Student Loan Nudges: Experimental Evidence on Borrowing and Educational Attainment*

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Abstract

We experimentally test the impact of student loan "nudges" on community college students' borrowing decisions and subsequent educational attainment. We show that nonbinding loan offers listed in financial aid award letters do not affect students' choice sets yet significantly alter borrowing and attainment. Students randomly assigned to receive a nonzero loan offer were 40 percent more likely to borrow than those who received a $0 loan offer. On average, borrowing induced by the nudge increased both GPA and credits earned by roughly 30 percent. The nudge had no effect on students' enrollment decisions in the year of the intervention, but in the following year, students induced to borrow were 10 percentage points (nearly 200 percent) more likely to transfer to a four-year college. Taking into account these attainment gains and corresponding labor market returns estimated in prior studies, we predict that the average student would be better off receiving a nonzero loan offer for any discount rate from loan packaging below 12.4 percent. Given that over 5 million U.S. college students are offered $0 in loan aid, our results indicate the potential to achieve large gains in educational attainment through changes to the choice architecture around borrowing.

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1 Introduction

U.S. undergraduate enrollment grew has grown by more than 30 percent since 2000 and two-year institutions have absorbed the majority of new students. The plurality of students now attend open-access community colleges and face poor odds of success. In recent cohorts, just under 40 percent received any college credential within six years of entry.[1] At the same time, a growing literature shows that community college graduates receive substantial labor market returns.[2] While financial aid has been shown to help low-income students enter and complete college (Deming and Dynarski 2010), the design of federal student aid programs may hinder students' ability to take advantage of these resources. In particular, a growing literature suggests that student decisions are influenced by debt aversion (Field 2009; Caetano et al. 2011), cognitive costs associated with complexity (Dynarski and Scott-Clayton 2006; Bettinger et al. 2012), issues of self-control (Cadena and Keys 2013), and framing effects (Pallais 2015; Evans et al. 2016). These results suggest that students may be highly responsive to choice architecture – the design of the environment in which individuals make choices (Thaler et al. 2012).

Institutional decisions affecting the choice architecture around students' borrowing options may generate suboptimal choices. Colleges must make federal loans available to all qualifying students, but have discretion over whether a loan "offer" is included in students' financial aid award letters. Thus, loan offers may serve as nudges by altering the framing of borrowing options, even though students' choice sets are unaffected. Offers could affect borrowing decisions if they are perceived as providing information about eligibility, induce anchoring around the offered amount, or cause students to attend to their borrowing options.[3]

We study the effect of student loan nudges on borrowing and educational attainment with a field experiment at a large community college. Students were randomly assigned to receive a loan offer of either zero or a nonzero offer of $3500 (for students with "freshman" status) or $4500 (for "sophomores").[4] Students who received a nonzero loan offer were 40 percent more likely to borrow than those who received a $0 offer, with each additional borrower taking-up $4000 in loan aid, on average. Nonzero loan offers generated sizable gains in educational attainment. In the year of the experiment, while we find no evidence of statistically significant effects of loan offers on enrollment, nonzero loan offers significantly increased credit accumulation and students' grade point averages (GPAs). Using the random assignment of loan offers as an instrument,

[1] Among community college students in the fall 2010 entry cohort, 30 percent completed a certificate or associate degree and an additional 9.3 percent earned a credential from a four-year institution (Shapiro et al. 2016).

[2] See, for example, Jepsen et al. (2014), Bahr et al. (2015), Dadgar and Trimble (2015), Liu et al. (2015), Stevens et al. (2015), and Turner (2016).

[3] Both inattention and anchoring have been shown to influence consumers' decisions in other financial markets. For instance, Keys and Wang (2016) show that borrowers exhibit anchoring with respect to minimum credit card payments while Stango and Zinman (2014) provide evidence of limited attention with respect to bank overdraft fees. However, despite the growing importance of student loans in households' balance sheets, less research has focused on borrowing decisions relative to consumers decisions in other credit markets (Zinman 2015).

[4] Our experimental site classifies students as freshmen if they have accumulated less than 30 credits.

we find that students induced to take-up a loan earned 3.7 additional credits and 0.6 higher GPAs, both increases of roughly 30 percent. Using data from the National Student Clearinghouse, we find that students induced to borrow by the nudge are 10 percentage points (200 percent) more likely to transfer to a 4-year institution one year after the intervention.

Cost-benefit calculations based on these estimated short-run attainment gains indicate that nudging students to borrow is more cost-effective than the first-year effects of the most promising interventions at community colleges to-date (e.g., Scrivener et al. 2012; Mayer et al. 2015). We predict that students also benefit from receiving a nonzero offer, on average. Using existing estimates of the labor market gains from community college credit accumulation and from admission to a four-year institution, the attainment gains students experienced within two years are worth more on average than the present value of their additional debt for any discount rate below 12.4 percent.

Finally, we provide evidence of the mechanisms through which nonzero offers affect students' borrowing decisions. We find no evidence supporting anchoring as a driving factor behind our findings and instead focus on a model that allows for default bias, inattention, and costs associated with learning about loan eligibility. Among treated students, the distribution of loan amounts exhibits a spike at the exact amount of the offer, providing evidence of inattention to alternative amounts. Two additional patterns suggest nonzero loan offers provide information about eligibility. First, treatment increases the number of students borrowing amounts at all points in the distribution of loans and not just the offered amount. Second, compliers - students induced to borrow by the nudge - are more likely than always-takers to be new students and are less likely to have borrowed in the past. We estimate that information provided by nonzero loan offers accounts for at least 78 percent of the effect of treatment on loan take-up.

Outstanding student loan debt in the U.S. has grown steadily over the past decade, reaching $1.34 trillion in 2017 (Federal Reserve Bank of New York 2017). Despite the fact that community college students have greater unmet financial need and are less likely to borrow than students at private and more selective institutions, efforts to reduce borrowing have been especially pronounced within this sector.[5] Such policies range from completely opting out of federal loan programs to offering all students $0 in loan aid. Colleges may try to limit student loan debt out of concern over students' ability to repay their loans and a desire to avoid sanctions that the Department of Education can place on schools with high cohort default rates (CDRs).[6] Although sanctions can cause schools to lose access to federal student aid programs, these penalties

[5]According to the 2012 National Postsecondary Student Aid Study (NPSAS), 70 percent of community college students who applied for federal student aid faced a cost of attendance that exceeded their total resources (including grants, loans, work-study, and personal resources). Among four-year public and nonprofit undergraduate aid applicants, 58 and 60 percent had unmet need. Authors' calculations using PowerStats.

[6]A school's CDR equals the share of federal borrowers who default within three years of entering repayment. Schools with CDRs exceeding 30 percent for three consecutive years lose eligibility to provide students with federal Pell Grants and loans, while schools with CDRs exceeding 40 percent in any single year lose access to federal loans. Schools can appeal such sanctions

are rarely applied to community colleges.[7]

There is limited evidence on the extent to which loan aid affects outcomes in and beyond college. Two observational studies estimate the impact of *access* to federal loan aid using variation in community colleges' decisions to participate in federal loan programs (Dunlop 2013; Wiederspan 2016).[8] These studies rely on the identifying assumption that colleges' decisions of when and whether to participate in the federal student loan program are random, whereas we implement random assignment of non-binding loan offers within a college. Nonetheless, consistent with our findings, results from these studies suggest that student loans can have a positive effect on the educational attainment of students whose borrowing decisions are affected by institutional decisions that inhibit access to federal loans.

A handful of four-year institutions have also recently implemented interventions designed to reduce borrowing. The Indiana University system rolled out a number of concurrent programs aimed at reducing debt and increasing four-year graduation rates in 2013, and within two years, aggregate borrowing fell by 16 percent (Kennedy 2015).[9] Schmeiser et al. (2017) study a similar intervention at Montana State University that targeted students with high debt using a difference-in-differences design, and find that targeted students' borrowing decreased by only 2 percent.[10] Less costly interventions designed to help inform students' borrowing decisions have produced mixed results. Loan applicants at the Community College of Baltimore who were randomly assigned to a text messaging campaign combined with assistance from financial aid counselors experienced small reductions in both the amount borrowed and short-run attainment (Barr et al. 2017).[11] Experimental evidence from the U.S. and the Netherlands suggest that information alone does not significantly alter students' borrowing decisions, even when it increases students' understanding of loan terms and programs (Booij et al. 2012; Darolia and Harper forthcoming). Our findings suggest that the point in time at which students make borrowing decisions is an especially important one as even small nudges can have effects on borrowing that are as large as, if not larger than, initiatives that are more expensive and

for a variety of reasons, including serving a large number of low-income students or having a low number of borrowers in a given cohort. Prior to 2012, CDRs were measured on a two year basis with lower sanction thresholds. See the Federal Student Aid Cohort Default Rate Guide for additional details.

[7]Only three community colleges received CDR-related sanctions between 2002 and 2015. All three avoided federal aid loss through successful appeals. In September 2017, one additional community college was sanctioned and will likely appeal (see https://www.ed.gov/news/press-releases/us-department-education-releases-national-student-loan-fy-2014-cohort-default-rate).

[8]Dunlop (2013) finds that students in participating colleges are more likely to transfer to a four-year school, with some subgroups also being more likely to obtain bachelor's degrees. Wiederspan (2016) estimates that college-wide participation in federal loan programs leads Pell Grant-eligible students to attempt an additional 19 credits in their first year.

[9]These initiatives included the establishment of an Office of Financial Literacy and financial education program, one-on-one financial counseling, online financial training for new students, and annual student loan debt letters to all student borrowers with information on cumulative debt, estimated monthly repayment, and remaining federal loan eligibility. Completion initiatives included a campaign that encouraged students to take 15 credits per semester and freezing tuition and fees for students on track to complete their degree in four years.

[10]Starting in fall 2012, Montana State University sent letters to students with high outstanding debt that included an incentivized invitation to participate in a one-on-one counseling session with a certified financial counselor.

[11]Students in the treatment group received text messages for one month. Texts included information about students' loan options and terms (including lifetime limits and future payments), encouragement to complete required forms, access to one-on-one counseling with financial aid staff (70 percent of students sent at least one question), reminders that it was possible to borrow less than the offered amount, and suggested reference loan amounts less than the federal maximum.

broader in scope.

Furthermore, across-the-board reductions in student loan debt is not necessarily the objective that institutions and policy makers should pursue. Estimated returns to college completion suggest that the investment in college is worth the cost of borrowing to finance it for the average young adult (Avery and Turner 2012). Many low-income students already avoid loans, even subsidized loans that do not accrue interest in college (Cadena and Keys 2013). In previous work, we show that students attending schools that make $0 loan offers behave as though they face a fixed cost of borrowing (Marx and Turner forthcoming). Initiatives designed to reduce borrowing may also lower attainment, as our experimental evidence indicates, or encourage students to use more costly sources of debt.

Finally, our study also contributes to a growing literature on the importance of choice architecture. Madrian and Shea (2001), Choi et al. (2006), Chetty et al. (2014), and Bernheim et al. (2015) show that default options matter for decisions related to investment, saving, and 401(k) participation. All students must opt into borrowing and thus, face a default of $0. Our study provides evidence that nudges can influence behavior even when the nudge does not change the default option. Nudges have been shown to affect financial choices across a variety of settings (e.g. Thaler and Benartzi 2004; Duflo et al. 2011; Allcott and Rogers 2014) and within the context of postsecondary education, information and framing affect both college-going and post-college outcomes. Such effects have been found for nudges including a trivial reduction in the price of college applications (Pallais 2015), providing information to high-achieving, low-income students (Hoxby and Turner 2015), sending text messages about obtaining financial aid and advancing in college (Castleman and Page 2015; Castleman and Page 2016), and sending unemployed workers information about financial aid and the return to college (Barr and Turner 2015). Distinguishing features of our study include involvement of a general population of community college students, measurement of the impacts of policy choices currently being made by many community colleges, and the triviality of the costs of making changes to loan offers on the parts of both colleges and students.

2 Federal Student Loans in the U.S.

Low-income college students in the U.S. are eligible for federal grants and loans. In order to access federal aid, prospective students must fill out the free application for federal student aid (FAFSA), which requires information on family income, assets, siblings, and other family members' college attendance. These inputs are fed through a complicated, nonlinear formula to determine a student's expected family contribution (EFC), the federal government's measure of ability to pay. Eligibility for federal need-based grants, subsidized loans, and campus-based aid will generally depend on EFC, either directly (as in the case of Pell Grant aid)

or when combined with additional information (as in the case of work-study funding).

All students who are enrolled at least part-time and have completed a FAFSA are eligible to borrow through federal loan programs. The largest source of federal loan aid for undergraduate students is the Direct Loan Program. The terms of federal loan aid depend on a student's course load, dependency status, class standing, and unmet need. While students must attempt at least 6 credits to be eligible to borrow, above this threshold, the terms of borrowing do not explicitly depend on a student's course load. A student's unmet need, equal to her total cost of attendance (tuition, fees, and a cost of living allowance) minus her expected family contribution (the federal government's measure of need) and total grant aid from all sources, determines her eligibility for subsidized loans, which do not accrue interest while in school. Students classified as freshmen are eligible for subsidized loans equal to the lesser of remaining need and $3500.[12] Community college students who are classified as sophomores are eligible for an additional $1000 in subsidized loans.[13] Dependent first-year students can borrow an additional $2000 in unsubsidized loans while independent students can borrow an additional $6000.[14]

Students who do not qualify for subsidized loans can still borrow unsubsidized loans up to the overall maximum (e.g., $5500 for freshmen dependent students and $9500 for freshmen independent students). Unsubsidized loans begin accruing interest immediately after disbursement, but interest rates for both subsidized and unsubsidized loans are fixed over the lifetime of repayment.[15] Dependent undergraduate students face a lifetime eligibility limit of $31,000 in federal loans, while the limit for independent undergraduate students is $57,500.

Although the federal rules described in the previous paragraph dictate the amounts of subsidized and unsubsidized loans for which a college student is eligible, colleges can decide how much loan aid to offer in financial aid award letters.[16] In all cases, not borrowing is the default: students who take no further action do not receive loans, regardless of the amount offered. Students who receive nonzero loan offers must still accept the offer and complete federal requirements (entrance counseling and a Master Promissory Note) in order to receive their desired aid. Students who do not receive a loan offer (or receive a $0 offer) can still request a loan, with the specific request process varying across institutions. Nearly all four-year institutions offer students the maximum amount of loan aid for which they are eligible. In contrast, community colleges

[12]Subsidized loan eligibility is also reduced when a student's remaining lifetime eligibility for subsidized loans ($23,000) is less than these amounts.

[13]Students enrolled in four-year institutions are classified as upper-level students are eligible for an additional $2000 in subsidized loans. Regardless of credit accumulation, community college students cannot be classified as upper-level.

[14]An undergraduate student is classified as independent if she will be over the age of 24 by the end of the calendar year, is married, has dependent children, was in foster care or a ward of the court since age 13, is an emancipated minor, is a homeless unaccompanied youth, is currently serving on active duty, or is a veteran.

[15]Subsidized and unsubsidized loans disbursed after July 1, 2015 had an interest rate of 4.29 percent. Both types of Direct Loans disbursed after July 1, 2016 had an interest rate of 3.76 percent.

[16]The Department of Education and college financial aid administrators call this process "packaging".

are divided in how much loan aid they offer to students.

We collected information on loan offer policies for all community colleges that participate in federal loan programs through a combination of web searches, emails, and phone calls between March 2014 and July 2015. In Table 1 we describe each type of school using summary data from the Integrated Postsecondary Education System's 2012-13 Student Financial Aid and Net Price files and the Department of Education's official 3-year cohort default rates.[17] A handful of community colleges offer students a nonzero subsidized loan with zero unsubsidized loans, while the vast majority are split between either offering students both subsidized and unsubsidized loans or offering them no loans. Community college students are roughly evenly split between schools that offer both subsidized and unsubsidized loans and schools that do not offer loans. All three categories of colleges have comparable populations in terms of Pell Grant receipt, suggesting that loan offers are not correlated with average student need. Schools that make $0 loan offers tend to have lower borrowing rates (16 versus 30 percent for schools offering subsidized and unsubsidized loans). Differences in federal loan take-up may have important financial consequences: nationwide, low-income community college students are more likely to use a credit card to pay for school and are more likely to work if they have unmet need and forgo subsidized loans.[18] Though the risk of federal sanctions for high student loan default rates may motivate college policies intended to reduce student borrowing, cohort default rates among schools that package both subsidized and unsubsidized loans are comparable to rates among schools that do not offer their students federal loans (18.6 versus 18.9, respectively).

3 The Experiment

The experiment was implemented at "Community College A" (CCA), an anonymous community college, during the 2015-16 academic year.[19] As shown in Panel A of Table 2, CCA's costs are comparable to the costs faced by community college students nationwide. For instance, in-district tuition and fees for the 2014-15 academic year were approximately $3100 versus $3249 nationwide. However, CCA has a significantly larger student body than the average community college, with a 12-month full-time equivalent enrollment (FTE) of approximately 18,800 compared to 4,300 across all community colleges. This difference is by design, in that one of the requirements for being recruited into the experimental sample was a sufficiently

[17] An earlier version of this table appears in the online appendix to Marx and Turner (forthcoming). The current version of the table includes the packaging practices of 792 community colleges, representing 92 percent of community colleges that participate in federal loan programs.

[18] Authors' calculations using data from the 2012 NPSAS (via Powerstats). We limit the sample to community college students with at least $1000 in unmet need (and thus would qualify for subsidized loans) and who are eligible for a federal Pell Grant. Students in this sample who forgo federal loans are 9 percentage points (33 percent) more likely to use a credit card to pay for college and are 4 percentage points (7 percent) more likely to work while in school than those who take-up federal loans.

[19] We ran a similar experiment at a second community college ("Community College B" or CCB). However, the experimental sample of students was much smaller than anticipated, and the resulting estimates were not sufficiently precise to rule out the possibilities of either large effects or no effect. Appendix B contains details on the CCB experiment.

high enrollment to obtain a useful sample size. Financial aid receipt is similar between CCA students and community college students nationwide. For instance, approximately 45 percent of CCA students received Pell Grant aid and 25 percent received federal loans in 2013-14, compared to 41 and 19 percent of students at the average community college.

Students at CCA have substantially lower completion rates and slightly worse labor market outcomes than students at the average community college. Only 5 percent of CCA students completed a credential within 150 percent of the expected time to degree (e.g., 3 years for an associate degree), compared to 21 percent of students at the average community college.[20] Median earnings among federal aid recipients who were no longer enrolled 10 years after entry are similar for CCA and community colleges nationwide (approximately $28,000 versus $30,253, respectively). Other earnings outcomes follow similar patterns, with CCA students experiencing slightly worse labor market outcomes than national averages. While borrowers from the experimental site had lower student loan balances when entering repayment (approximately $4200 versus $6500 nationwide), CCA borrowers experienced worse repayment outcomes.

CCA had considered changing its loan packaging procedures prior to the experiment. During the 2014-15 academic year, CCA offered loans to all students with less than $25,000 in outstanding federal loan debt. All prospective students who listed CCA on their FAFSA received information relating to their financial aid packages electronically via a web-based system. In addition to federal requirements, CCA required students to actively confirm that they wish to borrow and specify the amount of loan aid they would like via an electronic loan request form. CCA's loan eligibility criteria and application procedures were not altered for the experiment. CCA disburses all funds, including loans, 35 days after the start of the semester.[21]

3.1 Experiment design

The experiment entailed random assignment of loan offers to students. On a roughly daily basis starting in May 2015, the CCA financial aid office provided data for each batch of students for whom an award letter was to be generated the following day. Using these data, students were assigned to either the treatment group or the control group using randomization stratified by Expected Family Contribution (EFC) bins and all possible combinations of binary variables for new vs. returning, freshman vs. sophomore, dependent vs. independent, and with vs. without outstanding student loan debt.[22]

[20] Degree completion measures are only available for students who entered the college as first-time, full-time, degree seeking students. For community colleges, this group contains fewer than 40 percent of all students in an entry cohort on average. Within CCA, the share of entering students for whom degree completion outcomes are reported is even smaller (roughly 25 percent).

[21] If a student does not complete the federal and CCA borrowing requirements or does not enroll in at least 6 credit hours until after the scheduled disbursement date , their loan is disbursed within two days after these criteria are met.

[22] Break points for stratification by EFC were determined within combination of the binary variables so as to roughly equate the number of students per strata based on data from the two preceding years. A separate category was created for the considerable number of students with a zero EFC, and the break points always included the $5198 threshold for Pell Grant

Loan-eligible students assigned to the treatment group received a nonzero loan offer in their award letter, while loan-eligible students assigned to the control group received a $0 offer.[23] Figure 1 displays screen shots from CCA's web page showing the financial aid package, including examples of both treatment and control student offers at the bottom of the page. These offers were pure nudges: they did not affect students' eligibility for federal loans or the requirement that the student actively accept a nonzero loan (and complete federal requirements) to obtain a loan. Figure 2 displays a screen shot of the online form that all CCA students must fill out to obtain a loan.

The amount of the loan offered to treatment-group students depended on the student's class standing; in keeping with CCA's loan packaging practice in the prior year, treatment group freshmen received $3500 loan offers, while sophomores received $4500 offers. Students with unmet need exceeding these amounts were offered the full amount as subsidized loans, while those with lower unmet need received a combination of subsidized and unsubsidized loan offers in their award letters. Students in the control group were informed of their eligibility for federal loans and the process for requesting a loan via email.[24] CCA clearly displayed information on student loan eligibility on its website and all students that complete a FAFSA are sent information on their anticipated eligibility for Pell Grants and federal loans from the U.S. Department of Education.

3.2 Data and descriptive statistics

Our CCA experimental sample includes students who were randomly assigned before February 2, 2016. Table 3 displays the characteristics of this group by treatment group assignment. We test for differences in treatment and control group members' class standing, past enrollment at CCA, dependency status, amount of outstanding student loan debt, resources (EFC), Pell Grant aid, work study aid, other grant aid (i.e., federal non-Pell, state, and institutional grants), other resources (i.e., private and employer-provided aid),

eligibility in the 2015-16 academic year.

[23]Students who were not eligible for loan aid did not have loan aid mentioned in the award letter, regardless of their assignment to treatment or control groups. Students with no unmet need who were assigned to the control group also did not receive an award letter that mentioned federal loans. Unfortunately, we do not observe the specific amount of subsidized and unsubsidized loans offered to treatment group members, as we learned during the experiment that when a student accepts a loan, CCA's information systems change the amount in the "offer" field to the amount the student choose to accept. However, our measure of imputed subsidized loan eligibility is strongly predictive of actual eligibility for the subset of students that take-up both subsidized and unsubsidized loans (i.e., students for whom we can reliably measure actual subsidized loan eligibility) and we find no evidence of heterogeneous treatment effects for students with an without subsidized loan eligibility.

[24]The email contained general information on federal student aid programs and a link to the online loan request form. The paragraph relating to loan eligibility read as follows:

"Based on the information provided to us, you have not been offered a student loan at this time. If you plan to enroll at least half time (minimum of 6 credits hours) and have not yet reached the aggregate loan limit for undergraduate students, you may request loan funds by completing the Loan Request Form. If you have additional questions please contact the Student Financial Aid & Scholarships Office at (■■■) ■■■ – ■■■■... If you do choose to request a loan, the Student Financial Aid & Scholarships Office encourages you to borrow wisely as loan eligibility is limited and it is possible to lose all loan eligibility before finishing your program."

baseline cumulative credits, and baseline cumulative GPA (for returning students). The first column displays the control group mean and standard deviation (in parentheses) of each characteristic. The second column displays the difference between the treatment-group mean and control-group mean and the standard error of this difference (in parentheses). None of the differences in characteristics between treatment and control groups are statistically significant. While this is to be expected for the variables in the first five rows, which were used for stratification, the lack of any significant differences in the other rows provides additional evidence that randomization was successful.

CCA students who complete a FAFSA (and thus were eligible for random assignment) are quite similar to FAFSA-completing community college students nationwide. Sixty-five percent of CCA students are classified as freshmen and 59 percent are independent. Furthermore, the average CCA student has outstanding student loans worth about $4,200 and a GPA of 2.67. Using data from the publicly available 2012 National Postsecondary Student Aid Study (NPSAS), we estimate that 60 percent of community college students nationwide were classified as freshman, 58 percent were independent, average outstanding debt was $4,400, and the average GPA was 2.42 in 2012.[25] The mean values of $6,769 EFC and Pell Grants worth $3,438 are both about 50 percent higher than national averages, indicating that CCA students have a relatively high dispersion of resources, with more low-EFC students that receive Pell Grants and more high-EFC students that bring up the college average.

3.3 Empirical Framework

To examine the impact of nonzero loan offers on borrowing and attainment, we estimate ordinary least squares (OLS) and instrument variables (IV) models:

$$D_i = \beta T_i + \boldsymbol{\eta}\mathbf{X_i} + \nu_i \tag{1}$$

$$Y_i = \pi D_i + \boldsymbol{\phi}\mathbf{X_i} + \epsilon_i \tag{2}$$

In equation (1), D_i is a dummy variable equal to one if a nonzero loan was offered in the financial aid award letter of student i, T_i is a dummy indicating assignment to the treatment group, and $\mathbf{X_i}$ includes a vector of strata fixed effects and a linear term in student expected family contribution (EFC). To reduce residual variation we include controls for cumulative credits earned and GPA at baseline as well as the month of random assignment. OLS estimates of β will represent the extent to which CCA's loan offers were correlated with the randomly-assigned treatment status. Compliance with treatment was imperfect because students who were assigned to the treatment group were not offered a loan if their past borrowing exceeded $20,000 or

[25] Statistics generated by PowerStats (https://nces.ed.gov/datalab/).

if their financial aid package was completed after their enrollment decision and they had not enrolled in the six credits necessary to be eligible for a loan. Given such discrepancies between treatment status and offer status we include among our estimates the "intent-to-treat" (ITT) effect of loan offers, i.e. the reduced-form OLS estimates of the impact of treatment group assignment on these outcomes.

We estimate the "treatment-on-the-treated" (TOT) impact of receiving a nonzero loan offer with instrumental variables (IV) models in which we instrument for receipt of a nonzero loan offer with assignment to the treatment group. In this case, equation (2) represents the second stage. Estimates of the coefficient π will represent the TOT effect of a nonzero loan offer on the borrowing or attainment outcome Y_i. Even if assignment to nonzero loan offers among students in the treatment group was not random, the use of the treatment assignment dummy T_i as an instrument isolates variation in offers that was randomized. To test for heterogeneous treatment effects, we jointly estimate IV models for each subgroup to allow for cross equation correlation in error terms. In all analyses, standard errors are clustered by strata.

We also use assignment to the treatment group as an instrument for loan take-up. This is because increases loan aid, rather than the loan offer itself, is the most likely driver of educational attainment responses. To do this we replace the binary variable D_i in equations (1) and (2) with a binary measure of borrowing. For this approach to produce causal estimates of the local average treatment effect of loan take-up, it must be the case that there are no students who would have borrowed had they received a zero offer and do not borrow when they receive a nonzero offer.

3.4 Adjustments for multiple hypothesis testing

The outcomes we examine fall into two categories: borrowing and educational attainment. In the first category, we consider two main measures - the probability of borrowing and the amount borrowed - which are highly correlated. In the second category, we observe several measures of educational attainment, including the number of credits attempted, credits earned, GPA, and credential receipt. Testing for effects on multiple outcomes increases the likelihood of finding at least one estimate to be statistically significantly different from zero when standard errors do not account for the fact that many hypotheses are being tested.

We address concerns over the multiple hypothesis testing in two ways. First, we generate a standardized index of treatment effects following Finkelstein et al. (2012) and the online appendix of Kling et al. (2007). This index represents the weighted average of the estimated treatment effect for each separate outcome, jointly estimated via seemingly unrelated regression, with weights equal to the standard deviation of the specific outcome in the control group. Standard errors are calculated using the delta method. Second, for each separate attainment outcome, we calculate familywise p-values using the Westfall and Young (1993)

free stepdown procedure.[26] The significance of estimated effects on the standardized treatment index will provide evidence of whether the family of null hypotheses relating to individual attainment outcomes can be rejected, whereas the familywise p-values will allow us to determine which, if any, attainment outcomes contribute the most to the significance of treatment effects on the index.

4 Effects of Nonzero Loan Offers on Borrowing

Figures 3 through 6 provide a visual preview of our estimated impacts on borrowing outcomes. In Figure 3, each bar represents the the probability of borrowing for students who were assigned to receive a nonzero loan offer ("treatment group" members) and those assigned to receive a $0 offer ("control group" members). Vertical capped lines represent 95 percent confidence intervals. Students in the treatment group were 7 percentage points more likely to borrow, a 30 percent increase relative to the control group borrowing rate of 23 percent. Figure 4 displays the probability that treatment and control group members borrowed the exact amount that was included or would have been included in the award letter if the student were assigned to the treatment group ($P = $3500 for students with "freshman" status and $4500 for those with "sophomore" status). A significantly greater percentage of treatment group borrowers took up the exact amount offered compared to control group members (11 versus 7 percent, respectively).

Next, we compare how the amount borrowed by treatment group assignment (Figure 5), looking across all students (Panel A) and students who borrowed (Panel B). Treatment group members borrow approximately $280 more than control group members, a 26 percent increase from the control group mean. However, once we condition on loan take-up, control group borrowers take on higher debt than students assigned to the treatment group (an approximately 5 percent increase from the treatment group mean amount of $4551). The reduction in the amount borrowed by treatment group borrowers is consistent with the mechanism described in Marx and Turner (forthcoming), whereby deviating from the offered amount generates a fixed cost. Control group students will only pay the fixed cost of deviating if their desired amount is sufficiently greater than their offer of $0, while treatment group students whose desired amount is greater than $0 may be induced to accept these lower amounts.

[26] This procedure involves four steps, described below.

Step 1: For each attainment outcome $k = 1, ..., 4$, we calculate the p-value p^k from the test of the hypothesis $\beta^k = 0$ from equation (1); we order the labeling of these outcomes such that p^1 represents the smallest p-value and p^4 is the largest p-value. Step 2: We draw $N = 10,000$ random samples of observations with replacement (drawing proportionately from random assignment strata), with treatment status assigned randomly so as to impose the null. For each sample i we calculate p_i^k, the p-value from the test of the null for outcome k. We then compute the adjusted sample p-value $q_k^i = \min\left\{p_k^i, ..., p_4^i\right\}$. Step 3: For each outcome k, we calculate the share of random samples for which the p-value generated from the original data exceeds the adjusted sample p-value: $\bar{p}^k = \frac{1}{N}\sum_{i=1}^{N} 1\left\{q_k^i \leq p^k\right\}$. Step 4: The final familywise p-value for each outcome k is $\tilde{p}^k = \max\left\{\bar{p}^1, ..., \bar{p}^k\right\}$.

We further explore how borrowing decisions are influenced by loan offers by comparing the distributions of loans taken up by borrowers in the treatment and control groups. We recenter the actual amount borrowed around $P, defined as the amount students would have been offered had they been assigned to the treatment group ($3500 for freshmen, $4500 for sophomores). Panel A of Figure 6 displays the distributions of amounts borrowed by students in the treatment and control groups, represented by light blue and dark blue bars, respectively. Assignment to the treatment group increases the likelihood of borrowing at almost every point in the distribution. However, students who received a nonzero offer are substantially more likely to borrow exactly the amount they were offered, suggesting that the offered amount serves as an anchor or reference point for at least some portion of students.[27] This finding is confirmed in Panel B. Dark bars represent the unconditional share of students in the control group who borrowed amounts within $500 bins centered around $P, solid circles represent the control mean plus estimated treatment effect of nonzero loan receipt, and the vertical capped line indicates the corresponding 95 percent confidence interval. While nonzero offers significant increase the probability of borrowing amounts both above and below $P by 0.4 to 0.9 percentage points, the estimated 2 percentage point increase in the probability of borrowing exactly $P is substantially larger in magnitude, representing an approximately 115 percent increase relative to the control group mean.

4.1 Nonzero loan offers increase borrowing

We formally estimate the impact of treatment assignment and nonzero loan offers on borrowing outcomes using OLS and IV models. The first column of Panel B in Table 4 displays "first-stage" estimates of the effect of assignment to the treatment group on the probability of receiving a nonzero loan offer. Treatment group assignment increased the probability of being offered a nonzero loan by 81 percentage points. The fact that most students assigned to the treatment group were in fact treated with a nonzero loan offer allows for precise estimates of impacts on borrowing outcomes.

Given the imperfect compliance with treatment assignment, we use IV models to generate TOT estimates of the effect of loan offers on students' borrowing decisions, as described in Section 3.3. As shown in Panel C of Table 4, receipt of a nonzero loan offer resulted in a 9 percentage point increase in the probability of borrowing. This response represents a 39 percent increase in borrowing relative to control students' mean borrowing rate of 23 percent. Furthermore, a nonzero loan offer increased the average amount borrowed (including zeroes) by $348 (a 32 percent increase relative to the control group mean). Both estimates are statistically significant at the 1 percent level.

[27] We observe heaping at many $500 intervals in both the treatment and control groups. Even in the control group, such heaping is especially pronounced at $P because this amount corresponds to the maximum subsidized loan for students with unmet need of at least $P. However, when we limit our sample to students whose subsidized loan eligibility falls below $P, we only observe excess bunching at $P in the treatment group (Appendix Figure A.1).

We also examine outcomes among students who borrowed, including the amount of loan aid taken up and the likelihood of borrowing exactly \$P, the amount offered to the treatment group (i.e., \$3500 for freshmen and \$4500 for sophomores).[28] Nonzero loan offers resulted in a marginally significant ($p < 0.1$) \$146 decline in conditional loan aid take-up among borrowers (a 3 percent decline relative to mean loan aid received by control group borrowers). Furthermore, students who received a nonzero loan offer were significantly more likely to borrow exactly the default amount offered to the treatment group (4 percentage points or 53 percent).

4.2 Heterogeneity

We test whether receipt of a nonzero loan offer had heterogeneous impacts across different student subgroups. Most community colleges serve a diverse student body that includes both traditional and nontraditional students from a variety of family backgrounds. Given the one-size-fits-all approach to loan packaging taken by most community colleges, it is important to understand whether the effects on CCA students are generalizable to schools with different student bodies. We focus on subgroups defined by past experience of borrowing (any outstanding debt versus no outstanding debt), student resources (Pell Grant eligible versus ineligible), prior CCA enrollment CCA (new versus returning), class standing (freshman versus sophomore status), and dependency status. To do so, we jointly estimate estimate IV models for each separate subgroup. Table 5 contains these results.

Across all subgroups, a nonzero loan offer resulted in significant increases in the probability of borrowing and the unconditional amount borrowed. We can reject the hypothesis of equal impacts of nonzero loan offers on borrowing and the amount borrowed across all subgroups ($p < 0.001$). The largest differences between subgroups arise when splitting the sample according to whether a student had borrowed in the past. Loan offers increased borrowing by 12 percentage points and \$539 among students with outstanding loan debt compared to only 6 percentage points and \$185 among students with no outstanding debt. If students who have borrowed in the past have more information about their federal loan eligibility, then the relatively large effects on these students suggest that the effect of loan offers is not simply due to incorrect beliefs about loan eligibility.

Loan offers also generate significantly different effects by dependency status and Pell Grant eligibility. Pell Grant eligible students' loan take-up is more responsive to nonzero loan offers than that of ineligible students ($p = 0.045$). The nudge also led independent students to borrow significantly greater amounts than students classified as dependents ($p = 0.003$), an effect that is likely driven by the higher borrowing limits

[28] Among borrowers, over 99 percent of students assigned to the treatment group received a nonzero loan offer. Thus, OLS estimates of the impact of treatment group assignment on conditional outcomes are essentially the same as IV estimates of the impact of nonzero loan offer receipt.

for independent students. We find no evidence of significant differential impacts of nonzero loan offers on conditional borrowing or borrowing exactly \$P (i.e., \$3500 for freshmen and \$4500 for sophomores) along any of these dimensions.

We do not find significant differences between explicit and implicit offers of \$0. Among colleges that do not offer loans, some send award letters that do not mention student loans while others show show "\$0" explicitly. CCA students in the control group with unmet need received award letters with an explicit \$0 offer, while those with no unmet need (who were still eligible for unsubsidized loans) received award letters that made no mention of loans. Regardless of treatment group assignment or unmet need, all students still had to opt into borrowing. As shown in Figure 8, which displays local linear regressions of the probability of borrowing for treatment and control group members by unmet need, this distinction made no difference in the effect of treatment on loan take-up for either prior borrowers (who presumably had some knowledge about federal loans) or students with no outstanding debt. Treatment group members who received a nonzero offer instead of a \$0 offer were no more likely to borrow than those that would have otherwise not received any mention of loans on their award letters.

5 Impacts on Attainment

We first test whether nonzero loan offers affected the likelihood that CCA applicants enrolled in fall 2015, the semester of the intervention. As shown in Figure 7, 72 percent of students assigned to the control group enrolled in fall courses compared to 71 percent of students in the treatment group. IV estimates of the impact of receiving a nonzero loan offer on enrollment produce precisely estimated null effects. For instance, the 95 percent confidence interval excludes effects larger than a 1 percent increase in enrollment and a 4 percent decrease in enrollment.[29] Because we do not observe loan take-up by applicants who do not enroll, and given that enrollment is balanced across treatment and control groups, we limit the sample of students used to estimate attainment effects to students who enrolled in at least one course. We further limit our sample to exclude students who received their financial aid packages after October 15, 2015 ($N = 1843$), which is the approximate drop/add deadline for the fall semester. This restriction is meant to focus our attention on students who could have adjusted their credit hours in response to the loan offer, although our results are robust to including these students.[30]

We observe attainment outcomes for the 2015-16 academic year including credits attempted, credits

[29] We test for heterogeneous effects of loan offers on enrollment within the 10 subgroups examined in Table 5. As shown in Appendix Table A.1, only one of the 10 point estimates is significant ($p < 0.05$) and we can reject the test of joint significance across subgroups ($p = 0.482$).

[30] Appendix Table A.2 shows that characteristics of the treatment and control groups are balanced in this restricted sample. In this sample the first stage coefficient is 0.835 with standard error of 0.033 and F statistic of 640.

earned, GPA, degree receipt, and the standardized treatment index constructed from all four of these variables. Control group means and standard deviations are displayed in Panel A of Table 6. We examine the effect of treatment group assignment and being offered a loan on these outcomes (Panels B and C, respectively) and then estimate the achievement gains experienced by students who were induced to increase borrowing by the nudge of a nonzero loan offer (Panel D).

As shown in Panel A, students assigned to the treatment group experienced significant increases in their attainment. The significance of these estimates is not due to the fact that we examine multiple measures of educational attainment; impacts on the standardized treatment index are significant at the 1 percent level. Familywise p-values, displayed in brackets below the point estimates and cluster-robust standard errors, show that impacts on credits earned and GPAs remain significant at the 5 and 10 percent levels, respectively, after accounting for the familywise error rate. However, estimated impacts on credits attempted are no longer significant at conventional levels, and effects on degree receipt remain small and insignificant.

Estimated impacts of nonzero loan offers on attainment outcomes are quite similar to ITT estimates of the effect of treatment assignment (Panel B). Since loan offers only affected a subset of students' borrowing decisions, it is not surprising that nonzero offers are associated with relatively small changes in educational attainment. Under the monotonicity assumption that no student who would have borrowed had they received a zero offer stops borrowing when receiving a nonzero offer, the IV estimates in Panel C will represent the local average treatment effect of loan take-up on educational attainment for students who were induced to borrow by the nudge (compliers). On average, compliers took up a loan of approximately $4000. Borrowing leads to a statistically significant ($p < 0.05$) 2.5 increase in credits attempted over the academic year. Impacts on credits earned are even larger; nudge-induced borrowing led to gains of 3.7 credits earned over the academic year ($p < 0.05$). Finally, borrowing also increased course performance. Students induced to take-up a loan earned significantly higher GPAs in each semester, with a cumulative increase of over 0.6 GPA points ($p < 0.01$). Borrowing did not increase the likelihood of degree receipt by the end of the academic year. This finding is not surprising given that most students in our sample were more than one year of full-time attendance away from completing their degree programs.[31]

As with the borrowing outcomes, we test for heterogeneous effects of borrowing on educational attainment over the 2015-16 academic year (Table 7). Column (1) contains the average loan amount for students in the specific subgroup who were induced to borrow by the nudge, as this amount varies across groups. When it comes to impacts of nudge-induced borrowing on academic outcomes, there is only one case in which the estimates for the subgroups on the two sides of each binary distinction are statistically distinguishable.

[31] Approximately 96 percent of CCA students in the experimental sample were pursing associate degrees that required 60 to 70 credits. Most had accumulated fewer than 30 at the start of the fall 2015 semester. Only a quarter of the sample started the fall semester with at least 40 credits.

Estimated impacts on the standardized treatment index significantly exceed zero for a number of subgroups, but as a whole, we cannot reject the hypothesis that treatment effects are jointly insignificant across all subgroups ($p = 0.273$).[32]

Using data from the National Student Clearinghouse (NSC), we observe a subset of these attainment outcomes in the year after the intervention (2016-17), including enrollment, degree receipt, and transfers. Within CCA, attainment effects remain positive, but we can no longer obtain unbiased estimates of effects on earned credits or grades because students select out of CCA based on assignment to the treatment group.

We focus on three attainment outcomes in year following the intervention: reenrollment in CCA, transfers to a four-year public institution, and degree receipt. As shown in Table 8, students induced to borrow by receipt of a nonzero loan offer in fall 2015 are 12 percentage points (23 percent relative to the control group mean) less likely to reenroll in CCA for the 2016-17 academic year. We find a similarly-sized positive effect of borrowing on transfers into bachelor's degree programs within four-year institutions. Given the relatively low rate of transfers from CCA into four-year public institutions, the statistically significant 11.4 percentage point increase in the probability of transfer represents a 178 percent increase relative to the control group mean. Borrowing induced by the nudge did not generate statistically significant increases in degree receipt for the average student. Although statistically insignificant, the 2.3 percentage point increase represents a 11 percent gain relative to the control group mean. As shown in the fourth column, impacts on the standardized treatment index are positive but insignificant.

Appendix Table A.4 contains additional IV estimates of the effect of 2015-16 borrowing on receipt of specific credentials. Certificates are short-term vocational credentials that require between one and three semesters of full-time enrollment. Associate of Arts (AA) and Associate of Science (AS) degrees are designed for students who wish to transfer their first two-years of liberal arts coursework to a four-year institution and Associate of Applied Science degrees are granted to students in terminal vocational programs. None of the estimated effects on receipt of specific degrees are significant at conventional levels but the pattern of results suggests that borrowing may have heterogeneous and potentially offsetting effects on receipt of academic versus vocational credentials. Nudge-induced borrowing leads to a 10 percentage point increase in receipt of an AA or AS degree, a 6 percentage point reduction in certificate receipt, and a 4 percentage point decrease in the probability of AAS degree receipt.[33]

[32] We also test for heterogeneity by degree program. Specifically, we compare students pursing an academic associate degree (e.g., Associate of Arts, Associate of Science; hereafter AA) that is designed for students who wish to transfer their first two-years of liberal arts coursework to a four-year institution to students pursing a terminal vocational associate degree (e.g., Associate of Applied Science, Associate of Applied Business, Associate of Technical Studies; hereafter AAS), excluding the small number of students in certificate programs. AA students induced to borrow by the nudge attempt and earn more credits, while both AA and AAS students experience GPA increases. However, only one of the differences in attainment effects (credits earned) is significant at the 10 percent level and effects on the standardized treatment index, while jointly significant, are not statistically distinguishable (Appendix Table A.3).

[33] We examine heterogeneity in impacts on selected 2016-17 outcomes by the type of degree being pursued at baseline, results

6 Cost-Benefit Analysis

To contextualize our findings, we compare costs and benefits from the perspectives of the government and the student. Loans appear beneficial from both perspectives based on observed attainment effects, potentially a lower bound for total attainment effects of induced borrowing.

We compare our estimated impacts to impacts of other RCTs targeting community college students' attainment, including the City University of New York (CUNY) Accelerated Study in Associate Programs (ASAP) and the Performance-Based Scholarship interventions. Both interventions involved student-level random assignment and were evaluated by MDRC. CUNY community college students assigned to the ASAP program were subject to a suite of requirements, additional supports, and financial assistance.[34] The long-run effects of the ASAP program included a doubling of the likelihood of graduation within three years of program entry (Scrivener et al. 2015), while early impacts included a significant increase of 2.1 credits earned per semester (Scrivener et al. 2012). These gains can be compared to an estimated annual cost of $3900 per student per year, suggesting an annual increase of 1.1 credits earned per $1000 (Scrivener et al. 2015). The Performance-Based Scholarship (PBS) Demonstration was implemented at several community colleges nationwide. Students were randomly assigned to be eligible to earn up to $1500 per semester in incentive payments if they met specific academic goals.[35] At the most successful PBS site, treatment group members earned significantly more credits than control group members, with first-year impacts of approximately 1 additional credit per $1000 of program expenditures (Barrow et al. 2014).

Our estimated effect of 3.7 additional credits due to a $4000 loan translates into approximately 0.9 credits earned per $1000 expenditure, which is comparable to the magnitude of estimated effects from the ASAP and PBS programs. However, in our setting, the additional $1000 is lent to the student rather than spent. Long-run costs to colleges and government may be substantially lower if the additional loan aid is repaid. If we assume that students induced to borrow by the experimental nudge will default on their loans at the same rate as other CCA borrowers, the federal government's expected cost per $4000 loan is $444. This suggests a cost-benefit ratio of 8.1 additional credits per $1000, far exceeding the short-run returns of ASAP and PBS.[36]

are displayed in Appendix Table A.5. The effect on transfers to four-year institutions is driven by students initially pursuing an AA or AS degree. For these students, transfers to four-year institutions increase by a statistically significant ($p < 0.05$) 27 percentage points relative to a control group mean of 11 percent.

[34]Specifically, students were required to enroll in at least 12 credits per semester (the threshold for full-time attendance), attend special seminars and engage in intensive advising. Students received a tuition waiver to cover unmet need, free use of textbooks, and subsidies for transportation expenses. Students in the program took block scheduled classes and had support to take winter and summer semester courses. See Scrivener et al. (2015) for additional details.

[35]The specific population eligible for participation and the structure and size of incentives varied across experimental sites. See Mayer et al. (2015) for additional details.

[36]CCA has a 23 percent three-year cohort default rate. We assume that all defaulters do so immediately and make no payments on their loan and that otherwise, borrowers enter into the standard 10-year repayment plan and face a 5 percent interest rate. The average interest earned on a $4000 unsubsidized loan over the repayment period would be $880 and thus, the expected value of interest received given the risk of default is $678. The average cost of default is $4880, while the expected

We also assess whether borrowing is financially beneficial for students by describing the financial trade-off implied by the observed effects on borrowing and educational attainment. Making such a comparison requires translating the attainment gains into financial terms. For the returns to credit completion within CCA, we use estimates from Jepsen et al. (2014), who use an individual fixed-effects approach to estimate the effect of community college credits and credentials on earnings and employment for two cohorts of students enrolling in the Kentucky Community & Technical College System.[37] For enrollment in a four-year public institution, we use estimates from Zimmerman (2014), who uses a regression discontinuity design to estimate effects of admission to a four-year public school that acts as a substitute for community colleges.

Implied earnings effects are substantial. Jepsen et al. (2014) estimate that whether or not a student earns a credential, each additional credit generates a $5.60 to $14 increase in quarterly earnings (in 2008 dollars). Applying the estimates of Jepsen et al. (2014) according to the gender mix of compliers within CCA, a student induced to take up a $4000 unsubsidized loan by the experimental nudge would see annual earnings increase by $169 in 2016 dollars. Zimmerman (2014) estimates that admission to a 4-year institution that acts as a substitute for community colleges increases annual earnings by 22 percent, or $1593 (in 2005 dollars). Based on these estimates, the 10 percentage-point increase in enrollment at 4-year institutions implies an annual earnings increase of $198 in 2016 dollars. Thus, we project the combined earnings effects of the nudge to be roughly $370 per year per student, on average.

If the earnings effects begin five years after loan receipt and grow at a nominal rate of 3 percent over a 30-year career, and if students repay loans at the interest rate of 4.29 percent that that applied to loans made in in 2015-16, then the loans are financially beneficial if future cash flows are discounted at any rate below 12.4 percent. Because roughly half of the earnings gains are due to credit accumulation, the ex-post break-even rate would be roughly half as large for students who experience average gains in credits completed but whose four-year enrollment is unaffected. Thus, the induced borrowing is almost certainly beneficial to students on average, and it appears likely that the majority of students benefit.

7 The Nature of the Nudge

To infer the welfare effects of the nudge, it is necessary to distinguish between potential channels through which offered loans affect behavior and whether the response to the nudge may defy standard models of rational choice (Bernheim and Rangel 2009; Handel 2013; Allcott and Kessler 2015; Allcott and Taubinsky 2015). We consider four broad models and provide evidence on their relevance using the observed distribution

cost of default per $4000 loan is $1122. Thus, the federal government's expected net cost of a $4000 loan is $444. Given that a $4000 loan buys 3.6 additional credits, we estimate that the experimental nudge produces 8.1 additional credits per $1000.

[37] An earlier study finds similar effects for an older population of displaced, high tenure workers (Jacobson et al. 2005).

of loan amounts among the treatment and control groups.[38]

Two models are fully consistent with rational choice. First, students may be uncertain of their eligibility for federal loans, and it may be costly to obtain this information. In the presence of such *information costs*, a nonzero offer reduces or eliminates the expected cost by providing a signal that increases the belief that loans are available. Second, students may consider the offered amount a recommendation, causing a student's belief about the optimal loan amount to update toward the offered amount, generating *anchoring* of loan amounts.[39]

Two behavioral models of potential relevance are *inattention* and *default bias* (Madrian and Shea 2001; Thaler and Sunstein 2003). Bernheim et al. (2015) "distinguish between the status quo, which determines the outcome if the worker fails to attend, and the default, which determines which outcomes require effort." In our setting, the decision-maker is the student, and if the student doesn't expend the effort to complete a loan application then she receives no loan. This implies a default loan amount of \$0. The offered amount has no effect on the default loan amount, and hence both the treatment and control group might exhibit default bias toward borrowing \$0. Thus, default bias would not explain the treatment-control difference in borrowing, and we do not attempt to estimate its role. Similarly, a status quo bias toward the amount borrowed in the previous year would not vary across treatment and control groups. However, if the student fails to attend to the options for borrowing then she may simply choose the offered amount. This inattention toward options other than the offered amount could induce the observed effects on borrowing.

We examine a model that allows for information costs, inattention, and default bias. In Appendix C, we examine anchoring as an alternative model and show that its empirical predictions are not consistent with our findings. Evidence against anchoring is also provided by a separate experiment at a college in which all students were offered a loan (Marx and Turner 2017). In this experiment, references to amounts borrowed by recent graduates affected loan take-up, but treated students were no more likely to borrow the suggested amounts than students that were not given an explicit reference point.

Consider a utility function $U(\ell|T)$, where ℓ is the chosen loan amount and T is an indicator for treatment with an offer of $\$P$. When $T = 0$ the offered amount is \$0. Let the utility function have the form

$$U(\ell|T) = -(\ell - \ell^*)^{\alpha} - Tc_a \mathbf{1}[\ell \neq P]\mathbf{1}[\ell \neq 0] - (c_d + (1-T)(c_i + c_a))\mathbf{1}[\ell \neq 0],$$

where $\ell^* \in \mathbb{R}$ is the latent desired loan amount, $\ell \geq 0$ is the amount borrowed, $c_a \geq 0$ is the cost of attending to options other than the offered amount, $c_d > 0$ is the cost of deviating from the default of zero, $c_i > 0$ is

[38] The analyses described in this section were not included in our pre-analysis plan.

[39] Alternatively, anchoring could arise if the offered amount generates an endowment effect at the reference point established by the offer.

the information cost of discovering eligibility for federal loans, and $\alpha \in \{2, 4, 6, ...\}$.[40] Such preferences can be obtained as the reduced form of a model in which latent borrowing demand is determined by the chosen amount of educational investment. Optimal loan amounts will take following form (without specifying a choice at points of indifference):

$$\ell = \begin{cases} 0 & T = 0,\ c_d + c_i + c_a > (\ell^*)^\alpha \\ \ell^* & T = 0,\ c_d + c_i + c_a < (\ell^*)^\alpha \\ 0 & T = 1,\ (\ell^*)^\alpha < (\ell^* - P)^\alpha + c_d \cap c_a + c_d > (\ell^*)^\alpha \\ P & T = 1,\ (\ell^*)^\alpha > (\ell^* - P)^\alpha + c_d \cap c_a > (\ell^* - P)^\alpha \\ \ell^* & T = 1,\ c_a < (\ell^* - P)^\alpha \cap c_a + c_d < (\ell^*)^\alpha \end{cases}$$

In this model, treatment with a loan offer of $\$P$ can increase the number of borrowers in two ways. First, inattentive students with $\ell^* \leq 0$ may be induced to borrow by going along with the offered amount in the award letter if c_a is sufficiently large. Second, students with $\ell^* > 0$ may not borrow when not treated, either by inattentively following the \$0 offer or because information costs are large enough to prevent them from borrowing. We assess two empirical predictions that offer tests for information costs and inattention, respectively.

Property 1: If $\Pr\left(\ell \in \left(0, \frac{P}{2}\right) | T = 0\right) < \Pr\left(\ell \in \left(0, \frac{P}{2}\right) | T = 1\right)$ *then there are students with* $\ell^* \in \left(0, \frac{P}{2}\right)$ with $c_i > 0$.

Proof: Regardless of treatment, $\ell \in \left(0, \frac{P}{2}\right)$ only if $\ell = \ell^* \in \left(0, \frac{P}{2}\right)$. If $T = 1$ then students with $\ell^* \in \left(0, \frac{P}{2}\right)$ will not choose $\ell = P$ because $U(P|T = 1) = -(P - \ell^*)^\alpha - c_d < -(0 - \ell^*)^\alpha = U(0|T = 1)$. For such students we can focus on the choice between $\ell = 0$ and $\ell = \ell^*$. $U(0|T = 0) = -(\ell^*)^\alpha = U(0|T = 1)$, i.e. the utility obtained from $\ell = 0$ does not depend on treatment status, but $U(\ell^*|T = 0) = -c_d - c_a - c_i$ and $U(\ell^*|T = 1) = -c_d - c_a$. Treatment raises the utility obtained from choosing $\ell = \ell^*$ and increases the probability that it is chosen only if some of these students have $c_i > 0$.□

Property 1 shows that we can obtain evidence on the existence of information costs by examining whether the treatment group exhibits more mass than the control group for $\ell \in \left(0, \frac{P}{2}\right)$. Experimental evidence is provided in Figure 9, which plots the distribution of loan amounts by class level. Among both freshmen and sophomores (for whom P differs), we find more treatment-group students than control-group students choosing loans in amounts between 0 and $\frac{P}{2}$. If we estimate IV models we find that treatment increases the likelihood of borrowing such amounts by 2.1 percentage points ($p < 0.001$) for all students. This evidence

[40] The negative quadratic form is frequently used to model single-peaked preferences, as in the example of the seminal work of Crawford and Sobel (1982).

implies that the treatment effects on borrowing cannot be driven entirely by inattention; information costs must be at play.

Two additional observations about this property are noteworthy. First, it should be noted that not all models predict a positive treatment effect on small amounts of loans. In Appendix Appendix C we show that the anchoring model predicts a negative effect on the probability of borrowing some amounts near \$P in our setting, contrary to what we observe (Figure 6, Panel B). Second, it should be noted that it is the symmetry around ℓ^* in our parametric model that makes $\ell = \frac{P}{2}$ the focal point of this prediction. More generally, the argument will hold for the smallest loan amounts as long as the loss from borrowing less than ℓ^* is not too much greater than the loss from borrowing more than ℓ^*, and this will be true for a wider range of ℓ values if c_d is large. We observe positive treatment effects throughout $\ell \in (0, P)$.

Property 2: Consider $\delta > 0$. If, for all students with $\ell^* \in (P - \delta, P + \delta)$, if $c_a = 0$, then when T=1, if $\ell^* \neq P$ then $\ell \neq P$.

Proof: From the solution above, $\ell = P$ when $(\ell^*)^\alpha + c_a > (\ell^* - P)^\alpha + c_d \cap c_a > (\ell^* - P)^\alpha$. If $c_a = 0$, then this requires $0 > (\ell^* - P)^\alpha$. The right-hand side of this expression is uniquely minimized to zero when $\ell^* = P$, and so for no other value of ℓ^* can it be that $\ell = P$.□

Property 2 shows that we can test for the existence of inattention by examining whether the treatment group exhibits excess mass at $\ell = P$. If students behave as if there is no cost to attending to options other than the offered amount then we should not see a spike in the distribution of loans at the offered amount. Figure 5 shows that we do see such a spike. Treatment leads to a 2 percentage-point increase in the probability of borrowing \$P ($p < 0.001$) among all students, and a 3.7 percentage point increase ($p < 0.001$) among borrowers. Among treated students, the number borrowing exactly $\ell = P$ is equal to or greater than the number borrowing any amount in a \$500 bin above or below P. For this pattern to be consistent with Property 2, there would need to be a mass of students with $\ell^* = P$. This possibility seems unlikely except for the fact that for some students P corresponds to the maximum subsidized loan. However, when we limit the sample to students who are ineligible for the maximum subsidized loan, the excess mass of students at P in the treatment group remains (Appendix Figure A.1). We conclude that the effect of the nudge on amounts borrowed cannot be driven entirely by information costs and must instead involve some degree of inattention.

Property 3: Assume $c_i + c_a > 0$. For all students, if $\ell > 0$ when $T = 0$ then $\ell > 0$ when $T = 1$.

Proof: From the solution above, for a treated student to not borrow when treated it is necessary that $c_d \geq (\ell^*)^\alpha$. If $c_i + c_a > 0$ then $c_d \geq (\ell^*)^\alpha \Rightarrow c_d + c_i + c_a > (\ell^*)^\alpha$, which implies that the student does not borrow when not treated.□

Property 3 shows that this model implies a monotonic, positive effect of treatment on the dummy variable

$\mathbf{1}(\ell > 0)$. Under such monotonicity, a combination of sample moments identifies the average characteristics of always-takers (those who borrow regardless of their loan offer), compliers (those who are induced to take-up a loan when receiving a nonzero offer), and never-takers (those who do not borrow regardless of their loan offer) (Abadie 2003).[41] If the cost borrowing includes an information cost, we would expect this cost to be decreasing in past borrowing and/or schooling experience. If this is the case then compliers for the outcome of $\mathbf{1}[\ell > 0]$ will be newer students and will have less experience with student loans.

Complier characteristics are consistent with the existence of information costs. Table 9 displays estimates of the characteristics of students according to how their decision of whether to borrow responds to the nudge. Always-takers are significantly more likely to have borrowed in the past compared with compliers (73 versus 63 percent, respectively, with $p < 0.1$) and are significantly less likely to be new to CCA (19 versus 29 percent, respectively, with $p < 0.1$). Among returning students, compliers are the group with the lowest baseline (consistent with either information or attention costs decreasing in ability), though the differences between groups are not statistically significant.

Property 4: For all students with $c_i = 0$ and $\ell^* > 0 \cap \ell^* \neq P$, *if* $\ell = \ell^*$, *when* $T = 1$ *then* $\ell = \ell^*$ *when* $T = 0$.

Proof: From the solution above, for a treated student to choose $\ell = \ell^*$ when treated it is necessary that $c_d + c_a < (\ell^*)^\alpha$. Because $c_i = 0$, this implies $c_d + c_i + c_a < (\ell^*)^\alpha$, which implies that the student chooses $\ell = \ell^*$ when not treated.□

Property 4 allows us to bound the share of the treatment effect on the borrowing rate that is due to information costs. This property notes that the information cost generates a positive treatment effect on loan amounts other than P, whereas we would only see a reduction in the share borrowing such amounts if students were only influenced by inattention, as some of these students would be induced to borrow P when treated. The treatment effect is 0.02 for $\ell = P$ and 0.07 for other positive amounts. If we assume that all students induced to choose $\ell = P$ would have chosen $\ell = 0$ when not treated, then the information cost explains 78 percent of the impact of the nudge on loan take-up. If we instead assume that all students induced to choose $\ell = P$ would have chosen some other positive amount when not treated, then the information cost explains 100 percent of the treatment effect on loan take-up.

Welfare analysis is generally difficult when people exhibit behavioral biases. Bernheim and Rangel (2009)

[41] As shown in Abadie (2003), mean characteristics for each group can be constructed using combinations of sample moments. Let π_{AT}, π_{NT}, and π_C represent the share of students that are always-takers, never-takers, and compliers, respectively, and B the probability of taking-up a loan. As in equation 1, T indicates assignment to the treatment group. Then $\pi_{AT} = \Pr(B = 1|T = 0)$ and $\pi_{NT} = \Pr(B = 0|T = 1)$. Under the assumption that assignment to the treatment group weakly increases the probability of borrowing for all students, $\pi_C = 1 - \pi_{AT} - \pi_{NT}$. For any characteristic X, $E[X|C] = \frac{1}{(1-\pi_{AT}-\pi_{NT})} \{((1 - \pi_{NT}) E[X|B = 1, T = 1]) - (\pi_{AT} E[X|B = 1, T = 0])\}$, $E[X|AT] = E[X|B = 1, T = 0]$, and $E[X|NT] = E[X|B = 0, T = 1]$. We estimate sample analogues for each of these terms and test for significant differences in characteristics across groups using the delta method.

propose a framework for behavioral welfare analysis in which one option is deemed better for an individual than another if, among her choices that are considered relevant, she consistently chooses the first option. If the cost of attending to borrowing options was paramount then we might consider all choices relevant, implying ambiguity in whether students would prefer to borrow or not because there is a set of students who do not consistently choose the same option. However, choices are not considered relevant if the individual does not understand her options. Given the information cost supported by our findings, only students who are offered a loan understand their options, and thus the students induced to borrow are students who prefer to do so. Welfare effects of a nonzero offer would therefore appear to be positive for at least 78 percent of those induced to borrow. This conclusion could be reversed by other behavioral biases that we have not modeled, such as if present-biased students take out loans that are harmful to them in the long run, but our cost-benefit calculations suggest that the average student who borrows only when offered a loan is made financially better off.

We conclude that the loan offer nudge appears to affect borrowing through a combination of student inattention and misperception about loan eligibility. Information costs associated with learning about loan eligibility can explain at least 78 percent of the effect of the nudge on loan take-up, while bunching at the offered amount shows that inattention also affects borrowing. These results can help guide future work on welfare analysis and the question of why students, and individuals more generally, respond to certain nudges.

8 Conclusion

We experimentally test the effect of nudges on community college students' borrowing decisions. Students randomly assigned a nonzero loan were approximately 40 percent more likely to borrow, resulting in an aggregate increase of $3.6 million in loans received by the treatment group. A simple presentational nudge has an effect on borrowing that is similar to or larger in magnitude than that of more complicated and costly interventions aimed at reducing student borrowing. The randomized trial provides compelling evidence that borrowing could be reduced at little administrative cost by offering students no loan aid in their award letters. However, the reduction in borrowing must be weighed against the higher average attainment generated by borrowing and the potential information gathering costs imposed upon students. Our estimates suggest that students induced to borrow $4000 – the average amount taken-up by students who complied with the nudge by borrowing – earned 3.7 additional credits and improved their GPAs by 0.6 points in the year of the intervention. In the following academic year, students induced to borrow by the nudge were over 10 percentage points (178 percent) more likely to transfer to a four-year public institution.

Our findings have important policy implications for both colleges and government bodies. Consistent with

Dunlop (2013) and Wiederspan (2016), our results suggest that colleges' decisions to exit from the federal loan program will, on average, reduce students' educational attainment. Among community colleges that participate in the federal loan program, approximately half offer their students $0 in loan aid. Our results suggest that such policies may significantly decrease educational attainment. At the same time, colleges that make nonzero loan offers to all students will significantly increase loan take-up, and we cannot rule out the possibility that some groups of students do not benefit from this additional loan aid. We estimate a trade-off whereby $1000 of additional loans buys short-run increases of approximately 1 credit completed and 0.16 GPA points. Colleges that adhere to a simple default policy should consider which side of this trade-off better serves their students, while also taking into account the possibility that debt may alter students' behavior when they enter the labor force (e.g., Rothstein and Rouse 2011; Chapman 2016).

U.S. federal policy could also take these results into account. Government guidance to colleges can reference the trade-off estimated in these experiments and suggest alternative choice architectures. The Department of Education currently recommends that colleges present financial aid to students using a standardized "shopping sheet," and it has been suggested that this sheet be made mandatory.[42] Because the standard shopping sheet includes a line for loan offers, mandating its use would force colleges to nudge their students towards a particular level of borrowing, whether zero or nonzero. It is likely that no fixed amount of loan offer is best for all students, because larger amounts will nudge some students toward over-borrowing, while smaller amounts will nudge some students toward lower educational attainment. The federal government should allow, and ideally promote, further experimentation on framing of student loans so as to tailor financial aid offers to the needs of each student.

Multiple decision-theoretic models could explain the effects of loan offers on borrowing in this setting. We provide evidence that the nudge affects borrowing through both information costs and inattention. We estimate that at least 78 percent of the effect on whether a student borrows can be explained by information costs. The next stage of our research will employ treatments designed to clarify psychological mechanisms and to improve the precision of our estimates of the effect of loans on educational attainment. Ideally, these treatments will lead the way to more tailored solutions that account for characteristics and circumstances and help each student obtain his or her ideal amount of student loan aid.

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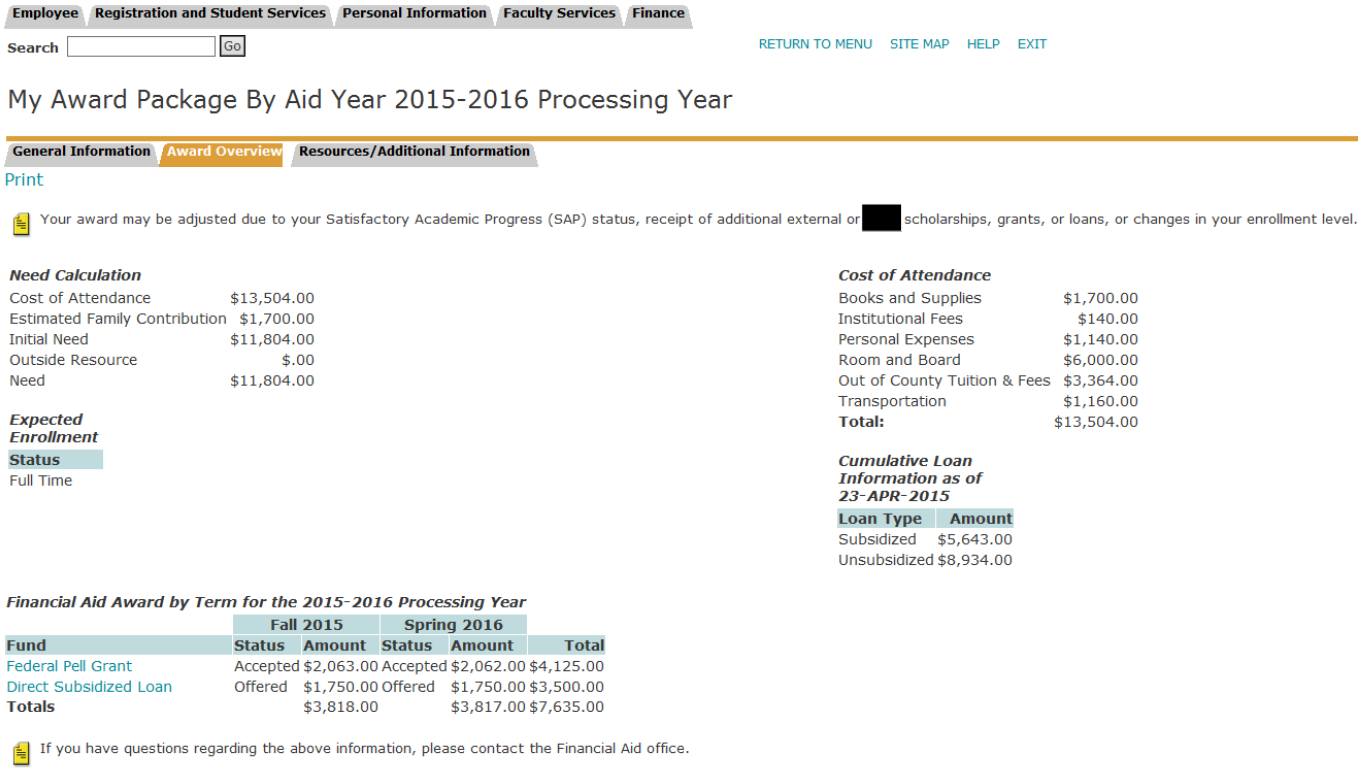
Figures and Tables

Figure 1: Screen Shots From CCA Financial Aid Web Pages

A. Information presented to both treatment and control group members



Employee | Registration and Student Services | Personal Information | Faculty Services | Finance

Search Go

RETURN TO MENU SITE MAP HELP EXIT

My Award Package By Aid Year 2015-2016 Processing Year

General Information | Award Overview | Resources/Additional Information

Print

Your award may be adjusted due to your Satisfactory Academic Progress (SAP) status, receipt of additional external or [redacted] scholarships, grants, or loans, or changes in your enrollment level.

Need Calculation

Cost of Attendance	$13,504.00
Estimated Family Contribution	$1,700.00
Initial Need	$11,804.00
Outside Resource	$.00
Need	$11,804.00

Expected Enrollment

Status
Full Time

Cost of Attendance

Books and Supplies	$1,700.00
Institutional Fees	$140.00
Personal Expenses	$1,140.00
Room and Board	$6,000.00
Out of County Tuition & Fees	$3,364.00
Transportation	$1,160.00
Total:	$13,504.00

Cumulative Loan Information as of 23-APR-2015

Loan Type	Amount
Subsidized	$5,643.00
Unsubsidized	$8,934.00

Financial Aid Award by Term for the 2015-2016 Processing Year

	Fall 2015		Spring 2016		
Fund	**Status**	**Amount**	**Status**	**Amount**	**Total**
Federal Pell Grant	Accepted	$2,063.00	Accepted	$2,062.00	$4,125.00
Direct Subsidized Loan	Offered	$1,750.00	Offered	$1,750.00	$3,500.00
Totals		$3,818.00		$3,817.00	$7,635.00

If you have questions regarding the above information, please contact the Financial Aid office.

B. Award information presented to treatment group members

Financial Aid Award by Term for the 2015-2016 Processing Year

	Fall 2015		Spring 2016		
Fund	**Status**	**Amount**	**Status**	**Amount**	**Total**
Federal Pell Grant	Accepted	$2,063.00	Accepted	$2,062.00	$4,125.00
Direct Subsidized Loan	Offered	$1,750.00	Offered	$1,750.00	$3,500.00
Totals		$3,818.00		$3,817.00	$7,635.00

If you have questions regarding the above information, please contact the Financial Aid office.

C. Award information presented to control group members

Financial Aid Award by Term for the 2015-2016 Processing Year

	Fall 2015		Spring 2016		
Fund	**Status**	**Amount**	**Status**	**Amount**	**Total**
Federal Pell Grant	Accepted	$2,063.00	Accepted	$2,062.00	$4,125.00
Direct Subsidized Loan	Offered	$ .00	Offered	$ .00	$ .00
Totals		$3,818.00		$3,817.00	$7,635.00

If you have questions regarding the above information, please contact the Financial Aid office.

Figure 2: Online Loan Request Form

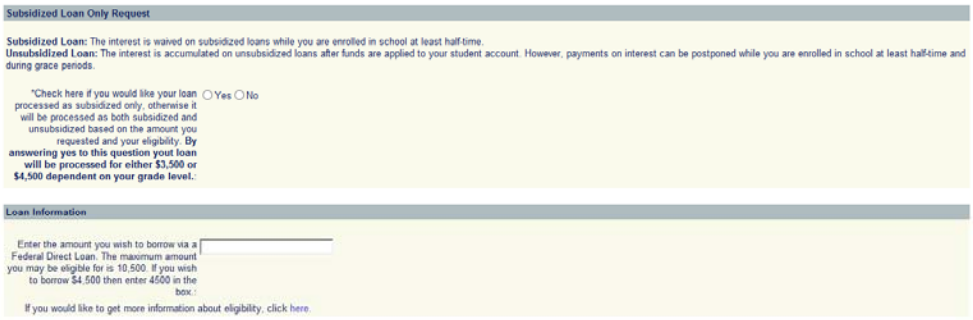
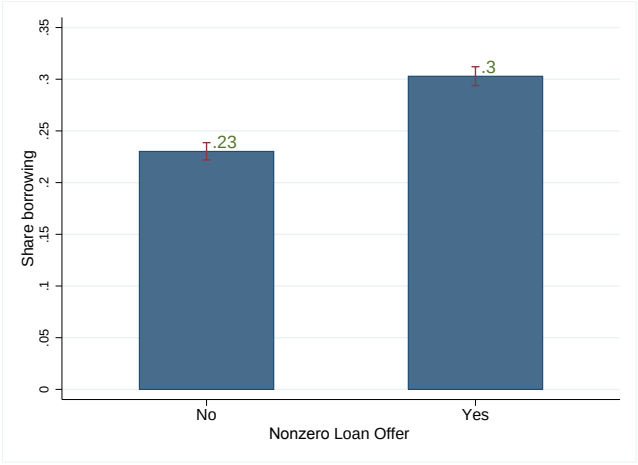
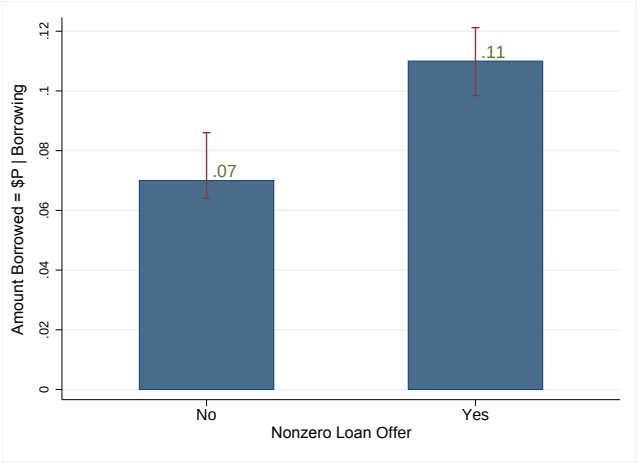


Figure 3: Proportion Borrowing by Treatment



Notes: CCA students randomly assigned before February 2, 2016. Each bar indicates the proportion of students in the treatment and control groups that borrowed. Capped vertical lines represent 95 percent confidence intervals.

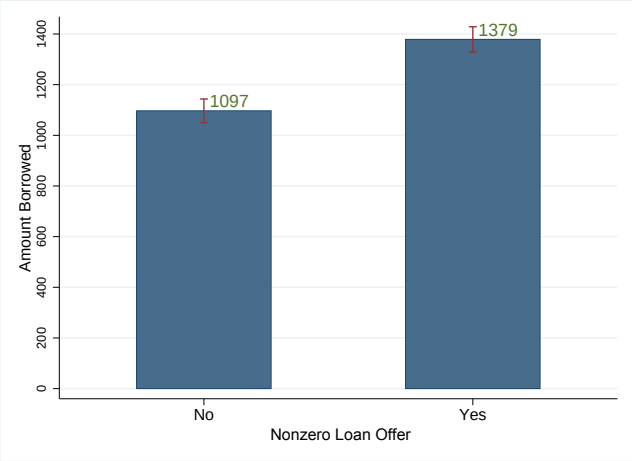
Figure 4: Proportion Accepting $P by Treatment



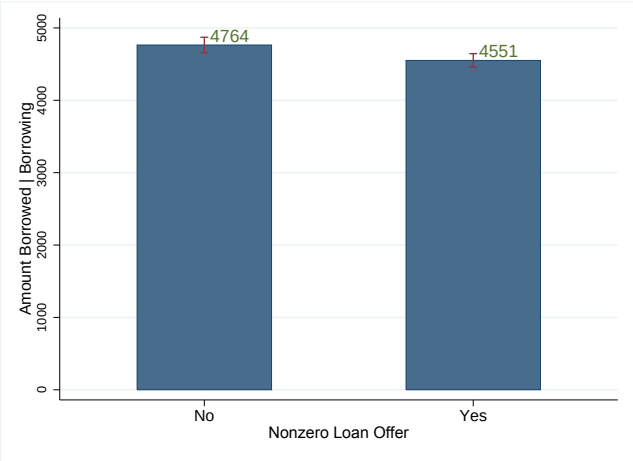
Notes: CCA borrowers randomly assigned before February 2, 2016. Each bar indicates the proportion of borrowers in the treatment and control groups that borrowed exactly $P, the amount that was offered to treatment group students ($3500 for freshmen and $4500 for sophomores). Capped vertical lines represent 95 percent confidence intervals.

Figure 5: Amount Borrowed (Conditional and Unconditional) by Treatment

A. Unconditional



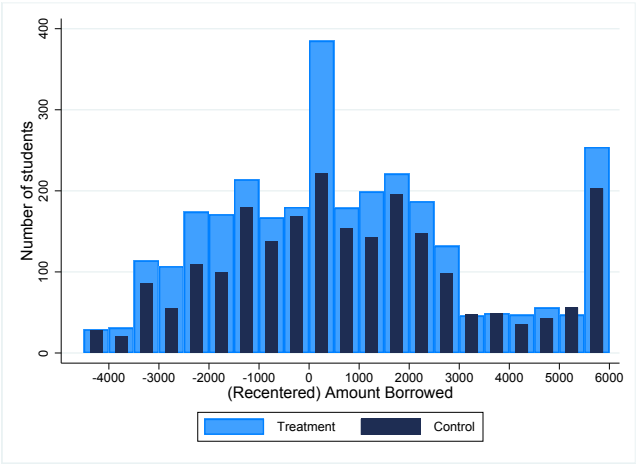
B. Conditional on Borrowing



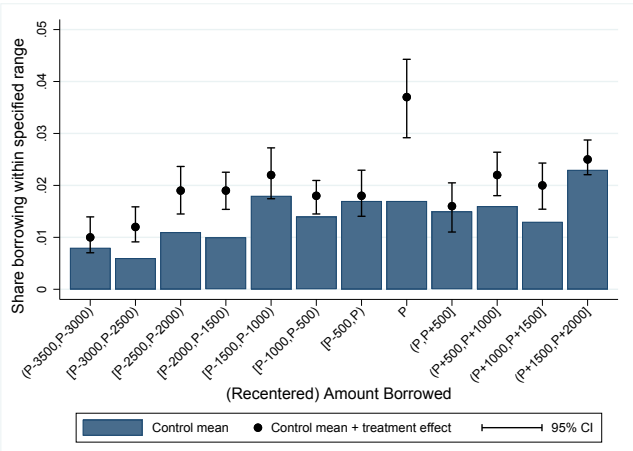
Notes: CCA students (Panel A) or CCA borrowers (Panel B) randomly assigned before February 2, 2016. Each bar indicates the average amount borrowed by students in the treatment and control groups. Capped vertical lines represent 95 percent confidence intervals.

Figure 6: Distribution of (Recentered) Amount Borrowed

A. Treatment and Control Group Borrowers

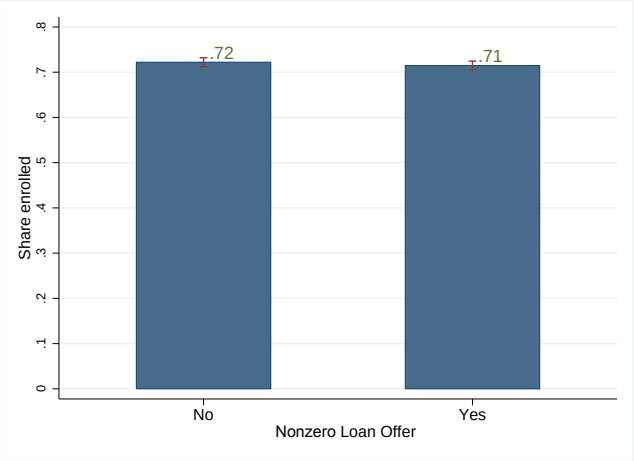


B. Effect of Nonzero Offers on the Probability of Borrowing Specific Amounts



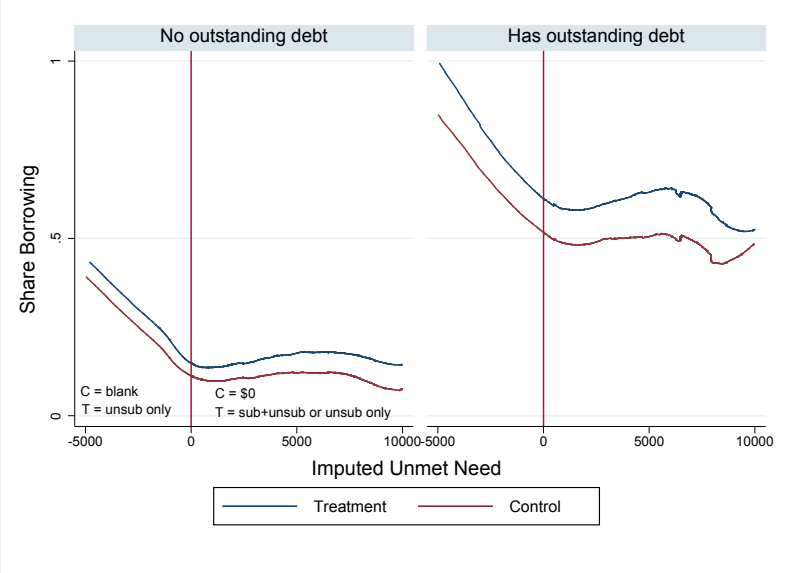
Notes: CCA borrowers randomly assigned before February 2, 2016. In both panels, the amount borrowed is recentered around the amount a student would have received had they been assigned to the treatment group ($3500 for freshmen and $4500 for sophomores). Panel A displays the number of students taking-up loans within the specified $500 bin. Panel B displays the control group mean unconditional probability of borrowing within the specified $500 bin, the estimated effect of nonzero offer receipt on the unconditional probability of borrowing within the specified bin, and the corresponding 95 percent confidence interval. Treatment effects are estimated via 2SLS where assignment to treatment group serves as an instrument for receipt of a nonzero loan offer.

Figure 7: Proportion Enrolled in Fall 2015



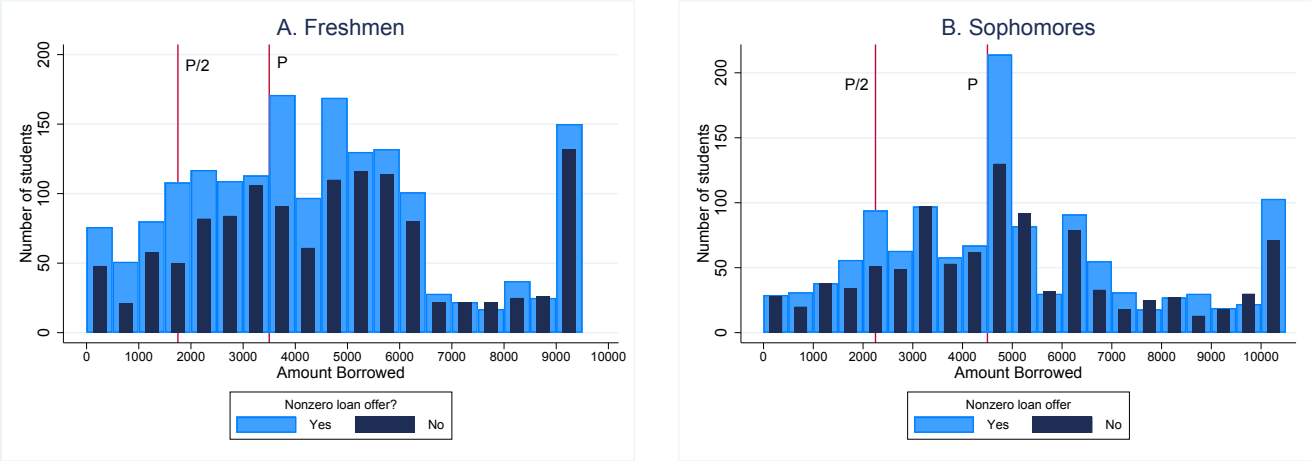
Notes: CCA students randomly assigned before October 15, 2015. Each bar indicates the proportion of students in the treatment and control groups that enrolled. Capped vertical lines represent 95 percent confidence intervals.

Figure 8: Explicit $0 Does Not Reduce Take-up Among Past Borrowers



Notes: Enrolled CCA students randomly assigned before October 15, 2015. Each line represents a local linear regression of the probability of borrowing on (imputed) unmet need (= gross need less EFC, grant aid, and work-study) by treatment assignment.

Figure 9: Distribution of Amount Borrowed by Level



Notes: CCA borrowers randomly assigned before February 2, 2016. Vertical lines indicate \$P (\$3500 for freshmen, \$4500 for sophomores) and $\frac{P}{2}$ (\$1750 for freshmen, \$2250 for sophomores). Light blue bars represent treatment group borrowers and dark blue bars represent control group borrowers.

Table 1: Characteristics of Community Colleges by Loan Packaging Procedures

	(1) Both	(2) Subsidized	(3) Neither
Number of institutions	323	19	454
Average undergraduate enrollment	14,037	18,483	11,642
Enrollment weighted percent of institutions	0.45	0.03	0.52
Offers BA degree(s)	0.12	0.05	0.07
Pell Grant aid			
Percent	0.40	0.36	0.37
Average | receipt	\$3,663	\$3,784	\$3,670
Federal loan aid			
Percent	0.30	0.26	0.16
Average | receipt	\$5,338	\$4,231	\$5,097
Cohort default rate	18.6	20.5	18.9

Notes: Community colleges participating in federal student loan programs, excluding the 69 community colleges for which we were unable to obtain loan packaging practice information (participation status and enrollment from http://projectonstudentdebt.org/files/pub/CC_participation_status_2013-14.pdf). Federal loan and Pell Grant recipient data from the Integrated Postsecondary Education Data System's 2012-13 Student Financial Aid and Net Price file. Information on whether a given community college offers bachelor's degree programs from the IPEDS 2012-13 Institutional Characteristics file. Cohort default rates from Department of Education, Office of Federal Student, official 3-year cohort default rates for borrowers entering repayment in 2012 (available at: http://www2.ed.gov/offices/OSFAP/defaultmanagement/cdr.html). All statistics are enrollment weighted except for average enrollment, the count of institutions in each category, and cohort default rates. Cohort default rates are weighted by cohort size. The number of schools with nonmissing cohort default rate information is in each category is 296 (both), 19 (subsidized), and 429 (neither).

Table 2: Community College A Characteristics and National Averages

	CCA	All community colleges
A. Prices		
Published tuition and fees		
In-district	$3,100	$3,249
In-state	$4,000	$3,375
Out-of-state	$7,500	$7,547
Cost of attendance (if living off campus)	$12,600	$16,434
B. Student body		
12-month FTE	18,800	4,335
Percent receiving Pell Grants	45	41
Percent with federal loans	25	19
Percent first generation	50	48
C. Attainment and Earnings Outcomes		
Percent grad w/in 150% time to degree	5	21
Percent with earnings, 10 years after entry	75	81
Percent earn > $25K, 10 years after entry	55	59
Median salary, 10 years after entry	$28,000	$30,253
D. Borrowing and Repayment Outcomes		
Percent defaulting in 3 years	20	19
Median debt at repayment entry	$4,200	$6,563
Percent paying down balance, 7 years later	60	67

*Note*s: Two-year public schools participating in Title IV federal student aid programs. Panel A measures from 2014-15 IPEDS institutional characteristics file. Dollar amounts for experimental sites rounded to nearest $100 to preserve confidentiality. Cost of attendance is equal to the sum of in-district tuition and fees and the estimated cost of books and supplies, off campus housing, and other living expenses. Panel B measures from 2013-14 IPEDS for all students except the last measure (percent of students that are first generation college students), which comes from the College Scorecard data and pools 2013-14 and 2014-15 cohorts. FTE = full-time equivalent enrollment. Enrollment for experimental sites is rounded to nearest 100 and percent measures are rounded to the nearest 5 to preserve confidentiality. Panel C measures from the College Scorecard data. The percent of students graduating within 150% of the expected time to degree is measured using first-time, full-time, degree-seeking undergraduates who entered college in fall 2010 and fall 2011. The percent of students with earnings and the percent of students earning more than $25,000 10 years after entry are measured for federal aid recipients who were not enrolled 10 years after college entry, belonging to the 2001-02 and 2002-03 entry cohorts. Earnings measured in 2012 and 2013, adjusted for inflation and reported in constant 2015$. Median salary is reported for students with earnings who received federal student aid in college and were not enrolled 10 years after college entry, belonging to the 2001-02 and 2002-03 entry cohorts. Percent measures for experimental sites rounded to nearest 5 and median salary for experimental sites rounded to nearest $1000 to preserve confidentiality. Panel D cohort default rate comes from the official three-year federal cohort default rate for students who entered repayment in FY2013. Borrowers are considered to have defaulted if they have not made payments on their federal loans for 270 days. Median debt from College Scorecard data and pools students entering repayment in 2014 and 2015. The percentage paying down their loan balance is from College Scorecard data and represents the share of students who entered repayment in FY2007 and FY 2008 who were not in default and had reduced their loan balance 7 years after entering repayment. Experimental site measures rounded to nearest 5 (percent measures) or nearest $100 to preserve confidentiality.

Table 3: Descriptive Statistics

	Control mean	Treatment effect
Characteristic		
<30 credits earned	0.65	-0.0002
		(0.007)
New	0.28	0.0002
		(0.006)
Independent	0.59	-0.0001
		(0.007)
Outstanding loan debt	4173	-5.2
	(6480)	(93)
Expected family contribution (EFC)	6769	115
	(8273)	(686)
Pell Grant aid	3438	16
	(2305)	(23)
Work study aid	45	0.1
	(508)	(4)
All other grant aid	122	0.4
	(453)	(5)
Total other resources	36	-0.5
	(272)	(3)
Cumulative credits[1]	32.1	0.05
	(24.8)	(0.27)
Cumulative GPA[2]	2.67	-0.01
	(0.92)	(0.02)
Test of joint significance (p -value)		
excluding cumulative credits, GPA		0.995
including cumulative credits, GPA		0.997
Observations	9,860	19,724

*Note*s: Sample includes students who were randomly assigned before February 2, 2016. 1. Continuous variable standard deviations in parentheses below means. 1. Cumulative credits only measured for students with prior attendance at experimental site (N = 13,566). 2. GPA only measured for students with prior attendance at experimental site and nonmissing GPA (N = 13,219). All other grant aid includes non-Pell federal grants, state grants, and institutional grants. Total other resources includes private and employer provided aid.

Table 4: The Impact of Nonzero Loan Offers on Borrowing

	(1) Offered loan	(2) Any borrowing	(3) Amount borrowed
A. Control group mean		0.23	$1,097
B. OLS estimates			
Assigned to treatment group	0.812	0.073	282
	(0.030)**	(0.009)**	(52)**
C. IV estimates			
Offered loan		0.090	348
		(0.009)**	(58)**
Observations	19,724	19,724	19,724

*Note*s: CCA students who were randomly assigned before February 2, 2016. Panel A contains OLS estimates of the impact of assignment to the treatment group on receiving a nonzero loan offer. Panels B contains IV estimates of the impact of being offered a nonzero loan on the specified outcome; assignment to the treatment group serves as the excluded instrument. Robust standard errors, clustered by strata, in parentheses; ** $p<0.01$, * $p<0.05$, + $p<0.1$. All regressions include controls for strata, randomization month, and baseline cumulative credits and cumulative GPA.

Table 5: Heterogeneity in the Impact of Loan Offers on Borrowing

	(1) Any borrowing	(2) Amount borrowed	Conditional Outcomes (3) Amount	(4) Amt. = $P
Subgroup				
No outstanding debt (N = 11,301)	0.060	185	-460	0.057
	(0.007)**	(52)**	(170)**	(0.020)**
Has outstanding debt (N = 8,424)	0.124	539	-80	0.040
	(0.009)**	(66)**	(101)	(0.013)**
	[<0.001]	[<0.001]	[0.054]	[0.474]
Pell eligible (N = 16,204)	0.096	358	-256	0.038
	(0.011)**	(72)**	(145)	(0.012)**
Pell ineligible (N = 3,521)	0.064	301	-60	0.055
	(0.011)**	(74)**	(94)	(0.021)*
	[0.045]	[0.579]	[0.329]	[0.518]
New student (N = 5,607)	0.097	313	-379	0.034
	(0.013)**	(69)**	(152)*	(0.013)**
Returning student (N = 14,117)	0.087	362	-131	0.049
	(0.012)**	(76)**	(118)	(0.014)**
	[0.571]	[0.629]	[0.199]	[0.425]
<30 credits earned (N = 12,763)	0.092	318	-292	0.037
	(0.010)**	(60)**	(101)**	(0.011)**
30 or more credits earned (N = 6,961)	0.085	399	-39	0.055
	(0.018)**	(114)**	(158)	(0.022)*
	[0.730]	[0.527]	[0.178]	[0.458]
Dependent student (N = 8,125)	0.076	179	-216	0.055
	(0.012)**	(34)**	(81)**	(0.019)**
Independent student (N = 11,599)	0.097	451	-156	0.041
	(0.012)**	(83)**	(136)	(0.013)**
	[0.213]	[0.003]	[0.703]	[0.548]
All subgroups				
Test of equality (*p* -value)	<0.001	<0.001	0.103	0.388
Test of joint significance (*p* -value)	<0.001	<0.001	0.113	0.011

*Note*s: CCA students who were randomly assigned before February 2, 2016. IV estimates of the impact of being offered a nonzero loan on the borrowing outcome specified in column, estimated separately for each specified subgroup. Amount = $P (column 4) is the probability that the student borrowed exactly how much they would have been offered if they were assigned to the treatment group ($3500 for freshmen and $4500 for sophomores). Assignment to treatment serves as an instrument for receipt of a nonzero loan offer. Brackets contain *p*-values from a test of the equality of prior two subgroup estimates. Robust standard errors, clustered by strata, in parentheses; ** p<0.01, * p<0.05, + p<0.1. All regressions include controls for strata, randomization month, and baseline cumulative credits and cumulative GPA.

Table 6: OLS and IV Estimates of the Impact of Nonzero Loan Offers on Attainment

	(1) Credits attempted	(2) Credits earned	(3) GPA	(4) Degree receipt	(5) Standardized treatment effect
A. Control mean					
	17.28	12.93	2.26	0.09	
	(7.65)	(8.75)	(1.27)	(0.29)	
B. OLS Estimates					
Assigned to treatment group	0.213	0.310	0.053	0.003	0.029
	(0.117)+	(0.132)*	(0.018)**	(0.005)	(0.011)**
	{0.158}	{0.067}	{0.021}	{0.637}	
C. IV Estimates					
Offered loan	0.255	0.371	0.063	0.003	0.034
	(0.134)+	(0.154)*	(0.021)**	(0.006)	(0.013)**
D. IV Estimates					
1[borrowed]	2.528	3.671	0.627	0.033	0.339
	(1.276)*	(1.585)*	(0.218)**	(0.065)	(0.130)**
Observations	11,774	11,774	11,774	11,774	11,774

*Note*s: Enrolled CCA students who were randomly assigned before October 15, 2015. Control group means and standard deviations (in parentheses) in Panel A. Panel B contains OLS estimates of the impact of assignment to the treatment group on the specified outcome; family-wise p-values (adjusted to account for multiple hypothesis testing) in brackets. Panels C and D contain IV estimates of the impact of being offered a nonzero loan (C) or loan take-up (D) on the specified outcome; assignment to the treatment group serves as the excluded instrument. See Section 3.4 for description of standardized treatment effects. Robust standard errors, clustered by strata, in parentheses; ** $p<0.01$, * $p<0.05$, + $p<0.1$. All regressions include controls for strata, randomization month, and baseline cumulative credits and cumulative GPA.

Table 7: Heterogeneity in the Impact of Borrowing on Attainment: 2015-16 Academic Year

	(1) Amount borrowed	(2) Credits attempted	(3) Credits earned	(4) GPA	(5) Degree receipt	(6) Std. TE
Subgroup						
No outstanding debt	3135	0.988	5.019	0.800	0.116	0.440
	(614)**	(2.718)	(3.581)	(0.408)+	(0.122)	(0.280)
Has outstanding debt	4602	3.458	2.739	0.497	-0.022	0.284
	(599)**	(1.114)**	(1.364)*	(0.249)*	(0.076)	(0.119)*
	[0.087]	[0.401]	[0.552]	[0.527]	[0.335]	[0.609]
Pell eligible	3924	2.822	4.325	0.788	-0.002	0.369
	(551)**	(1.341)*	(1.699)*	(0.258)**	(0.059)	(0.130)**
Pell ineligible	4585	1.154	0.483	-0.131	0.192	0.183
	(962)**	(3.348)	(4.277)	(0.369)	(0.236)	(0.406)
	[0.550]	[0.640]	[0.401]	[0.043]	[0.427]	[0.661]
New student	3201	0.745	0.561	0.266	0.021	0.143
	(581)**	(2.253)	(2.549)	(0.318)	(0.021)	(0.181)
Returning student	4423	3.153	4.764	0.733	0.039	0.426
	(612)**	(1.475)*	(1.983)*	(0.293)*	(0.096)	(0.168)*
	[0.148]	[0.371]	[0.193]	[0.280]	[0.857]	[0.251]
<30 credits earned	3629	2.494	2.882	0.440	-0.060	0.136
	(445)**	(1.469)+	(1.743)+	(0.259)+	(0.037)	(0.132)
30 or more credits earned	4961	3.333	5.600	0.911	0.183	0.607
	(947)**	(2.175)	(3.175)+	(0.321)**	(0.206)	(0.277)*
	[0.203]	[0.749]	[0.453]	[0.253]	[0.245]	[0.125]
Dependent student	2418	0.094	1.722	0.506	0.032	0.184
	(288)**	(2.527)	(2.980)	(0.425)	(0.101)	(0.243)
Independent student	4892	3.960	4.927	0.689	0.031	0.445
	(566)**	(1.234)**	(1.816)**	(0.255)**	(0.084)	(0.151)**
	[<0.001]	[0.170]	[0.314]	[0.713]	[0.884]	[0.362]
All subgroups						
Test of equality (*p* -value)	0.001	0.727	0.581	0.323	0.307	0.712
Test of joint significance (*p* -value)	<0.001	0.024	0.202	0.186	0.393	0.113

*Note*s: Enrolled CCA students who were randomly assigned before October 15, 2015. IV estimates of the impact of loan take-up on the outcome specified in column, estimated separately for each specified subgroup. Assignment to treatment, serves as an instrument for the amount borrowed. See Section 3.4 for description of standardized treatment effects. Brackets contain *p*-values from a test of the equality of prior two subgroup estimates. Robust standard errors, clustered by strata, in parentheses; ** p<0.01, * p<0.05, + p<0.1. Regressions also include controls for strata, randomization month, and baseline cumulative credits earned and cumulative GPA.

Table 8: The Impact of Borrowing on Attainment: 2016-17 Academic Year

	(1) Reenrolled at CCA	(2) Transfer to 4-year public	(3) Received any degree	(4) Standardized treatment effect
A. Control group mean	0.54	0.06	0.21	
B. IV estimates				
1[borrowed]	-0.123	0.114	0.023	0.121
	(0.105)	(0.051)*	(0.055)	(0.126)
Observations	11,774	11,774	11,774	11,774

*Note*s: Enrolled CCA students who were randomly assigned before October 15, 2015. IV estimates of the impact of loan take-up on the outcome specified in column. Assignment to treatment, serves as an instrument for the amount borrowed. Robust standard errors, clustered by strata, in parentheses; ** p<0.01, * p<0.05, + p<0.1. Regressions also include controls for strata, randomization month, and baseline cumulative credits earned and cumulative GPA.

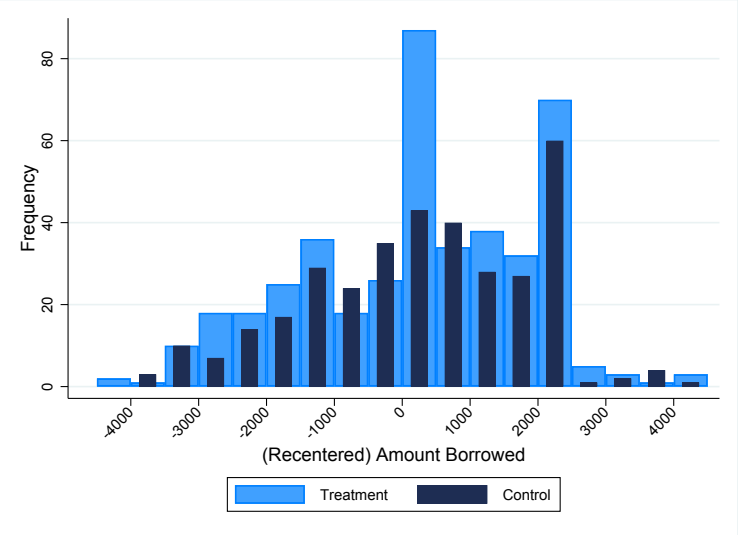
Table 9: Characteristics of CCA Students by Response to Treatment

Characteristic	E[X|AT]	E[X|C]	E[X|NT]	Tests of equality (p-value): E[X|C]=E[X|AT]	E[X|C]=E[X|NT]	E[X|C]=E[X|AT∪NT]	E[X|AT]=E[X|NT]
Female	0.659	0.652	0.620	0.902	0.510	0.655	<0.001
White	0.439	0.460	0.429	0.722	0.544	0.589	0.396
College educated parent	0.411	0.370	0.390	0.466	0.689	0.623	0.070
Age	30.2	28.7	26.6	0.239	0.058	0.291	<0.001
EFC	$4,143	$4,005	$2,854	0.874	0.143	0.301	<0.001
Cost of attendance	$11,698	$11,675	$9,464	0.959	<0.001	<0.001	<0.001
Pell Grant eligible	0.702	0.842	0.859	0.009	0.710	0.653	<0.001
Independent	0.682	0.670	0.548	0.818	0.011	0.071	<0.001
Has outstanding debt	0.727	0.635	0.306	0.083	<0.001	<0.001	<0.001
New student	0.210	0.297	0.308	0.074	0.800	0.763	<0.001
Freshman	0.559	0.645	0.676	0.139	0.539	0.966	<0.001
Baseline credits (N = 13,576)	35.2	31.8	31.0	0.298	0.797	0.891	<0.001
Baseline GPA (N = 13,576)	2.64	2.67	2.58	0.760	0.410	0.503	0.049
Baseline credits*GPA (N = 13,576)	100.9	95.1	91.0	0.571	0.657	0.891	<0.001

Notes: CCA students who were randomly assigned before February 2, 2016 (N = 19,724 except where noted). AT = always-takers (students who borrow regardless of treatment group assignment); C = complier (students induced to borrow by a non-zero offer), NT = never-takers (students who do not borrow regardless of treatment group assignment). Baseline credits and GPA sample limited to returning students.

Appendix A: Additional Tables and Figures

Figure A.1: Distribution of (Recentered) Amount Borrowed, Students with no Unmet Need



*Note*s: CCA borrowers randomly assigned before February 2, 2016 who were ineligible for subsidized loans based on imputed unmet need. Amount borrowed recentered around the amount a student would have received had they been assigned to the treatment group ($3500 for freshmen and $4500 for sophomores). Light blue bars represent treatment group borrowers and dark blue bars represent control group borrowers.

Table A.1: Heterogeneity in the Impact of Loan Offers on Fall 2015 Enrollment

	(1) Enrolled	(2) Control group mean
Subgroup		
No outstanding debt	-0.015	0.729
	(0.010)	
Has outstanding debt	-0.003	0.713
	(0.134)	
	[0.482]	
Pell eligible	-0.011	0.723
	(0.010)	
Pell ineligible	-0.003	0.716
	(0.016)	
	[0.694]	
New student	-0.021	0.684
	(0.018)	
Returning student	-0.004	0.736
	(0.010)	
	[0.428]	
<30 credits earned	-0.020	0.705
	(0.010)*	
30 or more credits earned	0.009	0.751
	(0.012)	
	[0.061]	
Dependent student	-0.008	0.763
	(0.013)	
Independent student	-0.011	0.693
	(0.012)	
	[0.874]	
All subgroups		
Test of equality (*p* -value)	0.416	
Test of joint significance (*p* -value)	0.482	

*Note*s: CCA students who were randomly assigned before February 2, 2016. IV estimates of the impact of being offered a nonzero loan on Fall 2015 enrollment, estimated separately for each specified subgroup. Assignment to treatment serves as an instrument for receipt of a nonzero loan offer. Brackets contain *p*-values from a test of the equality of prior two subgroup estimates. Robust standard errors, clustered by strata, in parentheses; ** p<0.01, * p<0.05, + p<0.1. All regressions include controls for strata, randomization month, and baseline cumulative credits and cumulative GPA.

Table A.2: Descriptive Statistics, Attainment Sample

Characteristic	Control mean	Treatment effect
<30 credits earned	0.61	-0.008
	(0.49)	(0.009)
New	0.26	-0.004
	(0.44)	(0.008)
Independent	0.56	-0.001
	(0.50)	(0.009)
Outstanding loan debt	4171	-22
	(6435)	(118)
Expected family contribution (EFC)	3026	208
	(7769)	(165)
Pell Grant aid	3358	12
	(2181)	(40)
Work study aid	77	1
	(651)	(12)
All other grant aid	192	2
	(558)	(10)
Total other resources	50	-3
	(323)	(6)
Cumulative credits[1]	34.6	0.31
	(24.2)	(0.52)
Cumulative GPA[2]	2.76	-0.01
	(0.86)	(0.02)
Test of joint significance (p -value)		
excluding cumulative credits, GPA		0.990
including cumulative credits, GPA		0.991
Number of observations	5,920	5,854

*Note*s: Enrolled CCA students who were randomly assigned before October 16, 2015. GPA only measured for students with prior attendance at CCA. All other grant aid includes non-Pell federal grants, state grants, and institutional grants. Total other resources includes private and employer provided aid.

Table A.3: Heterogeneity in the Impact of Loan Aid on 2015-16 Attainment Outcomes by Baseline Degree Program

	(1) Offered loan	(2) Any borrowing	(3) Amount borrowed	(4) Credits attempted	(5) Credits earned	(6) GPA	(7) Degree receipt	(8) Std. TE
A. OLS (assigned to treatment group)								
AA/AS (N = 3,156)	0.817							
	(0.036)**							
AAS (N = 8,109)	0.843							
	(0.032)**							
	[0.036]							
B. IV (offered loan)								
AA/AS (N = 3,156)		0.104	453					
		(0.017)**	(92)**					
AAS (N = 8,109)		0.108	408					
		(0.011)**	(78)**					
		[0.834]	[0.579]					
C. IV (borrowed)								
AA/AS (N = 3,156)			4373	5.700	9.385	0.601	-0.003	0.554
			(489)**	(3.031)+	(3.357)**	(0.405)	(0.136)	(0.273)*
AAS (N = 8,109)			3790	1.047	1.734	0.628	0.032	0.237
			(524)**	(1.811)	(1.971)	(0.259)*	(0.069)	(0.152)
			[0.258]	[0.265]	[0.079]	[0.957]	[0.825]	[0.345]
Test of joint sig (*p*- value):			<0.001	0.047	0.004	0.012	0.900	0.020

*Note*s: Enrolled associate degree-seeking CCA students who were randomly assigned before October 16, 2015. Each column within a panel contains estimates from a separate regression. Panel A contains OLS estimates of the impact of assignment to treatment on receiving a nonzero loan offer. Panel B contains IV estimates of the impact of a nonzero loan offer on borrowing outcomes, where assignment to treatment serves as an instrument for receipt of a nonzero loan offer. Panel C contains IV estimates of the effect of an additional $1000 of loan aid on 2015-16 academic year attainment, represented by standardized index of treatment effects (see Section 3.4 for details). The p-value from the test of equality of subgroup coefficients in brackets below estimates. Robust standard errors, clustered by strata, in parentheses; ** p<0.01, * p<0.05, + p<0.1. Regressions also include controls for strata, cumulative credits earned and cumulative GPA before the beginning of the fall 2015 semester.

Table A.4: The Impact of Borrowing on Receipt of Specific Credentials by Spring 2017

	(1) Certificate	(2) AA/AS	(3) AAS
A. Control group mean	0.06	0.08	0.10
B. IV estimates			
1[borrowed]	-0.055	0.100	-0.036
	(0.048)	(0.072)	(0.055)
Observations	11,774	11,774	11,774

*Note*s: Enrolled CCA students who were randomly assigned before October 15, 2015. IV estimates of the impact of loan take-up on receipt of the credential specified in column. Assignment to treatment, serves as an instrument for the amount borrowed. Robust standard errors, clustered by strata, in parentheses; ** p<0.01, * p<0.05, + p<0.1. Regressions also include controls for strata, randomization month, and baseline cumulative credits earned and cumulative GPA.

Table A.5: Heterogeneity in the Impact of Loan Aid on Selected 2016-17 Attainment Outcomes by Baseline Degree Program

	(1) Enrolled at CCA	(2) Transfer to 4-year public	(3) Any degree	(4) AA/AS degree	(5) AAS degree
A. IV (borrowed)					
AA/AS (N = 3,156)	-0.123	0.267	0.055	0.063	-0.019
	(0.214)	(0.041)*	(0.207)	(0.202)	(0.026)
AAS (N = 8,109)	-0.105	0.044	0.046	0.071	-0.021
	(0.115)	(0.055)	(0.072)	(0.046)	(0.071)
	[0.941]	[0.141]	[0.967]	[0.967]	[0.975]
Test of joint sig (*p*- value):	0.568	0.059	0.798	0.292	0.761
B. Control group means					
AA/AS	0.54	0.11	0.22	0.21	0.01
AAS	0.54	0.05	0.21	0.04	0.14

*Note*s: Associate degree-seeking CCA students who enrolled in fall 2015 and were randomly assigned before October 16, 2015. Each column contains estimates from a separate regression. IV estimates of the effect of borrowing on measures of 2016-17 attainment. The *p*-value from the test of equality of subgroup coefficients in brackets below estimates. Robust standard errors, clustered by strata, in parentheses; ** p<0.01, * p<0.05, + p<0.1. Regressions also include controls for strata, cumulative credits earned and cumulative GPA before the beginning of the fall 2015 semester.

Appendix B: Community College B Experiment

In this appendix, we describe the setting and design of the experiment that took place at Community College B (CCB). We then present preliminary estimates of the impact of loan offers on borrowing and attainment (fall semester enrollment and credits attempted).

B.1 CCB Experiment Design

In the year prior to the intervention (2014-15), CCB students were not offered loan aid. CCB only provides financial aid packages to students after they have registered for courses and sends students hard-copies of their financial aid package via mail. In addition to federal requirements (i.e., entrance counseling and completion of a master promissory note), CCB students who wish to borrow must complete several additional steps. These include filling out a budget, determining their expected future salary upon graduation and calculating estimated loan payments, and attending a one-on-one meeting with a college counselor.[43]

For the intervention, CCB's financial aid office offered students assigned to the treatment group their maximum subsidized loan and no unsubsidized loans. CCB students without subsidized loan eligibility were not included in the experimental sample. Offers continued to be made via paper award letters that were mailed to students (Figure B.1). Students in the control group did not receive an additional communications from CCB on their loan eligibility, although the school's financial aid website contained general information on federal loan programs.

CCB underperformed in terms of expected sample size. Based on past enrollment of degree-seeking students, we projected a sample size of roughly 8,000 students. However, the surprisingly small number of CCB students who completed a FAFSA and were eligible for subsidized loans reduced the number of students eligible to be included in random assignment to 2,221. At present time, we only observe borrowing outcomes for 2,102 of these students. As shown in Table B.1, predetermined characteristics are balanced between CCB treatment and control groups.

B.2 Results

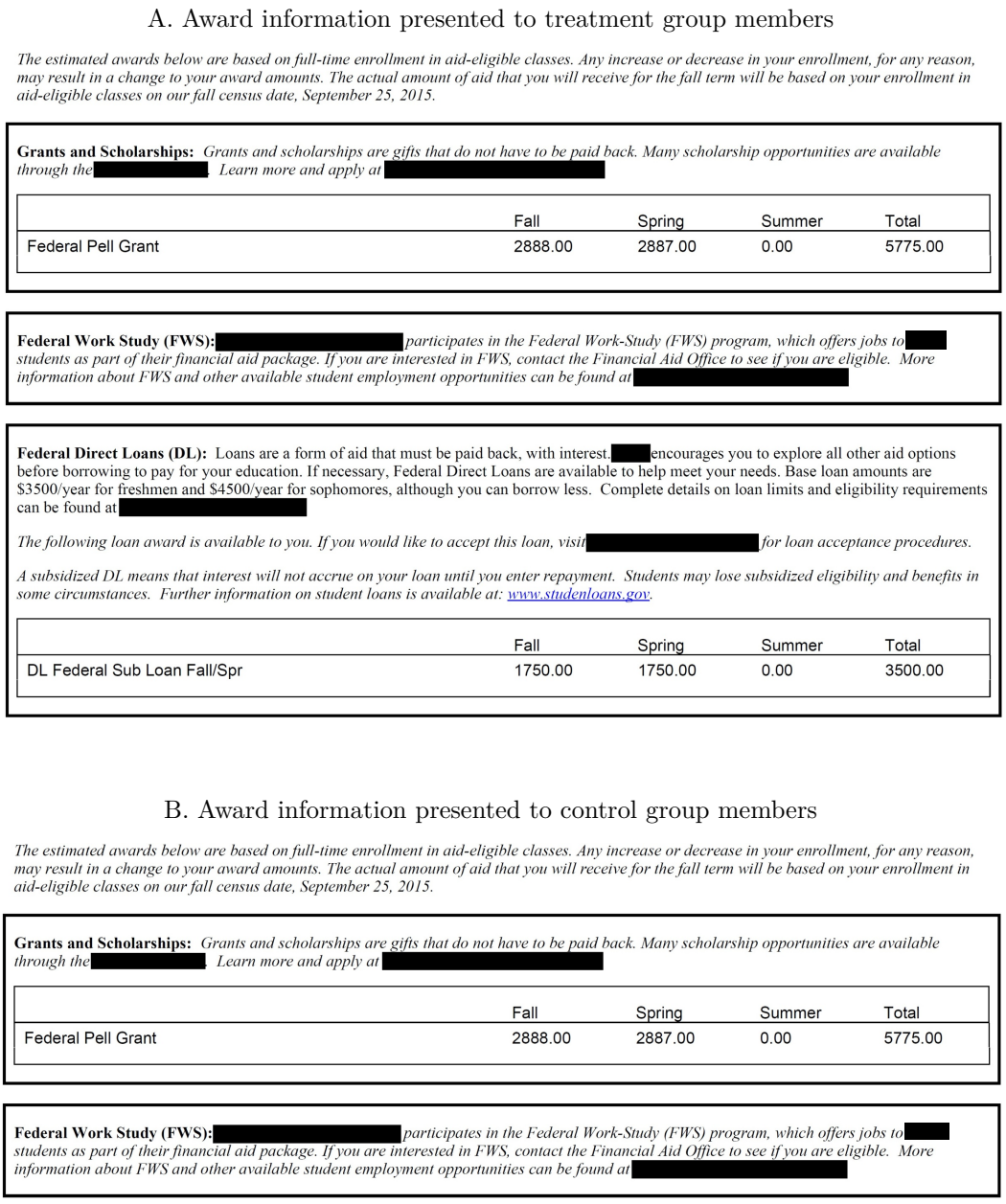
As shown in Table B.2, only 74 percent of treatment group members received a loan offer. The estimated effect of a nonzero loan offer on the likelihood of borrowing is small and statistically insignificant. However, the corresponding 95 percent confidence interval - $[-0.12, 0.14]$ - includes the estimated effect of the nudge within CCA. In contrast to CCA, we find large, negative impacts on conditional borrowing, suggesting that among borrowers, receipt of a nonzero offer led to a \$1093 reduction in loans ($p < 0.01$). This reduction is driven by a reduction in unsubsidized borrowing (available upon request), which is consistent with the fact that CCB treatment group members only received subsidized loan offers. We find evidence of patterns of heterogeneous treatment effects in the impact of nonzero offers on borrowing that are similar to those produced in CCA (Table B.3), but we are underpowered to distinguish between effects across groups.

[43]The budgeting worksheet requires students to estimate their fall and spring semester education-related expenses, financial resources, and unmet need.

Given that we do not find any first-stage effects of loan offers on borrowing, we are only able to estimate reduced form impacts of loan offers on attainment. As shown in Table B.4, estimated impacts on fall semester enrollment, credits attempted, and the likelihood of part-time or full-time enrollment are negative, insignificant, and sufficiently imprecise that we cannot rule out impacts of a similar magnitude to those found in CCA.

B.3 Figures and Tables

Figure B.1: CCB Financial Aid Award Letters



A. Award information presented to treatment group members

The estimated awards below are based on full-time enrollment in aid-eligible classes. Any increase or decrease in your enrollment, for any reason, may result in a change to your award amounts. The actual amount of aid that you will receive for the fall term will be based on your enrollment in aid-eligible classes on our fall census date, September 25, 2015.

Grants and Scholarships: *Grants and scholarships are gifts that do not have to be paid back. Many scholarship opportunities are available through the* [redacted]. *Learn more and apply at* [redacted]

	Fall	Spring	Summer	Total
Federal Pell Grant	2888.00	2887.00	0.00	5775.00

Federal Work Study (FWS): [redacted] *participates in the Federal Work-Study (FWS) program, which offers jobs to* [redacted] *students as part of their financial aid package. If you are interested in FWS, contact the Financial Aid Office to see if you are eligible. More information about FWS and other available student employment opportunities can be found at* [redacted]

Federal Direct Loans (DL): Loans are a form of aid that must be paid back, with interest. [redacted] encourages you to explore all other aid options before borrowing to pay for your education. If necessary, Federal Direct Loans are available to help meet your needs. Base loan amounts are $3500/year for freshmen and $4500/year for sophomores, although you can borrow less. Complete details on loan limits and eligibility requirements can be found at [redacted]

The following loan award is available to you. If you would like to accept this loan, visit [redacted] *for loan acceptance procedures.*

A subsidized DL means that interest will not accrue on your loan until you enter repayment. Students may lose subsidized eligibility and benefits in some circumstances. Further information on student loans is available at: www.studenloans.gov.

	Fall	Spring	Summer	Total
DL Federal Sub Loan Fall/Spr	1750.00	1750.00	0.00	3500.00

B. Award information presented to control group members

The estimated awards below are based on full-time enrollment in aid-eligible classes. Any increase or decrease in your enrollment, for any reason, may result in a change to your award amounts. The actual amount of aid that you will receive for the fall term will be based on your enrollment in aid-eligible classes on our fall census date, September 25, 2015.

Grants and Scholarships: *Grants and scholarships are gifts that do not have to be paid back. Many scholarship opportunities are available through the* [redacted]. *Learn more and apply at* [redacted]

	Fall	Spring	Summer	Total
Federal Pell Grant	2888.00	2887.00	0.00	5775.00

Federal Work Study (FWS): [redacted] *participates in the Federal Work-Study (FWS) program, which offers jobs to* [redacted] *students as part of their financial aid package. If you are interested in FWS, contact the Financial Aid Office to see if you are eligible. More information about FWS and other available student employment opportunities can be found at* [redacted]

Table B.1: Descriptive Statistics

Characteristic	Control mean (sd)	Treatment effect (se)
<30 credits earned	0.63	0.002
	(0.10)	(0.003)
New	0.22	-0.004
	(0.07)	(0.003)
Independent	0.43	0.005
	(0.10)	(0.003)
Outstanding loan debt	1904	97
	(74)	(147)
Expected family contribution (EFC)	2390	34
	(18)	(35)
Pell Grant aid	4397	-5
	(6)	(12)
All other grant aid	906	-9
	(25)	(49)
Test of joint significance (*p* -value)		0.543
Number of observations	1,047	1,055

*Note*s: CCB students randomly assigned before November 6, 2015. All other grant aid includes non-Pell federal grants, state grants, and institutional grants.

Table B.2: The Impact of Nonzero Loan Offers on Borrowing

	(1) Offered loan	(2) Any borrowing	Amount borrowed (3) Uncond.	(4) Cond.
A. OLS estimates				
Assigned to treatment group	0.741			
	(0.019)**			
B. IV estimates				
Offered loan		0.010	-41	-1,093
		(0.013)	(70)	(391)**
Observations	2,102	2,102	2,102	146
Mean control	0	0.066	$348	$5,287

*Note*s: CCB students randomly assigned before November 6, 2015. OLS estimates of the impact of assignment to treatment on being offered a loan (Panel A) and IV estimates of the impact of being offered a loan on borrowing outcomes (Panel B), where assignment to the treatment group serves as an instrument for being offered a loan. Robust standard errors, clustered by strata, in parentheses; ** p<0.01, * p<0.05, + p<0.1. All regressions also include controls for strata fixed effects.

Table B.3: Heterogeneity in the Impact of Loan Offers on Borrowing

	(1) Any borrowing	Amount borrowed (2) Uncond.	(3) Cond.
A. Outstanding debt			
Offered loan			
× No student loan debt	-0.001	-31	-651
	(0.012)	(61)	(626)
× Outstanding student loan debt	0.054	-78	-1,274
	(0.049)	(244)	(506)*
Test of equality (*p* -value)	0.265	0.853	0.464
B. Pell Grant eligibility			
Offered loan			
× Pell eligible	0.009	-49	-1,294
	(0.014)	(71)	(547)*
× Pell ineligible	0.010	-14	-887
	(0.033)	(178)	(575)
Test of equality (*p* -value)	0.978	0.854	0.627
C. Past enrollment			
Offered loan			
× New student	0.009	18	-889
	(0.032)	(159)	(878)
× Returning student	0.010	-57	-1,121
	(0.014)	(77)	(424)**
Test of equality (*p* -value)	0.989	0.672	0.812
D. Class standing			
Offered loan			
× <30 credits earned	-0.004	-67	-899
	(0.016)	(70)	(498)+
× 30 or more credits earned	0.033	3	-1,293
	(0.026)	(149)	(541)*
Test of equality (*p* -value)	0.221	0.674	0.592
E. Dependency status			
Offered loan			
× Dependent student	-0.011	-37	171
	(0.014)	(63)	(394)
× Independent student	0.041	-46	-1,652
	(0.028)	(149)	(410)**
Test of equality (*p* -value)	0.094	0.957	0.001
Observations	2,102	2,102	146

*Note*s: CCB students randomly assigned before November 6, 2015. IV estimates of the impact of being offered a nonzero loan on the borrowing outcome specified in column. Each panel contains estimates from a separate regression. Assignment to treatment, interacted with the specified characteristics, serves as an instrument for the interaction between the receiving a nonzero loan offer and the specified characteristic. Robust standard errors, clustered by strata, in parentheses; ** p<0.01, * p<0.05, + p<0.1. Regressions also include controls for strata fixed effects.

Table B.4: The Impact of Nonzero Loan Offers on Attainment

	(1) Enrolled	(2) Credits attempted	(3) ≥ 6 credits attempted	(4) ≥ 12 credits attempted
A. OLS estimates				
Assigned to treatment group	-0.019	-0.266	-0.010	-0.024
	(0.016)	(0.175)	(0.019)	(0.020)
B. IV estimates				
Offered loan	-0.025	-0.359	-0.013	-0.032
	(0.021)	(0.229)	(0.024)	(0.026)
Observations	2,102	2,102	2,102	2,102
Control mean	0.77	7.3	0.65	0.30

*Note*s: CCB students randomly assigned before November 6, 2015. Panel A contains OLS estimates of the impact of assignment to the treatment group on the specified outcome. Panel B contains IV estimates of the impact of being offered a nonzero loan on the specified outcome; assignment to the treatment group serves as an instrument for receipt of a nonzero loan offer. Robust standard errors, clustered by strata, in parentheses; ** p<0.01, * p<0.05, + p<0.1. Regressions also include controls for strata fixed effects.

Appendix C: Anchoring Model

We first consider a model with anchoring, which offers predictions that differ considerably from the other possible explanations discussed in Section 7. Let the utility function have the form

$$U(\ell|T) = -(\ell - \ell^*)^\alpha - Tc(\ell - P)^\alpha - (1-T)c(\ell - 0)^\alpha,$$

where $\ell^* \in \mathbb{R}$ is the latent desired loan amount, $\ell \geq 0$ is the amount borrowed, $c > 0$ is a parameter affecting the cost of deviating from the offered amount, and $\alpha \in \{2, 4, 6, ...\}$. We consider two testable properties of this model.

Property 1: When T=1, only if $\ell^* = P$ does $\ell = P$.

Proof: $\frac{d}{d\ell}\big|_P U(\ell|T=1) = -\alpha(\ell - \ell^*)^{\alpha-1} - \alpha T(\ell - P)^{\alpha-1} = -\alpha(P - \ell^*)^{\alpha-1}$. If $\ell = P$ and $\ell^* > P$ then the derivative is positive, and increasing ℓ would increase utility. If $\ell = P$ and $\ell^* < P$ then the derivative is negative, and decreasing ℓ would increase utility. Thus $\ell = P$ is only optimal if $\ell^* = P$.□

As Figure 5 shows, many students in the treatment group borrow exactly $\ell = P$, and this is not due to a shift in the distribution of loan amounts, suggesting that anchoring cannot be the only reason that the loan offer affects borrowing. Among treated students, the number borrowing exactly $\ell = P$ is similar to or greater than the number borrowing any amount in a \$500 bin above or below P. This increased mass at exactly $\ell = P$ could arise because for some students P corresponds to the maximum subsidized loan, but this is also true for the control group, for which we do not see a spike at exactly $\ell = P$. Figure 5 shows that a nonzero offer of P significantly increases the probability of borrowing exactly $\ell = P$ by a magnitude substantially larger than estimated impacts on the probability of borrowing other amounts.

Property 2: Suppose $\alpha = 2$ and $\epsilon \in \mathbb{R}^+$. If the density of ℓ^* is increasing (decreasing) over $[P, (1+c)(P+\epsilon)]$ then the probability $\Pr(\ell \in (P, P+\epsilon))$ will be greater (lesser) when T=1 than when T=0.

Proof: For $\alpha = 2$, the first-order condition and be rearranged to show that the utility function is maximized by $\ell = \frac{\ell^* + TcP}{1+c}$. The relevant probabilities are therefore $\Pr(\ell \in (P, P+\epsilon)|T=0) = \Pr\left(\frac{\ell^*}{1+c} \in (P, P+\epsilon)\right)$ $= \Pr(\ell^* \in ((1+c)P, (1+c)(P+\epsilon)))$ and $\Pr(\ell \in (P, P+\epsilon)|T=1) = \Pr\left(\frac{\ell^*+cP}{1+c} \in (P, P+\epsilon)\right) = \Pr(\ell^* \in (P, P+(1+c)\epsilon))$. Both the upper and lower bounds for the range of possible values of ℓ^* are decreased by cP when T=1 relative to when T=0. If the density of ℓ^* is increasing (decreasing) over the entire range then the higher values implied by T=1 occur with greater (lesser) probability.□

Empirical evidence on Property 2 indicates that anchoring is limited. Though we cannot directly observe the density of ℓ^*, when the offer is \$$P$, the distribution of ℓ near \$$P$ is not greatly distorted from that of ℓ^* (as noted in Property 1). Hence we can get a sense of the slope of the latent distribution around \$$P$

from the observed distribution among students treated with an offer of $\$P$. Panel A of Figure 6 shows that the loan amount density of treated students is increasing in the range up to \$2000 above $\$P$, at least among freshmen. By property 2, this would imply that in the bin just above $\$P$ we should observe more control-group students than treatment-group students. We observe the opposite, suggesting that anchoring is limited.

Property 3: Suppose $\alpha = 2$. There exists $\epsilon \in \mathbb{R}^+$ such that if the density of ℓ^* is increasing (decreasing) over $(0, P)$ then the probability $\Pr\left(\ell \in \left(\frac{P}{2} - \epsilon, \frac{P}{2} + \epsilon\right)\right)$ will be greater (lesser) when T=0 than when T=1.

Proof: For $\alpha = 2$, the utility function is maximized by $\ell = \frac{\ell^* + TcP}{1+c}$. The relevant probabilities are therefore $\Pr\left(\ell \in \left(\frac{P}{2} - \epsilon, \frac{P}{2} + \epsilon\right) | T = 0\right) = \Pr\left(\frac{\ell^*}{1+c} \in \left(\frac{P}{2} - \epsilon, \frac{P}{2} + \epsilon\right)\right) = \Pr(\ell^* \in ((1+c)\frac{P}{2} - (1+c)\epsilon, (1+c)\frac{P}{2} + (1+c)\epsilon))$ and $\Pr\left(\ell \in \left(\frac{P}{2} - \epsilon, \frac{P}{2} + \epsilon\right) | T = 1\right) = \Pr\left(\frac{\ell^* + cP}{1+c} \in \left(\frac{P}{2} - \epsilon, \frac{P}{2} + \epsilon\right)\right) = \Pr(\ell^* \in ((1-c)\frac{P}{2} - (1+c)\epsilon, (1-c)\frac{P}{2} + (1+c)\epsilon))$. Both the upper and lower bounds for the range of possible values of ℓ^* are decreased by cP when T=0 relative to when T=1. If the density of ℓ^* is increasing (decreasing) over the entire range then the higher values implied by T=1 occur with greater (lesser) probability. The entire range is $((1-c)\frac{P}{2} - (1+c)\epsilon, (1+c)\frac{P}{2} + (1+c)\epsilon)$, and for this to be contained in $(0, P)$, it must be that $\frac{P}{2} + \epsilon \leq \frac{P}{1+c} \Leftrightarrow \epsilon \leq \frac{P}{2}$. Choose ϵ small enough that this holds.□

Figure 9 shows that the density is increasing for both freshmen and sophomores, so the property implies more mass among the control group, but we observe the opposite.

The distributions of loan amounts among treatment and control groups does not support an anchoring explanation. Failure of Property 1 implies that anchoring cannot fully explain the borrowing effects, and failure of Properties 2 and 3 suggests that anchoring is limited. While there may be a small amount of anchoring that is obscured by offsetting factors, for the purpose of distinguishing between remaining possible mechanisms, we assume there is no anchoring.