I Z A Institute of Labor Economics
Initiated by Deutsche Post Foundation

DISCUSSION PAPER SERIES

IZA DP No. 12239

Maxed Out? The Effect of Larger Student Loan Limits on Borrowing and Education Outcomes

Jeffrey T. Denning
Todd R. Jones

MARCH 2019

I Z A Institute
of Labor Economics

Initiated by Deutsche Post Foundation

DISCUSSION PAPER SERIES

IZA DP No. 12239

Maxed Out? The Effect of Larger Student Loan Limits on Borrowing and Education Outcomes

Jeffrey T. Denning
Brigham Young University and IZA

Todd R. Jones
Georgia State University

MARCH 2019

ISSN: 2365-9793

IZA – Institute of Labor Economics

Schaumburg-Lippe-Straße 5–9
53113 Bonn, Germany

Phone: +49-228-3894-0
Email: publications@iza.org

www.iza.org

ABSTRACT

Maxed Out? The Effect of Larger Student Loan Limits on Borrowing and Education Outcomes*

Despite large and growing student loan balances, there is relatively little evidence on the effects of access to student loans on borrowing and educational outcomes. We examine the effect of access to credit by using policy variation in the maximum federal student loan amounts available to U.S. college students. In particular, first-, second-, and third-year students have access to different amounts of federal student loans. Using a regression discontinuity and administrative data from a state higher education system, we find that access to higher loan limits increases borrowing for at least 26 percent of borrowers. Despite this increase in borrowing, we find no evidence that eligibility for additional loans affects student GPA, persistence, or graduation.

JEL Classification: I22, D14
Keywords: student loans

Corresponding author:
Jeffrey T. Denning
Brigham Young University
Department of Economics
165 FOB
Provo, UT 84602
USA
E-mail: jeffdenning@byu.edu

* The authors would like to thank participants at the BYU Junior Faculty Seminar, Rich Patterson, Ross Rubenstein, and Jonathan Smith for useful comments. They would also like to thank the Utah System of Higher Education for providing the data and useful feedback. The views expressed in this paper do not reflect the official position of the Utah System of Higher Education or the state of Utah.

1. Introduction

Student loan balances are large and growing. Outstanding federal student loan balances in 2018 exceeded $1.5 trillion.[1] In 2014, there were 42 million borrowers with federal student debt. Moreover, student loans balances have grown by over 400% since 2000 (Looney and Yannelis, 2015). This level of student debt may be cause for concern for a number of reasons. A substantial number of borrowers are in default, which has serious consequences to the defaulter, including wage garnishment and damage to credit rating.[2] Default also represents a substantial cost to the federal government: in 2016, over $137 billion in student balances were in default (Mitchell, 2017).

Student loans can have human capital benefits if they lead students who would not otherwise attend college to do so. When policymakers increase access to student loans, they trade off potential benefits, including human capital accumulation, against potential costs, including increased default and distortions to economic decisions.[3] Beyond the direct effects of human capital accumulation and default, student loans affect homeownership (Goodman, Isen, and Yannelis, 2018; Mezza et al., 2018), earnings (Gervais and Ziebarth, 2016), and occupational choice (Rothstein and Rouse, 2011). In addition, schools capture a large amount of federal loans through tuition (Lucca et. al, 2018). Quantifying the human capital benefits of student loans is important given the large amount of student loans and the high costs of the program.

Relatively little is known about how access to student loans changes borrowing behavior and educational outcomes. This lack of evidence is largely due to U.S. students' near-universal access to student loans—most students attending college are able to borrow federal loans regardless of family need—limiting policy variation needed for identification. Knowing how credit take up changes in response to changes in loan eligibility informs household finance decisions and policy about the structure of student loan offers. Given that human capital

---

[1] Board of Governors of the Federal Reserve System (2018)

[2] The two-year cohort default rate (more than 270 days delinquent) in 2010 was approximately 10 percent (Looney and Yannelis, 2015).

[3] Another benefit may be the direct utility gain from additional consumption while students are in school (Denning, Marx, Turner 2018).

benefits are the motivation behind the large student loan program in the United States, there is comparatively little evidence on the benefits of student loans.

Using administrative data from all public universities in Utah, we consider how eligibility for higher student loan amounts affects borrowing and educational outcomes for enrolled college students. First-, second-, and third-year students have access to different annual federal loan limits. We use the cumulative credit cutoffs for classification to second- and third-year students in a regression discontinuity framework to study the effects of access to additional subsidized loans.[4] First, we document that having access to higher loan limits does increase borrowing. We then estimate that at least one quarter of borrowers in our sample change their student loan amounts in response to higher student loan limits; this suggests that credit constraints are important for this group of students. Finally, we consider the impact of higher federal loan limits on credits accumulated, grade point average (GPA), persistence, and graduation. We find that access to higher student loan amounts does not affect any of these educational outcomes.

Several studies have considered the human capital effects of student loans. These papers overcome the challenge of near-universal access in the U.S. by treating some students with information about loans, changing the loan offer (but not eligibility), making within-person across time comparisons, or focusing on another country. Marx and Turner (Forthcoming) randomize offers of student loans at a community college, but do not change student loan eligibility. In their setting, some students receive an offer of no student loans, while others receive an offer of their maximum student loan eligibility. Students who receive student loan offers increase borrowing, GPA, credit accumulation, and transfer to four-year institutions. Barr, Bird, and Castleman (2016) text students at a community college to prompt active choice about borrowing decisions. Students reduce student loan amounts and drop out of college a semester earlier as a result. Solis (2017) considers eligibility for student loans in Chile and finds that student loans increase enrollment and college attainment. Schmeiser et al. (Forthcoming) consider the effect of student loans at Montana State University and the University of Montana.

[4] Throughout, we use "cutoff" and "threshold" interchangeably.

They use a person fixed effect approach to quantify the effects of student loans. This approach assumes that students who opt to borrow student loans do not experience a contemporaneous shock that may affect student outcomes. They find that students who borrow have higher GPAs and attempt more credits.[5]

The current paper adds to this literature by measuring the academic benefits of higher loan limits in the United States, in particular at the public universities in the state of Utah.[6] Existing papers on the educational effects of student loans in the United States primarily focus on community colleges. We also consider another meaningful policy margin—higher student loan maximums, or the intensive margin of borrowing These maximums affect student loan balances for relatively high borrowers, which are an important group for dollars in default (Looney and Yannelis, 2018). Somewhat surprisingly, borrowers with low loan amounts are more likely to default (Looney and Yannelis, 2015). However, half of all dollars in default are from borrowers with balances over $25,000, making high-balance borrowers important for understanding default (Looney and Yannelis, 2018).

This study also offers insight into the presence and effects of credit constraints. Credit constraints are a classic explanation for underinvestment in human capital—people may wish to invest in additional schooling, but are unable to pay for it. Credit constraints may explain part of why many studies have shown that additional resources for college affect college enrollment and completion (Scott-Clayton, 2011; Castleman and Long, 2016; Denning, 2017; Bettinger et al., 2019; Barr, Forthcoming). A large literature discusses the extent of and consequences of credit constraints for college investment (Cameron and Heckman, 1998; Cameron and Taber, 2004; Keane and Wolpin, 2001; Belley and Lochner, 2007, Cowan, 2016; Stinebrickner and Stinebrickner, 2008; Lochner and Monge-Naranjo, 2011).[7]

---

[5] Another line of research has considered the effects of institutions disbursing student loans. Wiederspan (2016) examines the effects of community colleges opting out of the federal student loan program and finds that access to student loans increases credits attempted among poor students. Darolia (2013) shows that an institution losing eligibility to disburse federal financial aid (grants and loans) reduces enrollment.
[6] Schmeiser et al. (Forthcoming) is a notable exception.
[7] Lochner and Monge-Naranjo (2012) offers a nice review of this evidence.

Despite the frequent discussion of credit constraints and human capital investment, defining credit constraints is empirically difficult. Consider an experiment for measuring the prevalence of credit constraints where (prospective) students are randomly offered a college loan. If students take up the loan, they may do so because they are credit constrained—they wish to borrow more money for college, but cannot borrow from any other source. However, they may also take up the loan due to its interest rate. If the interest rate is sufficiently low, they may borrow in order to smooth consumption, substitute away from higher interest instruments such as credit cards, or finance college attendance; they may also borrow for a variety of other—including behavioral—reasons. Without a complete picture of a student's finances including credit balances, applications for credit, and interest rates for all potential lines of credit, it is impossible to distinguish between credit constraints and taking out additional loans due to lower interest rates.

As a result, testing for credit constraints often entails testing for *implications* of credit constraints. Stinebrickner and Stinebrickner (2008) survey students, asking if they would increase borrowing if the loan was offered at a "fair interest rate." Students who respond yes to this question are considered credit constrained. We advance this line of research by examining the revealed borrowing decisions of students who are actually offered access to more credit. We test whether students increase borrowing of federal loans when they have access to higher amounts.[8] While this is a necessary condition for credit constraints, it is not a sufficient condition.

Student loans are important for students making decisions about attending college. However, because students must pay tuition in every year they attend college, credit constraints may not initially bind for a student in the first year, but could bind in the second year or later (Stinebrickner and Stinebrickner, 2008). In this paper, we consider credit constraints for returning students. This is relevant because dropout is a pervasive feature of higher education.

[8] We note that we are unable to rule out that students with access to the lower loan amount substitute to private loans.

The six-year U.S. college completion rate is 67%, suggesting scope for credit constraints to affect student persistence and graduation (Shapiro et al., 2017).

The rest of the paper proceeds as follows. Section 2 discusses background and the policy environment. Section 3 describes the data and identification strategy. Section 4 presents results. Section 5 discusses the findings and concludes.

2. Background and Policy Environment

United States Federal Aid

The United States federal government makes loans available to students who file the Free Application for Federal Student Aid (FAFSA). This form contains many questions on student and parental income, assets, and demographics. These inputs are condensed into an Expected Family Contribution (EFC) that determines eligibility for federal need-based aid such as the Pell Grant. Students need to file a FAFSA once per year in order to have eligibility for aid throughout the school year.

Students who file the FAFSA may be eligible for the two primary types of federal direct loans—unsubsidized direct loans and subsidized direct loans. Subsidized loans are available to students who demonstrate financial need. This type of loan has lower interest rates, and interest does not begin accruing until after students leave college. A student's maximum subsidized loan is the minimum of 1) their Cost of Attendance[9] minus EFC and other financial aid, and 2) the statutory annual maximum for subsidized loans, detailed below. Unsubsidized student loans are available to all students who attend schools that participate in the federal student loan program. Unsubsidized loans have weakly worse interest rates than subsidized loans and begin accruing interest upon disbursement. In 2014-15, the U.S. disbursed $77.5 billion total in federal

[9] Cost of Attendance is set by schools and is defined at https://fafsa.ed.gov/help/costatt.htm as "the total amount it will cost you to go to college each year. The COA includes tuition and fees; on-campus room and board (or a housing and food allowance for off-campus students); and allowances for books, supplies, transportation, loan fees, and, if applicable, dependent care. It can also include other expenses like an allowance for the rental or purchase of a personal computer, costs related to a disability, or costs for eligible study-abroad programs."

subsidized and unsubsidized loans.[10] See the Appendix for information on other types of federal loan programs.

There are several relevant constraints on the amount students can borrow. This paper exploits statutory differences in maximum loans available to first-, second-, and third-year students. Over the period considered in this paper, the maximum amount of combined federal subsidized and unsubsidized loans for dependent students was $5,500 for first-year students, $6,500 for second-year students, and $7,500 for third-year students.[11] First-, second-, and third-year dependent students could borrow $3,500, $4,500, and $5,500 in subsidized loans respectively. Hence, moving from being a first- (second-) year student to a second- (third-) year student increases subsidized eligibility by $1,000 and total loan eligibility by $1,000. Students declared financially independent from their parents had higher maxima but saw the same marginal increase in loan eligibility: $9,500, $10,500, and $12,500 for first-, second-, and third-year students.

Universities and colleges have latitude in the exact definition of first-, second-, and third- year students. In Utah, the classifications correspond to 0-29, 30-59, and 60+ credits accumulated for freshman, sophomore, and junior students. These institutional features preview the identification strategy used in this paper: we compare students who are barely eligible for additional student loans to students who are barely ineligible.

Utah Higher Education

There are six public universities in Utah.[12] The most selective is classified as "More Selective" by the US News & World Report; other institutions are open access. Utah appears near the bottom in state rankings of in-state tuition. It is also notable because it is the state with the lowest levels of student debt in the nation.[13] Low student loan levels may raise questions

[10] Collegeboard (2016).

[11] There are also lifetime maxima for federal student loans. Dependent students may not borrow more than $23,000 in subsidized loans or $31,000 in unsubsidized loans.

[12] These are the University of Utah, Utah State University, Weber State University, Southern Utah University, Dixie State University, and Utah Valley University.

[13] The Institute for College Access & Success (2018).

about the external validity of our setting. However, loans may have larger effects on education outcomes in settings where take up is low (perhaps due to "loan aversion" or negative stigma of borrowing) because the students borrowing are likely to be more financially constrained—and thus have a higher marginal benefit of borrowing—than in settings where borrowing is high. Hence, our estimates of the effects on student outcomes may be an upper bound.

3. **Data and Identification Strategy**

The data come from the Utah System of Higher Education and were collected for administrative reasons. Because the financial aid data begin in 2011-12, our main results focus on the 2011-12 to 2017-18 school years.[14] The data contain information on student enrollment, student classification, demographics, credits completed, GPA, broad classes of financial aid, and graduation. As will be discussed below, our measure of federal aid is the total of all federal aid received (subsidized loans, unsubsidized loans, PLUS, Perkins, etc.); there are not separate amounts for subsidized loans and unsubsidized loans, or for any other breakdown.

Our identification strategy is to use a regression discontinuity design where completed credits is the running variable. The intuition is to compare students who are otherwise similar, but have access to different amounts of federal student loans due to being on either side of the credit cutoff. There are two cutoffs at which students have access to additional loans: 30 and 60 credits. In our analysis, we typically combine these cutoffs both in order to maximize power, and because students at either cutoff have the same $1,000 increase in loan eligibility. However, we also present results at each cutoff separately and find similar results. For student i, the estimating equation is:

$$Y_i = f(credits_i) + \alpha \cdot \mathbf{1}(credits_i > cutoff) + \boldsymbol{X_i \beta} + \varepsilon_i, \text{ with } |credits_i| \leq k,$$

where Y_i is an outcome such as college persistence; $credits_i$ is the number of credits a student has completed at the beginning of the fall semester, re-centered such that 30 or 60 credits is 0; $f(credits_i)$ is a flexible function of credits completed; $\mathbf{1}(credits_i > cutoff)$ is an indicator for a

[14] However, in supplemental analysis we consider years 2001-02 to 2017-18. Financial aid data is only available beginning in 2011-12, but enrollment and graduation data is available earlier.

student completing more than 30 or 60 credits; $\boldsymbol{X_i \beta}$ are covariates including student age, indicators for race and gender, and institution fixed effects; and ε_i is an idiosyncratic error term. We treat $f(credits_i)$ as linear and use local-linear ordinary least squares regression; we allow the slope to vary on either side of the cutoff. We estimate this equation in a bandwidth, k, which in practice is 6 credits. This bandwidth is approximately the median of the bandwidth for each outcome chosen by the procedure outlined in Imbens and Kalyanaraman (2012). Table 5 shows that the results are invariant to the use of smaller and larger bandwidths. Robust standard errors are used.

At the threshold, students have an increase in maximum subsidized loan eligibility, but there is no change in maximum unsubsidized loans availability. Hence, α captures both the effect of additional subsidized loan eligibility and the effect of total loan eligibility. We present evidence that students move on both margins—some students increase borrowing to more than would otherwise be available, and other students likely increase subsidized loan amounts while not borrowing any unsubsidized loans.

Sample selection

We focus on the beginning of the fall semester because this is when student classification for financial aid is easiest to determine. A student with 29 credits at the beginning of the fall semester is classified as a first-year student and, in this semester, has access to $500 less in loans than a student with 30 or more credits. However, if the student earns even one credit during the fall semester, they become second-year students in the spring semester, making them eligible for $500 more in loans than first-year students during the spring semester. Because schools can vary in whether students have to request that the higher loan eligibility be added to their loan offer, determining the amount of loans a student is offered is complicated. We consequently focus on the fall semester, when the difference in loan eligibility is clearest.

We present main results for the sample of students who take out any federal loan. The maximum student loan amount should not affect the probability of borrowing student loans unless students have a very high fixed cost of borrowing (Marx and Turner, 2015). Indeed, Table 1 shows that students do not change their probability of borrowing at the cutoff. The estimate on borrowing any federal loan is -.5 percentage points and is statistically insignificant. The top of the 95 percent confidence interval is .007, suggesting that students did not change their decision to borrow as a result of access to additional loans. As compared to all students, focusing on students who take out any federal loans generates a larger change in the probability of borrowing more student loans; we thus focus on this sample throughout the paper. However, the conclusions of the paper are unchanged if all students are included, as in Table 7 Panel B.

Diagnostics

In order for α to recover the causal effect of access to additional federal loan limits on student outcomes, two conditions must hold. The first is that students do not manipulate their credits to gain access to future loans. The second is that nothing else changes at the credits threshold that would affect student outcomes, including the type of student.

We test for manipulation in two ways. The first is by looking for manipulation of completed credits by examining the density of students in Figure 1 Panel A. There is a clear jump in the number of students with credits completed exactly equal to 30 or 60. This could occur for several reasons. First, because students wishing to graduate in four years must complete 15 credits a semester, one would expect there to be bunching at 30 and 60 credits completed.

Additionally, students are required to take 12 credits to be considered full-time for federal financial aid. Some students may take 13-14 credits, but students rarely take 10 or 11 credits. There are jumps at 12, 24, 36, 48, and 60. The jump at 60 appears to be more persistent; this may be because it is a multiple of both 12 and 15. As a result of these jumps, our preferred specification drops students with exactly 30 and 60 credits in a regression discontinuity donut estimator (Almond and Doyle, 2011). In the case of 30 credits, the density is smooth after removing 30, although there is somewhat of a jump at 60 credits. We present results including students with exactly 30 and 60 credits as a robustness exercise in Table 5 and our results are quantitatively very similar.

We show that there is bunching at 30 and 60 completed credits. However, we argue that this bunching is likely due to reasons unrelated to gaining access to loans. Students who do not take out any federal loans have no incentive to bunch at 30 or 60 credits for loan access. Hence, if bunching is observed at those credits for non-borrowers, it is likely due to factors unrelated to student loan access. In Figure 1 Panel B we show the density for borrowers and non-borrowers on the same figure, with a different vertical axis for each. Students who do not borrow have a similar jump at 30 credits. We observe a similar pattern at 60 credits in Figure 1 Panel C. The jump is larger for non-borrowers than for borrowers. We interpret the jump for non-borrowers as evidence that the bunching at 30 and 60 credits is likely unrelated to manipulating credits for access to additional student loans.

Our setting resembles those of Zimmerman (2014) and Ost, Pan, and Webber (2018), in which there is bunching in the running variable; in our setting, the bunching is in completed credits, while in their settings it is in GPA. In both of these cases—as in our case—observable student characteristics do not change at the threshold. Similarly, Zimmerman (2014) shows bunching among students who are not at risk of treatment—which is similar to our setting where there is bunching among non-borrowers. While the bunching in our setting is not ideal, we provide evidence that the identifying assumptions for regression discontinuity designs are likely to hold. Namely, that students are not sorting on the basis of completed credits to gain access to additional loans.

We must also assume that nothing else changes at the threshold that would affect student outcomes. One notable thing that does change is a student's classification, either from freshman to sophomore or from sophomore to junior. We show that other student aid amounts including federal grants, state grants, and institutional aid, do not change. However, the cutoffs for classification can affect other things such as registration priority. This change in registration priority would likely generate a positive bias in student outcomes. The University of Utah charges slightly more tuition to juniors versus sophomores (approximately $20-$40 more in total for students taking 12 credits). Omitting this one cutoff at this one school does not substantively change the conclusions of the paper. Overall, we conclude that there might be a slight upward bias on student outcomes at the cutoff. Given that we find no effect of access to student loans on education outcomes, this small upward bias appears to be insignificant for these outcomes.

As described previously, we condition the sample on students who are enrolled in the fall. However, it may be the case that students alter their enrollment in the fall in response to access to additional loans. There is not a perfect way to test this. One possibility is to use completed credits prior to the fall to see if there is a discontinuity in enrollment. However, students could manipulate their credits to gain access to additional loans. As seen in Figure 1 Panels B and C, the best evidence that enrollment seems to be unaffected is that the patterns in enrollment by completed credits are very similar among borrowers and non-borrowers, with the increase in density being remarkably similar among the two groups. If anything, it appears that non-borrowers have larger jumps in enrollment at the threshold. This visual evidence suggests that students do not appear to be enrolling in the fall at higher rates due to additional loan access. This non-enrollment result is not without precedent and is similar to Denning (Forthcoming).

While we find evidence that students are not manipulating their credits to gain access to student loans, students who are just above the threshold may be different than students just below the threshold. These differences could generate differential reactions to access to student loans. They may also produce differences in outcomes across the threshold that are unrelated to access to loans if, for example, the students above the threshold are positively selected, even if this selection occurs for reasons unrelated to access to loans. In Table 2, we test for continuity of available covariates of race, gender, and age.[15] These are continuous through the threshold. Further, we use these covariates are a predictor for graduation within four years to see if predicted graduation is smooth through the threshold. We find that predicted graduation does not change at the threshold.

Another concern is if students are systematically different on unobservable attributes across the threshold. Examples of this would be if there were some sort of other policy that

[15] A caveat here is that our dataset contains few covariates, so we are unable to test others.

affects students across the threshold (e.g. registration) or if students just above the threshold are more motivated than those just below. To address this issue, we run a regression discontinuity difference estimator. We compare the estimated discontinuities for borrowers versus non-borrowers by interacting our estimating equation with an indicator for borrowing any student loans. These results are presented in Table 7 Panel A. We find that, if anything, this reduces the point estimate for borrowers, though the differences are typically not statistically different. Another example is if students need to earn a minimum number of credits, e.g., 15, in order to maintain their merit scholarship. If this produces positive selection above the cutoff, this would bias our estimates upwards. Given that we find no effect of access to loans on educational outcomes, we do not view this as a concern for these outcomes.

4. Results

Effects on Borrowing

We first discuss the changes in borrowing resulting from changes in loan eligibility. Figure 2 Panel A shows that students who have 30 or 60 credits are much more likely to be classified by their institution as sophomores or juniors than students with 29 or 59 credits. The estimated change is 70.9 percent. Ideally, there would be a jump from 0 to 1 for this figure; the discrepancy likely has to do with reporting issues around student classification and completed credits.[16] This jump indicates that moving across the credit threshold increases the classification for the majority of students.

Figure 2 Panel B considers whether students (restricted to borrowers) borrow at the lower maximum amount (subsidized plus unsubsidized). In other words, if students near the 30 (60) credit threshold borrow $2,750 (3,250) in the fall.[17] As seen by the untreated mean of 25

---

[16] Measurement error could arise from the counting of transfer credits, AP courses, etc.

[17] Here, we note that our measure of federal borrowing is a combination of subsidized and unsubsidized loans, but also other types of federal aid, meaning that we will only observe students at the lower maximum if they are only borrowing subsidized and unsubsidized loans, or if, by chance, they are also borrowing other types and end up at the right amount. The annual maximum is $5,500 and $6,500, but we focus on borrowing in fall, which means the maximum is half the annual maximum. We also count someone as borrowing the maximum amount if their borrowing is within $50 of the maximum because universities often deduct small processing fees from the loan amounts.

percentage points, many students borrow at the lower maximum. There is a 12.1 percentage point reduction at the threshold in the probability of borrowing at the lower maximum (Table 3). Figure 2 Panel C shows an accompanying change of 8.8 percentage points in the probability of borrowing more than lower maximum amount. Both changes are clearly visible and highly statistically significant with t-statistics over 6. This translates into a change in borrowing of $131 for the fall semester. We additionally show in Table 3 that federal grant aid does not change at the threshold, nor do state aid or institutional aid.

A key question is how many students change their borrowing as a result of becoming a second- (third-) year student. Because many students take out only subsidized loans, the additional eligibility may induce such students to increase subsidized loan borrowing even though they do not borrow close the total maximum. Unfortunately, because the USHE data only contain a variable for all federal borrowing, we do not observe subsidized loan borrowing by itself. We therefore create a proxy for borrowing at the subsidized maximum by creating indicators for borrowing at the first- (second-) year subsidized maximum from the aggregated federal borrowing variable. These proxies capture all students who are borrowing the maximum subsidized loans and no unsubsidized loans, but also capture some students who are not borrowing the statutory maximum for subsidized loans (for example, if federal borrowing happens to equal the subsidized maximum, but consists of other sources, only part of which is subsidized). Table 3 shows how the proxy for maximum subsidized borrowing changes at the threshold: students are 9.9 percentage points less likely to borrow at the proxy for the subsidized maximum and 11.6 percentage points more likely to borrow at the higher maximum. This is consistent with many students borrowing the maximum subsidized loans available to them.

Given that 8.8 percent of students increase their borrowing above the total maximum and that 9.9 percent of students increase their borrowing of subsidized loans without borrowing the combined maximum,[18] we conclude that at least 18 percent of borrowers change their

---

[18] Here, we assume that all of the 9.9 percent of students who do not borrow at the proxy for the subsidized maximum increase borrowing. It is possible that some of them actually decrease borrowing

borrowing behavior.[19] Other students may change their behavior as a result of difference in loan eligibility. For instance, if students use heuristics in choosing their loan amounts (for example, $500 less than the maximum) then a changing total maximum could change their borrowing. In fact, our estimates imply that more than 18% of our sample changes borrowing. If we assume that all of the observed change in borrowing is driven by these 18 percent of students it would imply that each student increased borrowing by $698, which is larger than the maximum statutory change. [20] Hence, we know that more than 18 percent of students increased borrowing.[21]

Using similar logic, we know that if all borrowers who changed their behavior increased their borrowing by $500, 26 percent of our sample would have changed borrowing decisions.[22] Similarly, if the average change in loans among students who changed borrowing were instead $250, then over 52 percent of our sample would have changed their borrowing as a result of the change in loans available to them. Given our estimates we know that at least 26 percent of our sample changed borrowing in response to the higher maximums. This increase in borrowing is non-trivial. We view this as a notable result—many borrowers increase borrowing in response to higher loan eligibility.

Because we cannot perfectly observe eligibility for higher maxima,[23] our estimates of how much students change borrowing may be attenuated. To address this, we instrument for advancing to the next classification using the credit threshold. This reveals the effect on borrowing among students whose classification and eligibility changed. This estimate states

---

because this test represents only the change in borrowing *at* the maximum and does not distinguish the direction.

[19] 8.8+9.9=18.7, referred to 18 percent for simplicity.

[20] Recall that we are only looking at the fall semester, when students can borrow half of the $1,000 increase, or $500. $\frac{130.6}{.187} = 698.4$.

[21] This assumes that access to higher loan maxima did not decrease borrowing and that students who were previously not at the maximum of their borrowing eligibility did not increase borrowing by more than $500.

[22] Specifically, $\frac{130.6}{500} = .261$.

[23] See the first column of Table 3.

that changing classifications increases borrowing by $184 and implies that 37 percent[24] of borrowers increase borrowing in response to higher loan limits. Offering higher loan maximums substantively changes borrowing, increasing borrowing for at least 26-37 percent of borrowers. Our results are consistent with many borrowers being credit constrained.

We interpret the change in loan amounts in our paper to be similar in magnitude to existing estimates in two key papers studying the effect of borrowing decisions on education outcomes. Our estimated change of $131 is roughly half of the change observed in Marx and Turner (2018) ($282). Notably, our estimate is only for Fall semester rather than for an entire year. Hence, students are likely to experience a similar increase in average loan amounts for fall in our sample as compared to Marx and Turner (2018). Our estimate is larger than the change observed in Barr, Bird, and Castleman (2017). Hence, the loan increase is non-trivial, especially when compared with existing estimates in the literature.

Table 3 also includes several other financial aid outcomes including federal grants, state grants, and institutional aid. Federal grants do not depend on a student's classification, and as expected, we observe no significant change in federal grants. On the other hand, state and institutional aid could change if state or institutional support depends on student classification. However, the estimates of the change in institutional aid and state grants is small and statistically insignificant. Our results on other financial aid is consistent with the assumption that nothing else is changing at the student classification cutoff.

Effects on Student Outcomes

Figure 3 displays the change in student outcomes: GPA in the fall, enrolling in the spring; enrolling in the next school year; and graduating within four and six years of first enrollment. In no case is there a visual discontinuity. These null results are confirmed by the formal estimates in Table 4.

---

24 $\frac{184}{500} = .368$.

GPA may increase if students are able to work less and focus on studies more. We precisely estimate that student GPA (which we consider in the fall only) is unaffected by additional loans. The point estimate is .0004 GPA points. The top of the 95 percent confidence interval for GPA is .057, or a 2 percent increase relative to the untreated mean. Hence, we can rule out very small changes in student performance as a result of the changes in loan eligibility. Scaling our estimate by the change in loans, a $1,000 increase in loans would increase GPA by .003. This estimate implies that student GPA is not affected by access to additional loans.

Students may be able to attempt and pass more credits if they have access to additional loans—they may be able to focus more on schooling and less on work. The point estimate for completed credits is .03 credits, which represents a change of .3 percentage points. The top of the 95 percent confidence interval is .41 credits. A 1,000 increase in loans would increase credits attempted by .25 credits completed or 1/12 of a typical 3 credit class. As with GPA, increased access to student loans does not seem to affect credits earned.

Additional loans may allow students to enroll at higher rates in subsequent semesters by providing more resources to pay for college. We consider enrollment in the spring after additional loan eligibility and find a small decrease of .2 percentage points. The top of the 95 percent confidence interval is .016 or a 1.8 percent increase relative to the baseline. For enrollment in the next school year, the point estimate is -1.5 percentage points, and the top of the 95 percent confidence interval is .6 percentage points—a .7 percent increase relative to the untreated mean. The enrollment results are less precise than the academic results, but again indicate additional loans did not affect student outcomes

We also consider graduation outcomes, which are measured since a student's first time in college. We only observe two cohorts for up to six years, three cohorts for at least five, etc. and so we have limited power. However, in all cases we find no evidence of increases in graduation rates as a result of access to additional student loans. We lose power as we consider longer term outcomes, but the evidence is consistent—there is no measured effect on student outcomes.

Robustness

Our results are robust to a number of alternative specifications and samples. Table 5 Panel A shows that if we do not include covariates, the point estimates are very similar. If we include students within half a credit of 30 and 60 credits, our results do not change.[25] We similarly do not see different results as we vary the bandwidth to be three credits in Panel C or 9 credits in Panel D, with the exception of enrolling next year being marginally negative statistically significant in one case.

Table 6 shows that the results are similar at both the 60-credit hour cutoff and the 30-credit hour cutoff. The point estimates are quite similar but, as expected, the estimates are less precise than when both cutoffs are combined.

Table 7 expands the analysis sample to include students who did not take out any federal loans. Panel A runs a regression discontinuity difference estimator. The advantage of this estimator is that it differences out any common effect, such as registration priority, of moving from a first- (second-) year to second- (third-) year student. This specification interacts both the running variable (above and below the cutoff) and, separately, the indicator for the discontinuity with an indicator for borrowing any loans. We consistently find no effect for students who borrow, and that the point estimates for borrowers are smaller than for non-borrowers. This provides further evidence that increased loan amounts do not affect student outcomes. Non-borrowers see an increase in graduation within 4 years (and a marginally statistically signficant increase for Fall GPA and Completed Credits) that is not present for borrowers. However, any increase in graduation for non-borrowers is erased when considering graduation within six years.

We expand the sample for Table 7 Panel B to include all students, both borrowers and non-borrowers. The change in borrowers borrowing greater than the lower maximum and total federal loans is attenuated by the inclusion of many non-borrowers. However, most student outcomes are small and statistically insignificant. The one exception is graduation within four

[25] Credits do not always come in integer values in Utah.

years of entry. However, there is no effect for graduation within six years. Table 7 Panel B shows the increase in graduation within 4 years is driven by non-borrowers, which is inconsistent with additional loan access improving student outcomes.

5. **Discussion and Conclusion**

We show that access to additional student loans increases take up of student loans for at least one quarter of our estimating sample. However, over different policy discontinuities, different outcomes, and different specifications we find no effect of these loan maximums. We can rule out very small effects on GPA and credits completed. We also do not find evidence on future enrollment and graduation, but with less statistical precision.

Our work adds to the literature on the human capital benefits of student loans. Consistent with Barr, Bird, and Castleman (2017) and Schmeiser, Urban, and Stoddard (2018), we do not find large effects. This is in contrast to Solis (2017) and Marx and Turner (Forthcoming). The differences in results may be due to the use of a different sample (U.S. university students), a difference margin (changing the maximum amount), or some other factor.

Our results suggest that increasing the federal maximum student loan—at least in amounts similar to observed in this study—is unlikely to substantively change student human capital accumulation. Along with the finding in Lucca et. al (2018) that subsidized loan increases are passed through to tuition at a rate of around 60 cents, our results suggest that increasing *subsidized* loan amounts are unlikely to improve student academic outcomes. Overall, increasing maximum student loan amounts, at least by the amount considered in this paper, does not seem to generate any human capital benefits. Given the potential costs of higher student loan balances, our results suggest caution in changing the maximum amounts for federal student loans.

References

Almond, D., & Doyle, J. J. (2011). After Midnight: A Regression Discontinuity Design in Length of Postpartum Hospital Stays. *American Economic Journal: Economic Policy*, *3*(3), 1-34.

Barr, A. (Forthcoming). Fighting for Education: Veterans and Financial aid. *Journal of Labor Economics*

Barr, A., Bird, K., & Castleman, B. L. (2016). Prompting Active Choice among High-risk Borrowers: Evidence from a Student Loan Counseling Experiment. *Charlottesville, VA: EdPolicyWorks Working Paper*.

Belley, P., & Lochner, L. (2007). The changing role of family income and ability in determining educational achievement. *Journal of Human capital*, *1*(1), 37-89.

Bettinger, E., Gurantz, O., Kawano, L., & Sacerdote, B. (2016). The Long Run Impacts of Merit Aid: Evidence from California's Cal Grant. *American Economic Journal: Economic Policy*

Board of Governors of the Federal Reserve System (2018). Consumer Credit Outstanding (Levels). Accessed: https://www.federalreserve.gov/releases/g19/HIST/cc_hist_memo_levels.html

Brown, M., Karl Scholz, J., & Seshadri, A. (2011). A New Test of Borrowing Constraints for Education. *The Review of Economic Studies*, *79*(2), 511-538.

Cameron, S. V., & Heckman, J. J. (1998). Life cycle schooling and dynamic selection bias: Models and evidence for five cohorts of American males. *Journal of Political Economy*, *106*(2), 262-333.

Cameron, S. V., & Taber, C. (2004). Estimation of educational borrowing constraints using returns to schooling. *Journal of Political Economy*, *112*(1), 132-182.

Castleman, B. L., & Long, B. T. (2016). Looking Beyond Enrollment: The Causal Effect of Need-Based Grants on College Access, Persistence, and Graduation. *Journal of Labor Economics*, *34*(4), 1023-1073.

Cowan, B. W. (2016). Testing for Educational Credit Constraints using Heterogeneity in Individual Time Preferences. *Journal of Labor Economics*, *34*(2), 363-402.

CollegeBoard. (2016). "Trends in College Pricing 2016" *Trends in Higher Education Series.*

Darolia, R. (2013). Integrity Versus Access? The Effect of Federal Financial Aid Availability on Postsecondary Enrollment. *Journal of Public Economics*, *106*, 101-114.

Denning, J. T. (2017). College on the Cheap: Consequences of Community College Tuition Reductions. *American Economic Journal: Economic Policy*, *9*(2), 155-88.

Denning, J. T. (Forthcoming). Born Under a Lucky star: Financial Aid, College Completion, Labor Supply, and Credit Constraints. *Journal of Human Resources*.

Denning, J. T., Marx, B. M., & Turner, L. J. (2018). Welfare Effects of Changes in College Pricing.

Denning, J. T., Marx, B. M., & Turner, L. J. (Forthcoming). ProPelled: The Effects of Grants on Graduation, Earnings, and Welfare. *American Economic Journal: Applied Ecconomics.*

Keane, M. P., & Wolpin, K. I. (2001). The effect of parental transfers and borrowing constraints on educational attainment. *International Economic Review*, *42*(4), 1051-1103.

Gervais, M., & Ziebarth, N. L. (2016). Life after Debt: Post-Graduation Consequences of Federal Student Loans. *University of Iowa*.

Goodman, S., Isen, A., & Yannelis, C. (2018). *A Day Late and a Dollar Short: Liquidity and Household Formation among Student Borrowers* (No. 2018-025). Board of Governors of the Federal Reserve System (US).

Lochner, L. J., & Monge-Naranjo, A. (2011). The nature of credit constraints and human capital. *American economic review*, *101*(6), 2487-2529.

Lochner, L., & Monge-Naranjo, A. (2012). Credit Constraints in Education. *Annual Review of Economics*, 4(1), 225-256.

Looney, A., & Yannelis, C. (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.

Looney, A., & Yannelis, C. (2018). Borrowers with Large Balances: Rising Student Debt and Falling Repayment Rates.

Lovenheim, M. F. (2011). The effect of Liquid Housing Wealth on College Enrollment. *Journal of Labor Economics*, *29*(4), 741-771.

Lucca, D. O., Nadauld, T., & Shen, K. (2018). Credit Supply and the Rise in College Tuition: Evidence from the Expansion in Federal Student Aid Programs. *The Review of Financial Studies*.

Marx, B. M., & Turner, L. J. (2015). Borrowing Trouble? Student Loans, The Cost of Borrowing, and Implications for the Effectiveness of Need-based Grant Aid (No. w20850). National Bureau of Economic Research.

Marx, B. M., & Turner, L. J. (Forthcoming). Student Loan Nudges: Experimental Evidence on Borrowing and Educational Attainment. *American Economic Journal: Economic Policy.*

Mitchell, J. (2017, April 20). The U.S. Makes It Easy for Parents to Get College Loans—Repaying Them Is Another Story. Wall Street Journal.

Mezza, A., Ringo, D., Sherland, S., & Sommer, K. (2016). Student Loans and Homeownership.

Ost, B., Pan, W., & Webber, D. (2018). The Returns to College Persistence for Marginal Students: Regression Discontinuity Evidence from University Dismissal Policies. *Journal of Labor Economics*, *36*(3), 779-805.

Rothstein, J., & Rouse, C. E. (2011). Constrained after College: Student Loans and Early-career Occupational Choices. *Journal of Public Economics*, *95*(1-2), 149-163.

Schmeiser, M. D., Stoddard, C., & Urban, C. (Forthcoming). Selection of Student Loans and College Performance. *Journal of Consumer Affairs.*

Scott-Clayton, J. (2011). On Money and Motivation: A Quasi-experimental Analysis of Financial Incentives for College Achievement. *Journal of Human Resources*, *46*(3), 614-646.

Solis, A. (2017). Credit Access and College Enrollment. *Journal of Political Economy*, 125(2), 562-622.

Stinebrickner, R., & Stinebrickner, T. (2008). The Effect of Credit Constraints on the College Drop-out Decision: A Direct Approach Using a New Panel Study. *American Economic Review*, *98*(5), 2163-84.

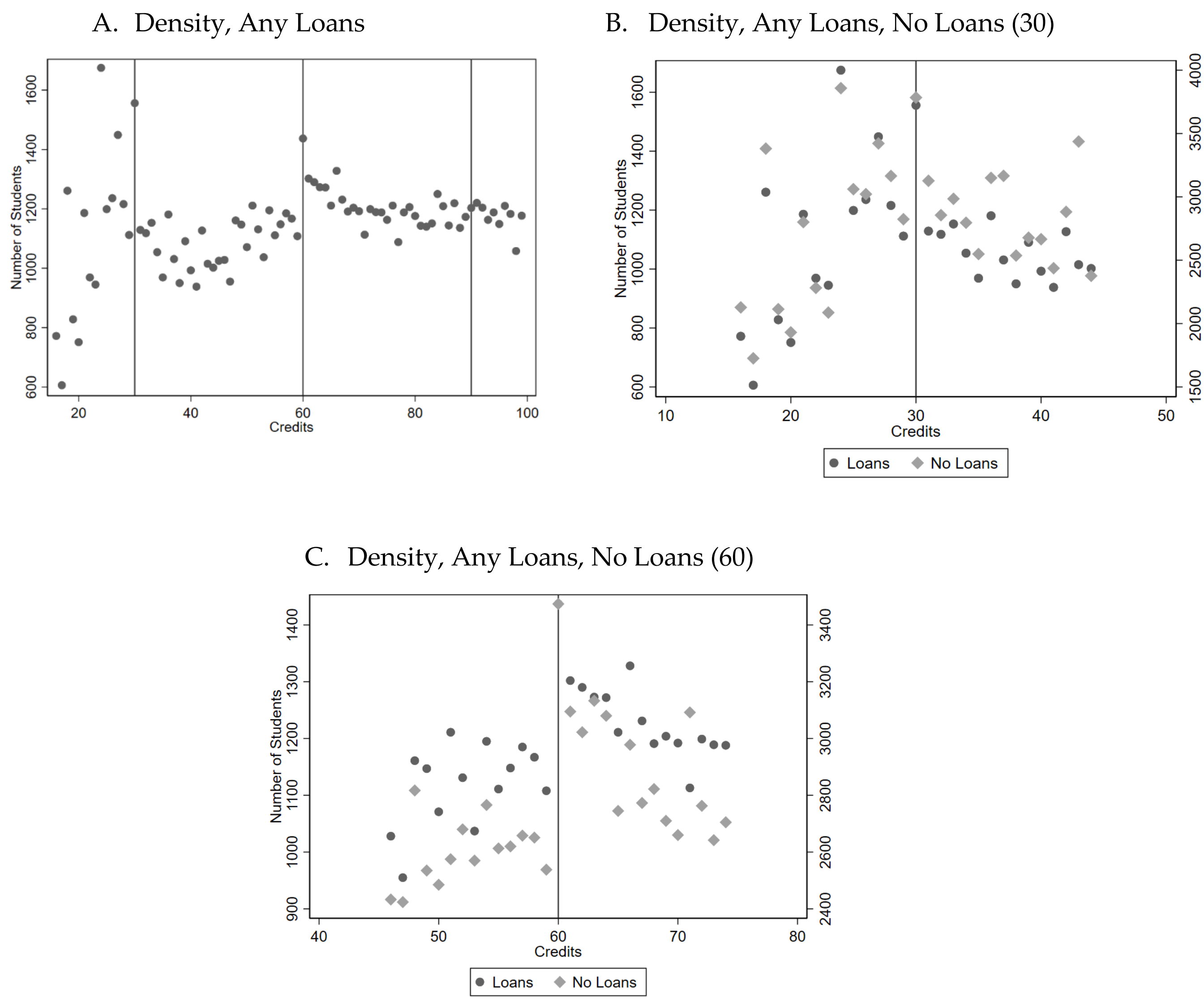
The Institute for College Access & Success. (2018). Student Debt and the Class of 2017. https://ticas.org/content/pub/student-debt-and-class-2017

Wiederspan, M. (2016). Denying Loan Access: The Student-level Consequences When Community Colleges Opt Out of the Stafford Loan Program. *Economics of Education Review*, *51*, 79-96.

Zimmerman, S. D. (2014). The Returns to College Admission for Academically Marginal Students. *Journal of Labor Economics*, *32*(4), 711-754.

Figures and Tables

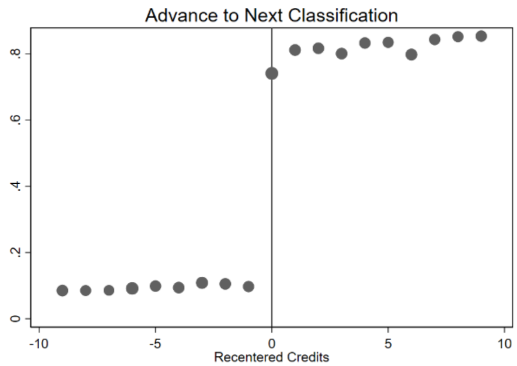
Figure 1: Density



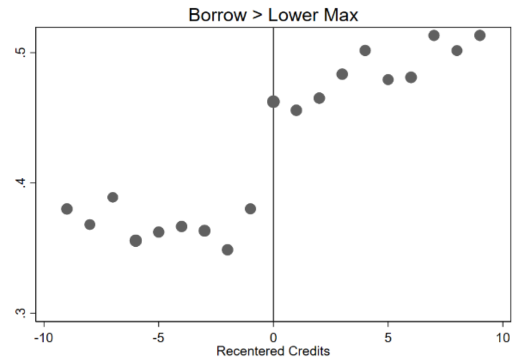
Notes: The data are from USHE administrative records and cover students enrolled from 2011-2017. Panel A displays the density of students by completed credits completed among students who took out any federal loans. Panels B, C, and D show both students who took out federal student loans and students who did not at 30, 60, and 90 credits respectively. Panels B, C, and D have different vertical axes for borrowers and non borrowers—the right axis is for non borrowers and the left axis is for borrowers.

Figure 2: First Stage and Borrowing

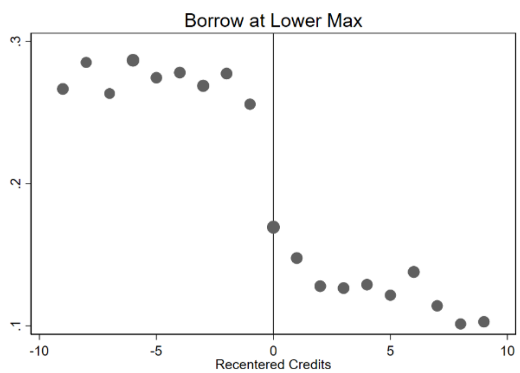
A. Advance to Next Classification



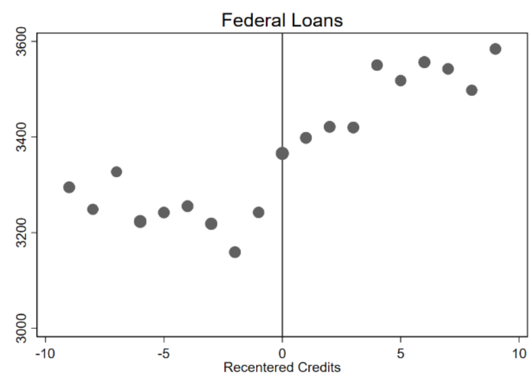
C. Borrow > Lower Max



B. Borrow at Lower Max



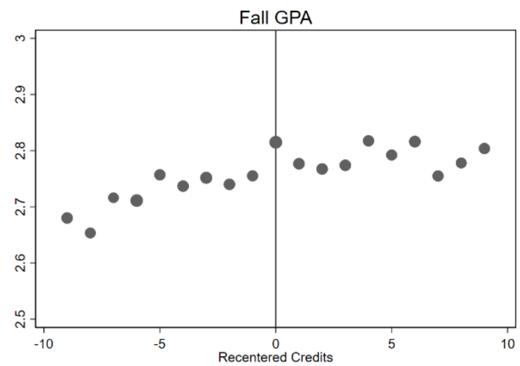
D. Total Loans



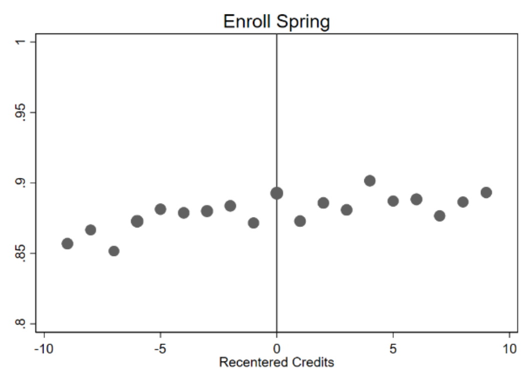
Notes: The data are from USHE administrative records and cover students enrolled from 2011-2017. These figures plot cell averages by re-centered cumulative completed credit hours. The size of the dot corresponds to the number of students in the cell. The sample is restricted to students who borrowed any federal student loans. Panel A plots the probability of being the higher student classification—for students near the 30 credit cutoff this is second-year, near the 60 credit cutoff this is third-year. Panel B plots the probability of borrowing at the federal max for the lower classification (e.g. for the 30 credit cutoff borrowing at the first year maximum). Panel C plots the probability of borrowing loans greater than the lower federal maximum. Panel D plots total federal loans in Fall.

Figure 3: Outcomes

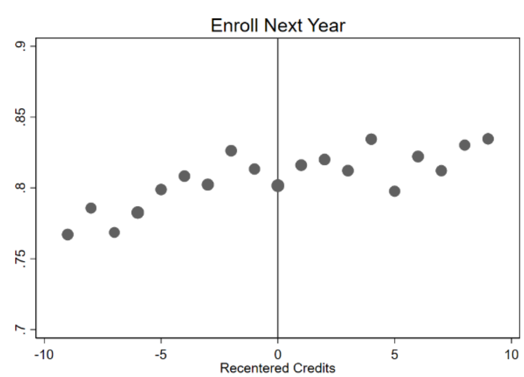
A. GPA



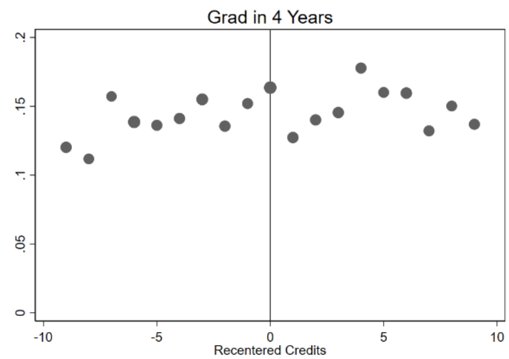
B. Enroll in Spring



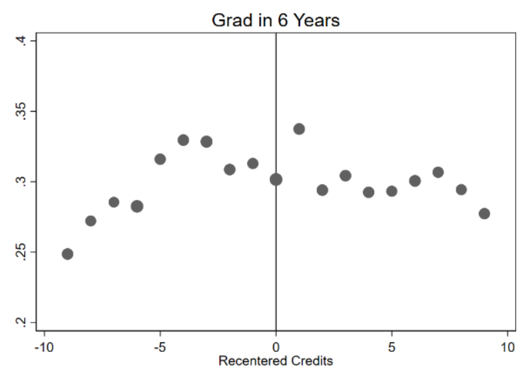
C. Enroll in 1 Year



D. Graduate in 4 Years



E. Graduate in 6 years



Notes: The data are from USHE administrative records and cover students enrolled from 2011-2017. These figures plot cell averages by re-centered cumulative completed credit hours and the 30 and 60 credit cutoffs are stacked. The sample is restricted to students who borrowed any federal student loans. The size of the dot corresponds to the number of students in the cell. Panel A plots the probability of enrolling in the spring. Panel B plots the probability of enrolling in the next school year. Panel C and D plot the probability of graduating within 4 and 6 years of entry respectively.

Table 1: Probability of taking out any federal loan

	Any Loan
>= 30/60 Credits	-0.0054
	(0.0065)
Mean | Untreated	0.292
N	99,020
Bandwidth	6
Controls	Yes

*Notes: The data are from USHE administrative records and cover students enrolled from 2011-2017. This table tests whether students are more likely to borrow any federal loans when they become eligible for larger loan maxima. The sample includes all students. * 0.10, ** 0.05, *** 0.01.*

Table 2: Covariate Balance

	Predicted Graduation	Male	White	Age
>= 30/60 credits	-0.001	-0.006	0.017	0.031
	(0.002)	(0.014)	(0.010)	(0.185)
Mean | Untreated	0.148	0.488	0.828	24.54
N	28,860	28,860	28,860	28,860
Bandwidth	6	6	6	6

*Notes: The data are from USHE administrative records and cover students enrolled from 2011-2017. This table tests whether student observable characteristics are smooth through the threshold. The sample includes only students who borrowed any federal loans. * 0.10, ** 0.05, *** 0.01.*

Table 3: First Stage and Borrowing Outcomes

	Advance	Borrow at Max	Borrow > Lower Max	Federal Loans	Borrow at Lower Sub. Max.	Borrow At Upper Sub Max
>=30/60 Credits	0.709***	-0.121***	0.0881***	130.6**	-0.099***	0.116***
	(0.00927)	(0.0104)	(0.0126)	(49.95)	(0.00838)	(0.00837)
Mean | Untreated	0.107	0.253	0.372	3,211.6	0.173	0.05
N	28,860	28,860	28,860	28,860	28,860	28,860
Bandwidth	6	6	6	6	6	6
Controls	Yes	Yes	Yes	Yes	Yes	Yes

	Fed. Grant Aid	State Aid	Institution Aid	Other Aid
>=30/60 Credits	21.77	8.8	18.3	12.1
	(35.47)	(13.3)	(38.3)	(11.2)
Mean | Untreated	1263.8	78.73	454.4	57.6
N	28,860	28,860	28,860	28,860
Bandwidth	6	6	6	6
Controls	Yes	Yes	Yes	Yes

*Notes: This table tests whether students who barely become eligible for additional loans see changes in their classification status and in their financial aid packages. All specifications control for the gender, race, and student age and have a bandwidth of six credits. Students within half a credit of 30 or 60 credits are excluded. The data are from USHE administrative records and cover students enrolled from 2011-2017. * 0.10, ** 0.05, *** 0.01.*

Table 4 Student Outcomes

	Fall GPA	Completed Credits	Enroll Spring	Enroll Next Year	Grad in 4 yr	Grad in 5 yr	Grad in 6 yr
>= 30/60 Credits	0.0004	0.033	-0.002	-0.015	-0.022	-0.008	0.009
	(0.029)	(0.194)	(0.009)	(0.011)	(0.012)	(0.017)	(0.022)
Mean | Untreated	2.76	10.13	0.879	0.829	0.15	0.27	0.326
N	28,743	28,822	28,860	24,185	17,722	13,660	9,043
Bandwidth	6	6	6	6	6	6	6
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes

*Notes: This table tests whether eligibility for additional loans affects educational outcomes. All specifications control for the gender, race, and institution fixed effects, and student age and have a bandwidth of six credits. Students within half a credit of 30 or 60 credits are excluded. The data are from USHE administrative records and cover students enrolled from 2011-2017. * 0.10, ** 0.05, *** 0.01.*

Table 5 Robustness

	Fall GPA	Completed Credits	Enroll Spring	Enroll Next Year	Grad in 4yr	Grad in 6yr
	A. Bandwidth=6, No Covariates					
Credits >=30/60	0.004	0.012	-0.002	-0.012	-0.022	0.001
	(0.029)	(0.209)	(0.009)	(0.012)	(0.012)	(0.022)
N	28,743	28,822	28,860	24,185	17,722	9,043
	B. Bandwidth=6, No Donut, Covariates					
Credits >=30/60	0.020	0.053	0.007	-0.018	-0.008	-0.006
	(0.024)	(0.161)	(0.008)	(0.010)	(0.010)	(0.019)
N	31,823	31,910	31,956	26,765	19,558	9,948
	C. Bandwidth=3, Covariates					
Credits >=30/60	0.041	0.332	0.00318	-0.0056	-0.0164	0.0618
	(0.048)	(0.310)	(0.015)	(0.019)	(0.020)	(0.036)
N	14,205	14,239	14,260	11,926	8,716	4,493
	D. Bandwidth=9, Covariates					
Credits >=30/60	0.000	-0.029	-0.005	-0.018*	-0.006	-0.011
	(0.023)	(0.153)	(0.007)	(0.009)	(0.009)	(0.017)
N	41,851	41,970	42,027	35,188	26,017	13,378

*Notes: This table shows robustness to excluding covariates (Panel A), including students within half a credit of exactly 30 or 60 credits (Panel B), or varying the bandwidth (Panel C and D). The data are from USHE administrative records and cover students enrolled from 2011-2017. * 0.10, ** 0.05, *** 0.01.*

Table 6 Cutoffs Analyzed Separately

A. Cutoff=60

	Fall GPA	Completed Credits	Enroll Spring	Enroll Next Year	Grad in 4 yr	Grad in 6 yr
Credits>=60	0.001	0.064	-0.017	-0.021	-0.019	0.036
	(0.040)	(0.318)	(0.012)	(0.016)	(0.017)	(0.029)
N	14,365	14,415	14,431	12,009	9,542	5,260
Bandwidth	6	6	6	6	6	6
Controls	Yes	Yes	Yes	Yes	Yes	Yes

B. Cutoff=30

	Fall GPA	Completed Credits	Enroll Spring	Enroll Next Year	Grad in 4 yr	Grad in 6 yr
Credits>=30	0.000	0.083	0.012	-0.006	-0.025	-0.027
	(0.041)	(0.218)	(0.013)	(0.017)	(0.017)	(0.033)
N	14,378	14,407	14,429	12,176	8,180	3,783
Bandwidth	6	6	6	6	6	6
Controls	Yes	Yes	Yes	Yes	Yes	Yes

N*otes: This table considers the 30 and 60 credit cutoff separately. Students within half a credit of 30 or 60 credits are excluded. The data are from USHE administrative records and cover students enrolled from 2011-2017. * 0.10, ** 0.05, *** 0.01.*

Table 7 RD Difference and All Students

	Fall GPA	Completed Credits	Enroll Spring	Enroll Next Year	Grad in 4 yr	Grad in 6 yr
A. RD Difference						
>=30/60 Credits * Any Loans	-0.057	-0.207	-0.016	-0.016	-0.043**	-0.002
	(0.036)	(0.230)	(0.014)	(0.014)	(0.014)	(0.025)
>=30/60 Credits	0.050*	0.25*	0.001	0.001	0.021**	0.009
	(0.021)	(0.123)	(0.008)	(0.008)	(0.008)	(0.013)
N	94,371	98,202	84,205	84,205	64,527	37,907
Bandwidth	6	6	6	6	6	6
Controls	Yes	Yes	Yes	Yes	Yes	Yes

B. All Students

	Advance	Borrow > Lower Max	Federal Loans	Fall GPA	Completed Credits	Enroll Spring	Enroll Next Year	Grad in 4 yr	Grad in 6 yr
>=30/60 Credits	0.681***	0.025***	27.3	0.032	0.057	-0.004	-0.0075	0.023***	0.014
	(0.00)	(0.00)	(26.19)	(0.02)	(0.09)	(0.00)	(0.01)	(0.01)	(0.01)
Mean | Untreated	0.123	0.109	938.0	2.68	9.76	0.857	0.406	0.2	0.453
N	176,959	99,020	99,020	94,371	174,971	176,959	162,144	108,339	81,706
Bandwidth	6	6	6	6	6	6	6	6	6
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

*Notes: This table uses students who did not borrow in estimation. Panel A uses all students in a regression discontinuity difference estimator. Panel B estimates on all available students and does not condition on borrowing. The data are from USHE administrative records and cover the maximum number of years available for each of the outcomes. * 0.10, ** 0.05, *** 0.01.*

Appendix

Information on Additional Federal Loan Programs

The United States also operates the PLUS loan program. PLUS loans are available to parents of dependent students. Application for the PLUS loan program checks that parents do not have an "adverse credit history." If approved, parents may borrow up to the Cost of Attendance less any other financial aid received. However, if parents are ineligible for the PLUS loan as a result of adverse credit, students may borrow up to the independent student annual direct loan maximum (subsidized plus unsubsidized loans). Parent PLUS loans totaled $10.7 billion in 2014-15 (Collegeboard, 2016).

There are several other student loan programs. The Perkins loan is a campus-based program that gives priority to students with exceptional financial need. The Perkins loan program is relatively small with disbursements in 2014-2015 of $1.16 billion. Students may also borrow private loans. These totaled $10.2 billion in 2014-15 (Collegeboard, 2016). Private student loans are available but generally offer higher interest rates and require that students or cosigners pass a credit check. Private loans are also not eligible for various forms of repayment such as Pay as You Earn Repayment, Income-Based Repayment, etc.