Paralysis by Analysis? Effects of Information on Student Loan Take-up*

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Abstract

Can relevant information influence student borrowing? In a field experiment with a large community college, we send emails about federal student loans to students who have received financial aid offers but have not made a borrowing decision. A treatment reminding students that they need not borrow the maximum amount of available loan aid has no effect. Treatments referencing amounts borrowed by recent graduates shift students from borrowing the maximum amount to not borrowing. Consistent with the hypothesis that students experience cognitive overload when presented with multiple loan amounts, the response is largest among low-performing students and arises from inaction. JEL: D12, D14, D91, I22, H31.

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1 Introduction

Outstanding student loan debt in the U.S. has grown steadily over the past decade, exceeding $1.5 trillion by the first quarter of 2020 (Federal Reserve Bank of New York 2020). While estimated returns to college completion suggest that borrowing to finance college is optimal for the average young adult (Avery and Turner 2012), the returns to educational investments are uncertain, and some students might take on debt without generating offsetting increases in human capital or post-college earnings.[1] Concerns that students are making uninformed borrowing decisions have led to calls for increased counseling, information, and guidance.[2] The effectiveness of such interventions will depend on the extent to which additional information about loan options, terms, and future payments can help improve borrowers' decisions and financial well-being. However, existing research provides mixed evidence on whether students benefit from information about borrowing options, outstanding loan debt, and expected repayments.

To help fill this gap, we present evidence from a field experiment at a large, anonymous community college (hereafter, LACC). Our experimental treatments were intended to help students with their borrowing decisions by providing relevant information via email. As is the case for students at other community colleges, most LACC students either forgo federal student loans or choose an amount exactly equal to the nonbinding loan "offer" listed in their financial aid award letters, suggesting they are inattentive to the fact that they can choose a different loan amount or that they view the listed amount as a recommendation (Marx and Turner 2019). The treatments were designed to test these explanations. Our first

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[1]In recent years, however, over half of all borrowers had not paid down even $1 of their debt two years after entering repayment, and 20 percent had defaulted on their loans within five years of repayment entry (Looney and Yannelis 2015).

[2]For example, the 2017 Understanding the True Cost of College Act, the 2019 Student Aid Improvement Act, and the 2019 College Affordability Act would standardize the way that colleges inform students of their loan options in financial aid award letters. The 2018 PROSPER Act would have allowed institutions to require students to complete additional counseling before borrowing through federal loan programs, while the 2019 Know Before You Owe Act would require institutions to inform student borrowers of their likely income and monthly student loan payments (based on program of study and estimated loan debt at graduation) and include a statement that the student can borrow less than the amount of loan aid displayed in their financial aid award letter.

treatment provided a reminder that students can borrow an amount other than the listed offer. Two additional treatments referenced the unconditional ($800) or conditional ($3000) mean annual amount borrowed by past LACC graduates, which allows for a test of whether students shift borrowing towards these reference points.

Our results indicate that students understand they have a choice over their loan amount but are easily overwhelmed by this choice. The reminder of the option to borrow less than the offered amount had no effect on loan take-up or the amount borrowed, suggesting that students were already aware of this possibility. Neither reference-point treatment led students to borrow the referenced amount. Instead, students who received either reference point treatment were 11 percent less likely to borrow at all. The reference-point treatments also caused an equally sized reduction in the probability that students borrowed the maximum available loan, suggesting that the treatments induced students who would have borrowed the maximum amount to instead not borrow.

Students' response to the reference-point treatments suggests that the mention of a second possible loan amount increased the perceived complexity of the borrowing decision and the corresponding cognitive cost of choosing how much to borrow. This response is consistent with evidence that complexity in other parts of the federal student aid application process reduces the effectiveness of financial aid (Dynarski and Scott-Clayton 2006; Bettinger et al. 2012) and a broader literature on the effects of "cognitive overload" on decisions along a variety of dimensions (Eppler and Mengis 2004). When it comes to other financial decisions, such as saving for retirement (Goldin et al. 2019) or charitable giving (Edwards and List 2014), research shows that reducing complexity related to the intensive margin of the choice (e.g., how much to contribute to a retirement account or how much to donate) can also affect the extensive margin (e.g., whether to save for retirement or donate). We find the same result for a marginal change in the other direction, with an increase in complexity on the intensive margin of how much to borrow causing a reduction on the extensive margin of whether to borrow at all.

Several results support the cognitive-overload explanation. First, we test whether the reference-point treatments affected the likelihood that a student made an active decision to decline the loan offer. We show that the reduction in borrowing is not explained by an increase in the number of students actively declining their loan offer, but rather an increase in the likelihood of making no decision (and thus receiving the default loan of $0). Next, we test for heterogeneous effects by student characteristics. Students who are unfamiliar with student loans, lack guidance when making a borrowing decision, are prone to using heuristics (e.g., complying with nudges), or have lower cognitive ability should be most at risk of cognitive overload. We find heterogeneity in treatment effects that is consistent with all of these predictions. Estimated treatment effect sizes are larger for new students and significantly larger for independent (nontraditional) students and those who have borrowed in the past. Finally, using past academic performance as a proxy for cognitive ability, we find that students whose baseline grade point average (GPA) is below the median for their level of credit accumulation are significantly more affected by the reference-point treatments than their above-median-GPA peers.

Our paper contributes to a body of evidence that 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. 2019), issues of self-control (Cadena and Keys 2013), framing effects (Pallais 2015; J. Evans et al. 2018; Abraham et al. 2020), opt-in costs (Marx and Turner 2018), default bias (Cox et al. 2018), and salience (Marx and Turner 2019). Many first-year students either do not realize that they have taken out student loans or cannot recall the amount that they have borrowed (Akers and Chingos 2014), and only 20 percent of borrowers attending public institutions could answer two "loan literacy" questions correctly (Anderson et al. 2018).[3] In a nationally representative survey, most student loan recipients stated that they did not calculate the size of their future monthly payments before

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[3]The questions tested for understanding of the consequences of defaulting on the loan and for knowledge of income-driven loan repayment plans.

deciding whether and how much to borrow (Lusardi et al. 2016).

Our focus on community college students' borrowing was motivated by the relatively high student loan default rates and low completion rates in this sector, which has served up to half of all college students in recent years (Looney and Yannelis 2015; National Center for Education Statistics 2019). Past interventions intended to help inform community college students' borrowing decisions have produced mixed results. Barr et al. (2019) show that text messages about loans and access to one-on-one counseling reduced the amount borrowed and educational attainment among community college students who had applied for loan aid and increased the probability of student loan default.[4] Changing the amount of loan aid listed in the financial aid award letter without changing the choices available to students significantly affects borrowing and attainment (Marx and Turner 2019).[5] In this study, we show that simply sending students an email showing average borrowing of past graduates reduced the amount borrowed by 13 percent. Based on the positive effects of student loans on academic attainment found in other studies (Dunlop 2013; Wiederspan 2016; Barr et al. 2019; Marx and Turner 2019), this result suggests that providing information intended to improve students' borrowing decisions may have unintended and adverse effects.[6]

The reduction in borrowing when students receive additional information is similar to choice overload - the theory that individuals may become overwhelmed when presented with

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[4]Treatment status included a variety of interventions, including the ability to ask for assistance from financial aid staff and text messages with information about loan terms (e.g., lifetime limits, future payments), encouragement to complete required financial aid forms, and, similar to our experiment, reminders of the option to borrow less than the offered amount of loan aid and references to smaller loan amounts. Our experiment allows us to isolate the effects of each of the reminder and reference messages.

[5]Interventions intended to reduce borrowing at bachelor's degree granting institutions have resulted in small to no changes in borrowing. 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 difference-in-differences design, Schmeiser et al. (2017) find that this intervention reduced the amount borrowed by only 2 percent, and had no effect on the probability of borrowing. Evidence from experiments in both the U.S. and the Netherlands suggest that student-loan-specific information alone does not significantly alter students' borrowing decisions, even when it increases students' understanding of loan terms and programs (Booij et al. 2012; Darolia and Harper 2018). These interventions provided information about the loan program or about the student's own outstanding debt, whereas our reference-point treatments describe the borrowing of others.

[6]Denning and Jones (forthcoming) find no evidence of increased attainment from an increase in the maximum amount a student can borrow.

too many options (Iyengar and Lepper 2000). Though our reference point emails did not actually expand students' choice sets, they may have expanded the perceived choice set by increasing the salience of the continuum of available loan amounts. In theoretical models, choice overload can arise from costs of searching for or evaluating options (Kuksov and Villas-Boas 2010) or from anticipated regret over a choice (Sarver 2008; Buturak and Evren 2017). Increasing the number of available options has been found to increase the likelihood that agents make inefficient or dominated choices over retirement plans and savings (Benartzi and Thaler 2001; Iyengar et al. 2004; Iyengar and Kamenica 2010) and health insurance plans (Heiss et al. 2010; Abaluck and Gruber 2011; Zhou and Zhang 2012).[7] Similar to our finding that seeing a second loan amount increases the likelihood of inaction, Shafir et al. (1993) find that subjects in a lab experiment are more likely to choose to wait for more information when given two purchase options than when given a single option. Meta-analyses by Scheibehenne et al. (2010) and Chernev et al. (2015) find heterogeneity in the effect of expanding options, with factors that appear to contribute to choice overload including choice set complexity and preference uncertainty. Both factors likely characterize students' choices of a specific dollar amount of loan aid needed for expenses to be incurred over the coming academic year.

The remainder of this paper proceeds as follows. In Section 2, we describe the student loan program and our experimental site. Section 3 details the design of our experiment, while Section 4 describes our empirical framework. We present results in Section 5, including the main effects of the emails on borrowing outcomes, heterogeneity in these effects, and estimates of effects on attainment. Section 6 concludes.

2   Student Financial Aid and LACC

Low-income college students in the United States are eligible for federal grants and loans. In order to access federal aid, prospective students must fill out the free application for federal

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[7]Evidence from lab experiments involving choice over hypothetical health insurance plans is also consistent with choice overload (Schram and Sonnemans 2011; Johnson et al. 2013; Kairies-Schwarz et al. 2014; Bhargava et al. 2017).

student aid (FAFSA) by providing information on their family income, assets, siblings, and other family members' college attendance. These inputs are fed through a complicated, nonlinear formula to determine a student's expected family contribution (EFC), the federal government's measure of ability to pay. Older students, those with dependents, and those who are married are classified as "independent" and need only include their own income and assets in the FAFSA, while "dependent" students' EFCs take into account the resources of both students and their parents. Eligibility for federal need-based grants, subsidized loans, and campus-based aid generally depend on EFC, either directly (as in the case of Pell Grant aid) or when combined with additional information (as in the case of work-study funding).

2.1 Federal student loans

All students who are enrolled at least part-time and have completed a FAFSA are eligible to borrow through federal loan programs. The primary source of federal loan aid for undergraduate students is the Direct Loan Program.[8] The terms of federal loan aid depend on a student's course load, dependency status, class standing, and unmet need. While students must attempt at least 6 credits to be eligible to borrow, above this threshold, the terms of borrowing do not explicitly depend on a student's course load. A student's unmet need, equal to her total cost of attendance (tuition, fees, and a cost of living allowance) minus her EFC and total grant aid from all sources, determines her eligibility for subsidized loans, which do not accrue interest while in school. Students classified as freshmen are eligible for subsidized loans equal to the lesser of remaining need and $3500.[9] Community college students who are classified as sophomores are eligible for an additional $1000 in subsidized loans.[10] Dependent

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[8]Student loan interest rates are pegged to the 10-year Treasury note interest rate plus 2.05 percent and are updated on an annual basis. Students in our study who had their loans disbursed before July 1, 2016 faced an interest rate of 4.29 percent, while those loans disbursed between July 1, 2016 and June 30, 2017 had an interest rate of 3.76 percent.

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

[10]Students enrolled in four-year institutions who are sophomores or juniors are classified as "upper-level" and are eligible for an additional $2000 in subsidized loans. Regardless of credit accumulation, community college students cannot be classified as upper-level. At most institutions, dependent students can borrow

undergraduate students face a lifetime eligibility limit of $31,000 in federal loans, while the limit for independent undergraduate students is $57,500.[11]

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. The Department of Education and college financial aid administrators call this process *packaging.* In all cases, not borrowing is the default: students who take no further action do not receive loans, regardless of the amount offered. Students who receive nonzero loan offers must still accept the offer, and new borrowers must also complete federal requirements (entrance counseling and a Master Promissory Note) in order to receive their desired aid.[12] 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. Community college students are roughly evenly split between colleges that offer loans and colleges that do not (Marx and Turner 2019).

2.2 LACC

We contacted LACC based on its relatively large student body. LACC had a 12-month full-time equivalent enrollment (FTE) of approximately 31,000 in 2014, compared to 4,300 at the average community college. Financial aid receipt is similar between LACC students and community college students nationwide. For instance, approximately 54 percent of LACC students received Pell Grant aid (compared to 41 percent nationwide) and 15 percent received

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an additional $2000 in unsubsidized loans, and independent students can borrow an additional $4000. At these institutions, students who do not qualify for subsidized loans can still borrow unsubsidized loans up to the overall maximum (e.g., $5500 for freshmen dependent students and $9500 for freshmen independent students). Unsubsidized loans begin accruing interest immediately after disbursement, but interest rates for both subsidized and unsubsidized loans are fixed over the lifetime of repayment.

[11]An undergraduate student is classified as independent if she will be over the age of 24 by the end of the calendar year in which she is enrolled, 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 in the military, or is a veteran.

[12]The Master Promissory Note is a legal document that states the student will repay her loan(s) and any accrued interest and fees to the U.S. Department of Education. See https://studentaid.gov/mpn/ for details.

federal loans (compared to 19 percent nationwide) in 2014.

LACC offers subsidized loans to all students with unmet need who are eligible for federal loan aid.[13] Prospective students who listed LACC on their FAFSA received information relating to their financial aid packages electronically via a web-based system. An example financial aid award letter from the year of the intervention appears in Figure 1. All students were offered the maximum amount of subsidized loan for which they were eligible. In addition to federal requirements, LACC required students to actively confirm that they wish to borrow and specify the amount of loan aid they would like - whether it be the amount offered or a different amount - via an electronic loan request form. New borrowers had to complete federal entrance counseling and a Master Promissory Note. Only a subset of LACC students were eligible for unsubsidized loans, and these students were required to complete and submit a request for the additional loan aid to the LACC financial aid office in person.[14] No LACC students used private student loans.[15]

3   The Experiment

The experiment was implemented prior to the start of the 2016-17 academic year and entailed random assignment of emails to students who had not yet chosen whether to borrow. Prior to the intervention, LACC students received information about their cost of attendance, available grant aid, and subsidized federal loan offers through the electronic financial aid

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[13]The practice of only offering subsidized loans is not limited to LACC. Marx and Turner (2018) report that about 6 percent of community colleges that make nonzero loan offers to all students only package subsidized loans. Many others package a "base amount" that is based on the subsidized loan maximum but includes unsubsidized loans as needed when a student's unmet need is less than this amount.

[14]During the year of the intervention and in prior years, LACC participated in a Department of Education initiative that allowed schools to limit unsubsidized borrowing for specific subsets of students. Students could determine their eligibility for unsubsidized loans and the process for requesting them through the LACC financial aid website but otherwise were not informed of this initiative. There was no change in this policy in the year of the intervention relative to prior years.

[15]Private student loans must be processed through the financial aid office and reported in the Integrated Postsecondary Education Data System. Nationwide, less than 2 percent of community college students had private student loans in 2016 (authors' calculations using the 2016 National Postsecondary Student Aid Study via PowerStats). Private loans entail a credit-worthiness requirement and/or require a cosigner, and most have higher interest rates than federal loans.

award letter shown in Figure 1. All students were offered the maximum amount of subsidized federal loan aid for which they were eligible. In late August 2016, the LACC financial aid office identified students who had not yet made a student loan choice.[16] These students were assigned to either the control group or one of three treatment groups. Randomization was stratified by 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.[17]

Table 1 displays the characteristics of students in the LACC experimental sample in comparison to the nationally representative sample of community college students from the 2016 National Postsecondary Student Aid Study (NPSAS) who were enrolled during the 2015-16 academic year. LACC students are similar to the nationally representative sample in terms of gender and class standing, but they are less likely to be white (36 versus 68 percent) and more likely to be black or Hispanic (43 versus 23 percent and 40 versus 24 percent, respectively). LACC students are substantially more likely to be eligible for a Pell Grant (92 versus 77 percent) and have lower average EFCs ($1301 versus $4325) but ultimately have similar levels of financial need.[18] LACC students had relatively low take-up of student loans, with a large difference at baseline due in part to the relatively high number of freshmen, but with a take-up rate of 0.16 in the fall semester compared to 0.25 nationally. LACC students earned lower academic-year GPAs (2.392 versus 2.759) but graduated at a rate similar to that of students in the national sample (15 versus 16 percent).

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[16]Students received award letters, including loan offers, when they applied for financial aid. The students in the experimental sample received their letters between June 14, 2016 and August 31, 2016, with a median date of July 24, 2016. They make up 50 percent of the students who received award letters. Other students in the population could be more or less responsive to the treatments.

[17]Break points for stratification by EFC were determined within combination of the binary variables so as to roughly equate the number of students per strata based on data from the two preceding years. A separate category was created for the considerable number of students with a zero EFC, and the break points always included the $5234 threshold for Pell Grant eligibility in the 2016-17 academic year.

[18]Financial need is equal to a student's total cost of attendance less EFC. Cost of attendance is the sum of tuition, fees, and estimated living expenses. Thus, Table 1 shows that LACC students faced a slightly lower cost of attendance compared to community college students nationwide (approximately $10,000 versus approximately $14,000, respectively). The measure of cost of attendance that we observe was determined prior to the intervention.

Figure 2 displays the email sent to students assigned to the control group. The control-group email was modeled after similar communications that LACC students receive before the start of a semester. Students in the three treatment groups also received this email with additional text displayed in a third paragraph at the end of the message (before the "Have a wonderful and successful year!").[19] As shown in Figure 3, the additional text in the email sent to the first treatment group includes a reminder that students need not borrow the offered amount ("You do not have to borrow this full amount"). The second and third treatments also remind students they have the option to borrow less than the packaged amount (using the same language as the first treatment group) and a reference to average annual amount conditional on any borrowing ($3000) or the average unconditional annual amount ($800) borrowed by LACC students who graduated last year.[20]

4   Empirical Framework

Random assignment allows us to obtain causal estimates from ordinary-least-squares (OLS) regressions of the form:

$$Y_i = \alpha + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T3_i + \epsilon_i \tag{1}$$

In equation (1), we examine outcome Y_i as a function of the treatment dummy variables: $T1_i$ for the reminder that one can borrow an amount other than that listed in the award letter, $T2_i$ for the reminder and the reference to graduates' conditional borrowing average of $3000, and $T3_i$ for the reminder and the reference to graduates' unconditional borrowing average of $800. In this regression, the constant (α) captures the average value of the outcome in the control group, and the β coefficients capture the differences between average outcomes

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[19]Results could be attenuated if students discuss financial aid with each other. We would consider this more likely in settings where students live together. In focus-group discussions with students at other community colleges, we found that it was quite rare for students to report discussing financial aid with their classmates.

[20]The goal of providing the conditional and unconditional average annual amount borrowed using similar language was to create variation in the reference point without employing deception.

of each treatment group and the control group. Estimated standard errors are robust to heteroskedasticity.

Our primary outcomes describe the amount that students choose to borrow. The first is a dummy variable indicating whether the student borrowed and the second is the amount of loan aid, including values of zero. Our third borrowing outcome is a dummy variable that indicates whether the student borrowed the maximum available loan.

To test for an effect of having any reference dollar amount listed in the email, we estimate regressions of the form:

$$Y_i = \alpha + \beta \left(T2_i + T3_i\right) + \epsilon_i \tag{2}$$

Equation (2) offers greater precision than equation (1) if $\beta_1 = 0$, a hypothesis that we test after estimating equation (1). That is, if the reminder of the possibility of borrowing less than the maximum amount has no effect on students' borrowing decisions, then the most efficient estimator of average effects of reference-point treatments compares students in these treatment arms to students in the control group and treatment arm without reference points. If reference-point treatments have the same effect ($\beta_2 = \beta_3$), then pooling also provides a more efficient estimate of this effect than estimating the effect of either reference-point treatment separately.

None of the treatments significantly affects enrollment. Of the sample that was sent emails, 89 percent enrolled at LACC, and we find no significant effects of treatment group assignment on enrollment: p-values all exceed 0.38 for individual treatments and are larger than 0.90 when comparing the reference-point treatment arms to the other arms. The 95 percent confidence interval for the pooled reference point treatment specification excludes effects larger in magnitude than a 1 percentage point (1.1 percent) increase or decrease in the probability of enrollment.[21] Thus, it does not appear that enrolled students constitute

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[21]Appendix Table A.1 contains these estimates as well as estimated effects for subgroups based on the characteristics used for stratification in random assignment.

a sample that is selected on treatment status, and hence we limit the sample in our main analysis to students who enrolled at LACC and could thereby obtain a loan that is observable in the data. We also show that our estimates are robust to including all students in the initial randomization sample, thereby including students who could not borrow from LACC but may have borrowed at a different institution.

To obtain evidence on the mechanisms through which treatment affected students' borrowing decisions, we test for heterogeneity in the treatment effect. To conduct these tests with maximal statistical power, we utilize equation (2). We jointly estimate this equation for partitions of the sample and then test for equality of effects across equations. For example, we split the sample into dependent students and independent students, estimate equation (2) for each of these subsamples, and test for equality of effects between the two samples.

Table 2 provides evidence that predetermined student characteristics are balanced across treatment arms. Balance is to be expected for inputs to the stratification (e.g., freshman, independent, new to LACC, prior borrowing, and EFC), but it also holds for other baseline characteristics including gender, race/ethnicity, financial need, and measures baseline attainment (credits earned and cumulative GPA). As would be expected for the number of characteristics and treatments over which we test sample balance, we find only a few differences that are statistically significant when tested individually; students in treatments $T1$ and $T2$ were about 3 percentage points more likely to be female than students in the control group; and students in treatment $T3$ received their financial aid package on average 1.4 days earlier than the control group. An overall F-test of joint significance of the treatments fails to reject a balanced sample for any of the baseline characteristics, and we find that controlling for baseline characteristics does not materially affect our estimates.[22]

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[22]Appendix Table A.2 shows that baseline characteristics are balanced in the initial randomization sample.

5 Results

In this section, we first present estimated treatment effects on borrowing outcomes.[23] Next we provide evidence suggesting cognitive overload is the most consistent explanation for our main results. In particular, we estimate whether treatments affect the number of students who make active choices about their loans, and we examine heterogeneity in treatment effects. Finally, we examine treatment effects on measures of academic attainment.

5.1 Borrowing outcomes

Table 3 displays our estimates for borrowing outcomes. Within the control group, 14 percent of students take out a loan, 12 percent borrow the maximum, and the unconditional mean amount borrowed is \$496 (Panel A). Panel B shows the estimated effects of each treatment on students' borrowing outcomes. The reminder that a student can borrow an amount other than the offered maximum (treatment $T1$) has no significant effects on borrowing. However, the treatments that reference a specific dollar amount borrowed by past graduates significantly reduce student borrowing.

Both reference-point treatments reduced the average amount borrowed by about \$65, or 13 percent of the control-group mean. As would be expected in this setting, in which the maximum available loan amount was offered in students' financial aid award letters, the reduction in borrowing is due to a reduction in the share of students who borrow the maximum, a treatment effect of roughly 1.5 percentage points. Less expected, we find changes of similar magnitude in the share of students who take up any amount of loan aid. For both reference-point treatments, students do not appear to shift towards the intermediate loan amounts referenced in the email, but rather to switch to not borrowing at all. This can be seen in Figure 4, which shows the distribution of loan amounts that are strictly between zero and the student's maximum available loan in each group. Students in the reference-

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[23]Borrowing outcomes are measured in the fall semester after the drop/add deadline. While it is possible for students to decide to borrow at a later point, most students who ultimately borrow choose to do so before this point.

point treatments are not induced to borrow the referenced amount or other amounts near it.[24]

Table 3 also shows the results from tests of the equality of effects across treatments. Our tests of equal effects of the two reference point treatments - $T2$ and $T3$ - produce p-values above 0.5 for all three loan outcomes. In contrast, we can reject a test of the equality of all three treatments with $p < 0.05$ in the case of effects on borrowing and $p < 0.1$ in the case of effects on the amount borrowed. Taken together, these results indicate that the reminder that a student can borrow less than the maximum available loan does not affect borrowing, but the emails with reference points describing borrowing among past students had equally sized effects on the likelihood that a student chooses to borrow.

We proceed by estimating models that compare the reference-point treatments with the reminder treatment and control group (i.e., equation (2)). These models provide greater precision when the reference-point treatments have similar effects and the reminder treatment has no effect, as we found and reported in Table 3. Estimates appear in the bottom panel of Table 3. Comparing the reference-point treatment effects to means among students not in reference-point treatments, we find that emails that referenced amounts borrowed by past students reduced loan take-up by 11 percent (1.6 percentage points; $p < 0.01$), the average amount borrowed by 13 percent ($65; $p < 0.01$), and the probability of borrowing the maximum available loan by 12 percent (1.4 percentage points; $p < 0.05$).[25]

The estimated effects of the reference point treatment are robust to the inclusion of additional controls and FAFSA filers who did not enroll in LACC. Panel A of Table 4 replicates our main estimates from Table 3. Panel B displays estimates from specifications that include additional controls for randomization strata fixed effects, while estimates in Panel C come

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[24]In other settings it might be possible to add an additional test based on whether the referenced amount was above or below the offered amount, but 95 percent of the students in the experiment received a loan offer greater than the larger of the two amounts referenced.

[25]Treatment effects for students who opened the email will likely be larger. Among current college students who received information about education tax credits via email in a recent field experiment, 43 percent opened the email (Bergman et al. 2019). If students in our sample opened emails at a similar rate, the effect of receiving a reference-point email on such students could be as large as a 27 percent reduction in borrowing.

from specifications that include controls for baseline attainment measures (whether the student is new to LACC and, for returning students, baseline cumulative credits earned and GPA at LACC). Panel D displays estimates from our main specification for the sample of both enrolled LACC students and FAFSA filers who did not enroll in LACC. Point estimates are virtually the same across these four specifications for all three borrowing outcomes. Finally, Panel E shows that when we reweight the sample to match the 2016 NPSAS nationally representative sample of community college students in Table 1 on observable characteristics, we obtain estimates that are less precise but larger in magnitude.[26]

5.2 Inaction as a Mechanism

We next explore the extent to which the reduction in borrowing among students who received emails containing reference points can be attributed to an active choice versus a failure to make a decision. To obtain a loan, LACC students must complete federal requirements and either actively accept the amount offered in their award letter or request a different loan amount. Students may actively decline their loan offer, but students who take no action also receive no loan. If reference points cause students to worry that they've borrowed too much, or if they led students to take the borrowing decision more seriously, then they might increase the number of students who actively decline the loan. Alternatively, if reference points induce cognitive overload, then they should increase the number of students who take no action. We observe whether a nonborrower made an active choice or took no action, and we estimate treatment effects on each.

Table 5 presents effects on whether students made an active borrowing choice. The first column replicates estimated effects on the extensive-margin of borrowing shown in the first column of Table 3.[27] Next, we examine whether the reference-point treatments affected the probability that a student actively declined the loan offer, an action taken by 22 percent

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[26]We obtain raked weights that match the analysis sample with the 2016 NPSAS sample based on race/ethnicity (black, Hispanic, white), gender, dependency status, class standing, EFC (Pell Grant eligible versus ineligible), unmet need (any unmet need versus no unmet need), and past borrowing.

[27]Appendix Table A.3 displays estimated effects of all three treatments.

of students who were not assigned to a reference-point treatment. We find no evidence of meaningful changes in the probability that a treated student actively declines her loan: the estimated 0.04 percentage point reduction in the probability of actively declining a loan is statistically insignificant and is less than 3 percent of the magnitude of the estimated impact on borrowing. Third, we test for treatment effects on the probability that a student made no active decision to accept or decline the loan offer. Students assigned to the reference-point treatment groups were 1.7 percentage points ($p < 0.05$) more likely than their counterparts to make no active decision. The magnitude of this effect is very similar in size to the magnitude of the estimated impact on borrowing (1.6 percentage points). Thus, it appears that many, if not all, of the students who would have borrowed in the absence of the reference-point treatment did not actively choose to decline their loan offers but rather failed to register any decision. That the financial information intended as guidance would instead cause inaction offers a cautionary finding for colleges and governments.

5.3 Heterogeneity

Next, we test whether emails had heterogeneous effects across student subgroups. We first examine subgroups defined by the baseline characteristics used for stratification in random assignment: past experience of borrowing (any outstanding debt versus no outstanding debt), student resources (Pell Grant eligible versus ineligible), prior LACC enrollment (new versus returning), class standing (freshman versus sophomore status, as determined by 30-credit threshold), and dependency status. To do so, we jointly estimate equation (1) for the two mutually exclusive subgroups defined by each of the five student characteristics and then test for equality of effects across the two equations for that characteristic. Table 6 contains these results in columns 2, 4, and 6, while columns 1, 3, and 5 display the mean outcome for control-group students.

We find two dimensions along which the effects of the reference-amount treatment are statistically distinguishable between stratification subgroups. First, we find large differences

when splitting the sample by whether a student had borrowed in the past. Differences in reductions in the probability that a student borrows, the amount borrowed, and the probability of borrowing the maximum available amount due to reference-point treatments are economically and statistically larger in magnitude for past borrowers than for students who have not borrowed before. Among past borrowers, the reference-point treatments decreased borrowing by 9.2 percentage points (23 percent relative to the control group mean) and \$362 (24 percent), while among students with no outstanding debt, borrowing fell by only 1.0 percentage point (9 percent) and \$40 (10 percent).[28] We would not expect these relatively large effects on past borrowers, or our finding of no effect from the information-only treatment, if students who have borrowed in the past have more information about their federal loan eligibility. Rather, it appears that these students are either closer to the margin of borrowing or that they are more likely to be students whose default action is to accept whatever number they see in the award letter (which, at LACC, had been the maximum available loan) unless they are also presented with another amount.[29] Most studies of borrowing include only past borrowers (e.g., Schmeiser et al. 2017; Darolia and Harper 2018; Barr et al. 2019) and cannot test such heterogeneity, but our finding here is consistent with the finding by Marx and Turner (2019) of past borrowers responding more to the offered loan amount.

The email referencing an explicit dollar amount also had significantly larger effects on borrowing for independent students than for dependent students. Differences in effects on the probability of borrowing and amount borrowed are both statistically significant at the 5 percent level, while the difference in effects on the share borrowing the maximum available loan amount are marginally significant ($p = 0.055$). This dimension of heterogeneity is consistent with dependent students being more likely to have someone with whom to discuss

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[28]We can reject tests of the equality of treatment effects by past borrowing for all three outcomes; p-values equal 0.008 (any borrowing), 0.009 (amount borrowed), and 0.022 (borrowing the maximum available amount).

[29]An alternative explanation for the relatively large effects of the reference-point treatments on past borrowers is that students who had accumulated more debt than the amount referenced in the email may have interpreted the message as an indication that they had already borrowed too much. We test whether past borrowers who had accumulated more than \$800 or \$3000 of debt were more likely to respond to the treatment but find no evidence of this (Appendix Table A.4).

their borrowing choice (e.g., a parent or high school guidance counselor) than independent students, resulting in independent students being more susceptible to cognitive overload. Point estimates are also relatively large for low-income students, measured by Pell Grant eligibility, though the difference in treatment effects is not statistically distinguishable from effects on Pell-ineligible students. Point estimates for freshmen are nearly identical to those for sophomores even though the latter are typically offered an extra $1000 of loan aid. These patterns of effects by both dependency and past borrowing status are quite similar to those produced by the loan-offer nudges examined by Marx and Turner (2019), providing further evidence of the characteristics of students who have trouble choosing a loan amount and instead simply adhere to the amount listed in the financial aid award letter.[30]

Next, we test for heterogeneous effects by baseline GPA. If a student's GPA serves as a proxy for her cognitive ability, and if the effect of reference-point emails operates through cognitive overload, then students with low baseline GPAs should exhibit larger borrowing responses to these emails. This is indeed what we find. Table 7 shows that effects are concentrated among those with below-median baseline GPAs.[31] The reference-point treatments reduce the share of low-GPA students who borrowed by 2.8 percentage points ($p < 0.01$), while effects on high-GPA students are small, positive, and statistically insignificant. Likewise, the amount borrowed falls by a statistically significant ($p < 0.01$) $106 among low-GPA students who received a reference point email, and the share of low-GPA students borrowing the maximum available loan decreases by a statistically significant 2.3 percentage points ($p < 0.01$), while effects on these outcomes among high-GPA students are positive and in-

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[30]Appendix Table A.5 provides corresponding estimates of heterogeneity in treatment effects on the probability of actively declining a loan offer and inaction. In all cases, treatment effects on the probability of inaction are larger in magnitude than treatment effects on the probability of being an active nonborrower, and there are no subgroups for which we find significant treatment effects on the probability that a student actively declines her loan. New students are significantly more likely to respond to reference-point treatments through inaction than returning students (4.1 versus 0.8 percent point increases; $p = 0.085$). None of the other differences in treatment effects on inaction between subgroups are statistically significant at conventional levels, but patterns are generally consistent with inexperienced and lower-income students being more likely to experience cognitive overload.

[31]Because a student's GPA is correlated with the number of classes she has taken, we calculate the median GPA for each level of baseline cumulative credits earned and split the sample of returning students by above versus below median.

significant. We can reject a test of the equality of treatment effects by baseline GPA with $p \leq 0.02$ across each of the three borrowing outcomes.[32]

Cognitive overload appears to provide a better explanation for our results than alternative explanations. For example, it is possible that a reference to past graduates and their debt reduces borrowing by increasing the salience of debt repayment. In this case, we might expect larger treatment effects among forward-looking students, but we find larger effects among low-performing students who are likely less forward-looking. Moreover, the repayment-salience explanation is not consistent with the null effects obtained in debt-letter experiments, where the information provided to students focused on the link between borrowing and post-college payments (Schmeiser et al. 2017; Darolia and Harper 2018). We find no evidence that students shift their borrowing towards the referenced amounts, as would be predicted by models with anchoring or students updating their beliefs about the optimal loan amount (Figure 4). Rather, introducing a second concrete option for students to consider increased the likelihood of inaction.

5.4 Attainment Effects

Borrowing can increase educational attainment if students are liquidity constrained or face trade-offs between working and studying (Lochner and Monge-Naranjo 2012). Recent studies provide evidence of positive average effects of student loans on educational attainment for students who borrow when they have access to loans (Dunlop 2013; Wiederspan 2016; Solis 2017). Nudges that reduce borrowing can have negative effects on attainment (Barr et al. 2019; Marx and Turner 2019). Given our finding that information about past students' average borrowing also led to a significant reduction in loan take-up, we also test for effects on educational attainment.

As discussed in Section 4, we can rule out all but negligible effects of our experimental

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[32] Appendix Table A.6 shows that while low-GPA students are more likely to respond to the reference-point treatments with inaction than high ability students, the difference in treatment effects is not statistically significant ($p = 0.105$).

treatments on the likelihood that a student enrolls in LACC for the fall semester. Because our experimental treatments have relatively small effects on borrowing patterns, we would not expect large effects on in-college attainment, even if changes in borrowing have large effects on attainment among students who respond to the treatments. For completeness, however, we proceed to estimate effects of emails on educational attainment, as measured by credits attempted, credits earned, grade point average, and degree receipt. In Appendix Table A.7, we provide these estimates for the pooled reference-point treatments that significantly affected borrowing.

We find no statistically significant effects on any of these measures of attainment.[33] The confidence intervals for these estimates are large, and so the lack of statistical significance should not be taken as evidence about the attainment effects of loan aid. Estimates from the experiment of Marx and Turner (2019) would imply that students in this experiment who were induced to forego a $3500 loan would complete 3.15 fewer credits and earn GPAs that were lowered by 0.56 points, effects that our confidence intervals do not exclude. The observed reductions in borrowing may also reduce attainment and therefore may not be in the best interest of students. Students may be better served by information or assistance tailored to their individual circumstances, particularly if they fall within one of the categories of students who appear to be overwhelmed by the student loan decision.

6   Conclusion

We experimentally test the effect of informational emails on community college students' borrowing decisions. Randomly assigned reminders that students could borrow less than the amount listed in the financial aid award letter do not affect borrowing decisions. However, when this information is combined with a reference to the average amount borrowed by past graduates, students are less likely to borrow at all. These letters referencing amounts of

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[33]There are also no effects that are significant at the 0.05 level when we separate each individual treatment (Appendix Table A.8).

past borrowing, rather than providing a helpful recommendation, appear to have induced cognitive overload: as the reduction in borrowing is driven by a reduction in the likelihood that a student makes any decision about borrowing, rather than an increase in the probability a student actively declines her loan. Consistent with this interpretation, effects were concentrated among older non-traditional students and those with lower academic ability.

A non-negligible share of students behave as though they are overwhelmed by the choice of how much to borrow, a key decision for human capital investment. While these students appear to know that they do not have to borrow the listed amount, many do so anyway, and those affected by treatments referencing the amounts borrowed by comparable peers appear unsure of how much to borrow. For colleges and the U. S. Department of Education, these results suggest that when it comes to decisions about student loans, simply providing information may not be sufficient to improve student outcomes (e.g., Bettinger et al. 2012; Bird et al. 2019; Bergman et al. 2019; Gurantz et al. 2019).[34] Students may benefit more from financial information when it is accompanied by human assistance (Bettinger et al. 2012; Castleman and Page 2015; Carrell and Sacerdote 2017; Barr and Castleman 2018; Barr and Turner 2018). Colleges' limited capacity to provide such assistance, however, may necessitate other mechanisms. We speculate that a budgeting exercise in which students consider their expected sources of income and expected expenses could help them identify their expected financial need and choose a corresponding loan amount.

Future work could explore interventions that provide assistance or require active choice. For researchers, our results suggest caution in drawing welfare conclusions from the observed choices of students. In particular, models of rational, forward-looking borrowing may be misspecified and give misleading impressions of college students' need for credit.

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[34]It may be that information provided before prospective students apply for college could be more effective: Stoddard and Urban (forthcoming) and Mangrum (2019) show that when states require high school students to complete financial education courses, federal student aid applications increase, students shift from using credit cards to federal student loans, and post-college loan repayment outcomes improve.

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Figures and Tables

Figure 1: Financial Aid Award Letter for Hypothetical Student "Finny Aid"

Shopping Sheet

Financial Aid Awards Status

2015 : (Fall 2014 - Summer 2015)
Welcome Finny Aid

Your initial award is based on full time enrollment.Credited amount is based on your current enrollment. If your enrollment changes, your awards will be adjusted accordingly. Future sessions will be disbursed later in the term.

To Accept/Decline/Reduce your Direct Subsidized Loan you must visit the Financial Aid "Application Status" screen.

Awards by Term	Fall		Winter		Summer	
Financial Aid	**Award**	**Credited**	**Award**	**Credited**	**Award**	**Credited**
DIRECT LOAN SUBSIDIZED	$750.00		$750.00			
Federal Pell Grant	$2,865.00		$2,865.00			
Total all awards	$3,615.00		$3,615.00			

Figure 2: Control Group Email

An important email from your ████████████ **Financial Aid Office**

YOUR ID#: ████████████

Dear ████████████████,

Don't forget, your estimated financial aid package for 2016-17 has been posted to your ██████ account.

Please make your student loan decision by Tuesday, September 6. You can accept, decline, or alter the Federal Direct Subsidized loan offer you have received by logging in to ██████ -> Financial Aid -> Application Status and select the Accept/ Decline loan document.

Have a wonderful and successful year!
████████████████
Office of Financial Aid
For more information please visit the financial aid website at ████████████.

Figure 3: Additional Text in Treatment Group Emails

Treatment arm	Additional text
1. Information	The amount of Federal Direct Subsidized loan offered in your award letter is the maximum amount you are eligible to borrow through the program. ***You do not have to borrow this full amount.*** Log into [online portal] if you wish to reduce or decline your loan.
2. Information + high reference point	The amount of Federal Direct Subsidized loan offered in your award letter is the maximum amount you are eligible to borrow through the program. ***You do not have to borrow this full amount.*** Last school year, the average subsidized loan borrower who graduated borrowed about $3000. Log into [online portal] if you wish to reduce or decline your loan.
3. Information + low reference point	The amount of Federal Direct Subsidized loan offered in your award letter is the maximum amount you are eligible to borrow through the program. ***You do not have to borrow this full amount.*** Last school year, the average student who graduated borrowed about $800. Log into [online portal] if you wish to reduce or decline your loan.

Figure 4: Distribution of Loans by Treatment Group for Students Borrowing Less than the Maximum Available Loan
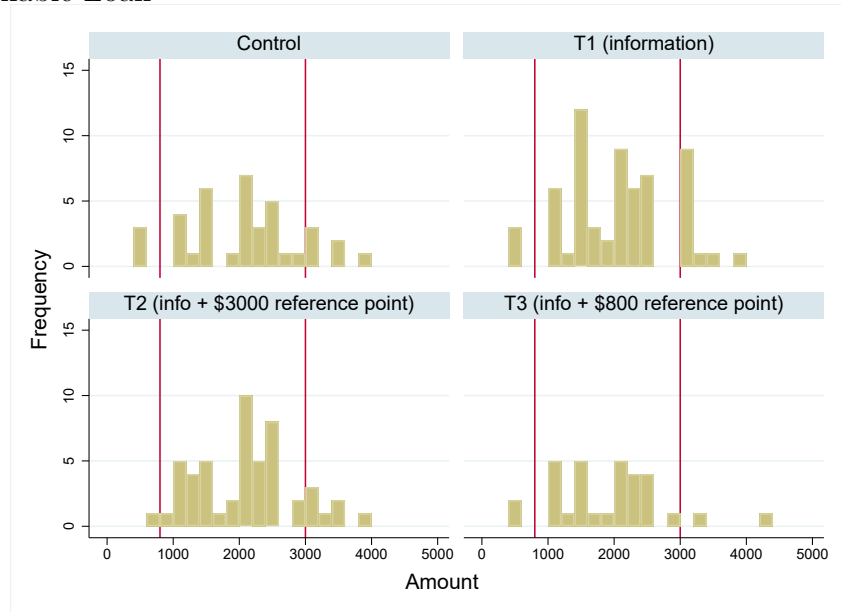
Notes: LACC experimental sample students who enrolled in fall 2016 and borrowed a positive amount that was less than the maximum amount available to the student (N = 181).

Table 1: Summary Statistics

	LACC experimental sample	2016 NPSAS community college students
A. Demographics		
Freshman	0.62	0.51
Gender = female	0.59	0.62
Race/ethnicity		
Black	0.43	0.23
Hispanic	0.40	0.24
White	0.36	0.68
Independent	0.42	0.48
B. Finances		
Expected family contribution	$1301	$4325
Pell Grant eligible	0.92	0.77
Any financial need	0.97	0.84
Financial need (conditional)	$8717	$7908
Any fed. student loan debt	0.07	0.27
Outstanding student loan debt (cond.)	$4965	$14,416
C. Outcomes		
Borrowed	0.16	0.25
Amount borrowed (conditional)	$3268	$5011
Academic year GPA	2.392	2.759
Degree reciept	0.15	0.16

Notes: Column 1 displays means for the LACC experimental sample, which includes who enrolled in fall 2016 and had not made a borrowing decision by August 31, 2016 (N=13,065). Column 2 displays means for FAFSA-filing community college students who enrolled in fall 2015 from the 2016 National Postsecondary Student Aid Study (NPSAS). Race/ethnicity categories are not mutually exclusive. Pell Grant eligible students are those with an expected family contribution at or below the Pell Grant eligibility threshold. Financial need is equal to a student's cost of attendance less their expected family contribution and grant aid.

Table 2: Balance Across Treatment Groups

Dependent variable:	(1) Freshman	(2) Female	(3) Black	(4) Hispanic	(5) White	(6) Independent	(7) New to LACC
Sample mean	0.624	0.586	0.424	0.396	0.347	0.420	0.258
T1 (can borrow other amount)	0.005 (0.012)	0.031 (0.012)*	-0.016 (0.012)	0.016 (0.012)	0.006 (0.012)	0.003 (0.012)	0.006 (0.011)
T2 (T1 + $3000 ref. point)	0.0002 (0.012)	0.028 (0.012)*	-0.003 (0.012)	0.007 (0.012)	-0.003 (0.012)	-0.0004 (0.012)	0.002 (0.011)
T4 (T1 + $800 ref. point)	0.001 (0.012)	0.012 (0.012)	-0.019 (0.012)	0.018 (0.012)	0.011 (0.012)	0.00005 (0.012)	0.003 (0.011)
Constant	0.623 (0.008)**	0.568 (0.009)**	0.434 (0.009)**	0.386 (0.009)**	0.343 (0.008)**	0.420 (0.009)**	0.255 (0.008)**
Test of joint sig. (p-value)	0.922	0.233	0.349	0.631	0.499	0.950	0.900
Observations	13,065	13,065	13,065	13,065	13,065	13,065	13,065

Dependent variable:	(8) EFC	(9) Need	(10) Prior borrowing	(11) Outst. student loans	(12) Packaged - days since 6/14	(13) Baseline credits earned	(14) Baseline GPA
Sample mean (std deviation)	1301 (2715)	$8419 (5786)	0.075	$371 (1699)	43.5 (26.1)	26.8 (19.2)	2.604 (1.093)
T1 (can borrow other amount)	-6 (66)	-95 (143)	-0.0003 (0.007)	-21 (44)	-1.0 (0.6)	-0.2 (0.6)	0.039 (0.031)
T2 (T1 + $3000 ref. point)	34 (68)	-32 (144)	-0.002 (0.006)	-56 (42)	-1.4 (0.6)*	-0.02 (0.6)	0.030 (0.032)
T3 (T1 + $800 ref. point)	-8 (66)	-97 (143)	0.002 (0.007)	-12 (44)	-0.2 (0.7)	-0.3 (0.6)	0.029 (0.032)
Constant	1,296 (47)**	8,475 (101)**	0.075 (0.005)**	393 (32)**	44.2 (0.5)**	27.0 (0.4)**	2.579 (0.023)**
Test of joint sig. (p-value)	0.788	0.874	0.811	0.500	0.143	0.86	0.946
Observations	13,065	13,065	13,065	13,065	13,065	9,692	9,692

Notes: LACC experimental sample students who enrolled in fall 2016. Estimates from OLS regressions of the characteristic denoted in the column on indicators for treatment group assignment. Each column displays estimates from separate regressions. Race/ethnicity categories are not mutually exclusive. EFC = expected family contribution. Robust standard errors in parentheses; ** $p<0.01$, * $p<0.05$, + $p<0.1$.

Table 3: The Impact of Information and Reference Points on Student Borrowing Outcomes

	(1) Any borrowing	(2) Amount borrowed	(3) Amount = max
A. Control group mean	0.14	496	0.12
		(1287)	
B. OLS estimates, all treatments			
T1 (can borrow other amt)	0.004	-4.9	-0.004
	(0.009)	(31.6)	(0.008)
T2 (T1 + $3000 ref point)	-0.012	-63.5	-0.016
	(0.008)	(30.7)*	(0.008)*
T3 (T1 + $800 ref point)	-0.017	-68.6	-0.015
	(0.008)*	(30.7)*	(0.008)+
Tests of equality (*p*-value)			
All treatments	0.038	0.071	0.216
T2 = T3	0.573	0.863	0.826
Observations	13,065	13,065	13,065
C. OLS estimates, pooled reference point treatments			
T2 + T3	-0.016	-64.7	-0.014
	(0.006)**	(21.7)**	(0.006)*
Observations	13,065	13,065	13,065

Notes: LACC experimental sample students who enrolled in fall 2016. Panel A displays outcome means for the control group and standard deviation of the amount borrowed in parentheses in column 2. Estimates from OLS regressions of the outcome denoted in the column on indicators for treatment group assignment (Panel B) or an indicator for assignment to one of the two reference point treatments (Panel C). Each column within a panel displays estimates from separate regressions. Robust standard errors in parentheses; ** p<0.01, * p<0.05, + p<0.1.

Table 4: Robustness of Estimated Effects on Borrowing

	(1) Any borrowing	(2) Amount borrowed	(3) Amount = max
A. Baseline estimates			
Pooled reference point treatments	-0.016	-64.7	-0.014
	(0.006)**	(21.7)**	(0.006)*
Observations	13,065	13,065	13,065
B. Strata fixed effects			
Pooled reference point treatments	-0.016	-62.4	-0.013
	(0.007)*	(27.6)*	(0.007)+
Observations	13,065	13,065	13,065
C. Controls for baseline attainment			
Pooled reference point treatments	-0.016	-63.3	-0.014
	(0.006)**	(21.6)**	(0.006)*
Observations	13,065	13,065	13,065
D. Initial randomization sample			
Pooled reference point treatments	-0.014	-53.7	-0.012
	(0.005)*	(19.9)**	(0.005)*
Observations	14,784	14,784	14,784
E. Reweighted			
Pooled reference point treatments	-0.028	-119.3	-0.025
	(0.014)*	(52.6)*	(0.013)+
Observations	13,065	13,065	13,065

Notes: LACC experimental sample students. Panels A, B, C, and E restrict the sample to students who enrolled in fall 2016. For Panel A specification, see Table 3 notes. Panel B displays results from OLS regressions that also include controls for random-assignment strata fixed effects. Panel C specification includes controls for whether the student is new to LACC, baseline credits earned, and cumulative GPA. Panel D sample includes observations of randomly assigned FAFSA filers that did not enroll in LACC. Panel E specification reweights the analysis sample to match the nationally representative 2016 NPSAS sample of community college students according to race, gender, dependency status, class standing, EFC, prior borrowing, and unmet need using raking. Robust standard errors in parentheses; ** p<0.01, * p<0.05, + p<0.1.

Table 5: Reference Point Treatments Lead to a Reduction in Active Choice

	(1) Any borrowing	(2) Active nonborrower	(3) No decision
A. Control group mean	0.14	0.22	0.64
B. OLS estimates			
Pooled reference point treatments	-0.016	-0.0004	0.017
	(0.006)**	(0.007)	(0.008)*
Observations	13,065	13,065	13,065

Notes: LACC experimental sample students who enrolled in fall 2016. Panel A displays outcome means for the control group. Panel B displays OLS estimates of treatment effects on the probability that a student accepts a loan of any amount (column 1), actively declines her loan (column 2), and does not make a borrowing decision (column 3). Robust standard errors in parentheses; ** p<0.01, * p<0.05, + p<0.1.

Table 6: Heterogeneous Effects by Stratification Variables

	Any borrowing		Amount borrowed		Amount = max	
	(1) Control mean	(2) Estimates	(3) Control mean, sd	(4) Estimates	(5) Control mean	(6) Estimates
Subgroup						
No outstanding debt (N = 12,088)	0.116	-0.010 (0.006)+	410 (1165)	-40 (21)+	0.104	-0.009 (0.005)
Has outstanding debt (N = 977)	0.400	-0.092 (0.030)** [0.008]	1532 (1959)	-362 (121)** [0.009]	0.349	-0.078 (0.029)** [0.022]
Pell eligible (N = 12,047)	0.135	-0.017 (0.006)**	488 (1272)	-67 (22)**	0.120	-0.014 (0.006)*
Pell ineligible (N = 1,018)	0.164	-0.005 (0.023) [0.595]	558 (1326)	-29 (82) [0.661]	0.153	-0.009 (0.022) [0.811]
New student (N = 3,373)	0.170	-0.023 (0.013)+	577 (1297)	-86 (43)*	0.160	-0.023 (0.012)+
Returning student (N = 9,692)	0.126	-0.014 (0.007)* [0.522]	464 (1267)	-56 (25)* [0.549]	0.110	-0.011 (0.006)+ [0.792]
<30 credits earned (N = 8,155)	0.150	-0.016 (0.008)*	501 (1240)	-47 (27)+	0.133	-0.013 (0.007)+
30 or more credits earned (N = 4,910)	0.123	-0.018 (0.009)+ [0.855]	480 (1334)	-91 (36)* [0.327]	0.104	-0.016 (0.008)** [0.527]
Dependent student (N = 7,575)	0.102	-0.006 (0.007)	347 (1061)	-23 (24)	0.090	-0.004 (0.006)
Independent student (N = 5,490)	0.186	-0.031 (0.010)** [0.038]	694 (1500)	-118 (39)** [0.037]	0.167	-0.027 (0.010)** [0.055]

Notes: LACC experimental sample students who enrolled in fall 2016. Columns 1, 3, and 5 display outcome means for control group members with the specified characteristic. Columns 2, 4, and 6 display OLS estimates of treatment effects on the outcome denoted in the column for students in the specified subgroup. Regressions for subgroups in each row are jointly estimated. Bracketed numbers contain *p*-values from tests of equality of effects between mutually exclusive subgroups. Robust standard errors in parentheses; ** p<0.01, * p<0.05, + p<0.1.

Table 7: Heterogeneous Effects by Baseline Academic Performance

	Any borrowing		Amount borrowed		Amount = max	
	(1) Control mean	(2) Estimates	(3) Control mean	(4) Estimates	(5) Control mean	(6) Estimates
Below median baseline GPA (N = 5,543)	0.141	-0.028 (0.009)**	518 (1320)	-106 (34)**	0.126	-0.023 (0.009)**
Above median baseline GPA (N = 4,149)	0.111	0.004 (0.009) [0.015]	410 (1210)	10 (36) [0.020]	0.094	0.006 (0.009) [0.019]

Notes: Returning LACC experimental sample students who enrolled in fall 2016 (N = 9,692). Columns 1, 3, and 5 display outcome means and standard deviations (in parentheses) for control group members with the specified characteristic. Columns 2, 4, and 6 display OLS estimates of treatment effects on the outcome denoted in the column for students in the specified subgroup. Regressions for above- and below-median baseline GPA are jointly estimated. Bracketed numbers contain p-values of tests of equality of effects between the two subgroups. Robust standard errors in parentheses; ** p<0.01, * p<0.05, + p<0.1.

Appendix A: Additional Figures and Tables

Table A.1: Information and Reference Point Treatments Have No Effect on Enrollment

	(1) Control mean	(2) Estimate
A. OLS estimates, all treatments		
T1 (can borrow other amt)		-0.005
		(0.007)
T2 (T1 + $3000 ref point)	0.887	-0.006
		(0.007)
T3 (T1 + $800 ref point)		-0.0002
		(0.007)
Tests of equality (*p*-val)		
All treatments		0.665
T1 = T2		0.889
T1 = T3		0.484
T2 = T3		0.401
Observations		14,784
B. OLS estimates, pooled reference point treatments		
Pooled reference point treatment		-0.001
		(0.005)
Observations		14,784
C. Heterogeneity by subgroup		
No outstanding debt (N = 12,088)	0.890	-0.001
		(0.005)
Has outstanding debt (N = 977)	0.816	-0.0004
		(0.022)
		[0.995]
Pell eligible (N = 12,047)	0.895	-0.0001
		(0.005)
Pell ineligible (N = 1,018)	0.767	-0.006
		(0.023)
		[0.803]
New student (N = 3,373)	0.890	-0.003
		(0.010)
Returning student (N = 9,692)	0.881	0.0003
		(0.006)
		[0.767]
<30 credits earned (N = 8,155)	0.879	-0.004
		(0.007)
30 or more credits earned (N = 4,910)	0.892	0.004
		(0.008)
		[0.462]
Dependent student (N = 7,575)	0.919	0.002
		(0.006)
Independent student (N = 5,490)	0.840	-0.004
		(0.009)
		[0.586]

*Note*s: Panels A and B contain OLS estimates of treatment effects on the probability of enrolling in LACC in the fall 2016 semester. Panel C contains OLS estimates of treatment effects on borrowing outcomes among specified subgroups. Regressions for subgroups in each row jointly estimated. Bracketed numbers contain *p*-values from tests of equality of effects between mutually exclusive subgroups. Robust standard errors in parentheses; ** p<0.01, * p<0.05, + p<0.1.

Table A.2: Balance Across Treatment Groups in Initial Sample

Dependent variable:	(1) Freshman	(2) Female)	(3) Black	(4) Hispanic	(5) White	(6) Independent	(7) New to LACC
Sample mean	0.628	0.589	0.43	0.393	0.345	0.442	0.256
T1 (can borrow other amount)	-0.001	0.023	-0.013	0.012	0.001	0.001	0.0003
	(0.011)	(0.011)*	(0.012)	(0.011)	(0.011)	(0.012)	(0.010)
T2 (T1 + $3000 ref. point)	0.000	0.024	0.002	0.004	-0.007	0.001	-0.0001
	(0.011)	(0.011)*	(0.012)	(0.011)	(0.011)	(0.012)	(0.010)
T3 (T1 + $800 ref. point)	-0.001	0.010	-0.008	0.011	-0.002	0.0002	-0.0002
	(0.011)	(0.011)	(0.012)	(0.011)	(0.011)	(0.012)	(0.010)
Constant	0.628	0.575	0.435	0.386	0.347	0.442	0.256
	(0.008)**	(0.008)**	(0.008)**	(0.008)**	(0.008)**	(0.008)**	(0.007)**
Test of joint sig. (p-val.)	0.993	0.376	0.385	0.704	0.727	0.998	0.999
Observations	14,784	14,784	14,784	14,784	14,784	14,784	14,784

Dependent variable:	(8) EFC	(9) Need	(10) Prior borrowing	(11) Outst. student loans	(12) Packaged - days since 6/14	(13) Baseline credits earned	(14) Baseline GPA
Sample mean	1433	$8643	0.081	$414	43.4	26.6	2.572
T1 (can borrow other amount)	-19	-81	-0.0001	-34	-0.8	-0.1	0.023
	(66)	(136)	(0.006)	(46)	(0.6)	(0.5)	(0.030)
T2 (T1 + $3000 ref. point)	39	-12	0.0002	-72	-0.9	-0.2	0.002
	(67)	(136)	(0.006)	(43)+	(0.6)	(0.5)	(0.030)
T3 (T1+ $800 ref. point)	-5	-95	0.001	-49	0.1	-0.6	0.002
	(67)	(135)	(0.006)	(45)	(0.6)	(0.5)	(0.030)
Constant	1429	8690	0.081	453	43.9	26.8	2.565
	(47)**	(96)**	(0.004)**	(35)**	(0.4)**	(0.4)**	(0.021)**
Test of joint sig. (p-val.)	0.664	0.808	0.991	0.624	0.190	0.611	0.700
Observations	14,784	14,784	14,784	14,784	14,784	10,995	10,995

Notes: See Table 2 notes.

Table A.3: <u>The Effect of Information and Reference Point Treatments on Active Choice</u>

	(1) Any borrowing	(2) Active nonborrower	(3) No decision
T1 (can borrow other amt)	0.004	-0.001	-0.001
	(0.009)	(0.010)	(0.012)
T2 (T1 + $3000 ref point)	-0.012	-0.0001	0.012
	(0.008)	(0.010)	(0.012)
T3 (T1 + $800 ref point)	-0.017	-0.003	0.020
	(0.008)*	(0.010)	(0.012)+
Tests of equality (*p*- val)			
All treatments	0.038	0.961	0.192
T1 = T2	0.058	0.842	0.244
T1 = T3	0.014	0.943	0.073
T2 = T3	0.573	0.786	0.532
Observations	13,065	13,065	13,065

*Note*s: OLS estimates of treatment effects on the probability that a student accepts a loan of any amount, actively declines her loan, and does not make a borrowing decision. Robust standard errors in parentheses; ** p<0.01, * p<0.05, + p<0.1.

Table A.4: Heterogeneous Effects of Reference Point Treatments for Prior Borrowers by Outstanding Debt

	(1) Any borrowing	(2) Active nonborrower	(3) No decision
T2 ($3000 ref point)			
ˣ Outstanding debt < $3000	-0.076	0.032	0.045
	(0.059)	(0.058)	(0.060)
ˣ Outstanding debt ≥ $3000	-0.075	0.027	0.048
	(0.043)+	(0.042)	(0.043)
	[0.990]	[0.950]	[0.962]
T3 ($800 ref point)			
ˣ Outstanding debt < $800	-0.067	0.054	0.013
	(0.273)	(0.274)	(0.274)
ˣ Outstanding debt ≥ $800	-0.108	0.053	0.056
	(0.036)**	(0.036)	(0.037)
	[0.880]	[0.996]	[0.877]

*Note*s: OLS estimates of reference point treatments on borrowing outcomes. Sample is limited to prior borrowers (N = 977). Each column contains estimates from a separate regression. Bracketed numbers contain *p*-values from tests of equality of effects between mutually exclusive subgroups. Robust standard errors in parentheses; ** p<0.01, * p<0.05, + p<0.1.

Table A.5: Heterogeneous Effects on the Probability of Making an Active Choice by Stratification Variables

	(1) Any borrowing	(2) Active nonborrower	(3) No decision
Subgroup			
No outstanding debt (N = 12,088)	-0.010	-0.004	0.014
	(0.006)+	(0.007)	(0.009)+
Has outstanding debt (N = 977)	-0.092	0.041	0.051
	(0.030)**	(0.029)	(0.030)+
	[0.008]	[0.137]	[0.242]
Pell eligible (N = 12,047)	-0.017	-0.0001	0.017
	(0.006)**	(0.008)	(0.009)*
Pell ineligible (N = 1,018)	-0.005	-0.005	0.009
	(0.023)	(0.024)	(0.030)
	[0.595]	[0.857]	[0.788]
New student (N = 3,373)	-0.023	-0.018	0.041
	(0.013)+	(0.013)	(0.016)*
Returning student (N = 9,692)	-0.014	0.005	0.008
	(0.007)*	(0.009)	(0.010)
	[0.522]	[0.130]	[0.085]
<30 credits earned (N = 8,155)	-0.016	-0.011	0.027
	(0.008)*	(0.009)	(0.010)*
30 or more credits earned (N = 4,910)	-0.018	0.017	0.001
	(0.009)+	(0.013)	(0.014)
	[0.855]	[0.070]	[0.136]
Dependent student (N = 7,575)	-0.006	-0.009	0.015
	(0.007)	(0.009)	(0.010)
Independent student (N = 5,490)	-0.031	0.012	0.019
	(0.010)**	(0.012)	(0.013)
	[0.038]	[0.165]	[0.784]

Notes: OLS estimates of treatment effects on borrowing outcomes among subgroups. Regressions for subgroups in each row jointly estimated. Bracketed numbers contain p-values from tests of equality of effects between mutually exclusive subgroups. Robust standard errors in parentheses; ** $p<0.01$, * $p<0.05$, + $p<0.1$.

Table A.6: Heterogeneous Effects on the Probability of Making an Active Choice by Baseline Academic Performance

	(1) Any borrowing	(2) Active nonborrower	(3) No decision
Below median baseline GPA	-0.028	-0.001	0.024
(N = 5,543)	(0.009)**	(0.012)	(0.014)+
Above median baseline GPA	0.004	0.013	-0.007
(N = 4,149)	(0.009)	(0.013)	(0.014)
	[0.015]	[0.428]	[0.105]

*Note*s: OLS estimates of treatment effects on borrowing outcomes among returning students (N = 9,692). Regressions for above- and below-median baseline GPA jointly estimated. Bracketed numbers contain *p*-values of tests of equality of effects between the two subgroups. Robust standard errors in parentheses; ** p<0.01, * p<0.05, + p<0.1.

Table A.7: The Impact of Nonzero Loan Offers on Attainment

	(1) Credits attempted	(2) Credits earned	(3) GPA	(4) Earned degree
T1 (can borrow other amt)	-0.181	-0.311	-0.050	-0.014
	(0.174)	(0.190)	(0.028)+	(0.009)+
T2 (T1 + $3000 ref point)	0.071	0.021	0.001	-0.008
	(0.175)	(0.189)	(0.027)	(0.009)
T3 (T1 + $800 ref point)	0.028	0.023	-0.010	-0.014
	(0.174)	(0.188)	(0.027)	(0.009)
Tests of equality (*p*- val.)				
All treatments	0.300	0.127	0.154	0.693
T2 = T3	0.806	0.990	0.673	0.483
Observations	13,065	13,065	13,065	13,065

*Note*s: See Table 3 notes.

Table A.8: The Impact of Nonzero Loan Offers on Attainment

	(1) Credits attempted	(2) Credits earned	(3) GPA	(4) Earned degree
A. Control group mean	15.8	12.7	2.392	0.15
	(7.1)	(7.7)	(1.112)	
B. OLS estimates				
Pooled reference point treatments	0.140	0.177	0.021	-0.004
	(0.123)	(0.133)	(0.019)	(0.006)
Observations	13,065	13,065	13,065	13,065
C. IV estimates				
Borrowed	-8.6	-10.8	-1.25	0.223
	(8.3)	(9.0)	(1.24)	(0.377)
Observations	13,065	13,065	13,065	13,065

*Note*s: For Panel B specification, see Table 3 notes. Panel C displays the second stage of 2SLS estimates using reference-point treatment as an instrument for whether a student borrowed; first stage appears in Panel C of Table 3. Robust standard errors in parentheses; ** p<0.01, * p<0.05, + p<0.1.