

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

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Abstract

We experimentally test the impact of student loan “nudges” on community college students’ borrowing decisions and subsequent educational attainment. We find that students are biased towards borrowing the amount listed in their financial aid award letters, even though this amount does not affect students’ choice sets. Students randomly assigned to receive a nonzero loan offer were 40 percent more likely to borrow than those who received a \$0 loan offer. Neither fall nor spring enrollment was affected by loan offers, but students induced to borrow by a nonzero offer earned significantly more credits and higher GPAs. An additional \$1000 in loans led to 0.9 additional credits earned and a 0.16 GPA increase in the first year. Given that nearly one quarter of U.S. college students are offered \$0 in loan aid, our results indicate the potential to achieve large gains in educational attainment by reforming the choice architecture around borrowing.

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1 Introduction

Undergraduate enrollment in U.S. colleges grew by more than 30 percent since the year 2000, and roughly 45 percent of undergraduates attend a 2-year college. Only 20 percent of students in recent cohorts earned a college credential within 150 percent of expected time to graduation.¹ While financial aid has been shown to help low-income students enter and complete college (e.g. Deming and Dynarski 2010), the design of federal student aid programs may hinder students' ability to take advantage of these resources. In particular, a growing literature suggests that student decisions are distorted by behavioral biases including debt aversion (Field 2009; Caetano et al. 2011), cognitive costs associated with complexity (Dynarski and Scott-Clayton 2006; Bettinger et al. 2012), issues of self-control (Cadena and Keys 2013), framing effects (Pallais 2015), and opt-in costs (Marx and Turner 2015).

Institutional decisions that affect the choice architecture around students' borrowing decisions may exacerbate students' behavioral biases. For example, financial aid award letters frequently "offer" a particular loan amount that is unrelated to students' actual loan eligibility.² Loan offers could affect students' borrowing decisions through anchoring (e.g. if students perceive the listed amount as a recommendation) or by altering students' beliefs about their federal loan eligibility. Thus, non-binding financial aid award letter offers may nudge students into borrowing more or less than is right for their circumstances. Nudging students to borrow more could increase educational attainment if students are debt averse or face trade-offs between working and studying (e.g., Lochner and Monge-Naranjo 2012; Avery and Turner 2012). However, it is also possible that such nudges could cause students to take on additional debt without generating offsetting human capital.

We study the effect of student loan nudges on borrowing and educational attainment with a field experiment at a large community college. Students were randomly assigned to receive a loan offer of either zero or an amount equal to \$3500 for students with "freshman" status and \$4500 for "sophomores."³ Students randomly assigned to receive a nonzero loan offer were 40 percent more likely to borrow than those who received a \$0 offer. Furthermore, loan amounts exhibit anchoring: among borrowers, students randomly assigned to receive a nonzero loan offer were more likely to borrow exactly the offered amount than any other amount. We test for heterogeneity in these effects by student resources, dependency status, class standing, and past experience of borrowing or attending the same school. We find evidence of positive impacts of nonzero offers on borrowing across all groups, with the largest changes in borrowing occurring among students with outstanding debt and among Pell Grant eligible (i.e., low-income) and independent (i.e., nontraditional)

¹Associate's degree-seeking students, 2009 entry cohort (National Center for Education Statistics 2014, Table 326.20).

²A student's dependency status and class standing determines the student's overall federal loan eligibility, while the composition of loans depends on unmet need. Section 2 provides additional details.

³The community college we worked with classifies students as freshmen if they have accumulated less than 30 credits.

students.

Although loan offers did not affect students' initial enrollment decisions in fall 2015 or reenrollment in the spring 2016 semester, they generated sizable gains along other dimensions of educational attainment. We use the random assignment of loan offers as an instrument for the amount borrowed and show that, on average, students who were induced to borrow by a nonzero loan offer earned significantly more credits and higher grade point averages (GPAs). Specifically, an additional \$1000 of borrowing induced by the experimental nudge resulted in a student earning 0.9 additional credits and a 0.16 higher GPA over the 2015-16 academic year. These short-run effects are comparable to the gains produced by other experimental interventions, including those involving student incentive payments and additional academic supports (e.g., Scrivener et al. 2012; Mayer et al. 2015), but at substantially lower cost if the majority of loans are repaid. If the government seeks to increase educational attainment, then student loans appear more cost-effective than the most promising interventions at community colleges. Using existing estimates of the labor market gains from community college (e.g., Jepsen et al. 2014), the one-year gains in credit completion are worth more on average than the present value of induced debt for any discount rate below 5.1 percent.

Outstanding student loan debt in the U.S. has grown steadily over the past decade, reaching \$1.26 trillion in 2016 (Federal Reserve Bank of New York 2016). 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.⁴ Such policies range from opting out of federal loan programs entirely to offering all students \$0 in loan aid. As of 2014, 238 community colleges had opted out of federal loan programs, leaving close to one million students without access to federal loan aid (Cochrane and Szabo-Kubitz 2014). Roughly half of all community college students who have access to federal loans attend a college that offers all students \$0 in loan aid (Marx and Turner 2015). Schools may try to limit student loan debt concerns over students' ability to repay their debt and a desire to avoid sanctions that the Department of Education places on schools with high cohort default rates (CDRs).⁵ Although such sanctions can result in loss of access to federal student aid programs, these penalties are rarely applied to community colleges.⁶

⁴According to the 2012 National Postsecondary Student Aid Study (NPSAS), 70 percent of community college students who applied for federal student aid faced a cost of attendance that exceeded their total resources (including grants, loans, work-study, and personal resources). Among four-year public and nonprofit undergraduate aid applicants, 58 and 60 percent had unmet need. Authors' calculations using PowerStats (<https://nces.ed.gov/datalab/powerstats>).

⁵A school's CDR is equal to the share of federal loan borrowers who default within three years of entering repayment. Schools with CDRs exceeding 30 percent for three consecutive years lose their eligibility to provide students with federal Pell Grants and federal loans, while schools with CDRs exceeding 40 percent in any single year lose access to federal loans. However, 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. Prior to 2012, CDRs were measured on a two year basis with lower sanction thresholds. See the Federal Student Aid Cohort Default Rate Guide for additional details.

⁶Only one community college was sanctioned between 2002 and 2013 and it avoided federal aid loss through a successful appeal. In September 2015, two additional community colleges were sanctioned but have appealed (see <http://www.ed.gov/news/press-releases/cohort-default-rate-continues-drop-across-all-higher-ed-sectors>).

Despite the attention paid to student debt, there is limited evidence on the extent to which loan aid affects outcomes in and beyond college. Two observational studies estimate the impact of *access* to federal loan aid using variation in community colleges' decisions to participate in federal loan programs and find large, positive impacts of loan access on educational attainment (Dunlop 2013; Wiederspan 2016).⁷ These studies rely on the identifying assumption that colleges' decisions of when and whether to participate in the student loan program are random, whereas we implement random assignment of non-binding loan offers within a college. Nonetheless, these results suggest that student loans have a positive effect on the educational attainment of students whose borrowing decisions depend on access to federal student loans.⁸

A handful of four-year institutions have also recently implemented interventions designed to reduce borrowing. Starting in 2013, the Indiana University system rolled out a number of concurrent programs aimed at reducing debt and increasing four-year graduation rates.⁹ In the two years following implementation, aggregate borrowing fell by 16 percent (Kennedy 2015). Similarly, starting in fall 2012, Montana State University sent letters to students with high outstanding debt that included an incentivized invitation to participate in a one-on-one counseling session with a certified financial counselor. Using a differences-in-differences design, Schmeiser et al. (2015) estimate that borrowing among targeted students decreased by 33 percent. Barr et al. (2016) find that Community College of Baltimore loan applicants who were randomly assigned to a text messaging campaign and access to assistance from financial aid counselors borrowed less.¹⁰ In contrast, evidence from loan information experiments in the U.S. and the Netherlands suggest that information alone does not significantly alter students' borrowing decisions, even in cases when it increases understanding of loan terms and programs (Booij et al. 2012; Darolia 2016). Our findings suggest that the point in time when students make borrowing decisions is an especially important one because even small nudges at this time can have effects on borrowing that are as large as, if not larger than, initiatives that are more expensive and broader in scope.

Furthermore, it is not clear that across-the-board reductions in student loan debt is the objective that

⁷Dunlop (2013) finds that students in participating colleges are more likely to transfer to a four-year school, with some subgroups also being more likely to obtain bachelor's degrees. Wiederspan (2016) estimates that college-wide participation in federal loan programs leads Pell Grant-eligible students to attempt an additional 19 credits in their first year.

⁸A larger set of studies examine the effects of grant aid on enrolled students' attainment. Using data on public college students in Ohio, Bettinger (2004) finds positive impacts of Pell Grant aid on reenrollment. Goldrick-Rab et al. (2012) show that Pell Grant recipients randomly assigned to receive an additional \$1000 in grants experienced a 2 to 4 percentage point increase in persistence. In Marx and Turner (2015), we provide suggestive evidence that when the fixed cost of borrowing does not bind, Pell Grant aid increases credits attempted. Denning (2016) finds positive effects of grant aid when combined with access to larger borrowing limits.

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

¹⁰Students in the treatment group received text messages for one month. Students were told via text that they did not have to borrow the amount that was offered to them in their award letters, that loan payments after college would depend on how much was borrowed, and the federal limits on lifetime borrowing.

institutions and policy makers should pursue. Estimated returns to college completion suggest that borrowing to finance college is optimal for the average young adult (Avery and Turner 2012). There is evidence that low-income students already avoid loans, even subsidized loans that do not accrue interest in college (Cadena and Keys 2013). In previous work, we provide evidence that \$0 loan offers generate a fixed cost of borrowing, resulting in a substantial reduction in loan take-up and potential reductions in attainment (Marx and Turner 2015). Thus, initiatives designed to reduce borrowing may also lower attainment or encourage students to use more costly sources of debt. Our findings are consistent with these concerns in that we show that policies that lower borrowing may harm some students by reducing human capital accumulation.

Our study also contributes to a growing literature on the importance of choice architecture design in other contexts. Madrian and Shea (2001), Choi et al. (2006), Chetty et al. (2014), and Bernheim et al. (2015) show that default options matter for decisions related to investment, saving, and 401(k) participation. In our setting, the loan offer is not a true default because all students still must opt into borrowing, but the offer may nudge students to take up loans. Nudges have been shown to affect financial choices across a variety of settings (e.g. Duflo et al. 2011; Allcott and Rogers 2014). Within the context of postsecondary education, information and its framing have been shown to affect both college-going and post-college outcomes. For example, a trivial reduction in the price of college applications has increased the number of colleges to which students apply (Pallais, 2015), and providing information to high-achieving, low-income students has had success in matching these students to high-quality colleges (Hoxby and Turner 2015; Andrews et al. 2016). Research has also shown significant effects of information and assistance with the federal student aid application (Bettinger et al. 2012), text messages about steps to obtain financial aid and advance in college (Castleman and Page. 2015, 2016), and mailings to unemployed workers with information about financial aid programs and the return to college (Barr and Turner 2015). Distinguishing features of our study include involvement of a general population of community college students, measurement of the impacts of policy choices currently being made by many community colleges, and the triviality of the costs of making changes to loan offers on the parts of both colleges and students.

The remainder of this paper proceeds as follows: in Section 2, we explain how students and institutions interact with the federal student loan program. We describe the setting, context, and design of our experiment in Section 3, while Section 4 details our methodology. We present estimated impacts of nonzero loan offers on borrowing outcomes in Section 5, while Section 6 presents impacts on attainment. Section 7 presents cost-benefit analyses from the perspectives of the government and the student, and in Section 8 we discuss the implications of our findings for institutional and governmental policies related to student loans.

2 Federal Student Loans in the U.S.

U.S. college students who are enrolled at least part-time are eligible to borrow through federal loan programs. The terms of federal loan aid depend on a student's course load, dependency status, class standing, and unmet need. 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 expected family contribution (the federal government's measure of need) and total grant aid from all sources, determines her eligibility for subsidized loans. Students classified as freshmen are eligible for subsidized loans equal to the lesser of remaining need and \$3500.¹¹ Community college students who have earned at least 30 credits and are classified as sophomores are eligible for an additional \$1000 in subsidized loans.¹² Dependent first-year students can borrow an additional \$2000 in unsubsidized loans while independent students can borrow an additional \$6000.¹³ Even 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). Subsidized loans do not accrue interest until six months after a student leaves school. Unsubsidized loans begin accruing interest immediately after disbursement, but interest rates are fixed over the lifetime of the loan.¹⁴ Dependent undergraduate students face a lifetime eligibility limit of \$31,000 in federal loans, while the limit for independent undergraduate students is \$57,500.

Although the federal rules described in the previous paragraph dictate the amounts of subsidized and unsubsidized loans for which a college student is eligible, colleges can decide how much loan aid to offer in financial aid award letters.¹⁵ In all cases, not borrowing is the default option in that students who take no further action do not receive loans, regardless of the amount offered. Students who receive nonzero loan offers must still accept the offer and complete federal requirements (entrance counseling and a Master Promissory Note) in order to receive their desired aid. Students who do not receive a loan offer (or receive a \$0 offer) can still request a loan, with the specific request process varying across institutions. Nearly all four-year institutions offer students the maximum amount of loan aid for which they are eligible. In contrast, community colleges are divided in how much loan aid they offer to students.

We collected information on loan offer policies for all community colleges that participate in federal loan

¹¹Subsidized loan eligibility is also reduced when a student's remaining lifetime eligibility for subsidized loans (\$23,000) is less than these amounts.

¹²Students enrolled in four-year institutions are classified as upper-level students are eligible for an additional \$2000 in subsidized loans. Regardless of credit accumulation, community college students cannot be classified as upper-level.

¹³An 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. See <https://studentaid.ed.gov/sa/fafsa/filling-out/dependency> for details.

¹⁴Subsidized and unsubsidized loans disbursed after July 1, 2015 had an interest rate of 4.29%.

¹⁵The Department of Education and college financial aid administrators call this process "packaging".

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

3 The Experiment

The experiment was implemented at “Community College A” (CCA), an anonymous community college, during the 2015-16 academic year.¹⁸ As shown in Panel A of Table 2, this school’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 equaled \$3136 versus \$3249 nationwide. However, CCA has a significantly larger student body than the average community college, with a 12-month full-time equivalent enrollment (FTE) of approximately 19,000 compared to 4,300 across all community colleges. This difference is by design, in that one of the requirements for being recruited into the experimental sample was a sufficiently high enrollment to obtain a useful sample size. Financial aid receipt is similar between CCA students and community college students nationwide. For instance, 45 percent of CCA students received Pell Grant aid

¹⁶An earlier version of this table appeared in an appendix to Marx and Turner (2015). The current version of the table includes the packaging practices of 792 community colleges, representing 92 percent of community colleges that participate in federal loan programs.

¹⁷Authors’ calculations using data from the 2012 NPSAS (via Powerstats). We limit the sample to community college students with at least \$1000 in unmet need (and thus would qualify for subsidized loans) and who are eligible for a federal Pell Grant. Students in this sample who forgo federal loans are 9 percentage points (33 percent) more likely to use a credit card to pay for college and are 4 percentage points (7 percent) more likely to work while in school than those who take-up federal loans.

¹⁸We ran a similar experiment at a second community college (“Community College B” or CCB). However, the experimental sample of students was much smaller than anticipated, and the resulting estimates were not sufficiently precise to rule out the possibilities of either large effects or no effect. Appendix C contains details on the CCB experiment.

and 26 percent received federal loans in 2013-14, compared to 41 and 19 percent of students at the average community college. Finally, students at CCA have substantially lower completion rates. Only 1 percent of CCA students completed a credential within 100 percent of the expected time to degree (e.g., 2 years for an associate degree), compared to 16 percent of students at the average community college. Only 8 percent of CCA students complete a degree within 200 percent of the expected time, compared to 31 percent of community college students nationwide.

Prior to the experiment, CCA was considering making changes to its loan packaging procedures. During the 2014-15 academic year, CCA offered loans to all students with less than \$25,000 in outstanding federal loan debt. All prospective students who listed CCA on their Free Application for Federal Student Aid (FAFSA) received information relating to their financial aid packages electronically via a web-based system. Nationwide, all first-time borrowers must complete federal entrance counseling and a master promissory note in order to receive requested federal student loans. In addition to these federal requirements, CCA required students to actively confirm that they wish to borrow and specify the amount of loan aid they would like via an electronic loan request form. CCA's loan eligibility criteria and application procedures were not altered for the experiment. CCA disburses all funds, including loans, 35 days after the start of the semester.¹⁹

3.1 Experiment design

The experiment entailed random assignment of loan offers to students. On a roughly daily basis starting in May 2015, the CCA financial aid office provided data for each batch of students for whom an award letter was to be generated the following day. Using these data, students were assigned to either the treatment group or the control group using randomization stratified by Expected Family Contribution (EFC) bins and all possible combinations of binary variables for new vs. returning, freshman vs. sophomore, dependent vs. independent, and with vs. without outstanding student loan debt.²⁰

Loan-eligible students assigned to the treatment group received a nonzero loan offer in their award letter, while loan-eligible students assigned to the control group received a \$0 offer.²¹ Figure 1 displays screenshots from CCA's web page showing the financial aid package, including examples of both treatment and control student offers at the bottom of the page. These offers were pure nudges: they did not affect students'

¹⁹If a student does not complete the federal and CCA borrowing requirements or does not enroll in at least 6 credit hours until after the scheduled disbursement date, their loan is disbursed within two days after these criteria are met.

²⁰Break points for stratification by EFC were determined within combination of the binary variables so as to roughly equate the number of students per strata based on data from the two preceding years. A separate category was created for the considerable number of students with a zero EFC, and the break points always included the \$5198 threshold for Pell Grant eligibility in the 2015-16 academic year.

²¹Students who were not eligible for loan aid did not have loan aid mentioned in the award letter, regardless of their assignment to treatment or control groups. Students with no unmet need who were assigned to the control group also did not receive an award letter that mentioned federal loans. Unfortunately, we do not observe the specific amount of subsidized and unsubsidized loans offered to treatment group members, as we learned during the experiment that when a student accepts a loan, CCA's information systems change the amount in the "offer" field to the amount the student choose to accept.

eligibility for federal loans or the requirement that the student actively accept a nonzero loan (and complete federal requirements) to obtain a loan. Figure 2 displays a screen shot of the online form that students filled out to accept a loan. The amount of the loan offered to treatment-group students depended on the student's class standing; in keeping with their loan packaging practice in the prior year, treatment group freshmen received \$3500 loan offers, while sophomores received \$4500 offers. Students with unmet need exceeding these amounts were offered the full amount as subsidized loans, while those with lower unmet need received a combination of subsidized and unsubsidized loan offers in their award letters. Students in the control group were informed of their eligibility for federal loans and the process for requesting a loan via email.²² CCA clearly displayed information on student loan eligibility on its website and all students that complete a FAFSA are sent information on their anticipated eligibility for Pell Grants and federal loans from the U.S. Department of Education.

3.2 Data and descriptive statistics

Our experimental sample includes students who were randomly assigned before February 2, 2016. Table 3 displays the characteristics of this group by treatment group assignment. We test for differences in treatment and control group members' class standing, past enrollment at CCA, dependency status, amount of outstanding student loan debt, resources (EFC), Pell Grant aid, work study aid, other grant aid (i.e., federal non-Pell, state, and institutional grants), other resources (i.e., private and employer-provided aid), baseline cumulative credits, and baseline cumulative GPA (for returning students). The first column displays the control group mean and standard deviation (in parentheses) of each characteristic. The second column displays the difference between the treatment-group mean and control-group mean and the standard error of this difference (in parentheses). None of the differences in characteristics between treatment and control groups are statistically significant. While this is to be expected for the variables in the first five rows, which were employed in the 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 (Table 3). Sixty-four percent of CCA students

²²The email contained general information on federal student aid programs and a link to the online loan request form. The paragraph relating to loan eligibility read as follows:

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

are classified as freshmen and 59 percent are independent. Furthermore, the average CCA student has outstanding student loans worth about \$4,200 and a GPA of 2.73. Using data from the publicly available 2012 National Postsecondary Student Aid Study (NPSAS), we estimate that 60 percent of community college students nationwide have freshman status for loan eligibility, 58 percent are independent, average outstanding debt is \$4,400, and the average GPA is 2.42.²³ The mean values of \$6,589 EFC and Pell Grants worth \$3,628 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.

4 Empirical Framework

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

$$D_i = \beta T_i + \boldsymbol{\eta} \mathbf{X}_i + \nu_i \quad (1)$$

$$Y_i = \pi D_i + \boldsymbol{\phi} \mathbf{X}_i + \epsilon_i \quad (2)$$

In equation (1), D_i is a dummy variable equal to one if a nonzero loan was offered in the financial aid award letter of student i , T_i is a dummy indicating assignment to the treatment group, and \mathbf{X}_i includes a vector of strata fixed effects and a linear term in student expected family contribution (EFC). To reduce residual variation we include controls for cumulative credits earned and GPA at baseline as well as the month of random assignment. OLS estimates of β will represent the extent to which CCA’s loan offers were correlated with the randomly-assigned treatment status. This correlation was imperfect because students who were assigned to the treatment group were not offered a loan if their past borrowing exceeded \$20,000 or if their financial aid package was completed after their enrollment decision and they had not enrolled in the six credits necessary to be eligible for a loan. Given such discrepancies between treatment status and offer status we include among our estimates the “intent-to-treat” (ITT) effect of loan offers, i.e. the reduced-form OLS estimates of the impact of treatment group assignment on these outcomes.

We estimate the “treatment-on-the-treated” (TOT) impact of receiving a nonzero loan offer with instrumental variables (IV) models in which we instrument for receipt of a nonzero loan offer with assignment to the treatment group. In this case, equation (2) represents the second stage. Estimates of the coefficient π will represent the TOT effect of a nonzero loan offer on the borrowing or attainment outcome Y_i . Even

²³Statistics generated by PowerStats (<https://nces.ed.gov/datalab/>).

if assignment to nonzero loan offers among students in the treatment group was not random, the use of the treatment assignment dummy T_i as an instrument isolates variation in offers that was randomized. To test for heterogeneous treatment effects, we jointly estimate IV models for each subgroup, which generates estimates equivalent to those produced by a single-equation, fully-interacted model. In all analyses, standard errors are clustered by strata.

We also use assignment to the treatment group as an instrument for loan aid. This is because increases in the amount borrowed, rather than the loan offer, are most likely what drives educational attainment responses. To do this we replace the binary variable D_i in equations (1) and (2) with a continuous measure of the amount borrowed. In Appendix B, we discuss the monotonicity assumption required to obtain local average treatment effect (LATE) estimates from this model and use results from our experiment to bound any potential bias in our estimates that would arise if some students borrow less when receiving a nonzero loan offer than they would have had they received a \$0 loan offer.

4.1 Adjustments for multiple hypothesis testing

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

We address concerns over the multiple hypothesis testing in two ways. First, we generate a standardized index of treatment effects following Finkelstein et al. (2012) and the online appendix of Kling et al. (2007). This index represents the weighted average of the estimated treatment effect for each separate outcome, jointly estimated via seemingly unrelated regression, with weights equal to the standard deviation of the specific outcome in the control group. Standard errors are calculated using the delta method. Second, for each separate attainment outcome, we calculate familywise p -values using the Westfall and Young (1993) free stepdown procedure.²⁴ The significance of estimated effects on the standardized treatment index will

²⁴This procedure involves four steps, described below.

Step 1: For each attainment outcome $k = 1, \dots, 4$, 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_k^i , the p -value from the test of the null for outcome k . We then compute the adjusted sample p -value $q_k^i = \min\{p_k^i, \dots, p_4^i\}$. Step 3: For each outcome k , we calculate the share of random samples for which the p -value generated from the original data exceeds the adjusted sample p -value: $\bar{p}^k = \frac{1}{N} \sum_{i=1}^N 1\{q_k^i \leq p^k\}$. Step 4: The final familywise p -value for each outcome k is $\tilde{p}^k = \max\{\bar{p}^1, \dots, \bar{p}^k\}$.

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

5 Impacts of Loan Offers on Borrowing

5.1 Visual evidence

Figures 3 through 6 provide a visual preview of our findings for borrowing outcomes. In Figure 3, each bar represents the the probability of borrowing for students who were assigned to receive a nonzero loan offer (“treatment group” members) and those assigned to receive a \$0 offer (“control group” members). Vertical capped lines represent 95 percent confidence intervals. Students in the treatment group were 7 percentage points more likely to borrow, a 30 percent increase relative to the control group borrowing rate of 23 percent. This difference is statistically significant at the 1 percent level. In Figure 4, we examine the probability of borrowing the exact amount that was included in nonzero loan offers ($\$P = \3500 for students with “freshman” status and \$4500 for those with “sophomore” status). A significantly greater percentage of treatment group borrowers took up the exact amount offered compared to control group members (11 versus 7 percent, respectively). This finding is consistent with anchoring increasing the likelihood that students borrow exactly the amount offered whether or not that amount is greater than zero.

Next, we compare how much is borrowed by students assigned to each treatment status (Figure 5), looking across all students (Panel A) and within the sample of students who borrowed (Panel B). Not surprisingly, given the increased rate of loan take-up, treatment group members borrow approximately \$280 more than control group members, a 26 percent increase from the control group mean. However, once we condition on loan take-up, control group borrowers take on significantly higher debt than students assigned to the treatment group (an approximately 5 percent increase from the treatment group mean amount of \$4551). The negative effect on loan aid among borrowers is consistent with the mechanism described in Marx and Turner (2015), whereby deviating from the offered amount generates a fixed cost. Control group students will only pay the fixed cost of deviating if their desired amount is sufficiently greater than their default offer of \$0, while treatment group students whose desired amount is greater than \$0 may be induced to accept these lower amounts.

We further explore how borrowing decisions are influenced by default loan offers by comparing the distributions of loans taken up by borrowers. We recenter the amount borrowed around $\$P$ - the amount students would have been offered had they been assigned to the treatment group (again, \$3500 for freshmen,

\$4500 for sophomores). Figure 6 displays the borrowing distributions of both the treatment and control group. Assignment to the treatment group increases the likelihood of borrowing at almost every point in the distribution. However, students who received a nonzero offer are substantially more likely to borrow exactly the amount they were offered, suggesting that the offered amount serves as an anchor or reference point for at least some portion of students.²⁵

5.2 Nonzero loan offers increase borrowing

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

Given the imperfect compliance with treatment assignment, we use IV models to generate TOT estimates of the effect of loan offers on students’ borrowing decisions, as described in Section 4. Receipt of a nonzero loan offer resulted in a 9 percentage point increase in the probability of student loan take-up (Table 4, column 2). This response represents a 39 percent increase in borrowing relative to control students’ mean borrowing rate of 23 percent. Furthermore, a nonzero loan offer increased the average amount borrowed (including zeroes) by \$348 (a 32 percent increase relative to the control group mean). Both estimates are statistically significant at the 1 percent level.

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

5.3 Heterogeneity

We test whether receipt of a nonzero loan offer had heterogeneous impacts across different student subgroups. We look at subgroups defined by past experience of borrowing (any outstanding debt versus no outstanding

²⁵We observe heaping at many \$500 intervals in both the treatment and control groups. Even in the control group, such heaping is especially pronounced at \$P because this amount corresponds to the maximum subsidized loan for students with unmet need of at least \$P. However, when we limit our sample to students whose subsidized loan eligibility falls below \$P, we observe excess bunching at \$P only in the treatment group.

debt), student resources (Pell Grant eligible versus ineligible), prior CCA enrollment CCA (new versus returning), class standing (freshman versus sophomore status), and dependency status. To do so, we jointly estimate IV models for each separate subgroup. Table 5 contains these results.

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

Loan offers also have heterogeneous effects by dependency status and Pell Grant eligibility. Pell Grant eligible students' loan take-up appears to be more responsive to nonzero loan offers than that of ineligible students ($p = 0.045$). Independent students borrowed 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.²⁶ We find no evidence of significant differential impacts of nonzero loan offers on conditional borrowing or borrowing exactly \$P (i.e., \$3500 for freshmen and \$4500 for sophomores) along these characteristics.

6 Impacts on Attainment

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

²⁶Following Abadie (2003), we estimate that the only characteristics on which compliers differed significantly from the average student were independent status (67 percent of compliers versus 56 percent of the enrolled students) and outstanding debt (63 percent of compliers versus 43 percent of enrolled students).

students who could have adjusted their credit hours in response to the loan offer, although our results are robust to including these students.²⁷

We observe attainment outcomes for the 2015-16 academic year including credits attempted, credits earned, GPA, degree receipt, and the standardized treatment index constructed from all four of these variables. Control group means and standard deviations are displayed in Panel A of Table 6. We examine the effect of treatment group assignment and being offered a loan on these outcomes (Panels B and C, respectively) and then estimate the achievement gains experienced by students who were induced to increase borrowing by the nudge of a nonzero loan offer (Panel D).

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

Estimated impacts of nonzero loan offers on attainment outcomes are quite similar to ITT estimates of the effect of treatment assignment (Panel B). Since loan offers only affect borrowing for a subset of students, it is not surprising that nonzero offers are associated with relatively small changes in educational attainment. Under the assumption that students do not borrow less when offered a loan (or, alternatively, homogeneous treatment effects of loan aid on attainment), the IV estimates in Panel C will represent the local average treatment effect of additional borrowing on educational attainment. An additional \$1000 borrowed leads to a statistically significant ($p < 0.05$) 0.63 increase in credits attempted over the academic year. Impacts on credits earned are even larger, with an additional \$1000 in loan aid leading to gains of 0.91 credits earned over the academic year ($p < 0.05$). Loan aid increases course performance as well. Students induced to borrow an additional \$1000 earned significantly higher GPAs in each semester, with a cumulative increase of 0.16 GPA points ($p < 0.05$). Loan aid did not increase the likelihood of degree receipt by the end of the academic year. This finding is not surprising given that most students in our sample required more than one year of full-time attendance to complete their degree programs.²⁸

As with the borrowing outcomes, we test for heterogeneous effects of borrowing on educational attainment over the 2015-16 academic year (Table 7). In all but one case, the estimates for the subgroups on the two

²⁷Appendix Table A.1 shows that characteristics of the treatment and control groups are balanced in this restricted sample. In this sample the first stage coefficient is 0.835 with standard error of 0.033 and F statistic of 640.

²⁸Approximately 96 percent of CCA students in the experimental sample were pursuing associate degrees that required 60 to 70 credits. Most had accumulated fewer than 30 at the start of the fall 2015 semester. Only a quarter of the sample started the fall semester with at least 40 credits.

sides of each binary distinction are not statistically distinguishable from each other. Estimated impacts on the standardized treatment index are significantly greater than zero for a number of subgroups, but as a whole, we cannot reject the hypothesis that treatment effects are jointly insignificant across all subgroups ($p = 0.273$). Additional experiments are needed to obtain more precise estimates of the effect of borrowing on attainment and to determine whether some groups benefit more than others from additional loan aid.²⁹

7 Cost-Benefit Analysis

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

We compare our estimated impacts to impacts of other RCTs targeting community college students' attainment, including the City University of New York (CUNY) Accelerated Study in Associate Programs (ASAP) and the Performance-Based Scholarship interventions. Both interventions involved student-level random assignment and were evaluated by MDRC. CUNY community college students assigned to the ASAP program were subject to a suite of requirements, additional supports, and financial assistance.³⁰ The long-run effects of the ASAP program included a doubling of the likelihood of graduation within three years of program entry (Schmeiser 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 (Schmeiser et al. 2015). The Performance-Based Scholarship (PBS) Demonstration was implemented at several community colleges nationwide. Students were randomly assigned to be eligible to earn up to \$1500 per semester in incentive payments if they met specific academic goals.³¹ Across all sites, 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 for the most successful site (Borrow et al. 2014).

Our estimated effect of 0.9 additional credits earned per \$1000 expenditure is comparable to the magnitude of estimated effects from the ASAP and PBS programs. However, in our setting, the additional \$1000 is

²⁹We also test for heterogeneity by degree program. Specifically, we compare students pursuing an academic associate degree (e.g., Associate of Arts, Associate of Science) that is designed for students who wish to transfer their first two-years of liberal arts coursework to a four-year institution to students pursuing a terminal vocational associate degree (e.g., Associate of Applied Science, Associate of Applied Business, Associate of Technical Studies, etc.), excluding the small number of students in certificate programs. We find no significant differences in the impact of loan offers or the amount borrowed by program type (Appendix Table A.2).

³⁰Specifically, students were required to enroll in at least 12 credits per semester (the threshold for full-time attendance), attend special seminars and engage in intensive advising. Students received a tuition waiver to cover unmet need, free use of textbooks, and subsidies for transportation expenses. Students in the program took block scheduled classes and had support to take winter and summer semester courses. See Schmeiser et al. (2015) for additional details.

³¹The specific population eligible for participation and the structure and size of incentives varied across experimental sites. See Mayer et al. (2015) for additional details.

lent to the student rather than spent. Long-run costs to colleges and government may be substantially lower if the additional loan aid is repaid. If we assume that students induced to borrow by the experimental nudge will default on their loans at the same rate as other CCA borrowers, the federal government's expected cost per \$4000 loan is \$444. This suggests a cost-benefit ratio of 8.1 additional credits per \$1000, far exceeding the returns of ASAP and PBS.³²

We also assess whether borrowing is financially beneficial for students. Because the behavioral effects of nudges often appear to defy standard models of rational choice, inferring the full welfare effects of nudges may require strong assumptions or additional experiments (Bernheim and Rangel 2009; Handel 2013; Allcott and Kessler 2015; Allcott and Taubinsky 2015). For example, in our setting students may respond to the offered amount of loan aid because of inattention to their borrowing options or because they take the offered amount as a recommendation, and the welfare implications of these alternative models can differ considerably (Bernheim et al. 2015). However, while it is difficult to quantify the effect of the treatment on student welfare, we can describe the economic trade-offs implied by the observed effects on borrowing and educational attainment.

The simplest assessment of the costs and benefits of offering nonzero loan aid compares induced loans to the educational attainment they enable. Making such a comparison requires translating the attainment gains into financial terms. We use estimates from Jepsen et al. (2014), who use an individual fixed-effects approach to estimate the effect of community college credits and credentials on earnings and employment for two cohorts of students enrolling in the Kentucky Community & Technical College System.³³ Partialling out the effects of completing credentials, each additional credit generates a \$5.6 to \$14 increase in quarterly earnings (in 2008 dollars). Applying the estimates of Jepsen et al. (2014) according to the gender mix of compliers in our experiment, a student induced to take up a \$4000 unsubsidized loan by the experimental nudge would see annual earnings increase by \$169.39 in 2016 dollars. If these earnings effects grow at a nominal rate of 3 percent, and students repay loans at the interest rate of 4.29 percent that prevailed in 2015-16, then the loans are financially beneficial if future cash flows are discounted at any rate below 5.1 percent. Given the relatively large earnings gains due to credential receipt estimated by Jepsen et al. (2014), CCA students' financial returns to borrowing will increase if loans lead to increases in completion in the long-run.

³²CCA 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.

³³An earlier study finds similar effects for an older population of displaced, high tenure workers (Jacobson et al. 2005).

8 Conclusions

We experimentally test the effect of nudges on community college students' borrowing decisions. Students randomly assigned a nonzero loan were approximately 40 percent more likely to borrow, resulting in an aggregate increase of \$3.6 million in loans received by the treatment group. A simple presentational nudge has an effect on borrowing that is similar to or larger than that of more complicated and costly interventions aimed at reducing student borrowing.³⁴ The randomized trial provides compelling evidence that borrowing could be reduced at little administrative cost by offering students no loan aid in their award letters. However, the reduction in borrowing must be weighed against the higher average attainment generated by borrowing. Our estimates suggest that students induced to borrow \$4000, the midpoint between the two amounts offered, earned 3.6 additional credits and improved their GPAs by 0.6 points. These are short-run attainment effects over one academic year, and there may be larger or smaller effects in the long-run.

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

U.S. federal policy could also take these results into account. Government guidance to colleges can reference the trade-off estimated in these experiments and suggest alternative choice architectures. The Department of Education currently recommends that colleges present financial aid to students using a standardized "shopping sheet," and it has been suggested that this sheet be made mandatory.³⁵ Because the standard shopping sheet includes a line for loan offers, mandating its use would force colleges to nudge their students towards a particular level of borrowing, whether zero or nonzero. It is likely that no fixed amount of loan offer is best for all student, because larger amounts will nudge some students toward over-borrowing, while smaller amounts will nudge some students toward lower educational attainment. The federal govern-

³⁴We obtain, for example, a 27 percent reduction in aggregate borrowing compared to the 16 percent reduction found by Kennedy (2015) and a 25 percent reduction in average amounts borrowed per student compared to the 33 percent of Schmeiser et al. (2015).

³⁵See <http://www2.ed.gov/policy/highered/guid/aid-offer/index.html> for additional details.

ment should allow, and ideally promote, further experimentation on framing of student loans so as to tailor financial aid offers to the needs of each student.

Multiple decision-theoretic models could explain the strong effects of loan offers on borrowing. In one class of models, the loan offer may establish a reference point and hence anchoring, either because students interpret the offer as a recommendation or because they exhibit loss aversion relative to the level of resources they would have if they accepted the offered amount. Alternatively, students may incorrectly infer that the offered amount is the maximum amount of loan aid for which they are eligible. While students are informed of their eligibility prior to seeing the loan offer, inattention or confirmatory bias could lead to such incorrect inferences. The next stage of our research will employ treatments designed to clarify psychological mechanisms and to improve the precision of our estimates of the effect of loans on educational attainment. Ideally, these treatments will lead the way to more tailored solutions that account for characteristics and circumstances and help each student obtain his or her ideal amount of student loan aid.

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Figures and Tables

Figure 1: Screen Shots From Financial Aid Web Pages

A. Information presented to both treatment and control group members

Employee Registration and Student Services Personal Information Faculty Services Finance

Search Go [RETURN TO MENU](#) [SITE MAP](#) [HELP](#) [EXIT](#)

My Award Package By Aid Year 2015-2016 Processing Year

General Information **Award Overview** Resources/Additional Information

Print

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

Need Calculation		Cost of Attendance	
Cost of Attendance	\$13,504.00	Books and Supplies	\$1,700.00
Estimated Family Contribution	\$1,700.00	Institutional Fees	\$140.00
Initial Need	\$11,804.00	Personal Expenses	\$1,140.00
Outside Resource	\$0.00	Room and Board	\$6,000.00
Need	\$11,804.00	Out of County Tuition & Fees	\$3,364.00
		Transportation	\$1,160.00
		Total:	\$13,504.00

Expected Enrollment Status
Full Time

Cumulative Loan Information as of 23-APR-2015

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

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

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

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

B. Award information presented to treatment group members

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

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

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

C. Award information presented to control group members

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

Fund	Fall 2015		Spring 2016		Total
	Status	Amount	Status	Amount	
Federal Pell Grant	Accepted	\$2,063.00	Accepted	\$2,062.00	\$4,125.00
Direct Subsidized Loan	Offered	\$0.00	Offered	\$0.00	\$0.00
Totals		\$3,818.00		\$3,817.00	\$7,635.00

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

Figure 2: Online Loan Request Form

Subsidized Loan Only Request

Subsidized Loan: The interest is waived on subsidized loans while you are enrolled in school at least half-time.
Unsubsidized Loan: The interest is accumulated on unsubsidized loans after funds are applied to your student account. However, payments on interest can be postponed while you are enrolled in school at least half-time and during grace periods.

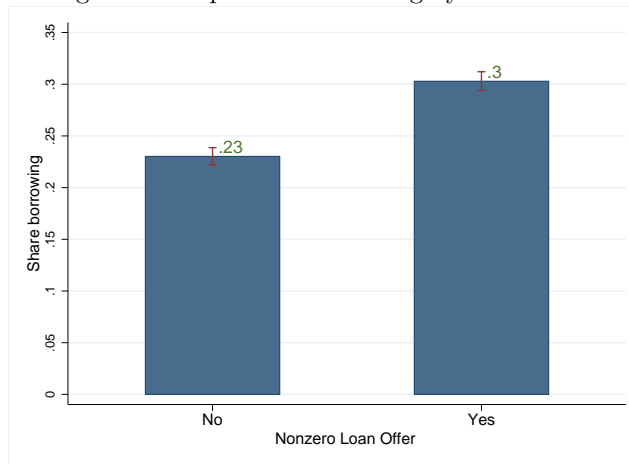
*Check here if you would like your loan processed as subsidized only, otherwise it will be processed as both subsidized and unsubsidized based on the amount you requested and your eligibility. **By answering yes to this question your loan will be processed for either \$3,500 or \$4,500 dependent on your grade level.** Yes No

Loan Information

Enter the amount you wish to borrow via a Federal Direct Loan. The maximum amount you may be eligible for is 10,500. If you wish to borrow \$4,500 then enter 4500 in the box.

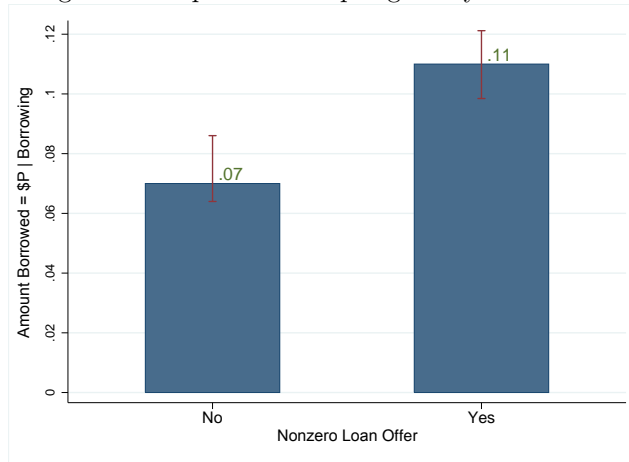
If you would like to get more information about eligibility, click here

Figure 3: Proportion Borrowing by Treatment



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

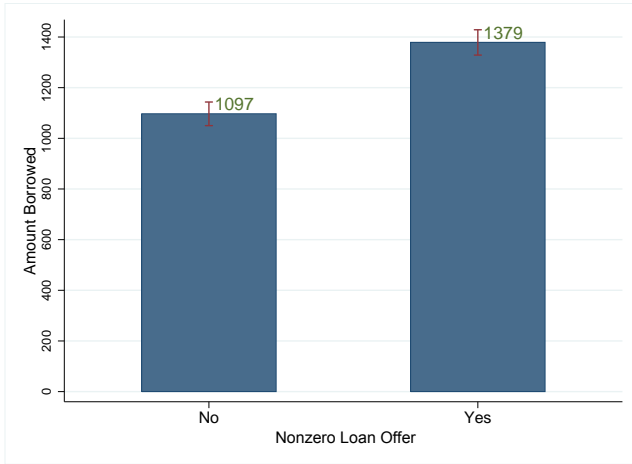
Figure 4: Proportion Accepting \$P by Treatment



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

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

A. Unconditional

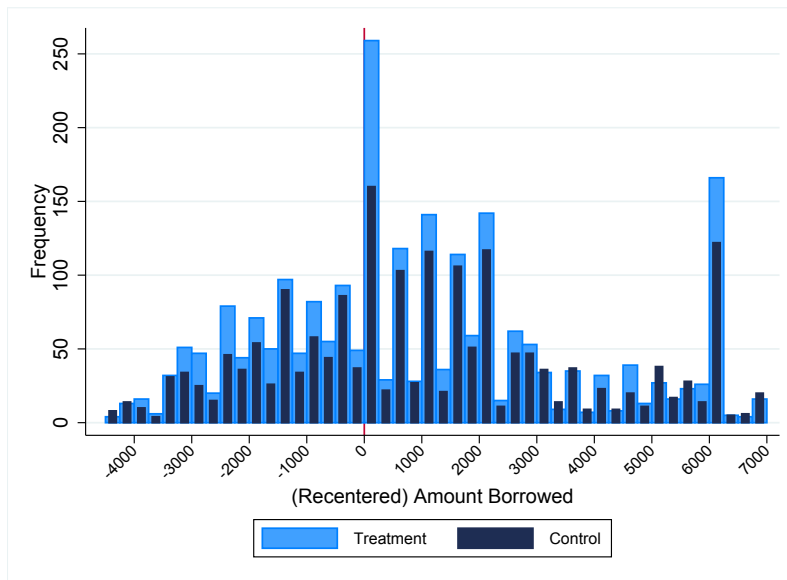


B. Conditional on Borrowing



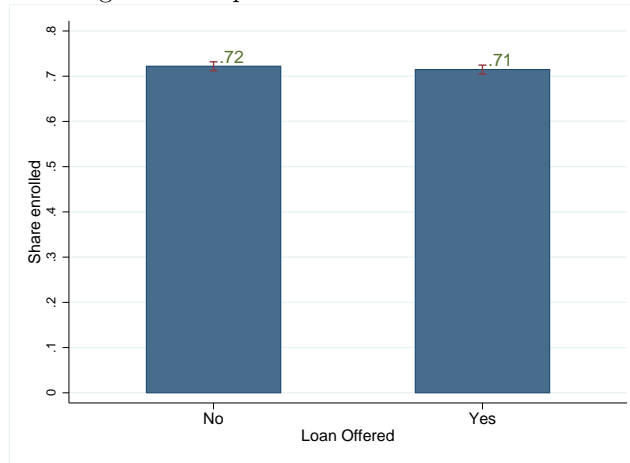
Notes: Students (Panel A) or borrowers (Panel B) randomly assigned before February 2, 2016. Each bar indicates the average amount borrowed by students in the treatment and control groups. Capped vertical lines represent 95 percent confidence intervals.

Figure 6: Distribution of (Recentered) Amount Borrowed



Notes: Borrowers randomly assigned before February 2, 2016. Amount borrowed recentered around the amount a student would have received had they been assigned to the treatment group (\$3500 for freshmen and \$4500 for sophomores).

Figure 7: Proportion Enrolled in Fall 2015



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

Table 1: Characteristics of Community Colleges by Loan Packaging Procedures

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

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

Table 2: Characteristics of Community College A and National Averages

	College A	All community colleges
<i>A. Costs (2014-15)</i>		
Published tuition and fees		
In-district	3,136	3,249
In-state	3,953	3,375
Out-of-state	7,468	7,547
Other COA components		
Books & supplies	1,700	1,389
Other expenses	1,840	3,880
Room & Board (if living off campus)	6,000	7,916
<i>B. Student body (2013-14)</i>		
12-month FTE	18,783	4,335
Percent receiving aid	45	57
Percent receiving Pell Grants	45	41
Percent receiving federal loans	26	19
<i>C. Outcomes (full-time, degree-seeking students in 2010 cohort)</i>		
Number	2,085	753
100% graduation rate	0.01	0.16
150% graduation rate	0.04	0.26
200% graduation rate	0.08	0.31

Notes: Two-year public schools participating in Title IV federal student aid programs that reported data to the Integrated Postsecondary Education Data System (IPEDS). Graduation rates in Panel C pertain to associate degree and certificate-seeking students.

Table 3: Descriptive Statistics

Characteristic	Control mean (sd)	Treatment effect (se)
<30 credits earned	0.64 (0.10)	0.0003 (0.0004)
New	0.28 (0.09)	0.0006+ (0.0003)
Independent	0.59 (0.10)	0.0003 (0.0004)
Outstanding loan debt	4170 (31)	0.1 (62)
Expected family contribution (EFC)	6633 (349)	217 (698)
Pell Grant aid	3628 (10)	11 (20)
Work study aid	46 (2)	2 (4)
All other grant aid	120 (3)	0.04 (5)
Total other resources	31 (2)	-0.3 (3)
Cumulative credits (N=13,964)	29.5 (0.1)	0.01 (0.25)
Cumulative GPA (N = 12,281)	2.73 (0.01)	-0.01 (0.02)
Test of joint significance (p -value)		
excluding cumulative credits, GPA		0.995
including cumulative credits, GPA		0.763
Number of observations	9,671	9,681

Notes: Students who were randomly assigned before February 2, 2016. Cumulative credits and GPA only measured for students with prior attendance at CCA. All other grant aid includes non-Pell federal grants, state grants, and institutional grants. Total other resources includes private and employer provided aid.

Table 4: The Impact of Nonzero Loan Offers on Borrowing

	(1) Offered loan	(2) Any borrowing	(3) Amount borrowed	<u>Conditional Outcomes</u>	
				(4) Amount	(5) Amt. = \$P
<i>A. OLS estimates</i>					
Assigned to treatment group	0.812 (0.030)**				
<i>B. IV estimates</i>					
Offered loan		0.090 (0.009)**	348 (58)**	-146 (81)+	0.037 (0.009)**
Observations	19,724	19,724	19,724	5,254	5,254

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

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

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

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

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

	(1) Credits attempted	(2) Credits earned	(3) GPA	(4) Degree receipt	(5) Standardized treatment effect
<i>A. Control mean</i>					
	17.28 (7.65)	12.93 (8.75)	2.26 (1.27)	0.09 (0.29)	
<i>B. OLS Estimates</i>					
Assigned to treatment group	0.213 (0.117)+ {0.158}	0.310 (0.132)* {0.067}	0.053 (0.018)** {0.021}	0.003 (0.005) {0.637}	0.029 (0.011)**
<i>C. IV Estimates</i>					
Offered loan	0.255 (0.134)+	0.371 (0.154)*	0.063 (0.021)**	0.003 (0.006)	0.034 (0.013)**
<i>D. IV Estimates</i>					
Loan aid (\$1k)	0.627 (0.304)*	0.910 (0.398)*	0.155 (0.061)*	0.008 (0.016)	0.084 (0.034)*
Observations	11,774	11,774	11,774	11,774	11,774

Notes: Enrolled students who were randomly assigned before October 16, 2015. Control group means and standard deviations (in parentheses) in Panel A. Panel B contains OLS estimates of the impact of assignment to the treatment group on the specified outcome; family-wise p -values (adjusted to account for multiple hypothesis testing) in brackets. Panels C and D contain IV estimates of the impact of being offered a nonzero loan (C) or receiving an additional \$1000 in loan aid (D) on the specified outcome; assignment to the treatment group serves as the excluded instrument. See Section 4.1 for description of standardized treatment effects. Robust standard errors, clustered by strata, in parentheses; ** $p < 0.01$, * $p < 0.05$, + $p < 0.1$. All regressions include controls for strata, randomization month, and baseline cumulative credits and cumulative GPA.

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

	(1) Credits attempted	(2) Credits earned	(3) GPA	(4) Degree receipt	(5) Standardized TE
<i>Subgroup</i>					
No outstanding debt	0.315 (0.870)	1.601 (1.234)	0.255 (0.157)	0.037 (0.039)	0.265 (0.157)+
Has outstanding debt	0.751 (0.217)** [0.627]	0.595 (0.270)* [0.425]	0.108 (0.057)+ [0.380]	-0.005 (0.016) [0.321]	0.109 (0.056)+ [0.435]
Pell eligible	0.719 (0.335)*	1.102 (0.467)*	0.201 (0.080)*	-0.001 (0.015)	0.094 (0.038)*
Pell ineligible	0.252 (0.709) [0.547]	0.105 (0.924) [0.332]	-0.029 (0.083) [0.047]	0.042 (0.050) [0.419]	-0.034 (0.081) [0.561]
New student	0.233 (0.700)	0.175 (0.795)	0.083 (0.103)	0.129 (0.110)	0.045 (0.057)
Returning student	0.713 (0.323)* [0.534]	1.077 (0.470)* [0.329]	0.166 (0.076)* [0.518]	0.009 (0.022) [0.925]	0.096 (0.041)* [0.466]
<30 credits earned	0.687 (0.395)+	0.782 (0.500)	0.121 (0.078)	-0.017 (0.010)	0.038 (0.037)
30 or more credits earned	0.672 (0.415) [0.979]	1.129 (0.669)+ [0.687]	0.184 (0.089)* [0.598]	0.037 (0.045) [0.247]	0.122 (0.065)+ [0.259]
Dependent student	0.039 (1.043)	0.712 (1.222)	0.209 (0.186)	0.013 (0.042)	0.076 (0.101)
Independent student	0.809 (0.263)** [0.475]	1.007 (0.396)* [0.819]	0.141 (0.059)* [0.762]	0.006 (0.017) [0.884]	0.091 (0.035)** [0.890]
<i>All subgroups</i>					
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 students who were randomly assigned before October 16, 2015. IV estimates of the impact of an additional \$1000 of loan aid 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 4.1 for description of standardized treatment effects. Brackets contain p -values from a test of the equality of prior two subgroup estimates. Robust standard errors, clustered by strata, in parentheses; ** $p < 0.01$, * $p < 0.05$, + $p < 0.1$. Regressions also include controls for strata, randomization month, and baseline cumulative credits earned and cumulative GPA.

Appendix A: Additional Tables and Figures

Table A.1: Descriptive Statistics, Attainment Sample

Characteristic	Control mean (sd)	Treatment effect (se)
<30 credits earned	0.61 (0.10)	-0.008 (0.005)+
New	0.26 (0.08)	-0.004 (0.005)
Independent	0.56 (0.10)	-0.001 (0.005)
Outstanding loan debt	4200 (38)	-80 (77)
Expected family contribution (EFC)	6316 (454)	571 (914)
Pell Grant aid	3351 (15)	27 (30)
Work study aid	74 (4)	1 (8)
All other grant aid	193 (4)	0.3 (9)
Total other resources	50 (3)	-3 (6)
Cumulative credits (N=11,705)	26 (0.1)	0.04 (0.26)
Cumulative GPA (N = 8,630)	2.8 (0.01)	-0.01 (0.02)
Test of joint significance (p -value)		
excluding cumulative credits, GPA		0.828
including cumulative credits, GPA		0.672
Number of observations	5,920	5,855

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

Table A.2: Heterogeneity in the Impact of Loan Aid by Degree Program

	(1) Offered loan	(2) Any borrowing	(3) Amount borrowed	(4) Std. attainment TI
<i>A. OLS (assigned to treatment group)</i>				
AA/AS (N = 3,156)	0.817 (0.036)**			
AAS (N = 8,109)	0.843 (0.032)** [0.036]			
<i>B. IV (offered loan)</i>				
AA/AS (N = 3,156)		0.104 (0.017)**	453 (92)**	
AAS (N = 8,109)		0.108 (0.011)** [0.834]	408 (78)** [0.579]	
<i>C. IV (amount borrowed \$1k)</i>				
AA/AS (N = 3,156)				0.127 (0.063)*
AAS (N = 8,109)				0.063 (0.042) [0.430]
Test of joint sig (<i>p</i>-value):	<0.001	<0.001	<0.001	0.027

Notes: Enrolled associate degree-seeking students who were randomly assigned before October 16, 2015. Each cell contains estimates from a separate regression. Panel A contains OLS estimates of the impact of assignment to treatment on receiving a nonzero loan offer. Panel B contains IV estimates of the impact of a nonzero loan offer on borrowing outcomes, where assignment to treatment serves as an instrument for receipt of a nonzero loan offer. Panel C contains IV estimates of the effect of an additional \$1000 of loan aid on 2015-16 academic year attainment, represented by standardized index of treatment effects (see Section 4.1 for details). The *p*-value from the test of equality of subgroup coefficients in brackets below estimates. Robust standard errors, clustered by strata, in parentheses; ** $p < 0.01$, * $p < 0.05$, + $p < 0.1$. Regressions also include controls for strata, cumulative credits earned and cumulative GPA before the beginning of the fall 2015 semester.

Appendix B: A Note on Monotonicity

In order for IV estimates of the effect of additional loan aid on attainment to represent a LATE, we must assume monotonicity: that assignment to treatment weakly increases borrowing for all students. We primarily expect that switching from a offer of zero to a nonzero offer would induce borrowing by students who would not have borrowed otherwise, an unambiguous increase. However, effects such as anchoring could also theoretically result in a student borrowing less if given a nonzero loan offer than if given a \$0 offer.³⁶

Without monotonicity, Estimates of the reduced form impact of assignment to treatment (T_i) on attainment outcome $Y_i(T_i, D_i(T_i))$, where $D_i(T_i)$ represents the amount borrowed, will produce a weighted average of the treatment effects for compliers and defiers (Angrist et al. 1996):

³⁶For instance, if the cost of not taking the offered amount is the same for \$0 and nonzero offers, and a student's preferred loan amount is close to, but above than the nonzero offer, she would take-up her desired debt if given a \$0 offer, but shift down to the offered amount if given a nonzero offer.

$$E [Y_i (1, D_i (1)) - Y_i (0, D_i (0))] =$$

$$\underbrace{E [Y_i (1, D_i (1)) - Y_i (0, D_i (0)) | D_i (1) > D_i (0)]}_{\text{effect of adtl loan aid on attainment | complier}} \cdot \underbrace{\Pr [D_i (1) > D_i (0)]}_{\text{pr(complier)}} \\ - \underbrace{E [Y_i (0, D_i (0)) - Y_i (1, D_i (1)) | D_i (1) < D_i (0)]}_{\text{effect of adtl loan aid on attainment | defier}} \cdot \underbrace{\Pr [D_i (1) < D_i (0)]}_{\text{pr(defier)}}$$

Thus, IV estimates can be written as:

$$\frac{E [Y_i (1, D_i (1)) - Y_i (0, D_i (0))]}{E [D_i (1) - D_i (0)]} = \\ \frac{E [\Delta^c Y_i | c] \cdot \pi_c - E [\Delta^d Y_i | d] \cdot \pi_d}{E [\Delta^c D_i | c] \cdot \pi_c - E [\Delta^d D_i | d] \cdot \pi_d}$$

Where $\Delta^c Y_i = Y_i (1, D_i (1)) - Y_i (0, D_i (0))$, $\Delta^d Y_i = Y_i (0, D_i (0)) - Y_i (1, D_i (1))$, $\Delta^c D_i = D_i (1) - D_i (0)$, $\Delta^d D_i = D_i (0) - D_i (1)$, $\pi_c = \Pr [D_i (1) > D_i (0)]$, and $\pi_d = \Pr [D_i (1) < D_i (0)]$.

If the parameter of interest is the effect of treatment on compliers, and the treatment of interest is binary, Angrist et al. (1996) show that the bias for the IV estimator takes the following form:

$$\hat{\beta}^{IV} = \beta^{LATE} - \left\{ \frac{\pi_d}{\pi_c - \pi_d} \right\} \{ E [\Delta^d Y_i | d] - E [\Delta^c Y_i | c] \}$$

The first term in brackets goes to zero as the share of the population that are compliers relative to the share that are defiers increases. The second term in brackets goes to zero as treatment effects for compliers and defiers become more similar. Thus, the bias in the IV estimator for the parameter of interest will be small if there are relatively few defiers or relatively homogeneous effects of loan aid on attainment. It is not possible to sign the bias in the IV estimator without assumptions over the sign of the second term in brackets (whether increases in loan aid have larger effects on the attainment of defiers or compliers).³⁷

In our setting, we can approximate an upper bound for the share of the population that are defiers under the assumptions that all students induced to reduce borrowing due to a nonzero loan offer borrow exactly

³⁷In the case of a continuous treatment:

$$\hat{\beta}^{IV} = \beta^{LATE} - \left\{ \frac{\pi_d}{\pi_c \cdot E [\Delta^c D_i | c] - \pi_d \cdot E [\Delta^d D_i | d]} \right\} * \left\{ E [\Delta^d Y_i | d] - E [\Delta^c Y_i | c] \left(\frac{E [\Delta^d D_i | d]}{E [\Delta^c D_i | c]} \right) \right\}$$

If $E [\Delta^d D_i | d] = E [\Delta^c D_i | c] = E [\Delta D_i]$, we obtain the same expression as was the case with a binary treatment.

the amount offered and that all students induced to increase borrowing would have otherwise not borrowed. As discussed in the following section, among borrowers, treatment group members are approximately 4 percentage points more likely to take up the exact amount of the default offer. Defiers in our setting are students who would have borrowed had they received a \$0 loan offer. Since 23 percent of control group students borrowed, at most 0.9 percent of sample members are defiers (e.g., 4 percent of 0.23). Treatment group members were 7 percentage points more likely to borrow than control group members. Therefore, we estimate an upper-bound for the ratio $\frac{\pi_d}{\pi_c - \pi_d}$ that is equal to $\frac{0.9}{7 - 0.9} = 0.13$. Thus, even if the effects on defiers' outcomes was twice the effect on compliers' outcomes, the IV estimates would only be biased by 13 percent of the true local average treatment effect.

Appendix C: Community College B Experiment

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

C.1 CCB Experiment Design

In the year prior to the intervention (2014-15), CCB students were not offered loan aid. CCB only provides financial aid packages to students after they have registered for courses and sends students hard-copies of their financial aid package via mail. In addition to federal requirements (i.e., entrance counseling and completion of a master promissory note), CCB students who wish to borrow must complete several additional steps. These include filling out a budget, determining their expected future salary upon graduation and calculating estimated loan payments, and attending a one-on-one meeting with a college counselor.³⁸

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

CCB underperformed in terms of expected sample size. Based on past enrollment of degree-seeking students, we projected a sample size of roughly 8,000 students. However, the surprisingly small number

³⁸The budgeting worksheet requires students to estimate their fall and spring semester education-related expenses, financial resources, and unmet need.

of CCB students who completed a FAFSA and were eligible for subsidized loans reduced the number of students eligible to be included in random assignment to 2,221. At present time, we only observe borrowing outcomes for 2,102 of these students. As shown in Table C.1, predetermined characteristics are balanced between CCB treatment and control groups.

C.2 Results

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

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

C.3 Figures and Tables

Figure C.1: CCB Financial Aid Award Letters

A. Award information presented to treatment group members

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

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

	Fall	Spring	Summer	Total
Federal Pell Grant	2888.00	2887.00	0.00	5775.00

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

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

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

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

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

B. Award information presented to control group members

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

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

	Fall	Spring	Summer	Total
Federal Pell Grant	2888.00	2887.00	0.00	5775.00

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

Table C.1: Descriptive Statistics

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

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

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

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

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

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

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

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

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

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

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