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

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Abstract

We provide the first experimental evidence on the effect of student loans on educational attainment. Loan amounts listed in financial aid award letters ("offers") do not alter students' choice sets but significantly affect borrowing. Students randomly receiving a nonzero offer were 40 percent more likely to borrow than those who received a $0 offer. Per additional borrower, loans increased by $4000, GPA and completed credits increased by 30 percent, and

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transfers to four-year public colleges increased by 11 percentage points. Cost-benefit and theoretical analyses suggest nonzero offers enhance welfare, yet over 5 million students are not currently offered loans. *JEL codes: I22, D91, D12, D14.*

1 Introduction

U.S. undergraduate enrollment has increased by more than 30 percent since 2000, with two-year institutions absorbing the majority of new students. The plurality of students now attend open-access community colleges and face poor odds of success (Shapiro et al. 2016). At the same time, numerous studies provide evidence that community college graduates receive substantial labor market returns (e.g., Jepsen et al. (2014), Bahr et al. (2015), Dadgar and Trimble (2015), Liu et al. (2015), Stevens et al. (2015), and Turner (2016).). While some grant programs 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 (e.g., Dynarski and Scott-Clayton 2006; Bettinger et al. 2012). Even though the federal government subsidizes loans for low-income students, it also incentivizes colleges to discourage borrowing, and there is little empirical evidence concerning the effect of borrowing on student outcomes to guide policy.

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. Such policies include offering all students $0 in loan aid or completely opting out of federal loan programs; colleges that have opted-out of federal loans serve nearly one million students (Cochrane and Szabo-Kubitz 2016). 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 imposes on schools with high cohort default rates.

In this paper, we provide evidence on the causal effect of student loans on attainment. We use experimental variation that mirrors the most common practices of community colleges. While many features of federal student loans, such as maximum amounts and eligibility requirements, are applied uniformly to students and institutions, colleges have discretion over whether to include loan "offers" in students' financial aid award letters. Listed loan amounts, commonly referred to as “offers,” do not alter students’ choice sets but may affect borrowing through choice architecture, i.e. the design of the decision-making environment (Thaler et al. 2012). Roughly half of the 10 million students attending community colleges that participate in the federal loan program receive a nonzero loan offer.

We study the effect of student loan offers on borrowing and educational attainment with a field experiment at a large community college. Students were randomly assigned to receive either a \$0 loan offer or a nonzero offer of \$3500 (for “freshman” who have accumulated less than 30 credits) or \$4500 (for “sophomores”). 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 a \$4000 loan, on average. Nonzero loan offers also generated sizable gains in educational attainment. While we find no evidence of economically meaningful or statistically significant enrollment effects in the year of the experiment, nonzero loan offers significantly increased credit accumulation and students’ grade point averages (GPAs).

We also provide evidence on the attainment effects of loans themselves by using the random assignment of loan offers as an instrument for borrowing. Our analyses indicate that, on average, students induced to borrow by the nonzero offer earned 3.7 additional credits and raised their GPAs by 0.6, both representing increases of roughly 30 percent. When scaled by the increase in resources due to additional loan aid, these estimates are comparable to estimated effects of need-based grant aid on short-run attainment in other settings. One year after the intervention, borrowers were 11 percentage points more likely to transfer to a four-year public institution.

Based on these first-year attainment gains, we estimate that nudging students to borrow through nonzero loan offers is more cost-effective than the most promising interventions at community colleges to-date (e.g., Scrivener et al. 2012; Mayer et al. 2015). Additionally, our intervention is distinguished by the fact that it mirrors community colleges’ actual behavior, rather than adding additional services and/or messaging on top of existing practices. The average student also likely benefits from receiving a nonzero loan offer. Using existing estimates of the earnings gains from community college credit accumulation and from admission to a four-year institution, we estimate that two years after the intervention, borrowers’ attainment gains are worth more, on average, than the present value of the students’ additional debt for any discount rate below 12.4 percent.

We provide evidence on the mechanisms through which nonzero offers affect students' borrowing decisions. Offers could affect borrowing decisions if they are perceived as providing information about loan availability, establish a reference point for students who are inattentive to alternatives, or induce anchoring around the offered amount.[1] The distribution of loans taken up by students who received a nonzero offer exhibits a spike at exactly the offered amount, providing evidence of some anchoring. But predictions from a model where loan offers affect students only through anchoring toward the offered loan amount are inconsistent with the observed loan distribution. Instead, our preferred model allows for a salience effect that causes borrowing of precisely the offered amount, costs associated with learning about loan availability, and default bias toward $0, which is the amount a student receives if she takes no action. Two patterns suggest nonzero loan offers also provide information about loan availability, even though students received an email explaining how to obtain a loan. First, nonzero loan offers increased take-up of loans of all sizes and not just the offered amount. Second, compared to always-borrowers, students who borrow due to receipt of a nonzero offer are more likely to be new students and are less likely to have borrowed in the past. The effect of a nonzero loan offer on the probability a student borrows the exact amount offered to treatment-group students with her class standing is just under one quarter of the treatment effect on the probability of borrowing any nonzero amount, implying that information about loan availability gained from nonzero offers can explain over 75 percent of the total effect on loan take-up. Thus, nonzero loan offers appear to generate welfare gains for most students that they influence.

This paper contributes to the literature in three ways. First, we directly demonstrate the effects of loan offers on community college students' attainment. Using a regression discontinuity design, Solis (2017) shows that access to credit significantly increases marginally-eligible students' college attainment in Chile. Two

[1] Inattention and anchoring have been shown to influence consumers' decisions in other financial markets. Keys and Wang (2016) show that borrowers exhibit anchoring with respect to minimum credit card payments while Stango and Zinman (2014) provide evidence of consumers' inattention to to bank overdraft fees. Despite the growing importance of student loans in households' balance sheets, research on students' borrowing and repayment decisions is limited (Zinman 2015).

observational studies based in the U.S. estimate the impact of access to loan aid using variation in community colleges' decisions to participate in the federal loan program (Dunlop 2013; Wiederspan 2016). Under the identifying assumption that colleges' of when and whether to participate in the federal student loan program are random, both studies find that restricting access to federal loans may also reduce educational attainment. In contrast, we implement random assignment of non-binding loan offers within a college to study a ubiquitous practice in the U.S. that affects borrowing without altering access to credit.

Second, we contribute to the broader literature on student borrowing. Students' borrowing decisions have been shown to be influenced by debt aversion (Field 2009; Caetano et al. 2011), concerns over self-control (Cadena and Keys 2013), and framing effects (Evans et al. 2016). Interventions involving information and text messaging, accompanied by assistance from financial aid counselors, have produced only small effects on borrowing (Schmeiser et al. 2017; Barr et al. 2017. Experimental evidence from the U.S. and the Netherlands suggests 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. Small changes to choice architecture at this point in time can have effects on borrowing that are as large as, if not larger, than initiatives that are more expensive and broader in scope.

Finally, we contribute to a large literature on the importance of "nudges" and other choice architecture.[2] Nudges affect financial choices in a variety of settings (e.g. Thaler and Benartzi 2004; Duflo et al. 2011; Allcott and Rogers 2014). Within the context of postsecondary education, attainment effects have been found for reductions in already-small costs 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 giving unemployed workers information about financial aid and the return to college (Barr and Turner 2015). Our

[2] As a small change to choice architecture that is intended to influence behavior without altering the choice set, student loan offers fit the definition of a policy nudge (Sunstein and Thaler 2009).

study contributes to this literature by showing that a policy choice currently in use or being contemplated by nearly a thousand colleges has especially large effects on a diverse population of community college students. The broader choice architecture literature shows that default options affect decisions related to investment, saving, and 401(k) participation (Madrian and Shea 2001, Choi et al. 2006, Chetty et al. 2014, Bernheim et al. 2015). As the default option for all students in our setting is \$0, regardless of the offer received, our study provides evidence that nudges that establish reference points can influence behavior even when the nudge does not alter the default.

The remainder of this paper is organized as follows. In Section 2, we describe how students and institutions interact with the federal student loan program. We describe the setting, context, and design of our experiment and our methodology in Section 3. We present estimated effects of nonzero loan offers on borrowing outcomes in Section 4 and describe empirical evidence on the mechanisms by which loan offers might affect behavior in Section 5. In Section 6, we present estimates of the effects of nonzero loan offers and borrowing on attainment, while Section 7 presents cost-benefit analyses from the perspectives of the government and of students. In Section 8, we conclude by discussing the implications of our findings for institutional and governmental policies related to student loans.

2 Federal Student Loans in the U.S.

Low-income college students in the U.S. are eligible for federal grants and loans. 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 the Pell Grant) or when combined with additional information (as in the case of work-study positions).

All students who have completed a FAFSA, are enrolled at least part-time, and have not defaulted on federal loans in the past are eligible to borrow from the Department of Education's Direct Loan Program. The terms of federal loan aid depend on a student's dependency status, class standing, and unmet need.[3] While students must attempt at least 6 credits to be eligible for federal loans, 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 EFC 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. Community college students who are classified as sophomores are eligible for an additional \$1000 in subsidized loans.[4] Dependent first-year students can borrow an additional \$2000 in unsubsidized loans while independent students can borrow an additional \$6000.

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 and generally offer interest rates well below those attached to private student loans. No student in our sample took out a private loan. 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 above dictate the maximum amounts of subsidized and unsubsidized loans for which a college student is eligible, colleges have discretion over how much, if any, loan aid to offer in students' financial aid award letters. The Department of Education and college financial aid administrators call this process "packaging". In all cases, not borrowing is the default: students

[3] 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.

[4] Students face a lifetime limit on subsidized loans of \$23,000. Bachelor's degree-seeking students classified as upper-level students are eligible for an additional \$2000 in subsidized loans. Community college students cannot be classified as upper-level.

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 loan. Students who do not receive a loan offer can still request a loan, with the specific request process varying across institutions. Thus, an offer of $0 may be justified as an accurate reflection of the default option, though students may misinterpret it as indicating ineligibility. Nearly all four-year institutions offer students the maximum amount of loan aid for which they are eligible. In contrast, community colleges are divided in how much loan aid they offer to students.

We collected information on loan offer policies for community colleges that participate in federal loan programs through a combination of web searches, emails, and phone calls between March 2014 and July 2015. We obtained information on the packaging practices of 92 percent of all community colleges participating in federal loan programs. Table 1 describes each type of school using summary data from the Integrated Postsecondary Education System's (IPEDS) 2012-13 Student Financial Aid and Net Price files and the Department of Education's official 3-year cohort default rates (CDRs). Less than half of the schools we surveyed provide students with nonzero loan offers. In total, over 5 million students attend schools that do not offer loans, including those that either included $0 loan offers or made no mention of loans in students' financial aid award letters. The two groups have comparable populations in terms of Pell Grant receipt, suggesting that loan offers are not correlated with average student need. Schools that do not offer loans have lower borrowing rates (16 versus 29 percent for schools providing nonzero offers).

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. Using data from the 2012 NPSAS (via Powerstats), we estimate that among 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, those 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. The risk of federal sanctions for high CDRs may motivate policies intended to reduce student borrowing.[5] However, borrowers who attended schools that package loans default at comparable rates to borrowers who attended schools that did not offer loans (18.7 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. Panel A of Table 2 shows that 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. As at most community colleges, pricing depends on residency and is a linear function of credits attempted. Because we recruited from the group of colleges with sufficient enrollment to obtain a useful sample size, 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 an average of 4300 across all community colleges. Financial aid receipt is similar between CCA students and community college students nationwide. 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, respectively, 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

[5] 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 any federal student aid, while schools with CDRs exceeding 40 percent in any single year lose access to federal loans. Schools can appeal such sanctions 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. Although sanctioned schools can lose access to federal student aid programs, 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).

time to degree, compared to 21 percent of students nationwide.[6] 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 and \$30,253, respectively). Other earnings outcomes follow similar patterns, with CCA students experiencing slightly worse labor market outcomes than national averages. While past CCA borrowers had lower student loan balances when entering repayment (approximately \$4200 versus \$6563 nationwide), they also experienced slightly 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 student loan debt. All prospective students who listed CCA on their FAFSA received their financial aid packages electronically via a web-based system. In addition to federal requirements, CCA students had to confirm that they wish to borrow and to 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, meaning that the default loan amount was \$0. Thus, CCA students who did not borrow may have either actively chosen to do so or simply not completed the loan request form or federal requirements. CCA disburses all funds, including loans, 35 days after the start of the semester.

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 on each batch of students for whom an award letter was to be generated the following day. Approximately 84 percent of students in the experimental sample were packaged before the start of the Fall 2015 semester (Appendix Table A.1). The experimental sample included all students eligible for financial aid; restricting to students who

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

were eligible for loans would have increased statistical power but was deemed infeasible by the financial aid administrator. 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.[7]

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 were not offered a loan. Students who were not eligible for loan aid did not have loan aid mentioned in their financial aid package, regardless of their assignment to treatment or control groups. Appendix Figure A.1 displays screen shots from CCA's web page showing the financial aid package, including examples of both treatment and control student offers in Panels B and C.[8] These offers were pure nudges: they did not affect students' eligibility for federal loans or the requirement that the student actively specify and accept a nonzero loan (and complete federal requirements) to borrow. Students in both the treatment and control groups were informed of their eligibility and the process for requesting a loan via email. The emails contained general information on federal student aid programs and a link to the online loan request form using language that is standard in the community college setting (summarized in Appendix Figure A.2). Both versions of email included language that could discourage borrowing, including an "Important Notice" of loan limits in the treatment-group email and encouragement to "borrow wisely" in the control-group email.

Students who wished to borrow followed the same process that was in place

[7]Break points for stratification by EFC were determined within combinations 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 eligibility.

[8]Among colleges that do not offer loans, some send award letters that do not mention student loans while others 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. We show in Appendix Figure A.4 that 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.

in prior years – all borrowers had to complete CCA's electronic loan request form (Appendix Figure A.3), which could be accessed either via the emailed link or from the financial aid award letter. First-time borrowers were also required to complete federal entrance counseling and a Master Promissory Note. CCA clearly displayed information on student loan eligibility on its website, and all students that complete a FAFSA receive information on their anticipated eligibility for Pell Grants and federal loans from the U.S. Department of Education.

The amount of the loan offered to students in the treatment group 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.[9]

We intended to run a similar experiment at a second community college ("Community College B" or CCB). However, the experimental sample of students was much smaller than expected because fewer students completed a FAFSA (and thus were eligible for random assignment) than anticipated, CCB administrators were only willing to package subsidized loans, and a surprisingly small number of CCB students who completed the FAFSA were eligible for subsidized loans. Within this small sample, we find small and statistically insignificant effects of nonzero subsidized loan offers on borrowing. We can reject the hypothesis that the effect of treatment assignment on borrowing is the same for CCA and CCB students. However, CCB had a much lower borrowing rate than CCA and most other colleges and thus, we cannot reject the hypothesis that the percentage change in borrowing (relative to the control group) is the same in both schools. Other important differences between CCA and CCB that may have contributed to the differences in both baseline

[9] 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 chose to accept. However, our measure of imputed subsidized loan eligibility is strongly predictive of actual eligibility for the subset of students who 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 and without subsidized loan eligibility.

borrowing and treatment effects include CCB's use of paper rather than electronic award letter notices and responses, extension of offers only after enrollment rather than after FAFSA completion, and the requirement of additional paperwork and other hurdles to obtain a loan at CCB. We cannot say for certain which of these or other factors explain the difference in the effect of nonzero offers on borrowing across sites. We include additional details regarding the CCB experiment and point estimates in Online Appendix B.

3.2 Data and descriptive statistics

Our experimental sample includes students who were randomly assigned before February 2, 2016. We test for differences in treatment and control group members' predetermined characteristics including 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), baseline cumulative credits, and baseline cumulative GPA (for returning students). The first column of Table 3 displays the control group means and standard deviations in parentheses. The second column displays placebo treatment effects – the difference between the treatment and control group means and the standard error of this difference (in parentheses). None of the differences 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 $4200 and a GPA of 2.67. Among community college students who completed a FAFSA nationwide in 2012, 60 percent of were classified as freshman, 58 percent were independent, average outstanding debt was $4400, and the average GPA was 2.42 (Authors' calculations using data from the 2012

NPSAS, via Powerstats). Average EFCs and Pell Grant aid ($6769 and $3438, respectively) 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:

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

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

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 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 represent the extent to which CCA's loan offers were correlated with randomly assignment. Compliance with treatment was imperfect because students who were assigned to the treatment group were not offered a loan if their financial aid package was completed after their enrollment decision and they had not enrolled in at least six credits. Given such discrepancies between treatment status and offer status, we also estimate the "intent-to-treat" (ITT) effect of loan offers (i.e. the reduced-form OLS estimates of the impact of treatment group assignment on student outcomes).

We estimate the "treatment-on-the-treated" (TOT) impact of receiving a nonzero loan offer with instrumental variables (IV) models in which assignment to the treatment group serves as the omitted variable. 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 outcome Y_i. Even if assignment to nonzero loan offers to treatment group students is not random, the use of the treatment assignment dummy T_i as an instrument isolates the variation in offers that was randomized. To test for heterogeneous treatment effects, we estimate IV models for each subgroup and allow for cross-equation correlation in error terms via seemingly-unrelated regression. In all analyses, standard errors are clustered by strata.

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 in the year of the experiment, 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 that is 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 estimated treatment effects for each separate outcome, jointly estimated via seemingly unrelated regression, with weights equal to the inverse of 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.[10] The significance of estimated treatment effects on the

[10] This procedure focuses on false positives and is therefore more stringent on this metric than controlling for the familywise error rate as proposed by Romano et al. (2008) and Romano et al. (2010). It involves four steps. 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 $q_k^i = \min\{p_k^i, ..., p_4^i\}$. Step 3: For each outcome k, we calculate the

standardized treatment index provides 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 Nonzero Loan Offers Increase Borrowing

We quantify the impact of treatment assignment and nonzero loan offers on borrowing outcomes using OLS and IV models. The first column 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 receiving a nonzero loan offer by 81 percentage points. The fact that most students assigned to the treatment group were in fact treated allows for precise estimates of impacts on borrowing outcomes.

Panel B presents OLS estimates of the reduced form effect of treatment group assignment on borrowing outcomes. Students assigned to 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.[11] Treatment group members borrow approximately $280 more than control group members, a 26 percent increase from the control group mean.

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 shown in Panel C of Table 4, receipt of a nonzero loan offer generated a 9 percentage point increase in the probability of borrowing. This response represents a 39 percent increase relative to control students' mean borrowing rate of

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 $\tilde{p}^k = \max\left\{\bar{p}^1, ..., \bar{p}^k\right\}$.

[11]The treatment-group borrowing rate of 30 percent was comparable to the borrowing rate in prior years (when all students were treated). Table 2 shows that approximately 25 percent of all CCA students borrowed during the 2013-14 academic year. Borrowing rates for prior cohorts of CCA students who filed a FAFSA - a necessary prerequisite for borrowing and one of the requirements for selection into the experimental sample - would be higher.

23 percent. Nonzero loan offers also increased the average amount borrowed by \$348 (a 32 percent increase relative to the control group mean). Both estimates are statistically significant at the 1 percent level.

We next explore how borrowing decisions are influenced by loan offers by comparing the distributions of amounts borrowed by treatment assignment. We recenter the actual amount borrowed around P, defined as the amount a student would have been offered had she been assigned to the treatment group (i.e., \$3500 for freshmen, \$4500 for sophomores). Panel A of Figure 1 displays the number of treatment and control group students borrowing within \$500 intervals centered around P. There are more treatment group students borrowing at almost every point in the distribution of loan amounts. Although P corresponds to the maximum subsidized loan available to students with unmet need of at least P, when we limit our sample to students whose subsidized loan eligibility falls below this amount, we still observe heaping at P in the treatment group (Appendix Figure A.5).

Our finding of significant heaping at P 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 offers, and the vertical capped line represent the corresponding 95 percent confidence intervals. While nonzero offers significantly 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 increase of approximately 115 percent relative to the control mean.

Most community colleges serve a diverse student body that includes both traditional and nontraditional students from a variety of backgrounds. Given the one-size-fits-all approach to loan packaging used by most community colleges, it is important to understand whether the effects on CCA students are generalizable to schools with different student bodies. Thus, we test whether nonzero loan offers had heterogeneous effects on borrowing across student subgroups defined by past experience with borrowing (any outstanding debt versus no outstanding debt), student resources (Pell Grant eligible versus ineligible), prior CCA enrollment (new versus returning), class standing (freshman versus sophomore status), and dependency status.

As shown in Table 5, a nonzero loan offer significantly increased the probability of borrowing and the unconditional amount borrowed for all subgroups. We can reject the hypothesis of equal impacts of nonzero loan offers on borrowing and the amount borrowed across 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. However, relative to loan take-up among control group members who were past borrowers (39 percent) and nonborrowers (10 percent), the estimated change in loan take-up for students without outstanding debt represents a larger increase in percentage terms.

Loan offers also generated significantly different effects by dependency status and Pell Grant eligibility. Pell Grant eligible students' loan take-up was more responsive to nonzero loan offers than that of ineligible students ($p = 0.045$). Pell-eligible students have lower family resources than ineligible students and thus, may be less informed of their borrowing options. 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 for independent students.

5 The Nature of the Nudge

In this section, we discuss evidence that allows us to distinguish between mechanisms by which loan offers might affect behavior. These mechanisms differ in their implications for welfare analysis and for our empirical approach to estimating the effect of borrowing on attainment. We consider four potential mechanisms and provide evidence on their relevance using the observed distribution of loan amounts among the treatment and control groups. The analyses described in this section were not included in our pre-analysis plan.

Two models are consistent with rational choice. First, students may be uncertain of the availability of federal loans, and it may be costly to obtain this information. In the presence of such *information costs*, a nonzero offer signals that loans are available, thereby reducing this cost. Second, a student may consider the offered amount a recommendation, causing her belief about the optimal loan amount to update toward the offered amount, generating *anchoring*. Among behavioral explanations, one possibility is *default bias* (Madrian and Shea 2001; Thaler and Sunstein 2003). In our setting, the student receives no loan if she does not expend effort to complete a loan application, implying a default of $0. Loan offers do not affect the default, so both the treatment and control group could exhibit default bias toward $0, and default bias cannot explain differences in borrowing between treatment and control groups.[12] Another potential behavioral mechanism is a *salience effect,* as the amount offered in the award letter is made more salient than any other amount. If there is a cognitive cost of considering other options the salience effect would lead students to be especially likely to choose the offered amount.

Appendix C presents formal models of the borrowing decision. We consider a model with anchoring and show that its empirical predictions are not consistent with our findings or those in an information experiment (Marx and Turner 2017).[13] Our preferred model allows for information costs, a salience effect, and default bias. Consider a student choosing a loan amount subject to three costs that could cause deviations from her most-preferred amount. First, she pays a default-bias cost for any nonzero loan amount. This is because the default for all students is to receive no loan aid, and obtaining a loan entails the hassle involved in the application process and perhaps other psychic costs of holding student loan debt. Second, she pays an informational cost to take up a nonzero loan if her offer is $0. Students who are unaware that student loans exist or that they are eligible for such loans face this cost of gathering accurate information on student loan availability and eligibility. Third, the student pays a cost from borrowing any amounts other than the amount offered, which represents the potential proclivity of students to choose the most

[12]Likewise, status quo bias toward amounts previously borrowed should not vary by treatment assignment (Bernheim et al. 2015).

[13]This experiment involved students at a community college that makes nonzero subsidized loan offers to all eligible students. They received informational emails that included an implicit recommended loan amount by referencing average annual borrowing among recent graduates. These randomly-assigned reference points only affected loan take-up and did not cause any student to shift toward the referenced amount.

salient amount. We impose no structure on the latent distribution of most-preferred loan amounts. Nonetheless, comparison of the loan distributions of treated and control students, as well as decomposition of the sample by potential borrowing, offers insight into the importance of information and salience costs.

5.1 Evidence from the distribution of loan amounts

The presence of an information cost predicts a positive treatment effect over a range of loan amounts, including small loan amounts close to zero. Intuitively, students with any level of desired borrowing could be dissuaded by a \$0 loan offer if they misinterpret the offer as indicating that loans are not available to them. If the information cost is sizeable, then the treatment group could exhibit more mass than the control group for any nonzero loan amount. In particular, we would expect to find a positive treatment effect on borrowing small, positive amounts, which would not affect utility enough to make paying the information cost worthwhile.[14] Figure 2 plots the distribution of loan amounts relative to P using \$500 bins. For all amounts less than P, there are more treatment group students than control group students who borrow. IV estimates suggest that treatment increases the likelihood of borrowing a relatively small amount (between \$0 and $0.5P$) by 2.1 percentage points ($p < 0.001$).

The salience effect predicts a positive treatment effect on the probability of borrowing exactly P. Figure 1 shows there is a spike in the treatment-group probability of borrowing P. Among treated students, the number borrowing exactly P is equal to or greater than the number borrowing any amount in a \$500 bin above or below P. Receiving a non-zero offer leads to a 2 percentage-point increase in the unconditional probability of borrowing P ($p < 0.001$) and a 3.7 percentage-point increase

[14]Not all models predict a positive treatment effect on the probability of borrowing small loan amounts. The anchoring model in Appendix C predicts with a negative effect. Our preferred model in Appendix C does so in a specific way because it incorporates simplifying parametric structure including symmetry of preferences around the most-preferred loan amount, making $0.5P$ the value below which the prediction of a positive treatment effect must hold. More generally, the argument will hold for the smallest loan amounts as long as the loss from borrowing less than the most-preferred amount is not too much larger than the loss from borrowing more than this amount, and this will be true for a wider range of loan values if the information cost is large.

($p < 0.001$) for borrowers. Borrowing P is also the modal choice among control-group borrowers, most likely because P corresponds to the maximum subsidized loan for some students. However, when we limit the sample to students who are ineligible for subsidized loans, the excess mass of students at P in the treatment group remains (Appendix Figure A.5). Thus, both information costs and the salience effect appear to play a role in the effect of the nudge on borrowing, and these costs may be present for the same or different students.

The share of the treatment effect on the probability of borrowing due to a reduction in information costs can be bounded from below. Relaxing information costs should increase the probability of borrowing amounts other than P. In contrast, if loan offers only influence students' decisions through the salience effect, then we would see a reduction in the probability of borrowing amounts other than P, since some students would be induced to shift the amount borrowed to P when treated. This raises the untestable question of how many students who were induced to borrow P would have instead borrowed some other positive amount if they had not been treated. The lower bound for this share is zero, meaning that all students induced to choose P would have chosen \$0 when not treated. In this case, the 2 percentage-point treatment effect on the probability of borrowing P is driven entirely by new borrowing, explaining 28 percent of the 9 percentage-point treatment effect on borrowing. This is an upper bound for the salience effect both numerically and because even borrowers who chose to borrow P may have done so simply because of the information cost. Thus, the information cost can explain at least 78 percent of the impact of the nudge on loan take-up.

5.2 Decompositions

Our models all exhibit a property that is not unique but is useful for further empirical analysis. An information cost, a salience effect, or an anchoring effect would each imply a monotonic, positive effect of treatment on the probability of borrowing. Under such monotonicity, a combination of sample moments will identify 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). This decomposition can also provide evidence on the decision model because we would expect the information cost to be decreasing in past borrowing and/or schooling experience. If this is the case, then compliers will be newer students and will have less experience with student loans.

Table 6 displays estimated characteristics of students according to how their decision of whether to borrow responds to treatment. Differences between characteristics of compliers and always-takers are consistent with the existence of information costs. 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 $p < 0.1$).

6 Impacts of Borrowing on Attainment

Student loans may affect educational attainment in a variety of ways. Like grants, loans provide resources that can allow students to pay for additional courses, shift time use from working to studying, or weather negative financial shocks. Unlike grants, loans must be repaid and therefore may also provide an incentive to succeed in school in order to earn more income afterwards. On the other hand, loans could increase attainment by less than grants or even decrease attainment if debt causes stress, loans are used to fund harmful leisure activities, or if concerns about repayment induce more work, less investment, or early exit from schooling. Extensive margin borrowing responses could affect attainment through the information provided via federal loan counseling, the requirement to attempt at least 6 credits to obtain a loan, and effects of any other aid that is crowded in or out by a loan. We discuss conditions under which our results can be interpreted as effects of student loan offers or of student loans themselves. We then present our experimental evidence.

6.1 Estimating effects of loans on attainment

Our finding that information costs generate the majority of the effect of nonzero offers on borrowing has important implications for interpreting our estimates of effects on attainment. TOT estimates of the effect of nonzero offers on attainment will represent local average treatment effects, because treatment is randomly assigned (instrument independence), treatment assignment should not affect attainment except through loan offers (exclusion restriction), and only treatment-group students received a loan offer (monotonicity) (Imbens and Angrist 1994). However, loan offers are unlikely to affect attainment except by inducing changes in students' borrowing decisions. The effect of borrowing on attainment could be estimated by using treatment assignment as an instrument for taking up a loan (that is, on average, \$4000). This specification should satisfy monotonicity, as loan offers should not make any students *less* likely to borrow, but the exclusion restriction will not be satisfied if treatment also affects attainment through intensive-margin changes in the amount borrowed. Alternatively, we could instrument for the amount borrowed, which implies a more plausible exclusion restriction. However, if treatment induces students who would have borrowed some amount above P to borrow exactly P instead, it will not have a monotone effect on the amount borrowed. If the amount borrowed has heterogeneous effects on attainment and monotonicity does not hold, 2SLS will not necessarily produce a LATE.

The theoretical and empirical results in Section 5.1 suggest that 2SLS will recover the LATE of borrowing in our setting. Students who responded to treatment appear to belong to one or two groups: the uninformed, who only borrow when receiving a nonzero offer, and those who are influenced by the salience of the amount offered, who are induced to borrow exactly P. That the latter group may respond to treatment on the intensive margin generates concerns about instrumenting for either borrowing or amount borrowed. However, as shown in Section 5.1, the number of intensive-margin responders is no more than 22 percent of the number of extensive-margin responders and is possibly zero. If students induced to borrow P would not have borrowed otherwise, then monotonicity would hold for either endogenous borrowing regressor. Moreover, we expect any intensive-margin responses to be relatively small; as is assumed with bunching estimation, such adjustments are only cost-effective for those who would otherwise locate near P (Saez 2010).

Our simplified parametric model also predicts that intensive-margin responses will be symmetric around P. This prediction is consistent with Figure 1, which shows that the distribution of borrowers is symmetric within several thousand dollars of P. Thus, any effects of intensive-margin reductions in borrowing are likely offset by effects of intensive-margin increases in borrowing. de Chaisemartin (2017) shows that if the negative treatment effect on defiers is offset by the positive effect on a subset of compliers, then 2SLS produces estimates of the LATE for remaining compliers. In our setting, attainment effects for students who increase borrowing by a small amount are likely similar in magnitude to effects for those who decrease borrowing by a small amount. Thus, instrumenting for loan take-up should capture the average causal effect of borrowing for students induced to borrow by the nudge. Instrumenting for the amount borrowed will scale effects by the number of dollars these students were induced to borrow on average, which is useful for conducting a cost-benefit analysis for this intervention, but effect sizes could be different for pure intensive-margin changes in the amount borrowed.

6.2 Results

We first test whether nonzero loan offers affected the likelihood that CCA applicants enrolled in fall 2015, the semester of the intervention. Among sample members who were randomly assigned before the drop/add date of the fall semester (N = 16,390), and thus could potentially respond to treatment assignment in their enrollment decisions, 72 percent of control group students took at least one class (Appendix Table A.2). IV estimates of the impact of receiving a nonzero loan offer on enrollment produce precisely-estimated null effects, with the 95 percent confidence interval excluding effects larger than a 0.6 percentage point increase in enrollment and a 2 percentage point decrease in enrollment. We also test for heterogeneous effects of loan offers on fall enrollment within the 10 subgroups examined in Table 5. As shown in Appendix Table A.2, only one of the 10 point estimates is significant ($p < 0.05$) and we can reject the test of joint significance across subgroups ($p = 0.482$).

Furthermore, the intervention had no effect on enrollment outside of CCA. Using matched National Student Clearinghouse (NSC) data, we test whether nonzero loan offers affected the probability of enrolling in a different institution in fall 2015. The 95 percent confidence interval rules out effects larger in magnitude than a 0.5 percentage point decrease and a 0.8 percentage point increase in the probability of enrolling in a non-CCA school.[15] Considering the mixed evidence on the enrollment effects of grants (Deming and Dynarski 2010), which are pure transfers, it is perhaps not surprising that the loan offer did not significantly affect enrollment.[16]

Because applicants who do not enroll cannot borrow, we limit the sample of students used to estimate attainment effects to those who enrolled in at least one course. Given that enrollment is balanced across treatment and control groups, adding the 30 percent of sample members without borrowing or attainment to each group produces ITT estimates that are approximately 30 percent smaller. Characteristics of the treatment and control groups are balanced in this restricted sample (Appendix Table A.3). We also conduct a bounding exercise in the spirit of Lee (2009) in which we assume that the insignificant 0.6 percentage point reduction in enrollment for students who received nonzero loan offers comes from the top and bottom of the distribution of potential outcomes. To construct bounds, we trim the top and bottom 0.6 percent of the control group for each continuous attainment outcome. Results are reported in brackets beneath the point estimates and standard errors for IV estimates in Table 7.

The attainment outcomes we observe for the 2015-16 academic year include

[15]We find no effect of loan offers on enrollment in the spring 2016 semester, either in the full sample, among students who enrolled before the fall semester drop/add deadline, or within any of the 10 subgroups.

[16]The lack of an enrollment response is consistent with prior research that finds similarly null effects of Pell Grant aid on college enrollment and choice of institution (e.g., Rubin 2011; Turner 2014; Marx and Turner 2018). One reason why Pell Grants and federal loans may not affect marginal students' enrollment decisions is the complexity of the federal aid application process (Bettinger et al. 2012) and the fact that students are not notified of their financial aid eligibility until after they have applied and been accepted to at least one college. Nationwide, 80 percent of community college students only list one school on their FAFSA, suggesting that there is limited scope for loan offers to affect students' choice of where to enroll. In four-year colleges, where students are more likely to have listed multiple institutions on their FAFSA, loan offers may affect the student's choice of institution. This may be a factor contributing to the fact that nearly all four-year institutions offer students the maximum amount of loan aid.

credits attempted, credits earned, GPA, and degree receipt. We construct a standardized treatment index from all four of these variables. Control group means and standard deviations are displayed in Panel A of Table 7. These are followed by the reduced form (OLS) estimates (Panel B) and the specifications in which we instrument for having been offered a loan (Panel C), having borrowed (Panel D), or for \$1000s of loan aid (Panel E).

Students assigned to the treatment group experienced significant increases in credits attempted, credits earned, and their GPAs in the year of the intervention. 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.[17]

The IV estimates in Panel C scale the attainment effects of the nudge by the percentage of students who were induced to borrow by treatment assignment. On average, these students 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 increased course performance. Those who borrowed because of the nudge earned significantly higher GPAs in each semester, with a cumulative increase of over 0.6 GPA points ($p < 0.01$).[18] Our findings that effects on course completion were

[17]Appendix Figure A.6 displays the distributions of credits earned and GPA by treatment assignment. Treatment appears to shift both distributions to the right, resulting in a reduction in the probability of very low credit completion (< 10) and a reduction in the probability of low academic year GPAs (< 3.0) relative to the control group.

[18]Loan take-up also could increase students' incentive to meet Satisfactory Academic Progress (SAP) requirements. While schools have some flexibility in setting SAP standards, almost all schools require students to maintain a 2.0 GPA. To explore whether this SAP requirement can explain the overall increase in GPA for students induced to borrow by a nonzero loan offer, we estimate treatment effects of borrowing on the probability of obtaining a GPA within mutually exclusive intervals (Appendix Figure A.7). We find no evidence that the effect of borrowing on GPA shown in

larger than effects on credits attempted and that borrowing significantly increased students' GPAs suggest that borrowing helped students both afford more courses and do better in the courses that they would have taken had they not borrowed. Borrowing did not have significant effects on 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. Approximately 96 percent of CCA students in the experimental sample were pursing associate degrees that required 60 to 70 credits at baseline. Most had accumulated fewer than 30 at the start of the fall 2015 semester and only a quarter had earned at least 40 credits.

Panel E of Table 7, presents 2SLS estimates that scale treatment effects by the amount borrowed. For each \$1000 increase in loan aid, credits attempted increased by 0.63, credits completed by 0.91, and GPA by 0.16. This re-scaling allows us to compare the size of the attainment effects with estimated short-run effects of grant aid on enrolled students, and we find effects of comparable size. For example, Anderson and Goldrick-Rab (2016) examine the effect of randomly-assigned eligibility for additional grant aid on low-income community college students. Their estimates imply that a \$1000 increase in resources led to 0.16 additional credits attempted and 0.03 additional credits earned. Denning et al. (2017) estimate effects of increased grant aid due to eligibility for the maximum Pell Grant among four-year college entrants. They find an additional \$1000 of financial aid increases credits attempted by 0.53 and GPA by 0.08. In both cases, estimated effects are statistically insignificant and sufficiently imprecise that we cannot rule out increases similar in magnitude to the effects we find per \$1000 borrowed.

In Section 7, we show that our estimated attainment effects are similarly comparable to more general initiatives when scaled by the cost of the initiative. As suggestive evidence on the mechanism for these attainment effects, we estimate from the increase in credits attempted that for each additional \$1000 borrowed, tuition payments increased by only \$66 (if all students faced the in-county rate) to \$157 (if all students paid the out-of-state rate). Thus, the majority of the increase in aid from borrowing increases resources for non-tuition expenses.

Table 7 comes from an increase in the probability of earning a GPA in (2, 2.5].

We test for heterogeneous effects per $1000 borrowed on educational attainment over the 2015-16 academic year (Table 8). When it comes to impacts of nudge-induced borrowing on academic outcomes, there is only one case in which the estimates for the subgroups on the two sides of each binary distinction are statistically distinguishable at the 10 percent level. Estimated impacts on the standardized treatment index significantly exceed zero for a number of subgroups, but as a whole, we cannot reject the hypothesis that treatment effects are jointly insignificant across all subgroups ($p = 0.273$). Point estimates are largest for Pell eligibles, returning students, independents, and those with outstanding debt, but we cannot reject homogeneity of the effects on any dimension. This finding, along with the similarity of CCA students to other colleges in the shares of students with these characteristics, and the low degree of competition between community colleges (Hoxby forthcoming), suggest similar effects nationally if all schools were to package loans.

We also estimate attainment effects in the academic year following the intervention (2016-17). Using data from the NSC, we examine three attainment outcomes: reenrollment in CCA, transfers to four-year public institutions, and degree receipt. As shown in Table 9, nudge-induced borrowing led to a 12 percentage point (23 percent) drop in CCA reenrollment in the year following the intervention. We find similarly-sized positive impacts of borrowing on the probability of transferring into a bachelor's degree program within a four-year public institution. 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. The 2.8 percentage point increase in the probability of transfer per $1000 borrowed is comparable to the implied 4.6 percentage point increase due to $1000 in grant aid found by Park and Scott-Clayton (2017). Borrowing induced by the nudge did not generate statistically significant increases in degree receipt. Although statistically insignificant, the 5.8 percentage point increase represents a gain of 28 percent relative to the control group mean.

It may be possible to conduct a follow-up analysis in five years, when most students will have presumably completed their education. Long-run outcomes of interest would include degree receipt, wages, outstanding student loans and other debt, and student loan repayment. Given the amount of time that would need to pass before such an analysis, the importance of our short-run results for millions of current college students, and uncertainty about the possibility of obtaining each of these outcomes, we proceed in the current paper with a cost-benefit analysis that relates our short- and medium-run results to the extant literature.

7 Cost-Benefit Analysis

To contextualize our findings, we compare costs and benefits of nonzero loan offers from the perspective of the government and then from the perspective of the student. Loans appear beneficial from both perspectives based on observed attainment effects. We conclude the section with a brief discussion about welfare analysis and the welfare implications of our results.

We compare our estimates 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 (PBS) 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 and received additional supports and financial assistance.[19] 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 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; the specific population

[19] Students were required to enroll in at least 12 credits per semester (full-time), 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.

eligible for participation and the structure and size of incentives varied across experimental sites (Mayer et al. 2015). 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 0.9 credits earned per $1000 outlay is comparable to the magnitude of estimated effects per $1000 spent by 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 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.[20] This suggests a cost-benefit ratio of 8.1 additional credits per $1000, far exceeding the short-run returns of ASAP and PBS. We note, however, that we do not observe CCA students for the length of time that it took for the ASAP program to produce significant increases in degree completion, which prevents us from comparing effects of borrowing to ASAP program expenditures on degree receipt.

We 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), 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 with an individual fixed-effects approach. For enrollment in a four-year public institution, we use estimates from Zimmerman (2014), who uses a regression discontinuity design to estimate effects

[20] 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 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.

of admission to a four-year public institution that acts as a substitute for community colleges.

Implied earnings effects are substantial. Jepsen et al. (2014) estimate that for students who do not earn 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 earnings effects of admission to a four-year institution that acts as a substitute for community college enrollment for recent Florida high school graduates; 55 percent of control group students enroll in a community college, almost exactly the rate of re-enrollment that we find at CCA. Four-year admission increases annual earnings by 22 percent, or $1593 (in 2005 dollars). Based on these estimates, the 11 percentage-point increase in enrollment at 4-year institutions implies an annual earnings increase of $198 (in 2016 dollars).[21] Thus, we project the combined earnings effects of the nudge to be roughly $370 per year per student, on average. These effects are summarized in Appendix Table A.4.

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 applied to loans made in 2015-16, then the loans are financially beneficial if future cash flows are discounted at any rate below 12.4 percent.

[21]To explore whether the Zimmerman (2014) estimates represent a useful measure of the returns to four-year college attendance for marginal CCA students, we look to descriptive evidence on community college transfer students' degree completion and differences in earnings of bachelor's degree recipients that are first-time versus transfer students. Data on community college transfer students from the National Student Clearinghouse – which includes the universe of such students – shows that 65 percent earn a bachelor's degree in the six years after transferring to a four-year public institution (Shapiro et al. 2013). These graduation rates are slightly higher than the 59 percent average completion rate of first-time, full-time undergraduates who enter a four-year public institution (https://nces.ed.gov/fastfacts/display.asp?id=40). The four-year public institution attended by marginal students in Zimmerman (2014) had an overall graduation rate of 49 percent and the graduation rate for marginal attendees was 48 percent. Two studies provide evidence that earnings gains from bachelor's degree receipt are similar for students who start at four-year institutions and those who transfer from a community college (Gill and Leigh 2003; Miller 2007). While it is not certain that the marginal CCA transfer student would graduate at the rate of the average transfer student, if we were to make this assumption, the estimates from Zimmerman (2014) would likely provide a lower bound on the returns to transferring for CCA students.

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 do not transfer to a four-year institution. In this calculation, we assume that all debt is new, but the cost could actually be negative for students whose student-loan borrowing replaces higher-interest credit card debt. Thus, the induced borrowing is almost certainly beneficial to students on average, and it appears likely that the majority of students benefit.

Moving beyond cost-benefit analysis to welfare analysis is generally difficult when people exhibit behavioral biases (Bernheim and Rangel 2009; Handel 2013; Allcott and Taubinsky 2015; Allcott and Kessler forthcoming). Bernheim and Rangel (2009) 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 we had found that 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 our evidence for the information-cost mechanism, it appears that many students who are not offered a loan do not understand their options, and thus most compliers are students who prefer to borrow when they understand their options. Welfare effects of a nonzero offer would therefore appear to be positive for at least 78 percent of those induced to borrow by a reduction in information costs (Section 5). This conclusion could be reversed by the presence of other behavioral biases that we have not modeled (e.g., 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 in fact made financially better off. That our findings suggest a welfare gain for the vast majority of students is therefore consistent with our finding of large average financial gains.

8 Conclusion

We experimentally test the effect of student loan nudges on community college students' borrowing decisions. Randomly assigned nonzero loan offers generated a 40 percent increase in the probability of borrowing. This effect is larger than the changes in borrowing produced by much costlier and more intensive interventions (e.g., Kennedy 2015; Schmeiser et al. 2017) and consistent with prior evidence that students' borrowing decisions are disproportionately affected by information and administrative costs imposed at the time they are making the decision of whether to borrow. In prior work, we show that a dollar of grant aid reduces student loan aid by more than a dollar for students attending colleges that make $0 loan offers, providing evidence of a fixed cost of borrowing (Marx and Turner 2018). Our experimental results suggest that the cost of gathering information about loan availability contributes to this fixed borrowing cost.

Students induced to borrow by the nudge earned 3.7 additional credits and improved their GPAs by 0.6 points in the year of the intervention, on average. In the following academic year, they were 11 percentage points (178 percent) more likely to transfer to a four-year public institution. We estimate that nonzero loan offers increase short-run attainment of community college students by substantially more per expected dollar of government expenditure than other interventions that have been evaluated with experiments. We cannot conclude that offering a nonzero loan is welfare-enhancing for every student, but we project that the average responder benefits financially from borrowing, even with a discount rate as high as 12 percent. Using a simple theoretical model that allows for salience effects, information costs, and default bias, we provide evidence on the channels through which nonzero offers affect student behavior. The pattern of responses to the nudge suggests that at least 78 percent of the response of loan take-up is driven by a reduction in information costs, suggesting that nonzero offers improve most students' welfare.

Our findings are relevant for colleges, policymakers, and future research on the effects of nudges. Over 5 million students attend U.S. colleges that do not offer loans in financial aid award letters, and nearly one million more attend colleges that do not participate in federal loan programs. Our results suggest that offering loans to students enrolled in these colleges could generate substantial attainment increases. We also show that nudges can affect behavior by communicating information in a way that is more salient than other methods used to communicate the same information. Students appear to benefit substantially from clear communication of the opportunity to borrow at the point in time when they are making borrowing decisions, and thus should be made aware of their choice set. At the same time, choosing well within this set also requires knowledge of expected costs and benefits. Future research could examine how to help each student obtain an amount of loan aid that best serves his or her needs.

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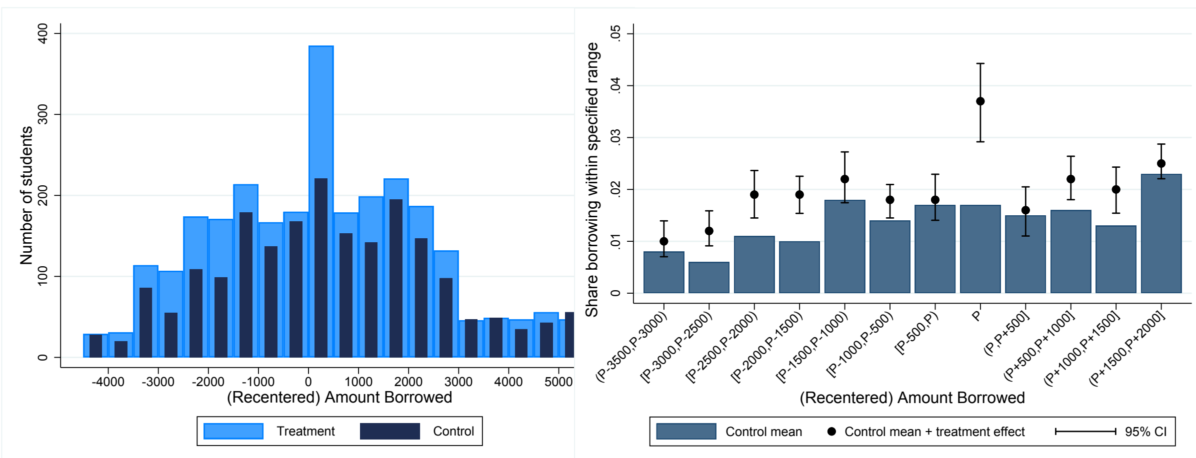
Figures and Tables

Figure 1: Distribution of (Recentered) Amount Borrowed

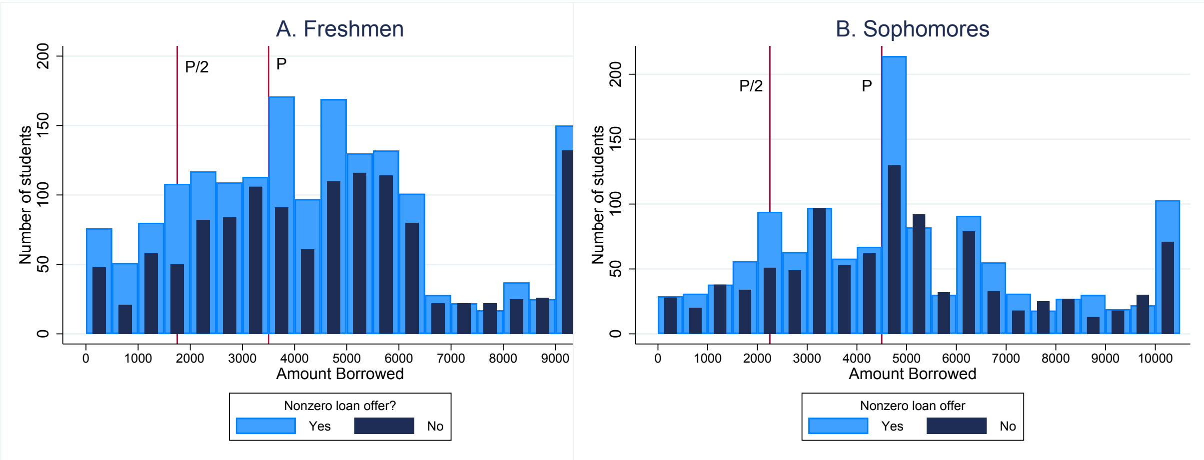
A. Treatment and Control Group Borrowers

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 2: 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

Nonzero loan offers?	(1) Yes	(2) No
Number of institutions	342	454
Average undergraduate enrollment	14,284	11,642
Enrollment weighted percent of institutions	0.48	0.52
Offers BA degree(s)	0.12	0.07
Pell Grant aid		
Percent	0.39	0.37
Average | receipt	$3,672	$3,670
Federal loan aid		
Percent	0.29	0.16
Average | receipt	$5,258	$5,097
Cohort default rate	18.7	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). The online appendix to Marx and Turner (2018) contains an earlier version of this table. Federal loan and Pell Grant recipient data from the IPEDS 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. Official 3-year cohort default rates (CDRs) for borrowers entering repayment in 2012 from Department of Education, Office of Federal Student, (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 CDRs. CDRs are weighted by cohort size. The number of schools with nonmissing CDRs is 315 (nonzero loans offered) and 429 (loans not offered).

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. CCA dollar amounts are 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 percent of students that are first generation college students, which comes from the College Scorecard 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. 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 and pools students entering repayment in 2014 and 2015. The percentage paying down their loan balance is from College Scorecard 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: Sample Characteristics at Baseline

	(1) Control mean	(2) Treatment effect
Characteristic		
<30 credits earned	0.65	-0.0002
	(0.48)	(0.007)
New	0.28	0.0002
	(0.45)	(0.006)
Independent	0.59	-0.0001
	(0.49)	(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	32.1	0.05
	(24.8)	(0.27)
Cumulative GPA	2.67	-0.01
	(0.92)	(0.02)

*Note*s: CCA students who were randomly assigned before February 2, 2016. Control group N = 9859; treatment group N = 9865. All characteristics measured prior to random assignment. Column 1 displays control group means and standard deviations. Column 2 displays OLS estimates from a regression of the specified characteristic on treatment group assignment. Baseline cumulative credits and GPA only measured for students with prior attendance at experimental site (N = 13,576). All other grant aid includes non-Pell federal grants, and state and institutional grants. Total other resources includes private and employer provided aid. p-values from test of joint significance of estimates is 0.995 (when estimated effects on baseline cumulative credits and GPA are excluded), 0.997 (when included).

Table 4: The Impact of Nonzero Loan Offers on Borrowing: 2015-16 Academic Year

	(1) Offered loan	(2) Any borrowing	(3) Amount borrowed
A. Control group mean		0.23	$1,097
B. OLS (reduced form) estimates			
1[Assigned to treatment group]	0.812	0.073	282
	(0.030)**	(0.009)**	(52)**
C. IV estimates			
1[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. Control group means (A), OLS estimates of the impact of assignment to the treatment group on the specified outcome (B) and IV estimates of the impact of being offered a nonzero loan on the specified outcome, in which assignment to the treatment group serves as the excluded instrument (C). 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, EFC, and baseline cumulative credits and cumulative GPA.

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

	(1) Any borrowing	(2) Amount borrowed
Subgroup		
No outstanding debt (N = 11,301)	0.060	185
	(0.007)**	(52)**
Has outstanding debt (N = 8,424)	0.124	539
	(0.009)**	(66)**
	[<0.001]	[<0.001]
Pell eligible (N = 16,204)	0.096	358
	(0.011)**	(72)**
Pell ineligible (N = 3,521)	0.064	301
	(0.011)**	(74)**
	[0.045]	[0.579]
New student (N = 5,607)	0.097	313
	(0.013)**	(69)**
Returning student (N = 14,117)	0.087	362
	(0.012)**	(76)**
	[0.571]	[0.629]
<30 credits earned (N = 12,763)	0.092	318
	(0.010)**	(60)**
30 or more credits earned (N = 6,961)	0.085	399
	(0.018)**	(114)**
	[0.730]	[0.527]
Dependent student (N = 8,125)	0.076	179
	(0.012)**	(34)**
Independent student (N = 11,599)	0.097	451
	(0.012)**	(83)**
	[0.213]	[0.003]
All subgroups		
Test of equality (*p* -value)	<0.001	<0.001
Test of joint significance (*p* -value)	<0.001	<0.001

Notes: 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. 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, EFC, and baseline cumulative credits and cumulative GPA.

Table 6: Characteristics of CCA Students by Response to Treatment

Characteristic	$E[X \mid AT]$	$E[X \mid C]$	$E[X \mid NT]$	Tests of equality (p-value): $E[X \mid C]=E[X \mid AT]$	$E[X \mid C]=E[X \mid NT]$	$E[X \mid C]=E[X \mid AT \cup NT]$	$E[X \mid AT]=E[X \mid NT]$
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

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 = compliers (students induced to borrow by a nonzero offer), NT = never-takers (students who do not borrow regardless of treatment group assignment).

Table 7: OLS and IV Estimates of the Impact of Nonzero Loan Offers on Attainment: 2015-16 Academic Year

	(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 (Reduced Form) Estimates					
1 [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					
1[Offered loan]	0.255	0.371	0.063	0.003	0.034
	(0.134)+	(0.154)*	(0.021)**	(0.006)	(0.013)**
	[0.149, 0.386]	[0.293, 0.520]	[0.050, 0.072]		
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)**
	[1.501, 3.805]	[2.995, 5.139]	[0.503, 0.719]		
E. IV Estimates					
Amount borrowed ($1k)	0.627	0.910	0.155	0.008	0.084
	(0.304)*	(0.398)*	(0.061)*	(0.016)	(0.034)*
	[0.375, 0.940]	[0.724, 1.267]	[0.125, 0.181]		
Observations	11,774	11,774	11,774	11,774	11,774

Notes: 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, D, and E contain IV estimates of the impact of being offered a nonzero loan (C), loan take-up (D), or $1000 borrowed (E) on the specified outcome; assignment to the treatment group serves as the excluded instrument. Ranges in brackets reflect lower and upper bounds accounting for possible selection on enrollment. 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, EFC, and baseline cumulative credits and cumulative GPA.

Table 8: Heterogeneity in the Impact of Amount Borrowed on Attainment: 2015-16 Academic Year

	(1) Credits attempted	(2) Credits earned	(3) GPA	(4) Degree receipt	(5) Standardized TE
Subgroup					
No outstanding debt	0.315	1.601	0.255	0.037	0.265
	(0.870)	(1.234)	(0.157)	(0.039)	(0.157)+
Has outstanding debt	0.751	0.595	0.108	-0.005	0.109
	(0.217)**	(0.270)*	(0.057)+	(0.016)	(0.056)+
	[0.627]	[0.425]	[0.380]	[0.321]	[0.435]
Pell eligible	0.719	1.102	0.201	-0.001	0.094
	(0.335)*	(0.467)*	(0.080)*	(0.015)	(0.038)*
Pell ineligible	0.252	0.105	-0.029	0.042	-0.034
	(0.709)	(0.924)	(0.083)	(0.050)	(0.081)
	[0.547]	[0.332]	[0.047]	[0.419]	[0.561]
New student	0.233	0.175	0.083	0.129	0.045
	(0.700)	(0.795)	(0.103)	(0.110)	(0.057)
Returning student	0.713	1.077	0.166	0.009	0.096
	(0.323)*	(0.470)*	(0.076)*	(0.022)	(0.041)*
	[0.534]	[0.329]	[0.518]	[0.925]	[0.466]
<30 credits earned	0.687	0.782	0.121	-0.017	0.038
	(0.395)+	(0.500)	(0.078)	(0.010)	(0.037)
30 or more credits earned	0.672	1.129	0.184	0.037	0.122
	(0.415)	(0.669)+	(0.089)*	(0.045)	(0.065)+
	[0.979]	[0.687]	[0.598]	[0.247]	[0.259]
Dependent student	0.039	0.712	0.209	0.013	0.076
	(1.043)	(1.222)	(0.186)	(0.042)	(0.101)
Independent student	0.809	1.007	0.141	0.006	0.091
	(0.263)**	(0.396)*	(0.059)*	(0.017)	(0.035)**
	[0.475]	[0.819]	[0.762]	[0.884]	[0.890]
All subgroups					
Test of equality (*p* -value)	0.962	0.581	0.446	0.485	0.803
Test of joint significance (*p* -value)	0.014	0.152	0.388	0.568	0.273

Notes: Enrolled CCA students who were randomly assigned before October 15, 2015. IV estimates of the impact of amount borrowed ($1k) 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, EFC, and baseline cumulative credits earned and cumulative GPA.

Table 9: The Impact of Borrowing on Attainment: 2016-17 Academic Year

	(1) Reenrolled at CCA	(2) Transfer to 4-year public	(3) Received any degree
A. Control group mean	0.54	0.06	0.21
B. OLS (Reduced Form) Estimates			
1[Assigned to treatment group]	-0.010	0.010	0.005
	(0.009)	(0.005)*	(0.007)
C. IV estimates			
1[Offered loan]	-0.012	0.012	0.006
	(0.011)	(0.005)*	(0.008)
D. IV estimates			
1[Borrowed]	-0.123	0.114	0.058
	(0.105)	(0.051)*	(0.082)
E. IV estimates			
Amount borrowed ($1k)	-0.030	0.028	0.014
	(0.026)	(0.013)*	(0.019)
Observations	11,774	11,774	11,774

Notes: 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. Panels C, D, and E contain IV estimates of the impact of being offered a nonzero loan (C), loan take-up (D), or $1000 borrowed (E) 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. Regressions include controls for strata, randomization month, EFC, and baseline cumulative credits earned and cumulative GPA.