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The Impact of Pell Grant Eligibility on Community College Students' Financial Aid Packages, Labor Supply, and Academic Outcomes

Rina Seung Eun Park
Community College Research Center
Columbia University
Judith Scott-Clayton
Community College Research Center
Columbia University
National Bureau of Economic Research

In this article, we examine the effects of receiving a modest Pell Grant on financial aid packages, labor supply while in school, and academic outcomes for community college students. Using administrative data from one state, we compare students just above and below the expected family contribution cutoff for receiving a Pell Grant. We find that other financial aid adjusts in ways that vary by institution: Students at schools that offer federal loans borrowed more if they just missed the Pell eligibility threshold, but at other schools, students were instead compensated with higher state grants. Focusing on the loan-offering schools, we find suggestive evidence that receiving a modest Pell Grant leads students to reduce labor supply and increase enrollment intensity. We also provide indirect evidence that students' initial enrollment choices are influenced by an offer of Pell Grants versus loans.

Keywords: *Pell Grant, financial aid, labor supply, regression-discontinuity*

Introduction

IN 1965, President Lyndon Johnson signed into law the Higher Education Act of 1965, which initiated the precursors to today's Pell Grant and Stafford Loan programs and solidified the federal government's role in higher education finance. Since then, the importance of federal financial aid policy has only increased. In 2014–2015, the federal government provided over US$120 billion in student loans, grants, and other forms of financial aid for undergraduates—more than 4 times the level of support provided in 1990–1991.

The federal Pell Grant program is the largest single source of grant aid, providing US$30.3 billion in grants to over 9 million students annually in 2014–2015, up to US$5,775 each per year. Students can use the grant at any eligible institution and receive the same amount regardless of where they go. Although the eligibility formula is complex, family income is the main component: Those with family income below US$30,000 typically receive the maximum award, while only about 5% of those with family incomes above US$70,000 receive any award. If the award exceeds tuition and fees, students can use the extra amount for books, food, or other living expenses.

Although a large body of research convincingly demonstrates that financial aid programs can influence student enrollments and completion (e.g., Deming & Dynarski, 2009; Long, 2008; Page & Scott-Clayton, 2016), evidence on the effects of Pell Grants specifically is more mixed. Two early studies of the introduction of Pell Grants find no evidence that college enrollments increased any faster for Pell-eligible students relative to ineligible students (Hansen, 1983; Kane, 1995). More recently, a regression-discontinuity (RD) analysis of urban community college students just above and below the eligibility cutoff for Pell finds no impact on college choice, course credits, or degree completion (Marx & Turner, 2015). A similar study using data on high school graduates in Tennessee generally finds no effect of minimum Pell eligibility on college sector, quality, and enrollment, though finds some small differences by gender (Carruthers & Welch, 2017). On the contrary, Pell Grants appear to positively influence enrollment rates for adult students (Seftor & Turner, 2002) and may increase persistence, graduation, and even postcollege earnings, conditional on enrollment (Bettinger, 2004; Denning, 2018; Denning, Marx, & Turner, 2017). A recent pair of RD studies using national administrative data to examine variation in Pell around various thresholds finds some evidence that larger Pell Grants might increase the likelihood of enrolling anywhere (Matsudaira, 2017), but conditional on enrollment, no effect of Pell eligibility on graduation and no clear effect on earnings (Eng & Matsudaira, 2017).

The ambiguous evidence regarding Pell has led researchers to investigate possible explanations. Several studies have suggested that the complexity of the federal aid application process and late notice of Pell eligibility may undermine the ability of the program to reach students who need aid most (Bettinger, Long, Oreopoulos, & Sanbonmatsu, 2012; Carruthers & Welch, 2017; Dynarski & Scott-Clayton, 2006; Dynarski & Wiederspan, 2012; Scott-Clayton, 2013).[1]

Another potential explanation is that state and institutional aid policies may interact with the federal aid formula in a way that makes it difficult to isolate the effect of Pell. For example, one concern often raised is whether institutions simply capture federal aid, either via increasing prices or via reducing institutional support that otherwise would have been provided. This is referred to as the "Bennett Hypothesis" after former U.S. Secretary of Education William Bennett, who prominently raised this concern. A similar problem can arise due to "fiscal vertical externalities" between federal, state, and local governments (Boadway & Tremblay, 2012; Johnson, 1988): The federal government acts as the "first mover" by establishing Pell as the foundation of financial aid packages (Pell Grants are never reduced as a result of other aid eligibility), but states or institutions as second movers can reduce or retarget their own aid dollars in response.

For example, research by Turner (2014) finds that selective nonprofit institutions capture, via reductions in institutional aid, 67 cents of every Pell dollar received by their students. Bettinger and Williams (2013) also find a negative correlation between Pell Grants and state aid. However, McPherson and Schapiro (1991) find a positive correlation between Pell Grants and overall institutional aid and Denning et al. (2017) find that Pell eligibility "crowds in" state aid in Texas.[2] Finally, studies have found that students may adjust their own borrowing decisions in response to grant eligibility, such that receiving an extra dollar of grant aid often leads to less a dollar of total additional aid received (Goldrick-Rab, Kelchen, Harris, & Benson, 2016; Marx & Turner, 2015). Interactions with state and institutional aid programs may also help explain why the estimated effects of Pell are not consistent from study to study, because state and institutional aid programs can vary substantially from context to context.

In this article, we use administrative data from a single state on a population of particular interest: community college enrollees. We implement a RD design that examines the effects of just barely qualifying for a Pell Grant on the composition of recipients' overall financial aid package, students' labor supply, and subsequent academic outcomes.

Examining the effect of a modest Pell Grant for students at community colleges has two advantages. First, even though the magnitude of the minimum Pell grant is relatively small, the monetary incentive is sharpest for community college students: The minimum Pell Grant, which averaged US$750 between 2008 and 2010, represented a more than 25% discount on tuition and fees during that time period.[3] Second, because of open-access admissions, community college enrollees are arguably more likely to be on the margin of college attendance and persistence (that is, potentially more likely to change behavior as a result of aid), and thus represent a key target population for need-based aid.

We find that even at community colleges, other sources of student aid do shift substantially around the cutoff for Pell, consistent with Turner (2014) and Marx and Turner (2015). We find distinctive patterns of financial packaging depending on whether or not institutions participated in federal loan programs. At institutions that participated in the federal student loan programs, students above the cutoff (who are ineligible for Pell) borrowed 55% more than those below the cutoff. This pattern replicates the findings in previous research by Marx and Turner (2015), though it appears even more strongly in our sample. On the contrary, at institutions that did not offer loans, students just above the Pell cutoff received state/institutional grants that offset the discontinuity in Pell Grants (i.e., at schools not participating in the loan programs, there is no discontinuity in overall grant aid around the Pell cutoff).

For our analysis of student labor supply and academic outcomes, we focus on the sample of students attending only loan-offering schools because they best satisfy the criteria for causal estimates.[4] We find that qualifying for the minimum Pell increases the intensity of enrollment, with recipients 4 to 7 percentage points more likely to enroll full-time from the spring of their first year to the spring of their second year. We also find evidence that those who are just barely eligible for Pell earn less in the first 2 years after entry, suggesting a reduction of labor supply equivalent to perhaps 1 or 2 hours per week. This is consistent with previous findings that grants decrease the need to work for pay and allow students to shift their time allocation from work to school (Broton, Goldrick-Rab, & Benson, 2016; Schudde, 2013). For cumulative outcomes at the end of 3 years—on cumulative grade point average (GPA), cumulative credits earned, degree completion, and transfer within 3 years of entry—we cannot detect statistically significant effects, though the point estimates are positive and of a magnitude consistent with the impacts on enrollment intensity throughout the first 2 years.

After presenting our main results, we examine their sensitivity to possible selection bias. Our analysis uses data on community college entrants, but Pell eligibility may shift who chooses to enroll in a community college in the first place. Indeed, we find a discontinuity in the density of observations around the cutoff that suggests students who qualify for Pell are disproportionately induced *not* to enroll in community college (perhaps because they attend either a 4-year or for-profit institution instead). Although we are reassured that student characteristics do not appear to shift around the cutoff, we also address the problem using two methods introduced in the literature: (a) limiting our analysis to a subset of colleges where we do not observe any evidence of differential selection, and (b) performing a bounding analysis under extreme assumptions about the missing population.

Unfortunately, because our main estimates are modest to begin with, they are not particularly robust to these rigorous sensitivity checks, leaving open the possibility that some of the positive effects we find may be due to differential selection into community colleges around the Pell grant cutoff. Still, because we find no differences in observed characteristics around the cutoff, we still view our main results as a reasonable "best guess" regarding the impact of receiving a small Pell grant. In addition, a valuable side effect of examining the potential selection problem is that we can provide some suggestive evidence regarding how Pell grant eligibility may influence institutional choice: The selection patterns we find are much more concentrated in areas with many nearby for-profit institutions.

Our article contributes to the literature in three ways. First, we take a step toward understanding how the nation's largest need-based grant program interacts with other aid programs. We find that other aid programs do respond to the federal Pell Grant. Not only so, we find clear distinctive patterns of financial aid packaging between institutions that participate in federal loans versus those that do not. Second, our article is one of the few that looks into the interaction of Pell eligibility with employment intensity during enrollment. Much interest in the Pell Grant program has focused particularly on the impacts on college enrollment of low-income students. We show that students who are just below the cutoff (Pell eligible) seem to shift their time allocation, reducing work while increasing their enrollment intensity. Finally, our results provide indirect evidence that Pell Grants may influence student enrollment decisions, in contrast to the findings of Marx and Turner (2015).

The remainder of the article proceeds as follows: The section "Financial Aid at Community Colleges" provides background on financial aid at community colleges and on the Pell Grant eligibility formula. The section "Data and Sample" describes our data and sample. In the section "Empirical Methodology," we describe our RD strategy and highlight key identification assumptions. The section "Results" presents our results, and the section "Discussion and Conclusion" discusses implications and open questions.

Financial Aid at Community Colleges

Among community college students enrolled in 2011–2012, on average, 38% of student enrolled received Pell and 17% received federal student loans with an average amount of US$1,140 and US$781 per enrollee, respectively.[5] Students qualify for the same amount of Pell regardless of where they enroll, and if the Pell Grant exceeds tuition and fees, students can receive the remainder back as a refund to cover other educational and living expenses.

Pell is by far the largest source of grant aid for community college students, but approximately 12% of students also receive state grant aid and 13% receive institutional grant aid. Although the average amounts of state and institutional aid (approximately US$190 and US$120, respectively) distributed per enrollee are much smaller than for Pell, our analysis below will suggest that these smaller programs can be particularly important for students around the margin of Pell eligibility. Moreover, institutions may have some discretion about how to distribute state grant aid. In the state we examine here, the state's need-based grant is given as a lump sum to institutions, which can then use their own formula to provide aid to students, as long as it is need-based.

To qualify for any federal aid, students must file a Free Application for Federal Student Aid (FAFSA). This application collects detailed information on students' income and assets, as well as similar information from the parents of dependent students. This information is used in a complex formula that provides an "expected family contribution" or EFC as its output. Although over a 100 pieces of information are required to precisely calculate the EFC, for the vast majority of students, the EFC is determined by income, family size, and number of children in college (Dynarski, Scott-Clayton, & Wiederspan, 2013). Lower income students will have lower EFCs. The EFC is used to distribute not just federal aid, but frequently state and institutional aid as well.

Pell eligibility is directly related to EFC: In general, Pell eligibility equals the maximum Pell in a given year, minus EFC. However, in most years, there is a minimum grant size such that the Pell does not decline continuously to zero, but may drop from several 100 dollars to zero at a certain point in the EFC distribution. The precise formula varies from year to year. In many years prior to 2008, the minimum grant size was US$400 (those with eligibility between US$200 and US$399 were rounded up, while those with eligibility below US$200 received nothing). In years since 2011, the minimum grant has been US$200. However, between 2008 and 2010, the minimum grant size was much larger than usual, in part due to additional American Reinvestment and Recovery Act funding. In 2008–2009 the minimum was US$690, rising to US$976 in 2009–2010, and falling back to US$555 in 2010–2011. We thus focus on the 2008–2010 academic years for our RD analysis.

Eligibility for subsidized student loans is calculated as the total cost of attendance (including estimated living expenses for students attending at least half-time), minus the EFC and other aid already received by the student, subject to annual loan maximums. Students are eligible for unsubsidized loans regardless of EFC. Between 2008 and 2010, the combined limit of subsidized and unsubsidized loans for first-year students was around US$5,500 annually for dependent students and US$9,500 annually for independent students.[6] It is also worth pointing out that total costs of attendance are high enough even at community colleges such that students receiving the minimum Pell Grant are very unlikely to have their state financial aid limited by the cost of attendance (in 2008, for example, average total cost of attendance for full-time students at community colleges was US$9,700).[7] In theory, student may also take out private loans to fund their schooling, but in practice, only 2% to 4% of students at public 2-year college take such loans (Baum, Ma, Pender, & Welch, 2017).

Finally, it is important to point out that not all students at community colleges have access to federal loans. Colleges sometimes choose to opt out of the Stafford loan program in fear of sanctions by the federal government.[8] For students who are eligible for the Pell Grant, those attending colleges that participate in the loan program have a higher likelihood and amount of borrowing as well as a higher number of attempted credit hours in the first year, relative to students attending colleges that do not participate (Wiederspan, 2016).

Data and Sample

The administrative data we use include information from all of the community colleges in a single state (more than 20 individual institutions). The data include five types of information: student demographics, first-year financial aid eligibility and receipt, transcript data, degree/transfer information, and quarterly earnings. Student demographics include race/ethnicity, gender, age, family income, and dependency status. Financial aid information includes the EFC (the summary measure of financial need which determines eligibility for Pell and other federal aid), and amounts of federal, state, and institutional aid actually received (broken out into detailed types of aid). The data do not include information on private loans; however, as noted above, as very few community college students take such loans this is not a major limitation for our analysis. Transcript data include remedial placement test scores for those who took such tests, credits attempted and earned, and grades for each term enrolled in any of the states' community colleges. Credential completion and transfer to 4-year institutions are measured using data from the National Student Clearinghouse (NSC), which include data for students who leave the community college system. Finally, student records are matched to quarterly earnings records, which we use to measure of student labor supply during the first 2 years postentry.[9]

The data are limited to fall entrants to the community college system who had not previously enrolled in any college (first-time beginners).[10] We focus on the 2008–2010 entry cohorts because of particularly large discontinuities in the Pell formula during those years (in earlier and later years, minimum awards were much smaller). In these years, the data include a total of 89,205 students. We further limit our sample to the 57% of students who filed a FAFSA (and thus have the financial information we need for the RD analysis) and have EFCs within US$2,000 of the Pell cutoff in the relevant year. Table 1 shows the characteristics and financial aid measures of our sample. The first three columns describe our analysis sample, while the fourth column provides statistics on the full sample of enrollees (regardless of EFC and including those who did not file a FAFSA) during these years, for comparison.[11] The majority of students in our sample are White students, about equally distributed in gender. On average, students in entry cohorts are slightly above 21 years old. About 60% of students in our analysis sample persisted to the subsequent fall, and about one-third transferred or received a degree within 3 years of entry. The final column provides national averages from the Beginning Postsecondary Students (BPS) 2012/2014 survey, representing first-time students who entered a public 2-year college during academic year 2011–2012. On average, compared with the BPS sample, our main analysis sample (column 3) has fewer Hispanic students, and has lower family income. In terms of financial aid, students in our sample received less state aid and borrowed less compared with the BPS sample.

Table 1 indicates that students above and below the EFC cutoff for receiving Pell are actually quite similar along most demographic dimensions other than family income. This confirms large differences in Pell receipt around the cutoff, but also highlights that students who are ineligible for Pell are also much more likely to take out student loans, and somewhat more likely to receive state grant aid. We will examine these patterns in more detail below.

TABLE 1

Sample Characteristics of 2008–2010 Cohort by Pell Grant Eligibility

	Mean (±2,000 bandwidth)				
Variable	(1) Pell eligible	(2) Pell ineligible	(3) Combined sample	(4) Full sample	(5) National average
Female	54%	55%	55%	53%	53%
Race					
Black	25%	23%	24%	24%	13.4%
Hispanic	7.0%	6.8%	6.9%	7.0%	23.9%
Asian	4.6%	5.2%	4.9%	6.0%	4.8%
White	62.7%	64.5%	63.5%	61.8%	53.1%
American Indian	0.6%	0.4%	0.5%	0.5%	0.8%
Age (years)	21.4	21.1	21.2	21.7	21.5
Any dual enrollment	24%	23%	24%	17%	NA
Persisted to spring term	83%	83%	83%	76%	NA
Persisted to next fall	61%	63%	61%	56%	NA
Transfer/degree within in 3 years	31%	34%	32%	28%	NA
Pretest scores					
Reading	53.7	55.3	54.4	51.9	NA
Writing	47.3	49.0	48.0	44.9	NA
Math	19.1	20.5	19.7	18.5	NA
Prior earnings					
1 year prior	US$2,760	US$3,109	US$2,911	US$2,740	NA
2 year prior	US$1,601	US$1,675	US$1,633	US$1,444	NA
Financial aid					
Applied for financial aid	100%	100%	100%	57%	NA
Dependent	80%	81%	80%	69%	71%
Family income	US$45,454	US$55,891	US$49,961	US$39,768	US$59,365
Family size	3.4	3.5	3.4	3.3	NA
EFC	US$3,495	US$5,523	US$4,371	US$4,545	US$6,494
Received Pell Grant	94%	0%	53%	41%	NA
Average Pell (including 0s)	US$1,261	US$1	US$717	US$1,368	US$1,501
Received total grant	96%	65%	83%	49%	NA
Average total grant (including 0s)	US$2,164	US$1,065	US$1,689	US$1,705	US$2,287
Received state aid	53%	59%	55%	21%	NA
Average state aid (including 0s)	US$618	US$735	US$669	US$188	US$293
Any fed loan	22%	39%	29%	12%	NA
Average loan amt (including 0s)	819	1,442	1,088	507	832
Sample size	4,463	3,392	7,855	89,205	9,587

Note. Columns 1 to 3 are restricted to samples of 2008–2010 fall entry cohort students who have filed FAFSA, for whom race/ethnicity is not missing, and who fall within US$±2,000 of the EFC cutoff for receiving Pell. Column 4 is for the entire 2008–2010 cohort, regardless of EFC or whether a FAFSA was filed (except for dependency, income, family size, and EFC, which are only available for FAFSA applicants). Column 5 shows averages for the nationally representative BPS 2012/14 sample, restricted to those who entered a public 2-year college for the first time in academic year 2011–2012. EFC = expected family contribution; FAFSA = Free Application for Federal Student Aid; BPS = beginning postsecondary students.

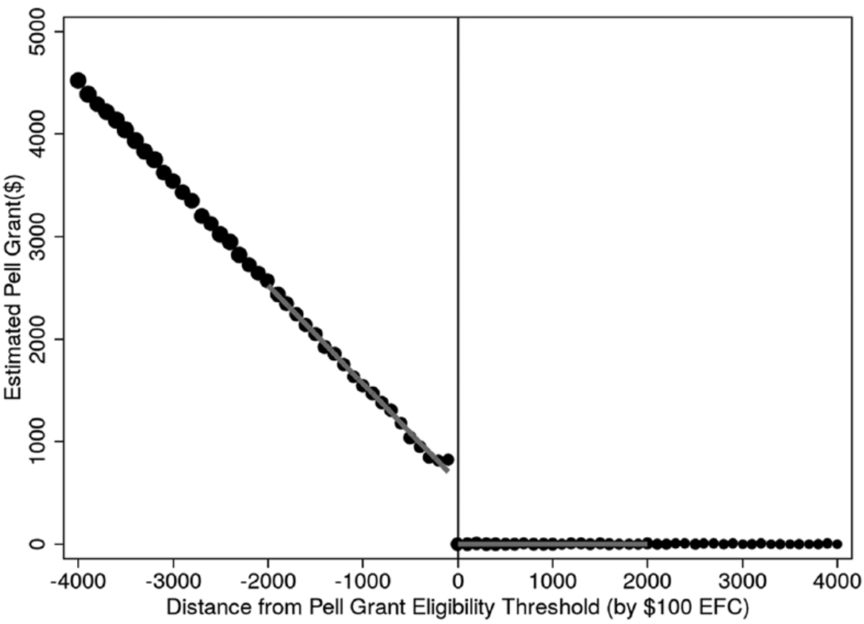

FIGURE 1. *Estimated Pell Grant by EFC (2008–2010 Cohort).*
Note. Samples are restricted to 2008–2010 cohort students who have filed FAFSA, for whom race/ethnicity is not missing, and who are nondual enrollees. Estimated Pell amount is computed by EFC assuming full-time enrollment intensity. Each point is a mean value of the outcome that falls within a bin of size US$100 EFC. Graph shows only points that fall within the US$±4,000 bandwidth. Gray line is a fitted line of mean points within a US$±2,000 bandwidth. EFC = expected family contribution; FAFSA = Free Application for Federal Student Aid.

Empirical Methodology

RD Design

We use a RD design to estimate the causal effect of Pell Grant eligibility for those near the EFC cutoff, using EFC as our forcing variable. The statutory discontinuity in Pell for a full-time student was US$690 in 2008–2009, US$976 in 2009–2010, and US$555 in 2010–2011 (awards are prorated for less-than-full-time enrollment).[12] The formula is reflected in Figure 1, which plots students' estimated Pell eligibility based on their EFC. We use estimated Pell eligibility here instead of actual Pell amounts received, because amounts received are endogenous to enrollment intensity. Later graphs that show actual Pell received will reflect a similar, if slightly muted pattern (as amounts received reflect enrollment intensity and can only be equal to or less than estimated eligibility).

The treatment of interest, which is fully determined by the forcing variable, is whether or not someone is *eligible* for the minimum Pell grant (Appendix D, in the online version of the journal, shows the relationship between EFC and the actual probability and amount of Pell receipt). This primary treatment could affect outcomes through multiple channels, including actual Pell grants received, changes in loan take-up, changes in other aid, or even via psychological effects (either positive or negative).

We implement the RD using a local linear regression estimator with a rectangular kernel (i.e., with all observations weighted equally) for observations within US$±2,000 from the EFC cutoff (Hahn, Todd, & Van der Klaauw, 2001; Imbens & Lemieux, 2008).[13] Specifically, we estimate

$$\begin{aligned} Y_{ist} &= \alpha + \beta_1\left(\text{PellEligible}_{it}\right) + \beta_2\left(\text{Dist}_{it}\right) \\ &+ \beta_3\left(\text{Dist}_{it} \times \text{Below}_{it}\right) + X_i\delta + \phi_s + \tau_t + \varepsilon_{ist}, \end{aligned} \tag{1}$$

where, $Dist_{it}$ is distance from the EFC cutoff for Pell eligibility in year t ($Dist_{it} = \text{EFC}_{it} - c_{0t}$), Below_{it} is a binary outcome indicating whether individual i in year t has EFC that is below the cutoff (individual is Pell-Eligible if their EFC is below the cutoff); X_i is a vector of individual-level covariates including race/ethnicity dummies, age, income, dependent status, whether the student had dual enrollment credits from high school, and placement math, reading, and writing scores (with flags for missing scores); φ_s is a vector of school fixed effects; and τ_t is vector of dummies for each cohort. If the RD assumptions hold, adding covariates (X_i) is not necessary for identification of causal effects, but will adjust for small sample bias and reduce standard errors. We are interested in β_1, treatment effect of Pell eligibility.

For RD estimates to be valid two assumptions need to be satisfied: (a) a discontinuity in treatment assignment $E\left[\text{PellEligible}_i|\text{EFC}_i = c\right]$ exists at the cutoff (c_0) and (b) in the absence of treatment, distribution of unobservables with respect to the running variable is continuous at the cutoff (c_0) (Hahn et al., 2001; Imbens & Lemieux, 2008) where, $\text{PellEligible}_i \in \{0,1\}$ are treatment status. To test this assumption, we follow the convention by checking smoothness in the density through McCrary test and estimating equation using pretreatment covariates as an outcome.

For our main analysis, we perform robustness checks through different bandwidths. In addition to testing for sensitivity across different bandwidths, we also use three bandwidth selection methods: cross-validation (Ludwig & Miller, 2005) and two plug-in rules—Imbens and Kalyanaraman (2012; hereafter, IK) and Calonico, Cattaneo, and Titiunik (2014; hereafter, CCT)—as a comparison to our baseline specification.[14] We estimate optimal bandwidths under each method for all the outcomes separately and examine their distribution.

Threats to Validity

A key assumption for an unbiased RD estimator is that individuals should not be able to systematically manipulate whether they fall above or below the cutoff of the forcing variable. As our sample is limited to FAFSA applicants, one concern is that students who do not expect to receive a Pell Grant would not bother to apply. This could create a loss of observations above the Pell cutoff. It is unlikely, however, that students/families can even predict let alone manipulate their EFCs so precisely around the threshold we use in our analysis, which separates students getting a small Pell Grant from those getting US$0. The EFC calculation is extremely opaque, relying upon hundreds of inputs from the FAFSA, and both the EFC formula and the relevant cutoffs change from year to year. Furthermore, a high proportion of financial aid applicants will have to submit tax documents to verify their income, so even if a savvy applicant knew the cutoffs it would not be straightforward to manipulate the inputs. Finally, even if a student expected to receive no Pell Grants, they may still apply to be considered for other types of federal, state, and institutional aid, many of which rely upon the federal EFC for eligibility.[15]

However, another way that the assumption of continuity in $f(\text{EFC}_i)$ can be violated is if there is differential selection into our community colleges sample around the cutoff. This is a bigger concern in this context, because our sample includes only students who ultimately enrolled in the community college system, and most students learn their aid eligibility prior to initial enrollment. If Pell eligibility induces some individuals to enroll in college who would not have otherwise, or if it influences students' choice of institution, this will cause a discontinuity in $f(\text{EFC}_i)$ within our sample frame.

This assumption can be tested by examining the density of observations around the cutoff. As shown in Figure 2, which plots density using US$100 EFC bins, we can see that there is a jump in the number of observations just to the right of the cutoff; that is, students are more likely to appear in our community colleges sample if they are *ineligible* for Pell. The direction of this enrollment jump is counterintuitive to what we would expect if Pell Grant induced student's enrollment choices. To confirm this discontinuity, we conduct a McCrary (2008) test, which rejects the null hypothesis that the density is smooth. Given the direction of enrollment jump, we hypothesize that the "missing" students to the left of the cutoff may be using their Pell Grants to attend schools other than community colleges. We explore this hypothesis further in the section following our main results.

Another approach to evaluating selection bias around the cutoff is to test for discontinuities in

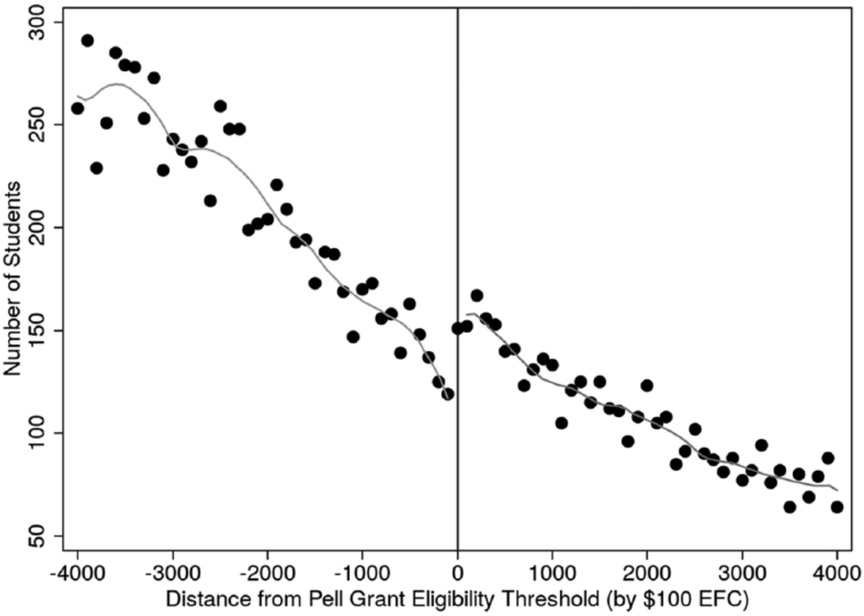

FIGURE 2. *Density plot for all schools.*
Note. Samples are restricted to 2008–2010 cohort students who have filed FAFSA, for whom race/ethnicity is not missing, and who are nondual enrollees. Points represent number of students (sum count) that fall within a bin of size US$100 EFC. Points within a US$±4,000 bandwidth are included in the figure. Gray line is a local smoothed polynomial line with Degree 2, using points within the US$±4,000 bandwidth. FAFSA = Free Application for Federal Student Aid; EFC = expected family contribution.

the baseline covariates around the EFC cutoff. Appendix Table C1 (in the online version of the journal) illustrates the relationship between covariates and EFC where we use a version of Equation 3 above with covariates on the left-hand side to test for any significant discontinuities. Reassuringly, despite the substantial discontinuity in the density, we find no evidence of discontinuities in any baseline covariates at the cutoff in our preferred US$2,000 bandwidth, including not just age, race/ethnicity, and gender, but also family income, dependency status, and placement test scores.[16] This conclusion holds even after limiting the sample to loan schools (see Appendix Table C1 and Appendix E in the online version of the journal), which have the largest discontinuity in density. Given the possibility that selection effects could occur in both directions, however, similar averages could mask differences in the distribution of student characteristics around the cutoff. For example, we do find that students who just barely qualify for Pell are slightly less likely to come from either the top or bottom quartile of family income, and more likely to come from the lower middle quartile (see Appendix Table C2 in the online version of the journal).

Our primary strategy to mitigate selection bias is to control for observable characteristics around the cutoff. In addition, to assess the possible role of selection on unobservable dimensions, we test the sensitivity of our results by following two procedures introduced in the literature: (a) analysis of impacts for a subset of institutions for which no discontinuity in the density of observations is observed (as proposed by Calcagno & Long, 2008), and (b) an RD bounding analysis (as proposed by Gerard, Rokkanen, & Rothe, 2016 [hereafter, GRR]). We describe these strategies in more detail after presenting our main results.

It is worth noting that while this discontinuity is problematic for an analysis of outcomes among community college enrollees, it also provides indirect evidence that Pell eligibility does influence initial enrollment decisions, which is an important margin of impact on its own. This is in contrast to findings in Marx and Turner (2015), who find no evidence that Pell eligibility affects either the enrollment margin or the choice of 2- versus 4-year college for students who applied to City University of New York (CUNY) colleges.[17]

Finally, EFC is available only for students who filed a FAFSA, which restricts our analysis among those who filed a FAFSA. It is worth exploring the sample we are excluding, the non-FAFSA applicants around the Pell cutoff. We expect sophisticated students with low chances of receiving Pell are less likely to apply for FAFSA. However, obscurity in EFC calculation and yearly changing cutoff scores makes it difficult for students to be sorting-out in a systematical way on one side of the cutoff. Alternatively, less sophisticated students may be missing from our sample from miscalculating their probability of receiving Pell, not knowing about FAFSA, or inability to complete all components in the application. In either case, we do not expect any systematic sorting of missing students in one side of the cutoff from the opaque nature of EFC calculation and cutoff. However, our results can have different generalization depending on the characteristics of students who are included in our sample. If students fail to submit a FAFSA primarily because they believe they do not need financial assistance to attend, then we might expect effects for nonapplicants around the cutoff to be smaller than they would be for our applicant sample. Alternatively, if students decline to submit a FAFSA primarily because they are uncertain about their eligibility or ambivalent about their enrollment, we might expect the impact of aid could be even larger for FAFSA nonapplicants around the cutoff than for our applicant sample. Unfortunately, without information about income, we have no way to select non-FAFSA students around the cutoff. As an alternative, Table C4 compares the characteristics, prior earnings, and financial aid measures of FAFSA and non-FAFSA students. We see that about 55% of the full sample applied to FAFSA. Comparing the two populations, non-FAFSA students are less likely to be female, more likely to be White, and have higher average earnings prior to entry (this can be a proxy that we are missing students from a wealthier background). Also, non-FAFSA students proportionally have lower take-up rates for pretest scores and the scores tend to be lower. Financial aid measures indicate that a few grants are available without filing a FAFSA, possibly nonneed-based grants.

Results

Effects of Pell Grant Eligibility on Composition of Overall Financial Aid Package

The two panels of Figure 3 illustrate how different components of students' aid packages change around the Pell eligibility cutoff, with observations grouped into US$100 EFC bins and the size of each circle reflecting the number of observations. All panels plot data for students at loan schools and no-loan schools separately, for reasons that will become clear. The left panel shows actual Pell Grant amounts received, and indicates an increase of approximately US$500 just to the left of the cutoff, with no difference between loan and no-loan schools.[18] However, a clear difference between these two institution types emerges when we look at the right panel, plotting average *total* grants by EFC. Across most of the EFC distribution, the institutions that do not offer student loans give out more in total grants. They also use state grant aid to compensate students just above the cutoff for Pell, such that at these institutions, there is no discontinuity in total grant aid around the Pell cutoff.[19] A large discontinuity in total grant aid exists only for institutions that participate in the student loan programs.

The left panel of Figure 4 shows student loan receipt by EFC. Of course, at no-loan schools, student loans are zero throughout the distribution.[20] At loan schools, we see a sizable jump in average loan amounts for students just above the Pell eligibility threshold. Considering all aid together, the right panel shows that for neither institution type is there any discontinuity in total aid received. For no-loan schools, state grant aid smooths out the discontinuity in Pell, while for loan schools, the discontinuity is smoothed out by loans. (Also note that the higher state grant aid at no-loan schools does not completely make up for the lack of loans: Students at no-loan schools receive substantially less in total aid than students at loan schools.)

Table 2 shows the regression results corresponding to the panels of Figures 3 and 4, with the top portion of the table showing results for loan schools and the bottom portion showing results for no-loan schools. Confirming what is visible in the pictures, there is a large

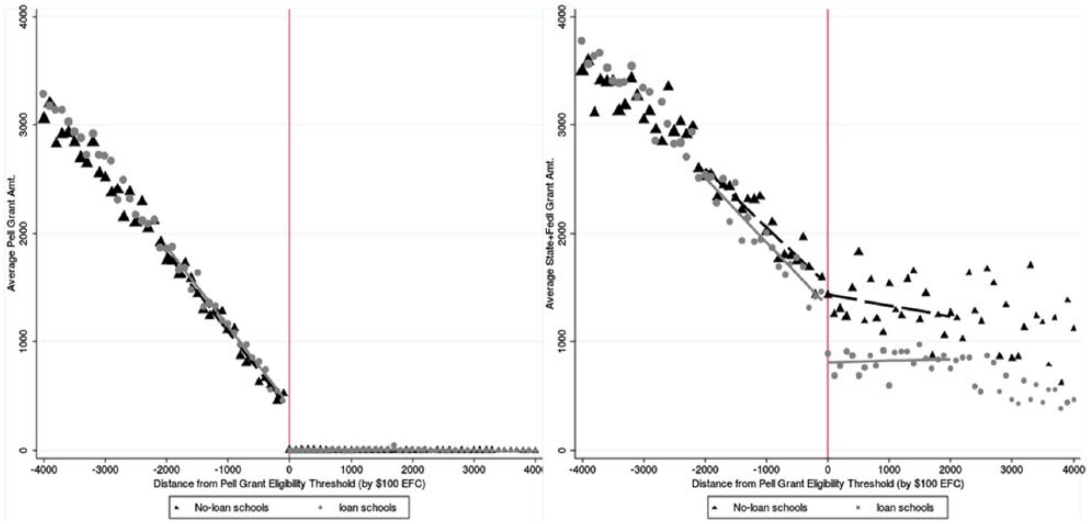

FIGURE 3. *Grant amounts (US$) for loan and no-loan schools.*
Note. Samples are restricted to 2008–2010 cohort students who have filed FAFSA, for whom race/ethnicity is not missing, and who are nondual enrollees. Averages are plotted separately for loan schools (triangle points) and no-loan schools (circle points). Each point represents mean outcomes for students that fall within a bin of size US$100 EFC. Only points within a US$±4,000 bandwidth are in the figure. Gray solid (loan schools) and black dashed (no-loan schools) lines are the linear fitted value of these points that fall within the US$±2,000 bandwidth. FAFSA = Free Application for Federal Student Aid; EFC = expected family contribution.

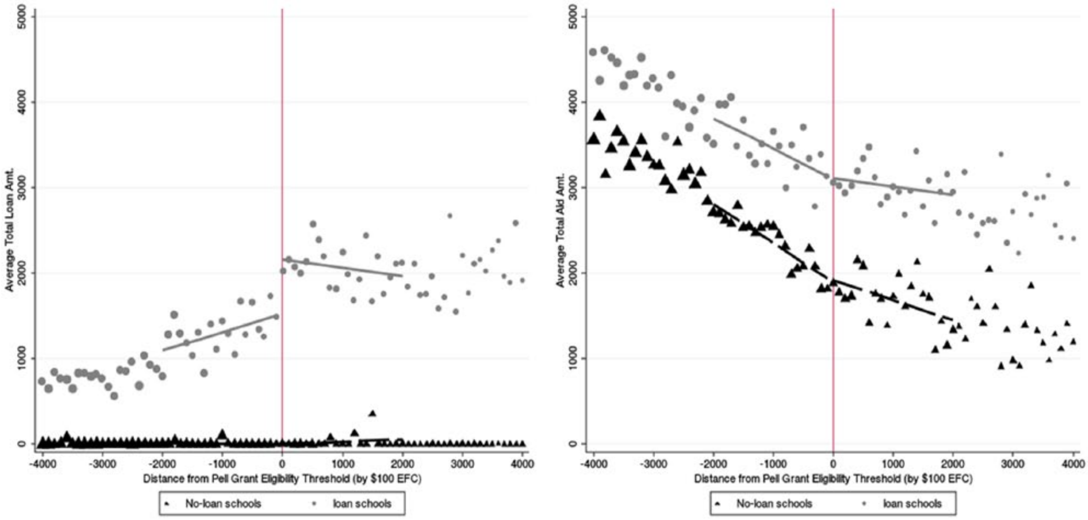

FIGURE 4. *Loan and total aid amounts (US$) for loan and no-loan schools.*
Note. Samples are restricted to 2008–2010 cohort students who have filed FAFSA, for whom race/ethnicity is not missing, and who are nondual enrollees. Averages are plotted separately for loan schools (triangle points) and no-loan schools (circle points). Each point represents mean outcomes for students that fall within a bin of size US$100 EFC. Only points within a US$±4,000 bandwidth are in the figure. Gray solid (loan schools) and black dashed (no-loan schools) lines are the linear fitted value of these points that fall within a US$±2,000 bandwidth. FAFSA = Free Application for Federal Student Aid; EFC = expected family contribution.

discontinuity in Pell Grants in both cases, but at no-loan schools, there is no significant discontinuity in total grant aid, loans, or total aid. For loan schools, there is a significant discontinuity in total grant aid (coefficient = US$560, $p < .01$), but an equal-and-opposite discontinuity in loan

TABLE 2

RD Estimates of Effect of Pell Eligibility on Composition of Financial Aid Packages

	Mean outcomes	(1) Basic 2,000 bandwidth		(2) Without covariate		(3) 1,000 bandwidth		(4) 4,000 bandwidth		(5) 4,000 bandwidth, quadratic	
Outcome	Just above cutoff	Coefficient	(*SE*)	Coefficient	(*SE*)	Coefficient	(*SE*)	Coefficient	(*SE*)	Coefficient	(*SE*)
Institutions offering federal loans											
Amount of Pell received	US$0	US$459	(17)***	US$467	(17)***	US$442	(20)***	US$445	(16)***	US$426	(22)***
Amount of Pell + State grants received	US$869	US$560	(64)***	US$574	(66)***	US$598	(92)***	US$413	(46)***	US$513	(69)***
Amount of loans received	US$1,953	US$−592	(113)***	US$−639	(118)***	US$−534	(163)***	US$−451	(79)***	US$−574	(120)***
Amount of total aid received	US$2,993	US$89	(129)	US$56	(131)	US$201	(185)	US$107	(91)	US$33	(137)
Sample size	1,421	5,753		5,753		2,828		11,944		11,944	
Institutions not offering federal loans											
Amount of Pell received	US$0	US$434	(25)***	US$427	(25)***	US$409	(32)***	US$481	(25)***	US$435	(34)***
Amount of Pell + State grants received	US$1,640	US$132	(105)	US$97	(118)	US$66	(151)	US$203	(77)***	US$90	(116)
Amount of loans received	US$4	US$3	(8)	US$6	(8)	US$−4	(11)	US$−9	(9)	US$7	(7)
Amount of total aid received	US$2,044	US$153	(123)	US$113	(136)	US$−40	(180)	US$268	(89)***	US$100	(136)
Sample size	456	2,102		2,102		1,048		4,421		4,421	

Note. Samples are restricted to students in the 2008–2010 fall entry cohorts who filed FAFSA and for whom race/ethnicity is not missing. Top panel estimates use only loan schools and bottom panel estimates use only no-loan schools. Coefficients indicate beta values for indicator of treatment status (i.e., 1 if eligible for Pell and 0 otherwise). Huber–White robust standard errors are in parentheses. Columns 1 and 2 are for samples within US$±2,000 bandwidth, column 3 for US$±1,000 bandwidth, columns 4 and 5 for US$±4,000 bandwidth. All specifications control for cohort fixed effects. All columns except column 2 control for covariates—female, Black, Hispanic, Asian, American Indian, age, income, dependent, dual enrollment, reading, writing, math score prior to entry, and flags on whether they have these test scores—and college fixed effects. All columns except for column 5 use local linear polynomial regression, while column 5 uses quadratic polynomial specification. Rectangular kernel is used in all specifications. RD = regression-discontinuity; FAFSA = Free Application for Federal Student Aid.

*$p < .1$. **$p < .05$. ***$p < .01$.

aid (coefficient = US$–592, $p < .01$), leading to no discontinuity in total aid.[21] The pattern of loan take-up at these schools replicates that found in previous research by Marx and Turner (2015), though it appears even more strongly in our sample. Figure 5 further shows that the "missing observations" below the Pell eligibility threshold are completely concentrated among loan schools; we see no discontinuity in the density of observations within the no-loan schools (consistent with the hypothesis that grant availability may affect initial enrollment decisions).

For no-loan schools, which in our sample represent about half of the institutions but only about one quarter of students enrolled, we see a very different pattern: Pell eligibility has no discontinuous effect on total grants, loans, or total aid. Therefore, we limit our subsequent analyses to students attending only loan-offering schools, where we do observe a significant discontinuity in overall grant aid. We later examine the no-loan schools as a type of placebo test. Even at loan institutions, these findings alter how we think about the treatment. In interpreting the effects that follow, it is important to recognize that for this sample, there is little difference in total aid received for those above and below the cutoff, but those who are Pell-eligible clearly receive more aid in the form of grants versus loans.[22]

Effects of Pell Grant Eligibility on Academic Outcomes and Labor Supply While Enrolled

Table 3 shows our estimated impacts on academic outcomes and student labor supply. We examine re-enrollment and enrollment intensity, cumulative GPA and credits completed, and earnings during each of the first 2 years. We also examine GPA, credits attained, credentials, and transfer at the end of our 3-year follow-up period. Note that for all outcomes, the difference in treatment is based on the first-year difference in aid received; this does not measure the cumulative effect of receiving Pell for more than 1 year.[23]

With a few exceptions, our results are mostly in a positive direction, but small and not statistically significant. Among the notable exceptions are that we do find significant positive effects on full-time enrollment in the spring of the first year (5 percentage point increase from a base of 52%), full-time enrollment in the fall of the second year (7 percentage point increase from a base of 37%), and full-time enrollment in the spring of the second year (4 percentage point increase from a base of 33%). In contrast, we find a negative effect on summer term enrollment between Years 1 and 2 (of about 5 percentage points), which is surprising taking into account that these include years in which summer Pell Grants were available.[24]

We also find consistently negative earnings effects during the first 2 years, though the reduction is only statistically significant in the first year. The negative earnings effects translate into about US$12 to US$20 less per week and are of the same order of magnitude as the increase in grant aid for Pell-eligible students. These reductions are consistent with a story in which Pell allows students to shift their time allocation, perhaps 1 or 2 hours per week, from work to school. If true, we might expect to see increases not just in credits but in GPA. Although effects on cumulative GPA were in a positive direction (between 0.06 and 0.08 points), they were not statistically significant (though they were very close by the end of our follow-up period).

Effects on cumulative credits earned, degree completion, and transfer measured 3 years after entry were generally in a positive direction and of a magnitude consistent with the positive effects observed in the time periods closest to the treatment. However, we do not have power to detect small effects on these distal outcomes, and it may simply be unrealistic to expect to see anything other than small effects given the treatment, which amounts to replacing US$500 in loans with US$500 in grants. In some respects, it might be considered surprising to find any effects of such a modest treatment.

For no-loan schools, we find little sign of any effects on student outcomes (these results are available in Appendix Table C6 in the online version of the journal). The sole exception is an isolated negative effect of Pell eligibility on cumulative GPA in some years and specifications.[25] Moreover, as shown in Figure 5, there is no discontinuity in the density of observations around the cutoff for these schools.

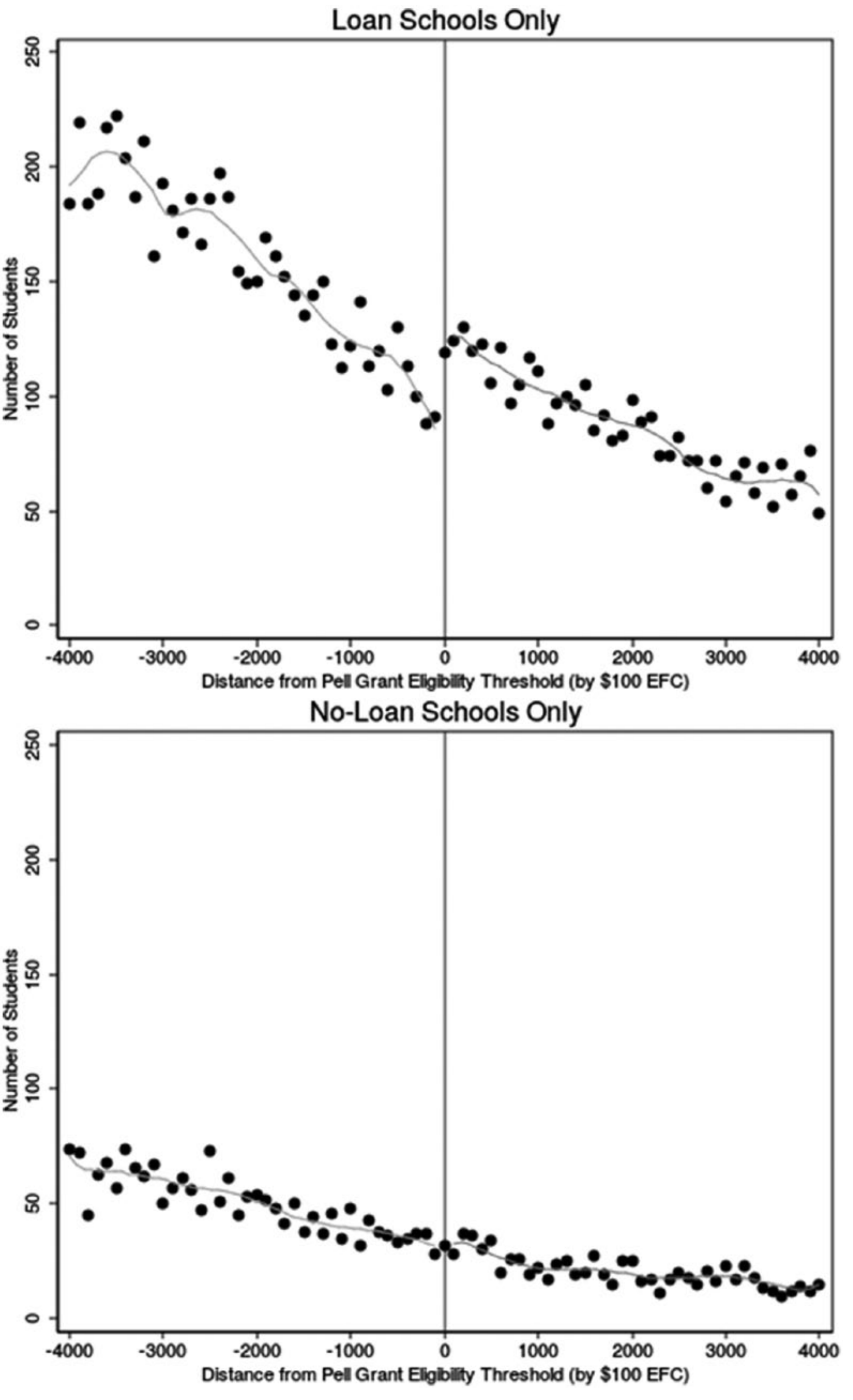

FIGURE 5. *Density plot for loan schools (top) and no-loan schools (bottom).*
Note. Samples are restricted to 2008–2010 cohort students who have filed FAFSA, for whom race/ethnicity is not missing, who are nondual enrollees, and only for students attending loan schools (top) or no-loan schools (bottom). Points represent number of students (sum count) that fall within a bin of size US$100 EFC. Points within a US$±4,000 bandwidth are included in the figure. Gray line is a local smoothed polynomial line with Degree 2, using points within the US$±4,000 bandwidth. FAFSA = Free Application for Federal Student Aid; EFC = expected family contribution.

TABLE 3

RD Estimates of Effect of Pell Eligibility on Academic Outcomes and Student Labor Supply (Loan Schools)

	Mean outcomes	(1) Basic 2,000 bandwidth		(2) Without covariate		(3) 1,000 bandwidth		(4) 4,000 bandwidth		(5) 4,000 bandwidth, quadratic	
Outcome	Just above cutoff	Coefficient	(*SE*)	Coefficient	(*SE*)	Coefficient	(*SE*)	Coefficient	(*SE*)	Coefficient	(*SE*)
Year 1 outcomes											
Enrolled full-time, Year 1 fall	0.657	0.020	(0.024)	0.030	(0.025)	−0.003	(0.034)	0.002	(0.017)	0.008	(0.025)
Reenrolled, Year 1 spring	0.842	−0.016	(0.020)	−0.012	(0.020)	−0.028	(0.029)	−0.001	(0.014)	−0.048	(0.021)**
Enrolled full-time, Year 1 spring	0.520	0.048	(0.026)*	0.058	(0.027)**	0.026	(0.037)	0.034	(0.018)*	0.019	(0.027)
Enrolled, Year 1 summer	0.290	−0.046	(0.023)**	−0.049	(0.024)**	−0.091	(0.033)***	−0.022	(0.017)	−0.065	(0.025)***
Cumulative GPA, end of year	2.473	0.061	(0.056)	0.060	(0.060)	−0.027	(0.081)	0.007	(0.040)	0.044	(0.060)
Cumulative credits completed, end of year	17.462	0.480	(0.559)	0.628	(0.589)	−0.647	(0.808)	−0.102	(0.390)	−0.312	(0.591)
Cumulative Year 1 earnings (Q4–Q3)	US$4,873	US$−806	(393)**	US$−911	(406)**	US$−710	(538)	US$−727	(282)**	US$−616	(420)
Year 2 outcomes											
Reenrolled, Year 2 fall	0.616	0.003	(0.026)	0.005	(0.026)	−0.014	(0.037)	0.014	(0.018)	−0.015	(0.027)
Enrolled full-time, Year 2 fall	0.371	0.074	(0.026)***	0.079	(0.026)***	0.065	(0.036)*	0.036	(0.018)**	0.047	(0.027)*
Reenrolled, Year 2 spring	0.580	0.005	(0.026)	0.005	(0.026)	0.034	(0.037)	−0.003	(0.018)	−0.002	(0.028)
Enrolled full-time, Year 2 spring	0.328	0.044	(0.025)*	0.047	(0.025)*	0.044	(0.036)	0.021	(0.017)	0.026	(0.026)
Enrolled, Year 2 summer	0.226	−0.004	(0.022)	−0.004	(0.022)	0.013	(0.031)	−0.011	(0.015)	0.005	(0.023)
Cumulative GPA, end of year	2.401	0.074	(0.053)	0.084	(0.057)	0.027	(0.075)	0.022	(0.037)	0.051	(0.056)
Cumulative credits completed, end of year	29.075	1.243	(1.063)	1.463	(1.123)	−0.033	(1.530)	0.011	(0.745)	−0.202	(1.127)
Cumulative Year 2 earnings (Q4–Q3)	US$5,323	US$−534	(445)	US$−627	(455)	US$−552	(620)	US$−364	(318)	US$−385	(475)
End of Year 3 attainment outcomes											
Cumulative GPA	2.392	0.084	(0.052)	0.097	(0.056)*	0.046	(0.074)	0.019	(0.036)	0.059	(0.055)
Cumulative credits earned	35.205	1.741	(1.342)	1.937	(1.406)	0.946	(1.935)	−0.041	(0.940)	0.312	(1.423)
Ever transferred to 4 year	0.215	0.026	(0.021)	0.028	(0.022)	−0.002	(0.031)	0.008	(0.015)	0.005	(0.023)
Earned any degree/certificate	0.206	0.010	(0.021)	0.016	(0.022)	0.000	(0.030)	−0.011	(0.014)	−0.012	(0.022)
Earned any degree/certificate or transferred	0.317	0.026	(0.024)	0.032	(0.025)	0.005	(0.035)	−0.003	(0.017)	0.002	(0.025)
Sample size	1,421	5,753		5,753		2,828		11,944		11,944	

Note. Samples are restricted to students in the 2008–2010 fall entry cohorts who filed FAFSA, for whom race/ethnicity is not missing, and among those attending loan schools. Coefficients indicate beta values for indicator of treatment status (i.e., 1 if eligible for Pell and 0 otherwise). Huber–White robust standard errors are in parentheses. Columns 1 and 2 are for samples within US$2,000 bandwidth, column 3 for US$±1,000 bandwidth, columns 4 and 5 for US$±4,000 bandwidth. All specifications control for cohort fixed effects. All columns except column 2 control for covariates—female, Black, Hispanic, Asian, American Indian, age, income, dependent, dual enrollment, reading, writing, math score prior to entry, and flags on whether they have these test scores—and college fixed effects. All columns except for column 5 use polynomial 1 degree specification, while column 5 uses quadratic polynomial specification. Rectangular kernel is used in all specifications. RD = regression-discontinuity; GPA = grade point average; FAFSA = Free Application for Federal Student Aid.
**p* < .1. ***p* < .05. ****p* < .01.

Sensitivity Checks

Optimal Bandwidth. Tables 2 and 3 assess the sensitivity of our RD estimators using bandwidths of ½ and 2 times our baseline bandwidth of US$±2,000 (US$±1,000 and US$±4,000, respectively). The general pattern and sign of our main results holds across different bandwidths; however, both magnitude and significance level fluctuates. For the wide bandwidth, coefficients are generally smaller. We also calculated optimal bandwidths under three different methods—cross-validation, IK, and CCT—separately for each outcome considered (see Appendix Table C3, in the online version of the journal, for a summary of these results).[26] Across outcomes, the average bandwidth suggested by cross-validation and IK is around US$±4,000, while CCT suggests US$±1,366. Our baseline US$±2,000 bandwidth lies at the lower end for cross-validation and IK but at the upper end for CCT. Given these results, we think our baseline bandwidth of US$±2,000 bandwidth is reasonable.

Degree of Polynomial. Misspecification of functional form can generate bias in our treatment estimator when calculating using linear regression (Lee & Lemieux, 2010). Thus, the last column of Tables 2 and 3 also provide results using a quadratic specification (with our widest bandwidth). Again, the overall pattern of results is similar to baseline, but magnitudes shift and here we see some negative results (on spring/summer enrollment in Year 1) become significant. To explore optimal degree of polynomial, we conduct a degree of polynomial test following Lee and Lemieux (2010) nonparametric approach by adding bin dummies to the polynomial regression and testing for joint significance of the bin dummies (equivalent to an F test using R^2 from with and without the bin dummies regression, see Appendix Table C4, in the online version of the journal, for full results).[27] For each outcome, polynomial degree is determined by the degree whereby adding a higher order term no longer makes the bin dummies jointly significant. In some cases, bin dummies remain significant regardless of the order of polynomial.[28] However, for variables where functional form does matter, a linear specification (polynomial of Degree 1) is generally supported.

Addressing Sample Selection Bias

Limit Analysis to Subgroup Where No Discontinuity Is Present. We first use a subgroup selection method introduced by Calcagno and Long (2008) to address the problem of discontinuous density in a different RD setting. Calcagno and Long (2008) examine the impact of a test score–based assignment to remediation and find discontinuities in the density of observations around the cutoff at some institutions in their sample but not others. They conduct a separate McCrary test for each institution and select only a subset of institutions with smooth densities for further analysis. When we follow a parallel approach, we find that nine smaller institutions exhibit no discontinuity in enrollments around the Pell Grant cutoff, while three large institutions do.[29] Hereafter, we refer to the former group of institutions as the continuous group, and the latter as the noncontinuous group.

Table 4, which examines how these two groups of institutions differ, is revealing in itself. Table 4 compares characteristics across the two subgroups, continuous and noncontinuous institutions. Initially, we look at averages of pretreatment covariates for all of our 2008–2010 cohorts. Students at noncontinuous schools are more males and have more students of color (Black, Hispanic, and Asians) and substantially fewer White students. Noncontinuous schools have more students who took remedial tests and have slightly higher writing and math scores, on average.[30] Exploring counts and distance of local schools, we find striking differences between the two groups. On average, continuous schools have no community colleges, 0.4 four-year schools, and 1.8 for-profit institutions within 10 miles. Schools with discontinuous enrollment around the Pell cutoff also have no community colleges, but more 4-year schools and many more for-profit schools within 10 miles (1.7 and 12.7, respectively). On average, a student at one of these schools is only about three miles away from either a 4-year or a for-profit institution, while at continuous schools the nearest alternatives in these sectors are about 20 miles away. (As one might expect, noncontinuous schools are located in more urban areas.) The large difference in nearby for-profit alternatives, in particular, suggests that perhaps the missing students

TABLE 4

Characteristics of Continuous Versus Noncontinuous Density (2008–2010 Cohort, Loan Schools)

Outcome	Continuous schools *M*	Noncontinuous schools *M*
Female (%)	0.528	0.511
Black (%)	0.223	0.285
Hispanic (%)	0.031	0.120
Asian (%)	0.024	0.107
White (%)	0.717	0.481
American Indian (%)	0.006	0.006
Age	21.616	21.601
Dual enrollment	0.278	0.055
Income	US$38,752	US$44,754
Depend	0.688	0.692
Has remedial reading (%)	0.536	0.680
Has remedial writing (%)	0.545	0.688
Has remedial math (%)	0.387	0.614
Remedial reading placement score	81.553	81.653
Remedial writing placement score	69.049	71.703
Remedial math placement score	34.190	36.007
Prior credits attempted	3.864	1.249
Prior credits earned	3.507	1.040
Prior year earnings (Q3–Q4–Q1–Q2)	US$2,921	US$2,597
Sample size	24,321	43,221
Local market		
Average number of nearby 2-year public schools (*N*)	0.0	0.0
Average distance to nearest 2-year school (miles)	27.7	25.3
Average number of nearby 4-year schools (*N*)	0.4	1.7
Average distance to the nearest 4-year school (miles)	20.4	3.0
Average number of nearby for-profit schools (*N*)	1.8	12.7
Average distance to nearest for-profit school (miles)	18.2	2.5

Source. College Scorecard Data (n.d.).
Note. Top panel: We take all samples from 2008–2010 cohorts and average the characteristics by whether student's school is in the noncontinuous or continuous group. Bottom panel: We define nearby schools as those located within less than 10 miles from our sample schools. Distance is calculated using latitude and longitude coordinates. All local market variables are averages for schools in the noncontinuous or continuous group. *M* = mean.

who are eligible for Pell may have switched their enrollment to attend for-profit schools instead of community colleges. This would be consistent with Cellini's (2010) finding that increases in the availability of Pell awards increased enrollment at for-profit colleges. Carruthers and Welch (2017) find that males who are just eligible for Pell are more likely to enroll in for-profit colleges, and less likely to enroll in public 2-year colleges, though the estimates are not always significant and the same patterns are not found for women. It is also possible, however, that students are using the Pell Grant to attend 4-year colleges as well. Although not specific to Pell Grants, several studies have found that financial aid can induce students to enroll in 4-year rather than 2-year colleges (e.g., Bettinger, Gurantz, Kawano, & Sacerdote, 2016; Castleman & Long, 2016; Scott-Clayton & Zafar, 2016).[31]

Unfortunately, the large differences in demographics across the two groups of institutions can make any differences in impacts harder to interpret. Although it would be reassuring if our analyses held up within our subset of continuous–density schools, if they do not, it is not clear whether this indicates that our results are driven by selection, or simply that Pell Grants have heterogeneous effects for different student populations. Nonetheless, we present our results for these two subsets of schools separately. First, we check for continuity in the density for all students attending continuous schools as a whole in Figure 6.[32] Figures 7 and 8 are similar graphical representations of grant amount, loan amount, and total aid amount around the cutoff as in Figures 3 and 4, but for the continuous group and the noncontinuous group separately. Table 5 shows our estimated regression effects on financial aid packages (top four rows) are consistent with our main results in Table 2. However, for academic and labor market outcomes, we see distinctive regression results between continuous and noncontinuous density groups. The general pattern is that few results are significant within the continuous group and some outcomes even have the opposite sign. The positive results that we observe in our main results appear concentrated within the three large institutions with noncontinuous density around the Pell cutoff. The fact that results are concentrated in the group where selection bias is most severe is not reassuring, but for the reasons explained above, neither is it definitive. The two groups are very demographically different and it is possible that the effect of Pell Grant is larger for younger, non-White students with higher test scores.

Bounding Analysis. Another way to account for potential selection bias is to bound our estimates, using a method proposed by Gerard et al. (2016) to deal with sample selection—including cases like ours where the missing data include the running variable itself, not just missing outcome data as in Dong (2017).[33] GRR introduce a way to identify partial treatment effects through estimating upper/lower bounds by making worst/best assumptions about the missing population.[34] For further details about this methodology, see Appendix B in the online version of the journal. GRR define “selectors” as those individuals, in this context, whose enrollment decision is influenced by whether or not they fall above or below the Pell cutoff. In this case, the selectors who fall below the cutoff, and hence qualify for Pell, are unobserved. Above the cutoff are a mix of nonselectors and selectors who would have enrolled elsewhere had they qualified for Pell. The goal of the GRR method is to estimate upper and lower bounds of the effects for only nonselectors by trimming the mixed side (in this case, above the cutoff, which includes both selectors and nonselectors) of the estimated proportion of selectors.

We first estimate the proportion of selectors (τ) by calculating the jump in enrollment at the cutoff from the height of the density curve using local polynomial smoothing with rectangular kernel (and Degree 1 polynomial). Second, assuming selectors have the best (worst) observed outcomes, the upper (lower) bound is estimated by the difference in expectation of outcome between the left and right side of the cutoff, where the side with more observations has been trimmed of observations below (above) the τ (or, respectively, $1-\tau$) quantile. See Appendix B, in the online version of the journal, for further details.

We perform two versions of this bounding analysis. First, we trim separately based on for each individual outcome, as indicated by the GRR method. This produces the widest bounds but is overly conservative in practice because different individuals are trimmed from the sample for each outcome (it is not the case that the best students on one outcome are the best students on all outcomes). So, as an alternative, we also examine results when we trim the sample just once, based on cumulative GPA in the first semester of the first year, and then calculate bounds on all outcomes using that same sample.

Table 6 reproduces our baseline regression estimates (US$±2,000 bandwidth including covariate controls), and then shows the results from these two versions of our bounding analysis. As expected, the GRR bounds in column 2 (in which the sample is trimmed separately for each sample) are very wide. In column 3, we tighten our bounds by trimming only once, based on a single outcome variable, then calculating bounds on different outcome variables using that same trimmed sample. We choose cumulative GPA in the fall semester of entrance to college, under the logic that whatever are the unobservable factors that influence enrollment decisions (e.g., student motivation) may correlate with academic performance as observed after enrollment. Our bounding results (column 3) are tighter with

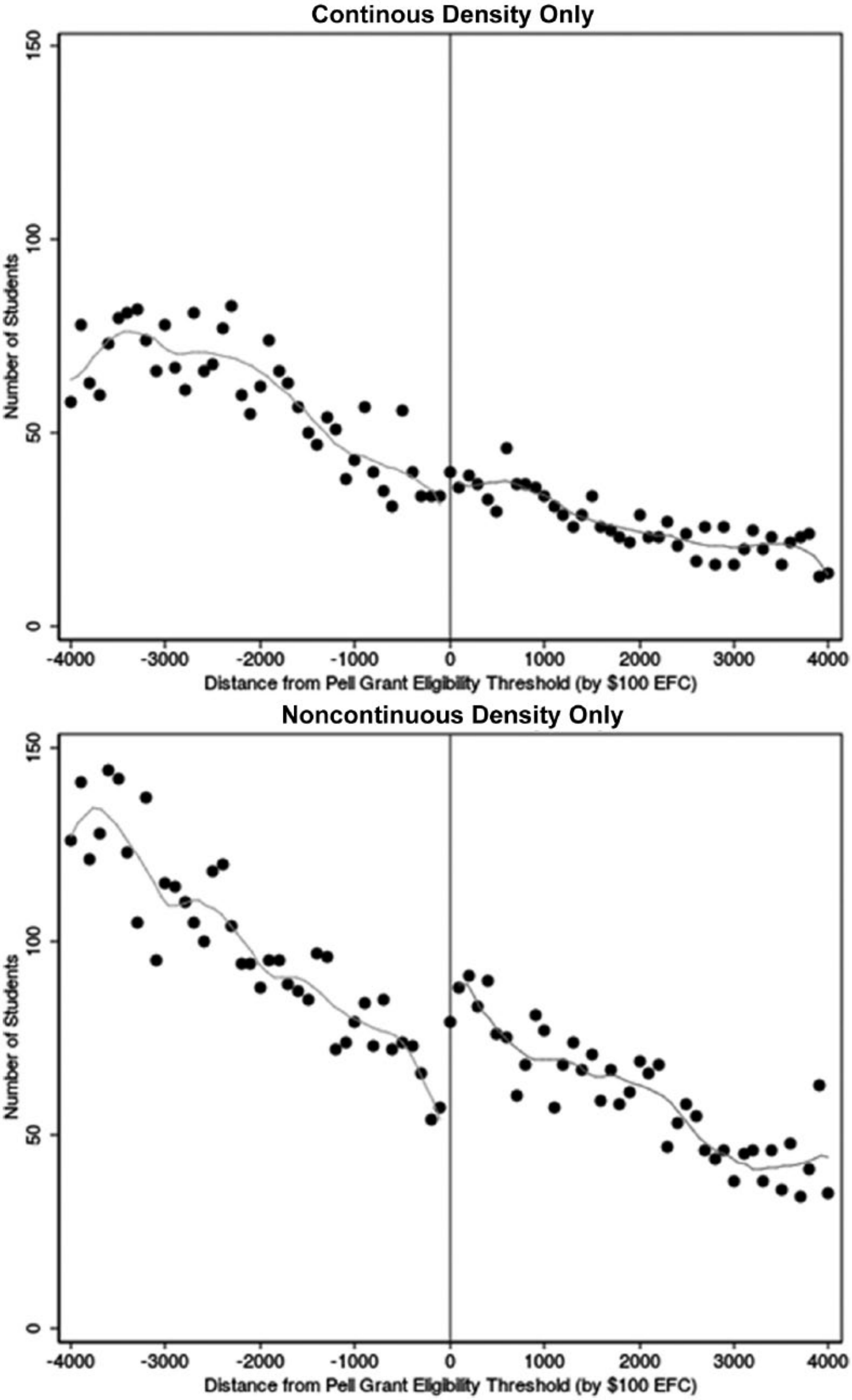

FIGURE 6. *Density plot for continuous schools (top) and noncontinuous schools (bottom).*
Note. Samples are restricted to 2008–2010 cohort students who have filed FAFSA, for whom race/ethnicity is not missing, who are nondual enrollees, and who are attending loan schools. Continuous (noncontinuous) schools are a subset of institutions that passed (failed to pass) individually conducted McCrary test at the institution level. Points represent number of students (sum count) that fall within a bin of size US$100 EFC. Points within US$±4,000 bandwidth are included in the figure. Gray line is a local smoothed polynomial line with Degree 2, using points within the US$±4,000 bandwidth. FAFSA = Free Application for Federal Student Aid; EFC = expected family contribution.

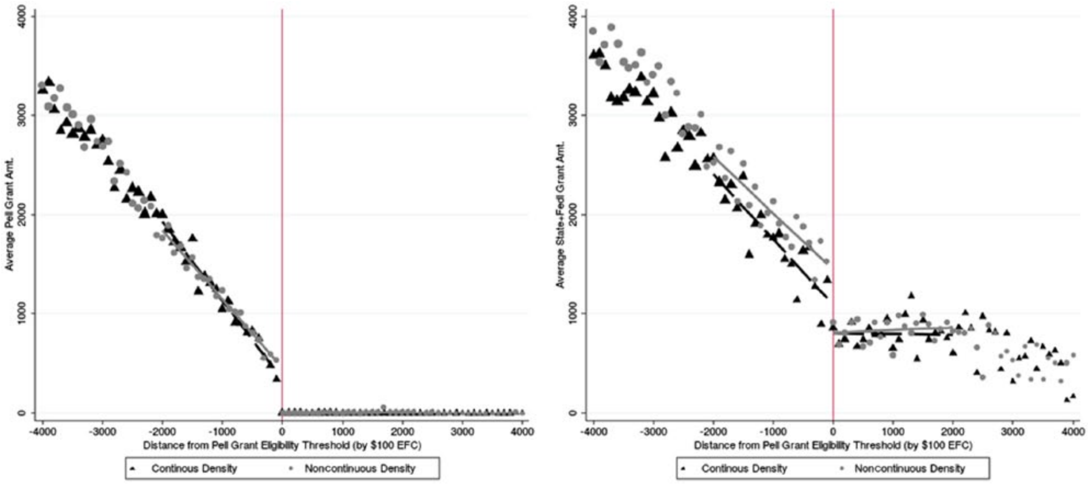

FIGURE 7. *Grant amounts (US$) for continuous and noncontinuous schools.*
Note. Samples are restricted to 2008–2010 cohort students who have filed FAFSA, for whom race/ethnicity is not missing, and who are nondual enrollees. Continuous (noncontinuous) schools are a subset of institutions that passed (failed to pass) individually conducted McCrary test at the institution level. Averages are plotted separately for continuous schools (triangle points) and noncontinuous schools (circle points). Each point represents mean outcomes for students that fall within a bin of size US$100 EFC. Only points within a US$±4,000 bandwidth are in the figure. Gray solid (continuous schools) and black dashed (noncontinuous schools) lines are the linear fitted value of these points that fall within a US$±2,000 bandwidth. FAFSA = Free Application for Federal Student Aid; EFC = expected family contribution.

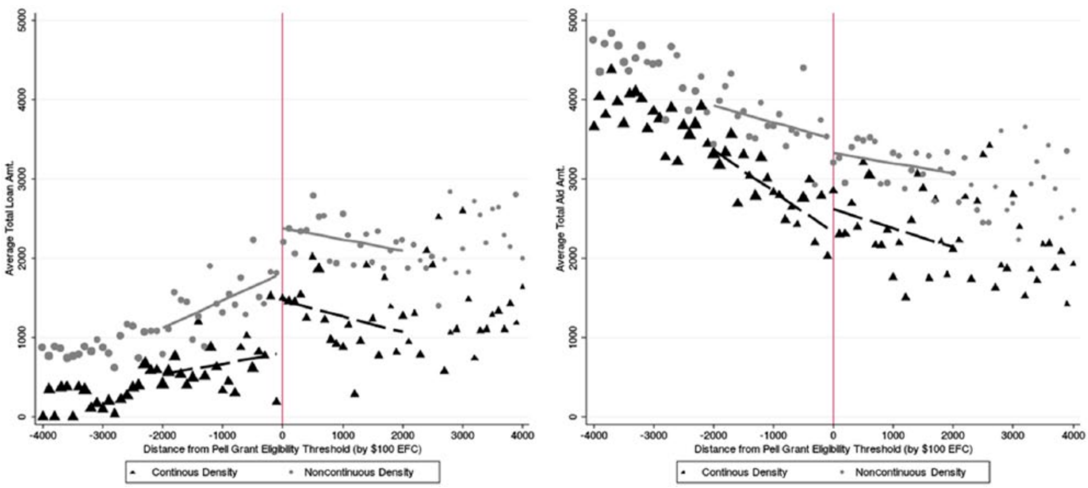

FIGURE 8. *Loan and total aid amounts (US$) for continuous and noncontinuous schools.*
Note. Samples are restricted to 2008–2010 cohort students who have filed FAFSA, for whom race/ethnicity is not missing, and who are nondual enrollees. Continuous (noncontinuous) schools are a subset of institutions that passed (failed to pass) individually conducted McCrary test at the institution level. Averages are plotted separately for continuous schools (triangle points) and noncontinuous schools (circle points). Each point represents mean outcomes for students that fall within a bin of size US$100 EFC. Only points within a US$±4,000 bandwidth are in the figure. Gray solid (continuous schools) and black dashed (noncontinuous schools) lines are the linear fitted value of these points that fall within a US$±2,000 bandwidth. FAFSA = Free Application for Federal Student Aid; EFC = expected family contribution.

more zero-excluding bounds (indicated in bold). Effects of Pell eligibility on financial aid packaging holds with all zero-excluding bounds. The bounds on full-time enrollment still fail to exclude zero but is shifted toward more positive impacts. Academic earnings and summer

TABLE 5

RD Estimates of Impact of First-Year Pell Eligibility (2008–2010 Cohort, Loan Schools, US$2,000 Bandwidth With Covariates)

	Continuous density schools				Noncontinuous density schools			
	(1) Mean outcomes	(2)	(3)	(4)	(5) Mean outcomes	(6)	(7)	(8)
Outcome	Just above cutoff	Coefficient	(*SE*)		Just above cutoff	Coefficient	(*SE*)	
Amount of Pell received	US$0	US$436	(25)	***	US$0	US$485	(23)	***
Amount of Pell + State grants received	US$955	US$400	(90)	***	US$810	US$697	(90)	***
Amount of loans received	US$1,500	US$−600	(160)	***	US$2,263	US$−559	(159)	***
Amount of total aid received	US$2,664	US$−72	(183)		US$3,217	US$245	(179)	
Year 1 outcomes								
Enrolled full-time, Year 1 fall	0.683	0.022	(0.035)		0.639	0.021	(0.032)	
Reenrolled, Year 1 spring	0.837	−0.052	(0.031)	*	0.845	0.017	(0.025)	
Enrolled full-time, Year 1 spring	0.542	0.012	(0.039)		0.505	0.076	(0.034)	**
Enrolled, Year 1 summer	0.284	−0.066	(0.035)	*	0.294	−0.034	(0.032)	
Cumulative GPA, end of year	2.522	0.064	(0.082)		2.438	0.059	(0.078)	
Cumulative credits completed, end of year	18.075	−0.532	(0.864)		17.043	1.244	(0.728)	*
Cumulative Year 1 earnings (Q4–Q3)	US$4,643	US$38	(558)		US$5,030	US$−1,269	(545)	**
Year 2 outcomes								
Reenrolled, Year 2 fall	0.584	−0.004	(0.040)		0.637	0.014	(0.034)	
Enrolled full-time, Year 2 fall	0.367	0.045	(0.039)		0.373	0.094	(0.034)	***

(continued)

TABLE 5 (CONTINUED)

	Continuous density schools				Noncontinuous density schools			
	(1) Mean outcomes	(2)	(3)	(4)	(5) Mean outcomes	(6)	(7)	(8)
Outcome	Just above cutoff	Coefficient	(*SE*)		Just above cutoff	Coefficient	(*SE*)	
Reenrolled, Year 2 spring	0.537	0.018	(0.040)		0.609	−0.001	(0.034)	
Enrolled full-time, Year 2 spring	0.317	0.028	(0.037)		0.335	0.055	(0.034)	
Enrolled, Year 2 summer	0.189	0.008	(0.031)		0.251	−0.008	(0.030)	
Cumulative GPA, end of year	2.464	0.081	(0.077)		2.357	0.069	(0.071)	
Cumulative credits completed, end of year	29.140	0.148	(1.608)		29.030	2.186	(1.412)	
Cumulative Year 2 earnings (Q4–Q3)	US$5,270	US$423	(652)		US$5,359	US$−1,132	(607)	*
End of Year 3 attainment outcomes								
Cumulative GPA	2.454	0.076	(0.077)		2.349	0.090	(0.070)	
Cumulative credits earned	34.133	1.095	(1.986)		35.937	2.512	(1.814)	
Ever transferred to 4 year	0.243	−0.022	(0.033)		0.197	0.056	(0.029)	**
Earned any degree/certificate	0.255	−0.009	(0.033)		0.173	0.018	(0.027)	
Earned any degree/certificate or transferred	0.378	−0.011	(0.037)		0.275	0.045	(0.031)	
Sample size	577	2,506			844	3,247		

Note. Samples are restricted to 2008–2010 fall entry cohort students who filed FAFSA, for whom race/ethnicity is not missing, and who are attending loan schools. Columns 1 to 4 further restrict to the subset of schools that has continuous density by McCrary (2008) test. Columns 5 to 8 restrict to the subset of schools that fails continuous density test by McCrary (2008). Coefficients indicate beta values for indicator of treatment status (i.e., 1 if eligible for Pell and 0 otherwise). Huber–White robust standard errors are in parentheses. Both regressions are within a US$±2,000 bandwidth except mean outcomes (columns 1 and 5) and control for cohort fixed effects for covariates—female, Black, Hispanic, Asian, American Indian, age, income, dependent, dual enrollment, reading, writing, math score prior to entry, and flags on whether they have these test scores—and college fixed effects. Local linear polynomial is used with rectangular kernel in all specifications. RD = regression-discontinuity; GPA = grade point average; FAFSA = Free Application for Federal Student Aid.
*$p < .1$. **$p < .05$. ***$p < .01$.

TABLE 6

GRR Bounds on RD Estimates (2008–2010 Cohort, Loan Schools Only)

	(1)		(2)		(3)	
	Original estimates		Trim by each outcome		Trim by cumulative GPA fall semester, first year	
Outcome	Coefficient	(*SE*)	Low	Upper	Low	Upper
Amount of Pell received	US$459	(17)	—	—	—	—
Amount of Pell + State grants received	US$560	(64)	**[US$236**	**US$1,143]**	**[US$377**	**US$661]**
Amount of loans received	US$−592	(113)	[US$−1,909	US$847]	**[US$–692**	**US$–442]**
Amount of total aid received	US$89	(129)	[US$−1,176	US$1,599]	[**US$73**	**US$98**]
Year 1 outcomes						
Enrolled full-time, Year 1 fall	0.020	(0.024)	[−0.213	0.374]	[–0.017	0.021]
Reenrolled, Year 1 spring	−0.016	(0.020)	[−0.156	0.432]	[−0.099	0.029]
Enrolled full-time, Year 1 spring	0.048	(0.026)	[−0.248	0.339]	[−0.061	0.085]
Enrolled, Year 1 summer	−0.046	(0.023)	[−0.546	0.041]	[**–0.125**	**–0.011**]
Cumulative GPA, end of year	0.061	(0.056)	[−0.569	0.577]	[−0.516	0.511]
Cumulative credits completed, end of year	0.480	(0.559)	[−5.236	6.610]	[−3.885	3.048]
Cumulative Year 1 earnings (Q4–Q3)	US$−312	(192)	[US$−2,749	US$3,630]	**[US$–347**	**US$–121]**
Year 2 outcomes						
Reenrolled, Year 2 fall	0.003	(0.026)	[−0.323	0.264]	[−0.074	0.062]
Enrolled full-time, Year 2 fall	0.074	(0.026)	[−0.343	0.244]	[−0.017	0.121]
Reenrolled, Year 2 spring	0.005	(0.026)	[−0.394	0.193]	[−0.091	0.067]
Enrolled full-time, Year 2 spring	0.044	(0.025)	[−0.424	0.163]	[−0.027	0.090]
Enrolled, Year 2 summer	−0.004	(0.022)	[−0.491	0.096]	[−0.044	0.023]
Cumulative GPA, end of year	0.074	(0.053)	[−0.509	0.605]	[−0.447	0.453]
Cumulative credits completed, end of year	1.243	(1.063)	[−9.128	13.378]	[−6.046	5.703]
Cumulative Year 2 earnings (Q4–Q3)	US$−281	(224)	[US$−2,865	US$4,477]	[**US$–381**	**US$–54**]
End of Year 3 attainment outcomes						
Cumulative GPA	0.084	(0.052)	[−0.475	0.606]	[−0.421	0.451]
Cumulative credits earned	1.741	(1.342)	[−11.665	17.283]	[−6.900	6.815]
Ever transferred to 4 year	0.026	(0.021)	[−0.393	0.194]	[−0.048	0.094]
Earned any degree/certificate	0.010	(0.021)	[−0.516	0.072]	[−0.090	0.068]
Earned any degree/certificate or transferred	0.026	(0.024)	[−0.406	0.181]	[−0.091	0.108]
Sample size	5,753	5,753	[4,576	4,576]	[4,431	4,448]

Note. Samples are restricted to students in 2008–2010 fall entry cohorts who filed FAFSA, for whom race/ethnicity is not missing, and who are attending loan schools. Column 1 is from Table 2 and Table 3. Columns 2 and 3 are bound estimates using GRR-bounding exercise. Square brackets indicate lower and upper bounds of treatment effect after adjusting for sample selection bias. Column 2 trims and run a single regression separately for each outcome variable. Column 3 trims using a single variable, cumulative GPA fall semester of first year, and runs multiple regressions on different outcomes. All regressions are specified using local linear regression within US$±2,000 bandwidth with rectangular kernel, controls for cohort fixed effects, controls for covariates—female, Black, Hispanic, Asian, American Indian, age, income, dependent, dual enrollment, reading, writing, math score prior to entry, and flags on whether they have these test scores—and controls for college fixed effects. RD = regression-discontinuity; GPA = grade point average; FAFSA = Free Application for Federal Student Aid.

earnings in both Year 1 and Year 2 remain negative with bounds that exclude zero.

Discussion and Conclusion

In this article, we examine the effect of being eligible for Pell on financial aid packages, student outcomes, and labor supply among those community college students in a single state who are around the Pell Grant eligibility cutoff. First, we find that even at community colleges that have relatively little institutional aid to distribute, non-Pell aid awards are influenced by differences in Pell eligibility. Moreover, the pattern of response is distinctive depending on whether an institution offers federal student loans: For schools that offer loans, students who just miss qualifying for Pell borrow more (almost equivalent to Pell eligibility at the cutoff), such that students just above and below the Pell cutoff receive similar amounts of aid in total. For schools that do not offer loans, students who do not qualify for Pell receive higher state grants to compensate. We next examine the effect of receiving a modest Pell grant for students attending loan-offering schools. We find that students who just barely qualify for Pell are more likely to enroll full-time (about 4–7 percentage points more likely, depending upon the term) and at the same time reduce their labor supply by about US$12 to US$20 per week. These patterns are consistent with a story in which Pell allows students to shift their time allocation, perhaps 1 or 2 hours per week, from work to school.

We also find a discontinuity in enrollments around the Pell cutoff (only within loan-offering schools), which suggests that Pell eligibility may independently affect enrollment decisions as well. We find that this discontinuity in enrollments is concentrated at three large urban community colleges, which have a lot of local market competition, particularly from for-profit institutions. However, we suggest caution in interpreting these results given that we cannot directly observe where else students may have enrolled and since the interactions of financial aid can be local in nature. In fact, our result is contrast to Carruthers and Welch (2017) who look at the effect of minimum Pell eligibility among all Texas high school graduates and find no effects on college sector.

Unfortunately, this pattern of enrollments may introduce bias into our RD estimates. To examine this, we follow two methods in the literature: reestimating impacts only for the subset of schools with continuous density through the cutoff and a bounding analysis that makes extreme assumptions about the missing population. In both cases, our results are not entirely robust. Although this is not reassuring, neither does it provide affirmative evidence that our main results are biased. Our best guess regarding the likely effects of receiving a modest Pell, in comparison to an equivalent amount of additional loans, is still drawn from our main results in Tables 2 and 3, which control for a rich set of observable characteristics at entry. Still, the lack of robustness suggests that these results should be interpreted cautiously and alongside evidence from other studies.

It is important to acknowledge that our results focus on students near the cutoff for Pell eligibility, who receive small Pell Grants. It is possible that larger grants could have a more-than-proportional effect, if lower income students are more sensitive to financial aid, or if larger grants enable more meaningful changes in circumstance (such as quitting a job entirely) than small grants do. Still, understanding the impacts for students receiving small Pell Grants is relevant as policymakers consider whether the optimal cutoff for receiving aid might be higher or lower than it currently is.

Our findings have two implications. First, even at community colleges, which typically have very little "institutional" aid to distribute, institutions may have discretion to determine how Pell interacts with other state and federal aid programs. In our sample, we find a complex web of interactions, with state grants smoothing over the discontinuity in the Pell schedule at no-loan schools, and loans smoothing over the discontinuity at loan schools. In particular, students at no-loan offering schools who just missed Pell (coefficient = US$434, $p < .01$) were compensated by a similar amount of state grant aid, such that at these institutions, we find no significant discontinuity in total grant aid around the Pell cutoff (coefficient = US$132, no significance). However, students at loan-offering schools who just missed Pell were on average, taking more loan aid of similar amount (coefficient = US$–592, $p < .01$) leading to no discontinuity in total aid.

Second, although the resulting treatment even at the loan-offering schools is relatively small—with eligible students receiving only about US$500 more in grants—we nonetheless find some evidence that this alters some student behaviors. Students who are just below the cutoff (receiving Pell) seem to shift their time allocation, reducing work (translating into about US$12–US$20 less earnings per week) while increasing their enrollment intensity; we find significant increases in full-time enrollment (between 4 and 7 percentage points) and suggestive (but not significant) evidence of increases in GPAs (between .06 and .08 points). Moreover, we find indirect evidence that Pell eligibility may alter students' initial enrollment choices: Students just barely eligible for Pell are less likely to show up in our sample of community college enrollees (and this pattern is most pronounced when many for-profit colleges are located nearby).

Our main estimates are modest in magnitude (which is not surprising given the modest size of Pell awards for students who just barely qualify), and as such they are not always robust to rigorous sensitivity checks that we conduct. Although it is possible that some of the positive effects we find may be due to differential selection into community colleges, it is reassuring that we find no differences in observed student characteristics around the cutoff. We conclude that the results described above are a reasonable "best estimate" regarding the impact of receiving a small Pell grant. This best estimate indicates that even small Pell Grants can have meaningful impacts on student behaviors and outcomes, at least in the community college setting.

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Notes

1. Matsudaira (2017) finds no evidence that reducing the number of questions on the form, conditional on starting the form, increases the likelihood of enrollment, but his results still leave open the possibility that more dramatic simplification efforts could have larger effects.

2. Tuition levels are another channel through which the impact of Pell could be diminished (this is often referred to as the "Bennett hypothesis" after former Secretary of Education William Bennett), although empirical research on this question has found mixed results (Rizzo & Ehrenberg, 2004; Singell & Stone, 2007; Turner, 2014).

3. Based on estimated average tuition and fees of US$2,713 in 2010–2011 (Baum & Ma, 2011).

4. We distinguish loan-offering schools by looking at average loan take-up rates across cohorts. Although no-loan schools include some with nonzero take-up rates, the rates at those schools were always very close to zero. Loan schools, no-loan schools, and "switchers" were clearly distinguishable.

5. Authors' tabulations using National Center for Education Statistics (NCES) QuickStats with National Postsecondary Student Aid Study (NPSAS): 2012 data split by institution type.

6. Federal loan limits are resourced from http://www.finaid.org/loans/historicallimits.phtml

7. The 2008 figure based on NPSAS: 2008 data, using "student budget (attendance adjusted)" variable for full-time students.

8. If an institution has more than a 30% cohort default rate for 3 consecutive years that school is prohibited to offer any federal financial aid, including Pell Grant, for 3 years (Wiederspan, 2016).

9. Although work-study earnings are not required to be reported to the unemployment insurance (UI) database, some institutions may find it easier to report them rather than to specifically identify and exclude them. In any case, work-study is a trivial component of student aid at community colleges and thus whether or not it is included has little implication for the earnings

data. About 1.2% of students in our data have any amount of federal work-study aid during their year of entry.

10. The administrative data we received were limited to first-time fall entrants to the community college system. That said, focusing on first-time entrants provides the cleanest analysis of the effect of Pell access, as continuing students may be a more self-selected group.

11. For dependent status, family income, family size, and expected family contribution (EFC), our data have information only on those who have filed a Free Application for Federal Student Aid (FAFSA).

12. In 2008 and 2009, Pell simply rises linearly below the cutoff until it reaches the maximum. In 2010, the formula takes a particularly weird shape, with eligibility fixed at US$555 for students within a range below the threshold, then rising linearly for a range, then discontinuously jumping again by about US$327 at an EFC approximately US$500 below the cutoff. This odd pattern in 2010 can be detected in Figure 1.

13. When using a subset of points to fit a local regression, different weights can be used to the fit data points (mostly, weight is given as a function of distance to the point estimator). This weight function is referred to as a kernel. In the RD literature, there is no consensus in an optimal choice of kernel because in practice different weight functions should have little impact on the estimator (DesJardins & McCall, 2014; Fan & Gijbels, 1996; Lee & Lemieux, 2010; McCrary & Royer, 2011). For consistency, we use a rectangular kernel, giving equal weights to all local points within the bandwidth, throughout the article as suggested by Lee and Lemieux (2010).

14. Lee and Lemieux (2010) also use the rule-of-thumb bandwidth procedure introduced by DesJardins and McCall (2014). We also run the rule-of-thumb procedure and find it suggests similar, but slightly smaller bandwidths than the IK (Imbens and Kalyanaraman, 2012) procedure.

15. Although we do not expect a discontinuity in the likelihood of FAFSA application around the cutoff (we cannot directly test this because we lack family income data for those who did not fill out a FAFSA), it is important to consider how the limitation to FAFSA applicants may affect the generalizability of our findings. Table C4 in our online appendix indicates that FAFSA applicants are more likely to be female, less likely to be White, and have lower average UI earnings prior to entry.

16. For the US$4,000 bandwidth specification, we see dual enrollment, age, and dependent variables as significantly different.

17. The City University of New York (CUNY) system is substantially more expensive, and arguably more stratified by ability, than the system under consideration in this article. Although purely speculative, this provides possible explanations for why Pell eligibility may impact college choice in this context but not in the CUNY context.

18. This amount is less than the statutory discontinuity in Pell eligibility largely because of less-than-full-time enrollment.

19. State grant aid in this state follows a decentralized financial aid system where institutions receive an aggregated amount of grant from the state and have autonomy in distributing the funds as long as it is need-based. The initial amount that institutions receive from the state is calculated through a standard formula based on aggregate student need at the institution level.

20. We suspect that the few observations off the line are either data errors or possibly students who switched institutions midyear.

21. Note that total aid includes some other small aid programs, so that it may be slightly more than the sum of grants and loans.

22. Moreover, as noted by Marx and Turner (2015), these averages mask important heterogeneity, because everyone to the left of the cutoff qualifies for a US$500 Pell Grant, but to the right of the cutoff, some students take out large loans while others take out nothing. Thus, some students who are bumped just below the cutoff will experience an increase in total aid, while others may actually take up less total aid than if they had not been Pell eligible.

23. Although we cannot confirm it in our sample because we only have 1 year of aid data, Marx and Turner (2015) find no discontinuities in subsequent years' Pell Grants for students around the EFC cutoff in a given year.

24. When we focus on the cohort most likely to have been eligible for summer Pell, the negative effect is no smaller.

25. Although we are hesitant to overinterpret this isolated contrary result, it is possible that at no-loan schools, the other state grant programs students receive in lieu of Pell may have more stringent or salient academic performance criteria for renewal.

26. For implementation, we use the rdbwselect_2014 function in the Stata rdrobust package (Calonico, Cattaneo, Farrell, & Titiunik, 2017).

27. Lee and Lemieux (2010) also use the Akaike information criterion (AIC) model selection for selection of degree of polynomial, however, they recommend a nonparametric F test because of a lack of visibility to compare across different models (see Lee & Lemieux, 2010).

28. We run this test including up to Polynomial Degree 6. There are no major changes when we add these extra degrees.

29. It is possible that at some of these small institutions, there is a real discontinuity that is simply too noisy to detect. When we aggregate across all of the nine small institutions, the aggregate discontinuity is still an insignificant 0.27 log points compared with a significant 0.46 log points at the three large institutions that show a clear discontinuity. Still the confidence intervals are overlapping, and because of the large standard errors at individual institutions, we cannot rule out substantively meaningful differences even at the set of nine "continuous" schools.

30. One relatively large school from the continuous group has an essentially zero remedial test take-up rate, which seems to drive the average down for the continuous group.

31. Castleman and Long (2016) find a positive effect on "any enrollment" that is similar in magnitude to their estimated effect on 4-year enrollment. Although they find no effect in either direction at community colleges, this is consistent with some students shifting from no college to community college while others shift from community college to a 4-year college.

32. The fact that individual institutions pass the McCrary test separately does not guarantee that they will do so in the aggregate. We test and confirm that our continuous group passes the McCrary test as a whole.

33. Dong (2017) addresses sample selection with missing outcome responses. In Dong's example, among the entire sample with data available for the running variable (first semester grade point average [GPA]) and treatment (probation), some students drop out of school and therefore have missing outcome data (e.g., final GPA). Our case is slightly different, as the missing observations are entirely missing, including on the running variable. GRR (Gerard, Rokkanen, & Rothe, 2016) bounds are inclusive of this case. Ultimately, however, Dong (2017)'s bounds under monotonic selection uses similar calculations as GRR bounding.

34. The GRR-bounding exercise is an extension to Lee's (2009) bounding exercise in the Sharp regression-discontinuity (RD) case. GRR require two additional assumptions regarding what they call the "selectors" (those students whose enrollment decisions shift as a result of their Pell eligibility): that the direction of selection is one-sided and that the conditional density is left-differentiable.

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Authors

RINA SEUNG EUN PARK is a PhD candidate in economics and education at Teachers College, Columbia University. She is also a research associate at the Community College Research Center (CCRC). Her research interest is in intersections between education, labor economics, and justice system. She holds a BA in both applied mathematics and economics from University of California (UC) Berkeley and MS in statistics from Stanford University.

JUDITH SCOTT-CLAYTON is an associate professor of economics and education at Teachers College, Columbia University, in the Department of Education Policy and Social Analysis (EPSA), where she directs the Economics and Education Program and teaches courses on the economics of education, labor economics, and causal inference. She is also a faculty research associate of the National Bureau of Economic Research, a nonresident senior fellow of the Brookings Institution, and a senior research associate at the CCRC. Her research lies at the intersection of labor economics and higher education, with a particular focus on financial aid, community colleges, and the outcomes of students after college, including labor market trajectories and patterns of student loan default. She holds a BA from Wellesley College and a PhD in public policy from Harvard University.

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