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ABSTRACT

By applying a regression discontinuity design to national data of students at four-year colleges, this study identifies the substitution effects of exogenously received increases of grant aid on hours of paid labor, earnings, and borrowing while in college. Results confirm students substitute grant aid for both paid labor and borrowing. An average increase of $1,100 in grant aid reduces weekly job hours by 1.5-2 hours per week for women, corresponding to a decline in annual earnings of $850, and reduces borrowing by an average of $300-$400 dollars among all students. We find limited evidence of grant aid's impact on academic outcomes.

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Monetary Substitution of Loans, Earnings, and Need-based Aid in Postsecondary Education: The Impact of Pell Grant Eligibility

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Abstract

By applying a regression discontinuity design to national data of students at four-year colleges, this study identifies the substitution effects of exogenously received increases of grant aid on hours of paid labor, earnings, and borrowing while in college. Results confirm students substitute grant aid for both paid labor and borrowing. An average increase of $1,100 in grant aid reduces weekly job hours by 1.5-2 hours per week for women, corresponding to a decline in annual earnings of $850, and reduces borrowing by an average of $300-$400 dollars among all students. We find limited evidence of grant aid's impact on academic outcomes.

Keywords: Need-based grant aid, college persistence, borrowing, financial aid

I. Introduction

The federal government, state governments, and private institutions annually invest billions of dollars in grant aid to improve human capital investment in postsecondary education. Ample evidence demonstrates that, in the absence of these sources of aid, college enrollment rates would suffer (see Deming and Dynarksi 2010, and Dynarski and Scott-Clayton 2013 for reviews of the literature). A growing body of evidence also suggests the investment in grant aid enhances persistence and degree completion among enrolled students. Observational studies demonstrate a positive association of grant receipt and persistence (Cabrera et al. 1992; Dowd 2004; Mendoza et al. 2009), and quasi-experimental (Alon 2011; Bettinger 2004; Bettinger et al. 2016; Castleman and Long 2016; Clotfelter et al. 2016; Henry et al. 2004; Schudde and Scott-Clayton 2016; Scott-Clayton 2011a) and experimental (Angrist et al. 2009; Angrist et al. 2014; Brock and Richburg-Hayes 2006; Goldrick-Rab et al. 2016) analyses provide causal evidence that grant aid has a positive effect on persistence.

Although grant aid appears to improve enrollment and persistence, we have less evidence on how the receipt of grant aid alters students' financial choices and behaviors, specifically how grant aid affects labor market participation and student loan borrowing decisions. Economic theory suggests students likely substitute some combination or earnings and borrowing with grant aid, but most of the extant literature examines only one form of substitution in isolation. For example, Broton, Goldrick-Rab and Benson (2016) measure the effect of aid on hours of paid labor while ignoring borrowing, and Marx and Turner (2017) identify the impact of grant aid on borrowing but do not observe hours of labor market participation. Students do not make the borrowing or work decisions in isolation. Over the course of a few months when enrolling in

postsecondary education, they jointly choose levels of borrowing and hours of labor market participation based on their remaining unmet need after accounting for grant aid.

This paper assesses the impact of grant aid on both borrowing and labor market participation to observe how students substitute grant money with student loans and earnings while enrolled. In order to identify these substitution effects, our analysis tests the exogenous receipt of grant aid on labor market participation, the likelihood of borrowing, and borrowing amounts. To provide causal evidence, we exploit an income threshold for federal Pell Grant eligibility by applying a regression discontinuity design to four waves of national data from 2000-2012. The Pell Grant is the largest single source of need-based grant aid for college attendance and is provided by the federal government to income eligible students. There is a threshold for Pell Grant eligibility roughly based on income such that students below the threshold are eligible to receive a portion of the maximum award. Previous work has found that Pell Grant eligibility has little effect on whether or where students enroll in college (Carruthers & Welch, 2017; Rubin, 2011); however, conditional on enrollment, eligibility for the Pell Grant at this threshold provides an average increase in grant aid of approximately $1,100 at four-year colleges and insignificant amount at two-year colleges relative to those just short of the eligibility threshold.

Better understanding the substitution effects of exogenous grant aid on borrowing and earnings enables a test of the mechanism by which grant aid may increase persistence. Receiving grant aid enables students to shift time away from the labor market toward academic studies. If students spend less time participating in the labor market while enrolled, they can allocate more time to accumulating additional college credit, increasing academic performance, or becoming

engaged with their institution, all of which may lead to enhanced persistence and human capital (Crisp and Nora 2010; Dundes and Marx 2006; Stinebrickner and Stinebrickner 2003).

The possible reduction in work hours while enrolled is a widely cited but rarely tested hypothesized mechanism of grant aid's effect on persistence (Castleman and Long 2016; Henry et al. 2004; King and Bannon 2002). While studies exist that identify the effects of grant aid on persistence and academic outcomes (Brock and Richburg-Hayes 2006; Castleman and Long 2016; Dynarski 2003; Goldrick-Rab et al. 2016), and the effect of grant aid on paid work while enrolled (Broton et al. 2016), rarely are these outcomes examined collectively with the exception of a few recent working papers we discuss below. Our analysis also considers the effect of grant aid receipt on academic outcomes of credit accumulation and performance to test this hypothesized mechanism. Under the assumption that our grant aid instrument meets the exclusion restriction, we also estimate the effect of labor market participation on academic outcomes.

Empirical estimates provide causal evidence that grant aid does substitute for other forms of postsecondary education financing. Receiving an additional $1,100 in grant aid reduces paid weekly work hours for women at four-year institutions by about 1.5 hours per week and reduces student loan borrowing among all recipients by $300-$400 per year; however, there is little evidence of differences in academic performance or increased credit attainment. Students are substituting money from student loans and paid labor for grant aid, but the results do not conclusively support the hypothesis that grant aid increases persistence through a reduction in hours of work.

Our findings contribute to the literature on the causal effects of grant aid on labor market participation and borrowing behavior of students investing in human capital through

postsecondary enrollment. Recently there has been a growing interest in this area, and there are currently a few working papers that examine the joint decisions of borrowing and labor market participation (Denning, Marx, & Turner, 2017; Page, Kehoe, Castleman, & Sahadewo, 2017; Park & Scott-Clayton, 2017). Our work contributes to this budding area of research by providing estimates at a different spot in the income distribution for four-year institutions using national data.

How students choose to substitute between grant aid, work, and loans is of growing interest to researchers and more importantly, it also has implications for the design of federal, state, and institutional financial aid programs. Although we find no immediate implications of these substitution effects on academic performance, changes in work behavior and reduced borrowing likely have effects on more distal outcomes we cannot observe in our data. If one goal of grant aid is to reduce borrowing among low- and middle-income populations, we must better understand how students' labor market participation and borrowing decisions are affected by grants.

The rest of the paper is structured as follows. In Section II, we provide an economic framework for considering the substitution of grant aid with earnings and borrowing and outline the theory of mechanisms that underlie grant aid's effect on persistence. We describe our data and empirical strategy in Section III. We employ the standard suite of regression discontinuity checks to ensure that its application in this context is appropriate. Section IV provides our main models' results and tests alternate specifications for robustness. We discuss our results and conclude in Section V with a broader discussion of our findings.

II. **Theory and Literature**

Economic Framework

In the postsecondary human capital investment decision, students are generally viewed as attempting to maximize lifetime utility by investing in education for an expected future payoff (Becker 1962). Within that investment decision, students must decide how to finance the initial sticker price of higher education given the constraints of budget and time. Students may have some savings that can be applied toward the cost of college, and dependent students often rely on parental resources thereby increasing their budget. But even in the absence of savings and parental assistance, two additional financial resources are available: loans and paid labor. Student loans relax the budget constraint at the cost of needing to be repaid in the future, and students can choose to work while enrolled to finance their education subject to the cost of time. The combination of these financial resources must weakly exceed the direct costs of higher education (tuition, fees, books, etc.) and allow the students to purchase necessary goods such as housing and food while enrolled. For low-income students, the likelihood of having substantial parental resources and personal savings is low, so they rely predominately on a combination of need-based grant aid, earnings, and borrowing. In practice, postsecondary students frequently employ both borrowing and labor while enrolled.

The receipt of grant aid alters students' financing calculus. Grant aid is free money that generates a price effect by directly lowering the net cost of postsecondary education. It may also have an income effect as students may make different consumption decisions as the grant aid serves to increase their budget.

We are primarily interested in investigating the potential substitution effects of grant aid with other financial resources applied to finance human capital investment, specifically loans and earnings from labor. Grant aid is preferable to either labor, which costs time, or loans, which

require future repayment with interest. Conditional on enrollment, we expect an exogenous increase in grant aid will result in students substituting the grant aid for both loans and labor and hence expect to see a reduction in both.

If students perceive each of these resources equally, we may expect the marginal rate of substitution between grants, loans, and labor to be one, that is, they are perfect substitutes. An additional dollar of grant aid may decrease bowering by a dollar or earnings from employment by a dollar or some combination of the two. However, as noted above, these resources are not financially equivalent. One dollar of grant aid is more valuable than either one dollar of earnings from labor or a borrowed dollar due to costs of time and interest payments respectively. If students function as rationale economic agents, we would expect to observe a joint reduction in borrowing and earnings that exceeds the value of additional grant aid.

Currently available empirical evidence confirms that perfect substitution is not true. Among City University of New York students, Marx and Turner (2017) find an additional dollar of grant aid reduces borrowing by $0.43 to $0.51 on average over all students; however, among borrowers, the dollar of grant aid reduces borrowing by $1.80 to $1.90. They explain this greater than 100 percent crowd-out of loans by suggesting there is a substantial non-monetary fixed cost to borrowing, such as a psychic cost. Students that would otherwise borrow small amounts are deterred from borrowing any money when they receive grant aid. Their explanation is certainly plausible, especially in light of loan averse tendencies of a substantial number of high school and college students who exhibit a preference to avoid borrowing (Boatman et al. 2017; Caetano et al. 2011).

However, it is also likely students shift employment behavior in response to additional grant aid; that is, the substitution between grants, loans, and labor is a joint decision. The

majority of previous studies that have investigated the impact of grant aid on borrowing have neglected to consider students' work behavior (e.g. Marx & Turner 2017), and studies that have previously identified the effect of grant aid on labor (e.g. Broton, Goldrick-Rab, & Benson 2016) have ignored the effects on borrowing. When grant aid alters the choice of a level of borrowing, students also choose a level of labor market participation, and therefore earnings. Grant aid may reasonably be expected to alter a utility maximizing student's decision on levels of borrowing and working. Given the psychic costs of borrowing and time costs of earnings, an optimal allocation of borrowing and earnings is likely some combination of the two from the student's perspective. For example, a loan averse student may want to minimize borrowing while maximizing time spent on academic efforts and therefore limiting hours of paid labor. Such a student will likely choose to substitute some combination of work hours and borrowing amounts when receiving grant aid. We therefore extend the prior literature by estimating the effect of grant aid on both borrowing and labor decisions to better understand how students perceive the tradeoffs between grant aid, borrowing, and working while enrolled.

Our work joins a recent slate of working papers to examine the joint decision of borrowing and labor market participation (Denning, Marx, & Turner, 2017; Page, Kehoe, Castleman, & Sahadewo, 2017; Park & Scott-Clayton, 2017). For instance, Page et al. (2017) examine the effects of the Dell Scholar program on postsecondary outcomes including borrowing and labor market decisions. They find that Dell Scholars are less likely to have federal loans and private loans, but there are mixed findings regarding labor market participation. In particular, they find the students who receive additional grant aid are more likely to work while enrolled, with students more likely to work at least 10 hours per week but less likely to work over 30 hours per week. The authors argue that the financial reward and the counseling provided to the

Dell Scholars encouraged students against taking on a larger amount of work hours. This is useful evidence of the substitution decisions made by postsecondary students; however, there are several factors that limit the generalizability of their work. Dell Scholars are substantially different than average low-income students. To be eligible, they must have participated in an affiliated college-readiness program, earn a minimum GPA of 2.4, be low-income per Pell Grant eligibility, and plan to enroll in a four-year college. They also undergo an intensive selection process that considers factors such as academic achievement, disadvantage, and responsibility such that only 5.1% of applicants receive the award (Page et al., 2017). Furthermore, Dell Scholars receive up to $20,000 in scholarship funds, a laptop, and textbook support, far more money that the average student receives in grant funds. These features somewhat limit the external validity of their study.

Relatedly, leveraging the automatic zero expected family contribution (EFC) threshold, Denning, Marx, and Turner (2017) examine the effects of Pell Grants on borrowing and labor market participation for Texas students. However, they do not consider contemporaneous labor market participation but rather post-college participation, so they do not directly measure the substitution effects we investigate. The working paper most similar to our study is Park and Scott-Clayton (2017). They leverage the EFC cutoff for Pell Grant eligibility to examine the effects of eligibility for community college students within a single state. They find that Pell Grant eligibility only increased grant aid for students at institutions that offer federal loans but not at no-loan institutions. Among students at the loan offering two-year colleges, they find Pell Grant eligible students borrow about $500-$600 less and earn $700-$800 less.

Our work is distinct from these papers in several ways. While we also examine the effects of Pell Grant eligibility on postsecondary outcomes, unlike Denning et al. (2017), we examine

contemporaneous labor market decisions and not post-college earnings, and we provide estimates at a different location in the income distribution. Relative to Park and Scott-Clayton (2017), we examine the effects of Pell Grant eligibility predominately at four-year institutions using cross-sectional national data, which is more generalizable than findings for only one state and for only loan-offering institutions. Lastly, our paper also has greater generalizability than that of Page et al. (2017). In short, our paper makes substantively unique contributions to this area of research.

Potential Mechanisms of Grant Aid's Impact on Persistence and Attainment

Better understanding this substitution is important because the reduction of labor and borrowing may explain why grant aid improves college outcomes such as persistence and attainment. Despite the evidence that grant aid affects persistence and attainment, the mechanisms by which grant aid improves college outcomes are not well identified and documented. A popular hypothesis is that grant aid reduces the amount that students spend in the labor market while enrolled, and this reduction in work hours leads them to presumably spend more time on coursework thereby increasing persistence through a positive effect on academic outcomes such as credit accumulation and performance. This mechanism is underexplored in the empirical literature, and we consider both components of this theory.

First, limited empirical evidence exists to support the idea that grant aid reduces hours of paid work. For a small group of low-income students, Broton, Goldrick-Rab, and Benson (2016) find that randomly receiving $3,500 of grant aid in Wisconsin reduces hours of employment by 1.7 per week. Additionally, DesJardins and McCall (2014) find that receiving the Gates Millennium Scholarship reduces hours employed, earnings, and borrowing. Neither of these studies estimate the effects of grant aid on persistence nor provide a strong link between aid,

work, and academic performance, although DesJardins and McCall provide somewhat tenuous evidence of increased academic performance as measured by GPA. Furthermore, both are focused on small programs with small sample sizes and limited external validity.

The second component of the mechanism of grant aid improving persistence is that reducing hours spent in the labor market enhances academic performance because a greater share of time can be spent on academic pursuits (Dundes and Marx 2007; Scott-Clayton 2011b; Stinebrickner and Stinebrickner 2003). If true, we should observe that a reduction in hours of paid labor increases credit accumulation and/or GPA. Several causal studies have examined this relationship, but results are not consistent. Both Dadgar (2012) and Stinebrickner and Stinebrickner (2003) find increased work hours has a negative effect on academic performance. Soliz and Long (2016) distinguish between academic outcomes and find increased work causes small negative effects on first year GPA but positive effects on first year credits earned. Scott-Clayton (2011b) identifies differential effects by gender; work has positive effects on first-year GPA and credits earned for men but negative effects for women. Given the literature's mixed results, it remains unclear if the mechanism by which aid enhances persistence is through a reduction in work leading to increased academic achievement.

Park and Scott-Clayton (2017) and Carruthers and Özek (2016) are the best extant examples linking grant aid, earnings, academic outcomes, and persistence. Park and Scott-Clayton (2017) find that, at loan-offering institutions, Pell eligible students receive additional grant aid, which is substituted with reductions in both loans and earnings, resulting in a decrease of one to two hours of weekly job hours, relative to Pell ineligible students around the EFC threshold. However, they find little to no effects on persistence and attainment outcomes. On the other hand, Carruthers and Özek (2016) find grant aid increases persistence and shifts students

away from labor market earnings while enrolled, reducing weekly job hours by nearly two hours at minimum wage

Our study has several salient differences from Carruthers and Özek, who focus on the effects of losing grant aid in their second year of college for students already receiving aid in their first year. In contrast, we compare students across all years of undergraduate education that do and do not have additional grant aid at the beginning of the year regardless of the amount of aid they had the prior year. To the extent that an asymmetry in student behavior exists when the grant aid is framed as a loss relative to a gain, we might expect different results. Furthermore, while the aid we examine is need-based, the aid studied by Carruthers and Özek is merit-based, and they exploit an academic achievement discontinuity to estimate a treatment effect. Other studies have suggested an academic requirement produces a separate mechanism by which merit-based grant aid may induce higher persistence (Schudde & Scott-Clayton 2016; Scott-Clayton 2011a). Finally, the data from Carruthers and Özek are taken from Tennessee public colleges for the entering 2004-2005 cohorts while our analysis employs repeated cross-sections of national data on college students across all years of undergraduate education from 2000 through 2012 in public and private four-year colleges thereby providing greater external validity.

In summary, this paper contributes to the literature in several ways. First, it increases our understanding of how students choose to substitute earnings and borrowing for grant aid improving upon existing studies that examine one of these outcomes in isolation. By examining both outcomes together, we better understand consumer behavior in the tradeoff between labor market participation and borrowing. Additionally, our analysis tests a hypothesized mechanism of grant aid's impact on attainment through reducing hours worked for pay leading to increased

academic outcomes and therefore increased persistence with enhanced generalizability relative to existing studies.

III. Data and Empirical Strategy

Data and Sample

Our data come from the National Postsecondary Student Aid Study (NPSAS), a repeated cross-section of college students nationally collected by the National Center for Education Statistics (NCES). NPSAS uses clustered stratified random sampling by first selecting institutions stratified by sectors of higher education and then randomly selecting students within institutions stratified by undergraduate and graduate status and STEM majors (Wine et al. 2011). Our sample includes the four most recently available NPSAS waves: 2000, 2004, 2008, and 2012. We exclude all students who are noncitizens and non-permanent residents as they are ineligible for federal student aid, and we exclude students who did not file for federal financial aid. We also exclude all graduate students so that the analytic sample consists of students across all years of undergraduate education.

The data contain students' demographic characteristics; parental education; Expected Family Contribution (EFC); exact amount of multiple forms of aid received including grants and loans from the federal government, state government, institutions, and other private sources; and a number of outcome variables including hours worked per week, annual earnings, GPA for the academic year, and number of credits attempted by term. Given that work-study is an additional form of financial aid, we primarily focus on hours worked per week exclusive of work-study, but we also consider work-study hours as a secondary outcome. Because NPSAS only observes

students for one academic year, we are limited to estimating within academic year term-to-term persistence.

Table 1 provides the descriptive statistics for demographics and grant aid variables for the pooled NPSAS sample (all four waves 2000-2012), our pooled analytic sample based on a $2,000 bandwidth around the EFC threshold for Pell Grant eligibility[1], and the individual waves around a $2,000 bandwidth.[2] In our pooled analytic sample around a $2,000 bandwidth (second column of numbers), 58 percent of the students are female, 64 percent are white, 14 percent are black, 14 percent are Hispanic, and 4 percent are Asian. Forty percent of the students reported that their parental education is high school or less, 28 percent reported some college, and 33 percent reported their parents have a bachelor degree or higher. In comparison to the full, pooled NPSAS sample, our bandwidth-restricted analytic sample has slightly more males, more white students, fewer black students and higher levels of parental education. In terms of grant aid for our bandwidth-restricted sample (provided in Panel B), a little over half received the Pell Grant with an average amount of $651, 31 percent received state grant, 31 percent received institutional grant, and about three quarters received any grant with an average amount of $4,177. For comparability, all dollar amounts are reported in 2012 dollars throughout the paper.

Table 2 provides the descriptive statistics for work, borrowing, and academic outcomes. Compared to the pooled NPSAS sample, the pooled bandwidth-restricted analytic sample has students who are more likely to have a job, report working more hours per week (excluding work-study/assistantship), are more likely to have a loan with a slightly larger loan amount, and have comparable academic outcomes. Seventy seven percent of the analytic sample had a job

[1] The threshold varies by year.
[2] We choose a $2,000 bandwidth to illustrate the reduction in sample and changes in covariate means. We show robustness to bandwidth selection for our major outcomes in the tables and appendix.

during the academic year and, those that did, worked nearly 20 hours per week on average. Over 70 percent, borrowed to finance their education with an average loan amount of $5,373. First to second term, within year persistence was fairly high at 78 percent based on attempting credits in both the first and second terms.[3] Not all outcomes are available for every wave; specifically the 2000 wave is missing hours worked per week excluding work-study and the 2012 wave did not record credits attempted in a measure that is comparable across institutions.

We note the small differences between the analytic sample and full NPSAS sample because our regression discontinuity estimation technique provides local average treatment effects around the Pell Grant eligibility threshold and does not necessarily generalize to the full sample. NCES provides sample weights to make the full sample representative of the nation, but these weights are not designed to function for a subset of the income distribution of the full sample, hence we do not employ sample weights in our analysis. Our analytic sample, therefore, may not be perfectly representative of the national college student population but is an advantage over prior studies focusing on a single institution, system, or state.

Empirical Strategy

The treatment is whether students are eligible to receive the federal Pell Grant thereby receiving additional grant aid. Treatment is determined by a strict dollar threshold in the EFC distribution. The EFC is calculated by the federal government using an opaque formula based on information provided by students and their parents (if dependent) on the Free Application for Federal Student Aid (FAFSA). Lower EFCs indicate a student is expected to contribute less to the cost of college and is therefore eligible for more financial aid. The Pell Grant eligibility threshold varies by NPSAS wave, but a dollar higher than the threshold and the student is not

[3] To make terms comparable across institutions, NCES uses credits attempted in the second major term for trimester and quarter institutions.

eligible to receive the Pell Grant. EFCs at or below the threshold provide at least a minimum amount of Pell Grant that varies by NPSAS wave.

For each entering cohort, we used the federally posted Pell Grant schedule to determine the EFC threshold for eligibility. Noncompliance between Pell eligibility and actual Pell Grant receipt is an issue, predominately in one direction (Table 3). In the $2,000 bandwidth-restricted sample, only 10 students out of nearly 16,000 students (.05 percent) received the Pell Grant even though their EFC indicated that they were not eligible; however, about 3,200 students out of 24,000 Pell eligible students (13.3 percent) did not receive a Pell award. Consequently, we employ fuzzy regression discontinuity using eligibility as an instrument for receipt such that our results are local average treatment effects for compliers.

Pell Grant eligibility also implies eligibility for other federal programs such as the Supplemental Educational Opportunity Grant (SEOG), Academic Competitiveness Grant (ACG) and the National Science and Mathematics Access to Retain Talent (SMART) Grant programs (U.S. Department of Education 2015),[4] and Pell eligibility is related to need-based aid offered by states and institutions (Turner 2013). The treatment is thus the combination of all forms of grant aid that depend on Pell eligibility status. This is a limitation only if we are interested in isolating the impact of the federal Pell Grant program, which we are not. From a student's perspective, there is no rationale reason why need-based grant aid from one source should be treated differently than need-based grant aid from a different source, hence we estimate the causal effect of all forms of grant aid based on Pell eligibility on our set of outcomes.

[4] ACG program and SMART Grant began in 2006 and ended in 2011, overlapping with the 2008 NPSAS wave. The effect of ACG program and SMART Grant program are accounted for with the wave fixed effects employed in our analysis. See Denning & Turley (2017) and Evans (2017) for analyses of the effects of the SMART Grant.

We estimate a regression discontinuity model for student *i* at institution *j* in wave *k* as in equation (1).

$$Y_{ijk} = \beta_0 + \beta_1 Eligibility_{ijk} + \beta_2 EFC_{ijk} + \beta_3 EFC_{ijk} * Eligibility_{ijk} + X_{ijk} + \lambda_j + \psi_k + e_{ijk} \quad (1)$$

We test several outcomes including labor market participation while enrolled, the likelihood of borrowing, borrowing amounts, and academic outcomes such as first-to-second term persistence within academic year, GPA, and credits attempted. We use linear probability models for all binary outcomes. *Eligibility* is a binary indicator of whether students were eligible for additional grant aid and is the variable of interest: the difference in the intercepts at the threshold and the causal effect of just meeting eligibility. As discussed above, we use eligibility as an instrument for Pell Grant receipt due to noncompliance. We center the running variable, EFC, at the appropriate threshold for each NPSAS wave. We include a vector of student level covariates in X to control for race, gender, and parental education. λ is an institution fixed effects term, included to account for differential aid policies and practices across institutions, ψ is a wave fixed effects term to account for changes over time, and *e* is an individual error term. We employ clustered heteroskedastic-robust standard errors clustered at the institution level, although our results are robust to alternative standard error calculations (Cameron and Miller 2015). We use local linear regression to estimate the causal effect of aid on our outcomes allowing for different slopes on either side of the threshold with the inclusion of the EFC and eligibility interaction term. We test the sensitivity of our findings to bandwidth selection.

We employ the standard suite of regression discontinuity checks of manipulation and covariate balance (Imbens and Lemieux 2008; Lee and Lemieuxa 2010). It is very challenging to manipulate the EFC to become eligible for the Pell Grant. The EFC eligibility cutoffs change annually based on congressional appropriations, and the formula relies on using the prior year's

earning information. Hence we do not expect any evidence of manipulation of the running variable. Empirically, the density of the running variable around the cutoff for each cohort is observed to be smooth around the threshold, and the McCrary test (McCrary 2008) shows no evidence of manipulation (Figure 1).

The identifying assumption is that all unobserved determinants of the outcomes must be continuous across the threshold. Supporting evidence of this continuity is presented in Table 4 by establishing continuity of the observed covariates (race, gender, and parental education) on either side of the threshold. We use local linear regression and a bandwidth of $2,000 of EFC for these checks. The results show that there is no impact at the cutoff on race, gender, or parental education providing evidence in support of the identifying assumption.

We note that a potential limitation of including non-first year students is that results are conditional on reenrollment. There is a possibility that Pell eligibility, as determined by EFC, affects the subsequent year's enrollment, and, if true, we would observe more second-year Pell eligible students than Pell-ineligible students around the threshold for eligibility. Two empirical pieces of evidence alleviate this concern. First, a density test of returning students around the Pell eligibility threshold reveals no difference in density on either side of the threshold. Second, limiting the analysis solely to first-year students provides slightly smaller but similar results, albeit with less precision.

IV. Results

Treatment Contrast

We begin by demonstrating the treatment contrast in grant aid received between the just eligible and just ineligible students. We show the discontinuity of aid receipt for a variety of

sources of aid along with the total grant aid at the Pell eligibility threshold (Table 5). We confirm that Pell Grants do vary discontinuously at the threshold. The regression discontinuity estimate is lower than the minimum Pell Grant for full-time students ($400 in 2000, 2004, and 2008, and $555 in 2012) due to the inclusion of non-full-time students in the sample, the linear estimation method near the threshold, and noncompliance. We also find discontinuities in the amount of Supplemental Education Opportunity Grant (SEOG), a federal grant institutions can award to Pell recipients, and in the receipt of institutional grants. Pell eligibility increases sources of grant aid beyond the receipt of Pell Grants, thus the treatment contrast is the amount of total grant aid that students received based on their Pell eligibility status: an average of $798 based on a $2,000 bandwidth. The treatment contrast is concentrated at four-year institutions ($1,096) and negligible and statistically insignificant at two-year institutions ($154). The discontinuity of state grant aid is very small and statistically indistinguishable from zero. Appendix Table A1 demonstrates this treatment contrast is fairly stable at four-year institutions across alternate bandwidth choices.

That the total grant aid is less than the Pell award among two-year students is explained by the fact that Pell awards often supplant institutional dollars. Indeed, we observe a negative effect of Pell Grant eligibility on institutional awards among two-year students that creates little benefit of being just Pell eligible among two-year students. Turner (2013) documents this phenomenon in much greater detail in her assessment of Pell pass through rates. We note that Park and Scott-Clayton (2017) do observe a treatment contrast at the Pell eligibility threshold for two-year students in their state in 2008, which runs counter to our observed treatment contrast for two-year students. We have two hypotheses that may explain this discrepancy. First, it is possible the state used in their analysis has substantially higher state-based financial aid that

distributes funds based on the Pell eligibility threshold than observed nationally. Second, it is possible their results are limited to the time period they study (2008). Specifically, the Academic Competitiveness Grants were active during this period and provided additional grant aid at the Pell eligibility threshold for students in their first two years of college for a subset of students who studied a rigorous high school program. We consider this possibility by examining the treatment contrast for two-year institutions in the NPSAS 2008 wave. There we observe a statistically significant $460 increase in total grant aid among two-year college students across the Pell eligibility threshold lending some credence this hypothesis. Because we observe no treatment contrast among two-year students nationally across all waves, the remainder of our study focuses on students at four-year institutions; however, both hypotheses speak to the importance of examining these findings over a broader range of states, institutions, and time periods.

Work, Loan, and Monetary Substitution

With an increase in grant aid and no change in student savings or parental assistance,[5] we would expect students to substitute the increase with a combination of less borrowing and less job earnings. We test this substitution directly by using regression discontinuity to measure the effect of Pell eligibility on whether students are employed, weekly hours of paid labor, whether they borrowed student loans, and borrowing amounts. Our reduced form estimates are presented in Table 6, which provides the results of estimating equation (1) for students at four-year institutions.

[5] We examine parental PLUS loans as an outcome and find Pell Grant eligibility has no effect on the probability of having parental PLUS loans nor on the amount of the loan (results available upon request). Although we cannot observe all sources of family resources, this finding suggests parental resources are not discontinuous across the threshold.

We observe that Pell Grant eligibility does not greatly influence whether students have jobs; only two estimates are marginally significant for this outcome. Relatedly, Pell Grant eligibility has no discernable effects on whether students borrow. In other words, there is little to no effect on the extensive margin of whether students choose to borrow or work. However, we do observe that receiving approximately $1,100 in additional grant aid reduces the number of hours worked by a statistically significant one to two hours per week and a reduction in borrowing by a statistically significant three to four hundred dollars (depending on bandwidth selection). Hence, grant aid does have an effect on the intensive margin. These results suggest students are dividing their grant aid between reductions in both labor and borrowing. As a secondary work outcome, we examine whether Pell Grant eligibility affects work-study receipt and work-study hours but find no effect on either outcome (Appendix Table A2).

Heterogeneous Effects across Gender

A substantial amount of prior research on the effects of grant aid has documented differential responses by gender. Generally, the effects of financial aid, work-study, hours worked, and patterns of college persistence vary differentially for men and women (Angrist et al. 2014; Angrist, Lang, and Oreopoulos 2009; Leppel 2002; Scott-Clayton 2011b), and Scott-Clayton and Minaya (2016) find that participation in federal work-study may provide better results for women than men.

Because prior studies have identified heterogeneous effects of aid, working, and academic performance by gender, we separate the results by gender in Table 7. We provide continuity and balance checks within gender in Appendix Table A3. This heterogeneity examination reveals that results for the full sample are largely driven by women (Table 7, Panel B). Women appear 1 to 4 percentage points less likely to hold a job and to borrow for higher

education, although the estimates are not consistent across bandwidths. However, there is stronger and more consistent evidence for two other outcomes: weekly job hours and borrowing amounts. Female Pell-eligible students around the threshold work about 1.5-2 hours less per week and borrow $500-$600 less compared to female Pell-ineligible students. We note that we observe no statistically significant difference in the amount of grant aid received by men and women across the eligibility threshold (Table 8), so these differences are not driven by a difference in treatment contrast across gender.

For male students at four-year institutions, we see no evidence of increased grant aid changing employment behavior while enrolled. Point estimates on both holding a job while enrolled and on hours work are small and statistically insignificant. There is some evidence that male students are less likely to borrow with several point estimates suggesting Pell Grant eligibility reduces the likelihood of borrowing by three percentage points, although the precision of those results is not robust to variation in local linear regression bandwidth.[6]

We explore the observed monetary substitution further in Table 8 for a subset of students whose reported earnings were less than $60,000, including students who did not work. This cutoff corresponds to the 99th percentile of earnings in the data, which we limit to avoid large outliers in reported annual earnings. We show our findings for women are robust to this restriction in Appendix Table A4, which provides unrestricted earnings estimates and those limited to a more conservative restriction of $50,000. The results in Table 8 confirm that monetary substitution of grant aid for work is clearest for women at four-year institutions. For an increase in $1,142 in total grant aid, women worked less, resulting in a decrease of $854 in

[6] We note that we find statistically insignificant results in formal tests of the gender difference in job hours, although the point estimates are in the same directions as our subgroup analyses. Our results then only provide suggestive evidence of some differences among men and women, but they are not conclusive. Results are available upon request.

earnings. The dollar amount reduction of earnings aligns well with the reduction of 1.5-2 hours of less work per week at reasonable levels of hourly pay. Pell eligible women also borrowed $531 less in loans. These point estimates clearly demonstrate the substitution for grant aid is divided between reduced borrowing and reduced earnings. At least for women, the dollar amounts also suggest substitution levels that match theory: the value of a dollar in grant aid is worth more than a dollar achieved through borrowing or earnings. However, the overlapping confidence intervals do not allow us to completely rule out a dollar for dollar substitute and therefore perfect substitution for grant aid and combined work and borrowing. In contrast, there is little evidence of men reducing job earnings, although we observe a comparable reduction in borrowing.

Grant Aid's Impact on Academic Outcomes

We turn to considering how grant aid's effect on labor market participation may function as a mechanism by which grant aid improves persistence. The leading hypothesis is that grant aid reduces hours of paid labor resulting in more time spent on academics leading to increased credit accumulation and performance. Our results above provide convincing evidence that grant aid does reduce labor market participation, but it remains to be seen whether reduced work hours lead to increased academic performance. To test the hypothesized mechanism of the reduction in hours worked affecting academic outcomes, we must treat grant aid as an exogenous instrument for hours of labor. If grant aid only affects academic outcomes through its reduction in labor, it meets the exclusion restriction and we can conclude any academic outcome differences across the Pell eligibility threshold are due to reduced labor market participation.

Table 9 presents our empirical fuzzy regression discontinuity results of the effects of Pell Grant eligibility on academic outcomes for male and female students at four-year institutions.

For male students, we observe a robust significant increase of 0.07-0.09 in GPA across various bandwidths but neither significant nor consistent results in attempted credits in term 1, attempted credits in term 2, or within year persistence. For female students, we do not observe a significant increase in GPA or an increase in credits attempted in term 1 or term 2. The within year persistence estimates are consistently positive, but they are not significant at conventional levels. In some cases, they come close to the 10 percent significance level. For instance, with our $2,000 preferred bandwidth, Pell-eligible female students at four-year institutions persist, on average, 3.3 percentage points more than their Pell-ineligible counterparts, and this is significant at the 11.6 percent level. While these 2 to 3 percent points increase in persistence estimates for female students are not precisely estimated, they are fairly consistent across bandwidths, are close to significance, and are similar to within-year estimates from other studies (Castleman and Long 2016; Goldrick-Rab et al. 2016; Partridge 2013).

In summary, we observe little effect of grant aid on near-term academic outcomes. These results do not conclusively support the hypothesized mechanism of reduced hours spent in the labor market increasing credit accumulation and performance.

Robustness of Results

We have demonstrated robustness of our main findings across choices of bandwidth, but we also conduct a falsification test by assigning placebo treatment at various EFC cutoffs instead of the policy relevant threshold established by the federal government. The results of this falsification test are shown in Appendix Table A5. Although there are occasionally statistically significant results across the 24 tests, they appear random as there is no pattern of results across treatment contrast (grant aid) or outcomes at any placebo threshold. Additionally, we provide parametric results using a cubic functional form to estimate the treatment contrast and our main

outcomes in Appendix Table A6. Results are generally larger in magnitude relative to local linear regression, and the precision is greatly enhanced due to the much larger sample size without a bandwidth restriction. We prefer to rely on the more conservative local linear regression results to avoid the possibility of functional form misspecification.

V. Discussion and Conclusion

Figure 2 provides a visual summary of our results for the population for whom we see the greatest effects: women at four-year colleges. To summarize, our results show that Pell Grant eligibility for students near the threshold increases the total grant aid by $1,100. This increase in grant aid leads to substitution effects as students reduce borrowing and hours worked leading to lower earnings. In our specification using a $2,000 bandwidth, which provides conservative estimates in most outcomes, we observe that Pell eligibility causes women to work about a 1.5-2 hours less per week, earning about $850 less. They also borrow about $550 less than Pell ineligible women. Hence, we observe the expected substitution of grant aid reducing both borrowing and hours of paid work, although not perfectly on a dollar for dollar basis. On average, the substitution effect observed for women is sensible with the $1,142 increase in grant aid leading to a $1,385 decrease in combined borrowing and earnings, but for men, for whom we observe similar levels of student loan substitution but no changes in labor market participation, the substitution is incomplete leaving them with greater total financial resources.

These findings suggest differential responses to grant aid across gender, aligning with much of the prior literature. The gender differences in response to work hours we observe may be explained by male college students' labor supply being less elastic than for women. Perhaps men earn higher wages in the non-college degree labor market making them more reluctant to

reduce hours of work relative to women when receiving additional grant aid. There may also be structural barriers in jobs that men take while enrolled that prevent them from adjusting weekly hours relative to jobs that women hold while enrolled. Alternatively, men may be more inclined to increase consumption while enrolled relative to women and use the additional grant aid to increase their budget constraint. It is also possible that men are more likely to have more unobserved financial resources. For example, families may be more likely to provide financial support to men than women, and the increase in grant aid reduces familial support instead of altering labor market participation. On the other hand, effects on labor market participation for women may be related to childcare such as having children and providing childcare. We test having children and number of dependents and find Pell Grant eligibility has no effects on either outcomes for both men and women. We note that these outcomes can be thought of as pre-treatment covariates, which are then expected to be balanced across the threshold and unsurprisingly, we find null results here. Our data do not have the post-treatment childcare-related variables to test our particular hypotheses. Further research may shed light on this difference in behavior.

It is important to compare our findings to those in the prior literature. In their examination of borrowing response to Pell eligibility, Marx and Turner (2017) find an additional dollar of grant aid reduces borrowing by 43 to 51 cents on the dollar, but, because of financial aid packaging decisions by the institution they study, only 12 percent of their sample borrows student loans. This implies a large response by a small subset of students, and they argue a dollar of grant aid crowds out $1.80-$1.90 among students who would have borrowed in the absence of additional grant aid. They conclude a fixed cost of borrowing leads a small increase in grant aid to deter a subset of students from borrowing entirely.

We agree this conclusion is plausible, but our study suggests two important additional considerations. First, as we have demonstrated, grant aid affects labor market participation. It is possible that the greater than 100 percent level of loan crowd out among would be borrowers is offset by increases in labor market participation, which is unobserved in their study. Although, on average, we observe decreases in hours of employment due to increased grant aid, we observe heterogeneous effects, and it is possible that some students respond by increasing hours of employment while simultaneously reducing borrowing to zero. This is not inconsistent with the Marx and Turner conclusion that a fixed borrowing cost exists, but it emphasizes the need to observe work and borrowing behavior jointly.

Second, the low borrowing rates in their sample causes us to question the generalizability of their results given that we observe about 70 percent of students borrowing in the national data. We also find slightly lower rates of grant and loan substitution as an additional dollar of aid reduces borrowing by a value of 37 cents across all four-year students in our study.

Turning to our estimates of grant aid's impact on employment, our measured reduction in working hours for women of 1.5-2.0 hours per week somewhat aligns with prior literature (Broton, Goldrick-Rab, & Benson, 2016; Park & Scott-Clayton, 2017). Broton, Goldrick-Rab, and Benson (2016) estimated a 1.7 hour reduction per week in their study of the Wisconsin Scholars Grant (WSG). However, the amount of grant aid in that program is $3,500, more than three times the difference in grant aid we observe. Additionally, they find no difference across gender but a much larger reduction in the probability of working (nearly a 6 percentage point reduction). The additional grant aid in WSG likely reduces the need to hold a job thereby explaining the larger reduction in the probability of working, but, based on our results, we would expect the hour reduction in working to be greater than what we observe. Perhaps the difference

is driven by local labor market conditions in Wisconsin that are not present across the United States. We note that average hours worked of their population under study is substantially lower than the national average we observe in our study (roughly 11 hours per week compared to 20 hours per week in our study). It is also possible that the effect of grant aid is nonlinear possibly due to the inflexibility of working hours. They also do not evaluate changes in borrowing; it is possible students in their program are more likely to substitute grant aid for a larger reduction in borrowing and less reduction in earnings than we observe nationally.

Findings from two other grant aid sources provide mixed results of grant aid's impact on working while enrolled. Using a 35 academic week calendar and assuming minimum wage, we estimate that grant aid reduces weekly job hours by nearly two hours in Carruthers and Özek's (2016) study of the merit-based HOPE scholarship in Tennessee. In their study of Dell Scholarship recipients, Page et al. (2017) find that Dell Scholars tend to work more overall but there is a decrease in work hours for students working at least 30-40 hours a week.

Because we study Pell Grant eligibility, our findings can be most directly compared to Park and Scott-Clayton (2017), who find that Pell Grant eligibility reduces working by one to two hours per week, which are similar to our estimates. Their findings are not divided by gender and apply to all students at two-year loan-offering institutions in their state of analysis. Collectively, it appears students are responding to additional grant aid by changing work behavior and substituting need-based aid for earnings while enrolled.

To test the hypothesized mechanism of whether grant aid reducing hours spent in the labor market leads to increased persistence, we evaluate the effects of Pell eligibility on academic outcomes. We find no effects of the additional aid on GPA for women, although we do observe small, positive GPA effects for men, and we find small effects on term-to-term

persistence effects of two to three percentage points for women consistent with other studies (Castleman and Long 2016; Goldrick-Rab et al. 2016, Partridge 2013), although the latter have p-values just outside standard levels of significance. Unlike Carruthers and Özek (2016) who find losing a merit-based scholarship results in a reduction of credits attempted, we find no increase in academic performance or credits attempted for women when receiving grant aid. Pell-eligible women work almost two hours less per week providing them with more time to engage in educational activities; however, we find the reduction in work hours is not linked to observable changes in academic performance.

Our results do not support the hypothesized mechanism, so we consider alternative mechanisms for why grant aid may increase educational attainment. It is possible the additional time spent outside of the labor market increases social integration, such as participating in extracurricular activities at the institution. Scholarship in the field of postsecondary education suggests social integration leads to higher persistence (Tinto 1975; Tinto 1998). This hypothesis requires further investigation in future research. There is also some evidence that borrowing choices are linked to persistence (Barr, Bird, and Castleman, 2017; Dowd and Coury 2006; Kim 2007). Perhaps the reduction in borrowing caused by additional grant aid leads to increased persistence, although the economic rationale behind such a link is not clear.

We acknowledge several limitations in our analysis. First, fuzzy regression discontinuity only provides causal estimates for compliers close to the eligibility threshold in the EFC distribution. These students are low-income, but they are not the neediest students that attend college with an EFC of zero dollars. We are unable to estimate the effect of additional grant aid for the lowest income students. Second, because work hours and earnings are self-reported by students, there is likely measurement error. To the extent that the error is classical, it would

result in attenuation bias suggesting our estimates of the substitution effect between grant aid and earnings are underestimates. We believe this is of limited concern given that the earnings reports are closely aligned with the reduction in hours worked per week times reasonable hourly earnings of non-college educated laborers, but it may partially explain the smaller effects we observe relative to those in prior literature.

This paper also provides important contributions for policy. Policymakers are interested in the relationship between financial aid and postsecondary outcomes (Dynarski and Scott-Clayton 2013; Hossler et al. 2009; Slater 2009). If financial aid only affects students at the attendance margin, then it is inefficient to continue providing money to them after they have enrolled. However, if financial aid also helps students persist and move towards degree completion, then it is a valuable tool, worthy of continued public and private investment.

Although we cannot distinguish the effect of the Pell Grant from other sources of need-based grant aid, our results demonstrate that continued investment in Pell Grants by the federal government will limit borrowing and reduce the conflict between spending time on academic pursuits and time spent on paid labor while enrolled in postsecondary education. Although the effects on persistence are not precisely estimated, they suggest need-based aid does increase persistence among women. It also increases academic performance among men, so, to the extent performance is tied to learning, need-based aid leads to increased human capital. Beyond the federal government's investment in Pell Grants, these findings suggest continued investment in need-based aid by state governments and institutions will have positive effects on student outcomes.

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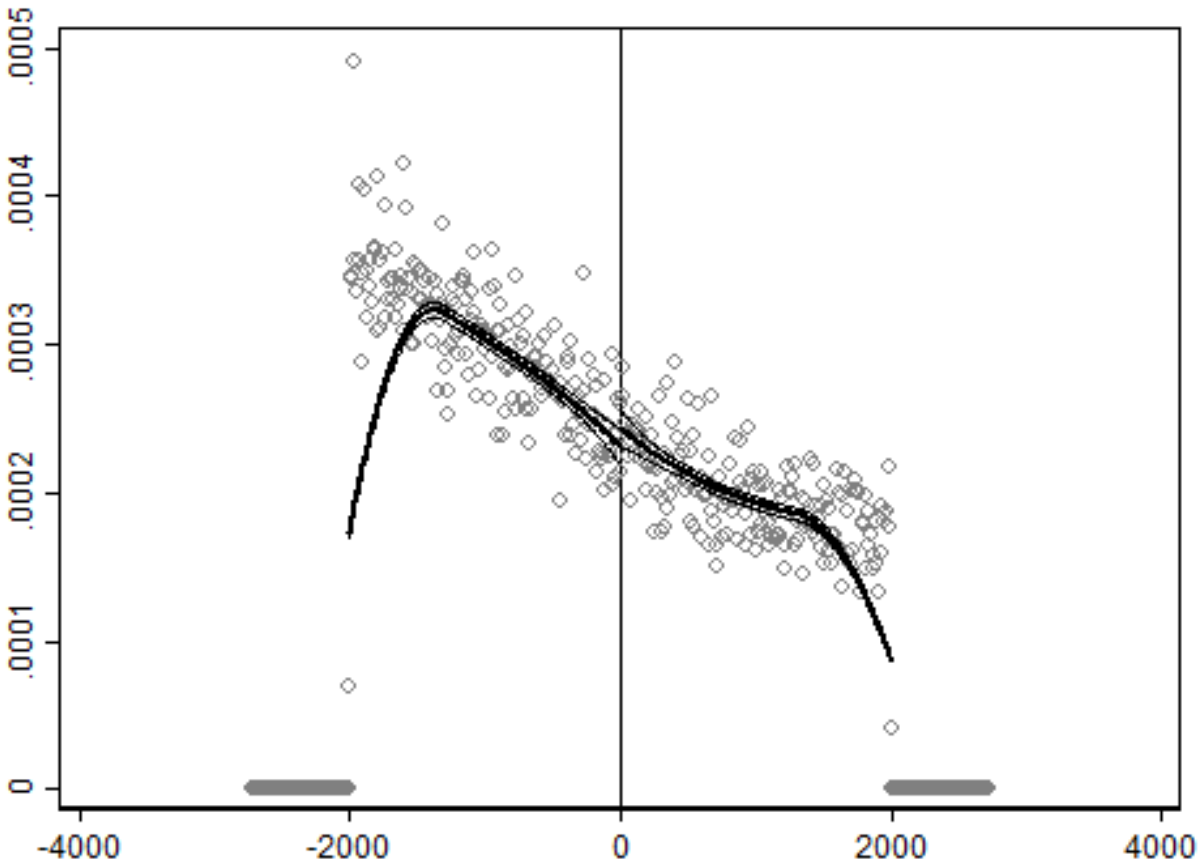
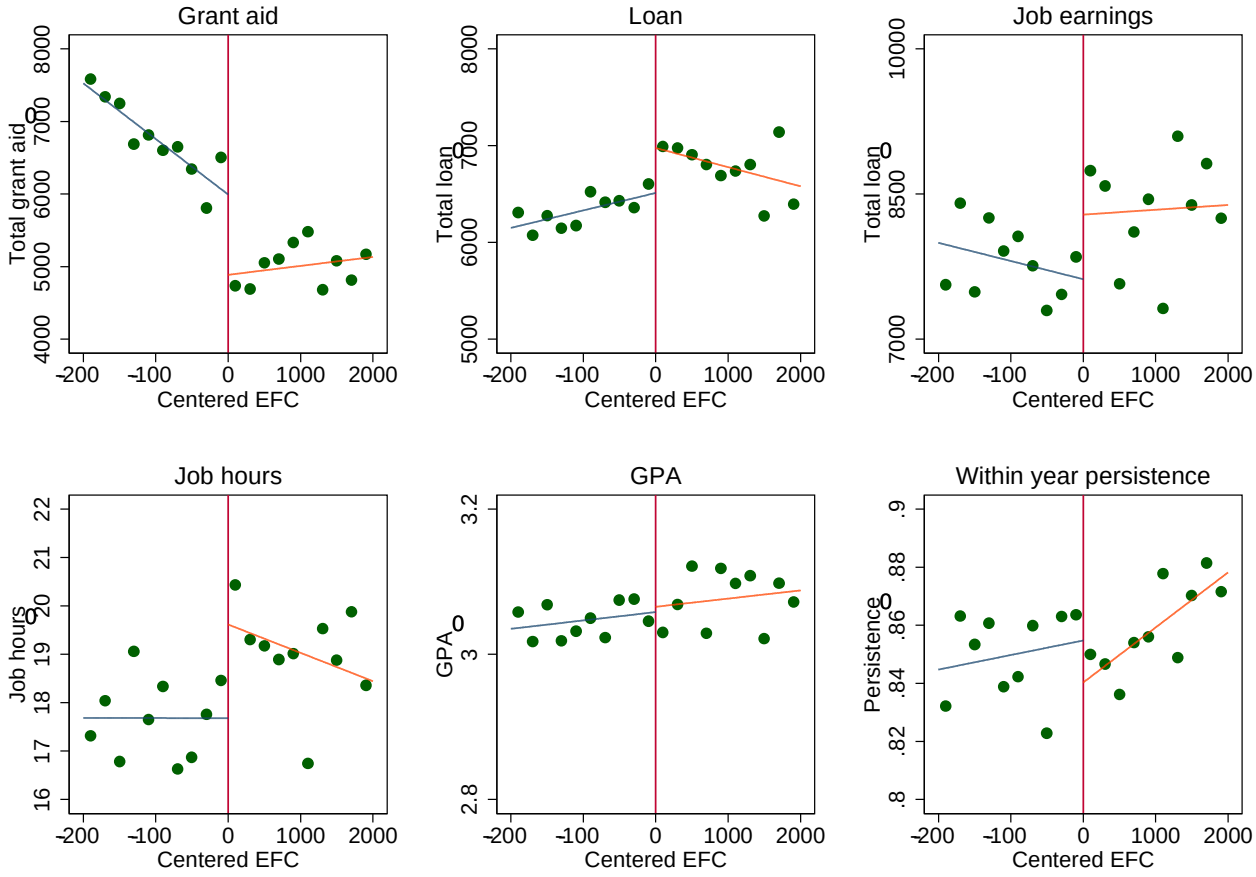
Figures and Tables

Figure 1. Formal McCrary graph to test for manipulation.



Note. The EFC is centered at the cutoff for each wave for the pooled NPSAS sample of all four waves. The density function of EFC was estimated using McCrary's (2008) test for manipulation of the forcing variable in regression discontinuity framework.

Figure 2. Relationships between EFC and selected outcomes for women at four-year institutions with local linear regressions on either side of the cutoff



Note. Each point represents the mean of the dependent variable within a $200 bin of EFC. The trend lines present local linear regressions within $2,000 of the Pell Grant eligibility cutoff controlling only for EFC, eligibility and their interaction.

Table 1. Descriptive Statistics: Demographics and Aid variables

Variable	(1) Pooled NPSAS	(2) Pooled sample of $2,000 bw	(3) 2000 analytic sample of $2,000 bw	(4) 2004 analytic sample of $2,000 bw	(5) 2008 analytic sample of $2,000 bw	(6) 2012 analytic sample of $2,000 bw
Panel A: Demographics						
Female	0.60	0.58	0.60	0.58	0.59	0.53
White	0.56	0.64	0.71	0.63	0.62	0.61
Black	0.18	0.14	0.12	0.15	0.15	0.14
Hispanic	0.16	0.14	0.10	0.13	0.14	0.18
Asian	0.04	0.04	0.03	0.04	0.05	0.04
Other	0.05	0.04	0.04	0.05	0.04	0.04
Parents' education: high school or less	0.43	0.40	0.43	0.41	0.37	0.38
Parents' education: some college	0.25	0.28	0.25	0.25	0.30	0.30
Parents' education: BA or higher	0.32	0.33	0.32	0.33	0.33	0.32
Panel B: Grant Aid						
Pell receipt	0.58	0.52	0.50	0.51	0.57	0.48
Pell grant amount	1846.92	650.80	720.07	654.40	669.14	559.45
	(1987.83)	(798.62)	(899.70)	(817.11)	(758.91)	(746.35)
SEOG receipt	0.14	0.09	0.11	0.09	0.10	0.05
SEOG amount	99.56	83.69	123.52	91.46	90.62	30.45
	(392.05)	(391.21)	(503.11)	(417.42)	(393.69)	(198.74)
State grant receipt	0.25	0.31	0.32	0.28	0.39	0.21
State grant amount	652.49	853.31	870.18	721.91	1117.41	546.78
	(1609.23)	(1784.28)	(1796.22)	(1628.62)	(1986.44)	(1501.38)
Inst. grant receipt	0.25	0.31	0.35	0.30	0.33	0.24
Inst. grant amount	1590.99	1935.78	2060.79	1746.85	2133.13	1726.24
	(4441.06)	(4803.37)	(4824.56)	(4390.98)	(4829.55)	(5187.16)
Any grant receipt	0.77	0.76	0.77	0.75	0.80	0.70
Total grant amount	4752.23	4176.76	4200.10	3646.48	4915.37	3535.58
	(6231.85)	(6486.23)	(6110.99)	(5753.21)	(6853.21)	(6831.85)
Observations	241,820	39,810	6,770	10,200	14,420	8,430

Note. $2,000 bw is the bandwidth around the Pell eligibility cutoff for each wave. Reported values are means of continuous variables and proportions of binary variables. Total grant includes federal grants such as Pell and Supplemental Educational Opportunity Grant (SEOG), state grants, institutional grants, outside grants including employee benefits, and grants from private sources. Standard deviations are in parentheses. Aid variables have been converted to constant 2012 dollars. All observations have been rounded to the nearest 10 per Institute for Education Sciences (IES) compliance.

Table 2. Descriptive Statistics: Work, borrowing, and academic outcomes

Variables	(1) Pooled NPSAS	(2) Pooled sample of $2,000 bw	(3) 2000 analytic sample of $2,000 bw	(4) 2004 analytic sample of $2,000 bw	(5) 2008 analytic sample of $2,000 bw	(6) 2012 analytic sample of $2,000 bw
Panel A. Work and Borrowing						
Has any job during school year	0.70	0.77	0.87	0.71	0.83	0.66
Hours worked per week (excluding work-study/assistantship)	17.79 (16.87)	19.72 (16.76)	.	19.52 (16.48)	21.15 (16.54)	17.52 (17.20)
Loan receipt	0.66	0.71	0.72	0.65	0.74	0.71
Total loan amount	4923.50 (5284.99)	5373.21 (5333.60)	5283.54 (5050.61)	4437.73 (4918.62)	6069.18 (5776.84)	5386.03 (5063.82)
Work study receipt	0.11	0.13	.	0.14	0.15	0.08
Hours of work study per week	1.52 (5.25)	1.71 (5.35)	.	1.73 (5.27)	2.06 (5.84)	1.08 (4.42)
Panel B. Academic Outcomes						
GPA in first academic year	2.88 (0.93)	2.96 (0.84)	2.95 (0.73)	2.96 (0.77)	3.08 (0.73)	2.74 (1.09)
Term 1 attempted credit hours	12.23 (9.81)	12.40 (11.81)	12.76 (4.79)	12.44 (4.96)	12.07 (4.33)	.
Term 2 attempted credit hours	12.10 (5.32)	12.17 (5.39)	12.52 (7.67)	12.14 (4.59)	12.55 (4.20)	.
Persisting from first to second term by credits attempted	0.79	0.78	0.78	0.75	0.72	.
Observations	241,820	39,810	6,770	10,200	14,420	8,430

Note. Credit hours are normalized to 15 credit hours per semester. Hours worked per week are only for students who reported having a job. Hours worked per week excluding work study/assistantship was not included in the survey for the 1999-2000 wave. Total loan includes federal, state, institutional, and private/alternative loans. In the 2011-2012 wave, institutions did not report attempted credit hours that were comparable to previous waves. Persistence is calculated by the authors as having attempted credit hours for both the first and second major terms. All observations have been rounded to the nearest 10 per IES compliance.

Table 3. Compliance of Pell eligibility and receipt

Variable	Whole sample	$2000 bw
Pell ineligible		
Did not receive Pell	87,730	15,750
Did receive Pell	100	10
Total	87,830	15,760
Pell eligible		
Did not receive Pell	15,630	3,200
Did receive Pell	141,470	20,860
Total	157,100	24,060
Observations	244,930	39,820

Note. This calculation uses all four waves of NPSAS.
All observations have been rounded to the nearest 10 per IES compliance.

Table 4. Continuity and balance checks of demographics at $2,000 bandwidth

Variable	(1) Female	(2) Par ed: HS or less	(3) Par ed: Some college	(4) Par ed: BA or higher	(5) Black	(6) Hispanic	(7) Asian	(8) Other	(9) White
Pell eligibility	0.007	0.004	0.010	-0.014	0.004	0.002	-0.007	0.005	-0.004
	(0.010)	(0.011)	(0.010)	(0.010)	(0.007)	(0.007)	(0.004)	(0.004)	(0.009)
N	39,810	39,810	39,810	39,810	39,810	39,810	39,810	39,810	39,810

Note. Reported values are the coefficient estimates on Pell eligibility after regressing the student observable variable on the running variable (EFC), Pell eligibility, and the interaction term of EFC and Pell eligibility around a bandwidth of $2,000 with institutional fixed effects. Heteroskedastic-robust institution-level clustered standard errors are in parentheses. All observations have been rounded to the nearest 10 per IES compliance.
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

Table 5. Treatment contrast at $2,000 bandwidth

Variable	(1) Pell	(2) SEOG	(3) State grant	(4) Institutional grant	(5) Total grant	(6) N
All institutions	244.80**	97.32**	43.17	153.92+	798.20**	39,810
	(7.83)	(9.83)	(38.18)	(81.97)	(114.21)	
Four-year institutions	250.75**	144.54**	63.04	285.87*	1,095.53**	22,000
	(9.23)	(15.96)	(56.91)	(130.30)	(177.12)	
Two-year institutions	235.80**	29.90**	-38.74	-133.54+	153.90	10,300
	(17.67)	(7.50)	(60.03)	(78.40)	(119.89)	

Note. Reported coefficients are fuzzy treatment estimates using eligibility as an instrument for receipt with the running variable (EFC) and the interaction between EFC and eligibility along with race, gender, and parental education as regressors with institution and wave fixed effects. About 7,500 students in the $2,000 bandwidth who attended less than 2-yr institutions or attended multiple institutions are included in the all institutions category but not the only four-year or only two-year categories. Total grant includes federal grants such as Pell and SEOG, state grants, institutional grants, outside grants including employee benefits, and grants from private sources. Heteroskedastic-robust institution-level clustered standard errors are in parentheses. All observations have been rounded to the nearest 10 per IES compliance.

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

Table 6. Work and loan substitution for grant aid at four-year institutions

	(1) $1,000 bw	(2) $1,500 bw	(3) $2,000 bw	(4) $2,500 bw	(5) $3,000 bw	(6) IK bw
Has job	-0.013	-0.026^{+}	-0.014	-0.020^{+}	-0.012	-0.011
	(0.020)	(0.015)	(0.013)	(0.012)	(0.010)	(0.010)
N	10,750	16,300	22,000	27,810	36,580	37,270
Weekly job hours	-1.876*	-1.619*	-1.040^{+}	-1.657**	-1.293**	-1.574**
	(0.852)	(0.659)	(0.555)	(0.484)	(0.434)	(0.456)
N	8,480	12,890	17,390	22,060	26,780	24,060
Weekly job hours (unconditional on having jobs)	-1.944*	-1.665**	-1.105*	-1.701**	-1.328**	-0.807*
	(0.826)	(0.640)	(0.540)	(0.470)	(0.418)	(0.378)
N	8,730	13,290	17,950	22,770	28,250	34,810
Loan receipt	-0.028	-0.023	-0.020	-0.014	-0.013	-0.013
	(0.017)	(0.014)	(0.013)	(0.011)	(0.010)	(0.009)
N	10,750	16,300	22,000	27,810	36,580	39,140
Total loan	-611.558*	-276.476	-299.882^{+}	-361.054*	-388.910**	-308.902**
	(248.390)	(195.372)	(167.577)	(141.122)	(126.602)	(114.521)
N	8,730	13,150	17,700	22,210	28,760	35,560
Total loan (unconditional on having loans)	-701.950**	-415.542*	-401.222*	-403.134**	-404.750**	-297.326**
	(251.426)	(196.400)	(165.384)	(141.377)	(126.808)	(101.890)
N	10,750	16,300	22,000	27,810	36,580	65,270

Note. Reported coefficients are fuzzy treatment estimates using eligibility as an instrument for receipt with the running variable (EFC) and the interaction between EFC and eligibility along with race, gender, and parental education as regressors with institution and wave fixed effects. Weekly job hours reported is conditional on having any job. Total loan is conditional on having loans. Heteroskedastic-robust institution-level clustered standard errors are in parentheses. All observations have been rounded to the nearest 10 per IES compliance.

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

Table 7. Work and loan substitution for grant aid at four-year institutions by gender

	(1) $1,000 bw	(2) $1,500 bw	(3) $2,000 bw	(4) $2,500 bw	(5) $3,000 bw	(6) IK bw
Panel A. Male 4-year						
Has job	0.038	-0.002	0.000	-0.010	-0.010	0.000
	(0.033)	(0.026)	(0.022)	(0.019)	(0.017)	(0.013)
N	4,750	7,180	9,660	12,190	15,880	28,390
Weekly job hours	0.595	-0.236	-0.318	-1.219	-0.704	0.344
	(1.336)	(1.054)	(0.888)	(0.782)	(0.692)	(0.510)
N	3,820	5,800	7,800	9,850	11,930	33,740
Weekly job hours	0.600	-0.311	-0.432	-1.259^{+}	-0.719	-0.204
(unconditional on having jobs)	(1.315)	(1.036)	(0.869)	(0.762)	(0.668)	(0.576)
N	3,920	5,950	8,010	10,120	12,490	18,780
Loan receipt	-0.035	-0.030	-0.031^{+}	-0.019	-0.016	-0.031**
	(0.028)	(0.022)	(0.019)	(0.016)	(0.014)	(0.012)
N	4,750	7,180	9,660	12,190	15,880	27,810
Total loan	-718.443^{+}	-179.416	-108.043	-135.866	-284.778	-261.152
	(417.175)	(309.301)	(263.641)	(221.086)	(197.621)	(187.505)
N	3,850	5,790	7,750	9,660	12,390	13,400
Total loan	-772.880^{+}	-351.315	-352.554	-265.288	-366.013^{+}	-323.704*
(unconditional on having loan)	(408.066)	(310.515)	(259.479)	(225.442)	(198.752)	(155.729)
N	4,750	7,180	9,660	12,190	15,880	30,040
Panel B. Female 4-year						
Has job	-0.047^{+}	-0.041*	-0.025	-0.028^{+}	-0.016	-0.012
	(0.027)	(0.021)	(0.018)	(0.016)	(0.014)	(0.013)
N	6,010	9,120	12,330	15,620	20,710	22,770
Weekly job hours	-2.747*	-2.092*	-1.447^{+}	-1.810**	-1.601**	-1.805**
	(1.253)	(0.959)	(0.784)	(0.681)	(0.609)	(0.636)
N	4,660	7,100	9,600	12,210	14,850	13,390
Weekly job hours	-2.899*	-2.187*	-1.569*	-1.925**	-1.684**	-1.581**
(unconditional on having jobs)	(1.213)	(0.932)	(0.764)	(0.660)	(0.587)	(0.545)
N	4,810	7,340	9,940	12,650	15,760	17,680
Loan receipt	-0.046^{+}	-0.035^{+}	-0.024	-0.018	-0.014	-0.014
	(0.025)	(0.020)	(0.017)	(0.015)	(0.013)	(0.013)
N	6,010	9,120	12,330	15,620	20,710	21,190
Total loan	-479.677	-391.706	-440.727^{+}	-529.426**	-513.419**	-454.052**
With loan	(339.890)	(271.720)	(227.913)	(200.668)	(182.166)	(174.564)
N	4,880	7,360	9,950	12,550	16,370	17,760
Total loan	-798.833*	-606.143*	-553.081*	-572.402**	-497.846**	-460.661**
(unconditional on having loan)	(359.072)	(276.142)	(229.469)	(198.462)	(179.179)	(163.777)
N	6,010	9,120	12,330	15,620	20,710	23,920

Note. Reported coefficients are fuzzy treatment estimates using eligibility as an instrument for receipt with the running variable (EFC) and the interaction between EFC and eligibility along with race, gender, and parental education as regressors with institution and wave fixed effects. Weekly job hours reported is conditional on having any job. The IK bandwidths for each panel by order of outcome variable are $4464, $5884, $3994, $4169, $5226, $3291, $3359, $2720, $3355, $3084, $3557, and $3314 respectively. Heteroskedastic-robust institution-level clustered standard errors are in parentheses. All observations have been rounded to the nearest 10 per IES compliance. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

Table 8. Monetary substitution of regression discontinuity estimates around a $2,000 bandwidth with job earnings less than $60,000

Variable	(1) Total grant	(2) Total loan	(3) Job earning
Four-year institutions	1191.82**	-444.47*	-516.81
	(205.65)	(192.94)	(351.06)
N	17,220	17,220	17,220
Male 4-yr	999.60**	-583.67+	268.88
	(314.49)	(308.70)	(554.46)
N	7,710	7,710	7,710
Female 4-yr	1141.98**	-530.88*	-853.99+
	(276.20)	(261.46)	(508.54)
N	9,510	9,510	9,510

Note. Reported coefficients are fuzzy estimates using eligibility as an instrument for receipt with the running variable (EFC) and the interaction between EFC and eligibility along with race, gender, and parental education as regressors with institution and wave fixed effects. These estimates do not include the 1999-2000 wave. Job earnings are restricted to those reporting earnings of less than $60,000 while enrolled. Heteroskedastic-robust institution-level clustered standard errors are in parentheses. All observations have been rounded to the nearest 10 per IES compliance.

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

Table 9. Regression discontinuity estimates of academic outcomes across various bandwidths

	(1) $1,000 bw	(2) $1,500 bw	(3) $2,000 bw	(4) $2,500 bw	(5) $3,000 bw	(6) IK bw
Panel A. Male 4-year institutions						
GPA	0.129*	0.086+	0.081*	0.084*	0.088**	0.072**
	(0.058)	(0.045)	(0.037)	(0.033)	(0.029)	(0.025)
N	4,720	7,150	9,630	12,140	15,790	19,570
Credit term1	0.197	0.404	0.424	0.314	0.329	0.263
	(0.565)	(0.431)	(0.406)	(0.279)	(0.207)	(0.161)
N	3,700	5,580	7,520	9,470	12,490	14,390
Cred term2	-0.089	0.039	0.055	-0.039	0.057	0.035
	(0.547)	(0.390)	(0.368)	(0.252)	(0.212)	(0.203)
N	3,210	4,850	6,530	8,240	10,810	9,270
Persistence	-0.001	0.019	0.006	0.006	0.014	0.003
	(0.036)	(0.028)	(0.023)	(0.021)	(0.018)	(0.022)
N	3,140	4,740	6,420	8,100	10,820	7,310
Panel B. Female 4-year institutions						
GPA	0.024	-0.011	-0.029	-0.039	-0.028	-0.040
	(0.053)	(0.041)	(0.034)	(0.030)	(0.027)	(0.028)
N	5,990	9,080	12,290	15,560	20,600	17,390
Credit term1	0.371	0.287	0.323	0.124	0.169	0.297
	(0.348)	(0.349)	(0.259)	(0.190)	(0.174)	(0.321)
N	4,800	7,300	9,910	12,550	16,810	8,070
Cred term2	0.188	-0.046	0.095	0.016	-0.018	-0.060
	(0.366)	(0.315)	(0.226)	(0.179)	(0.154)	(0.151)
N	4,170	6,390	8,710	11,010	14,650	16,060
Persistence	0.017	0.021	0.033	0.028	0.023	0.020
	(0.033)	(0.025)	(0.021)	(0.018)	(0.016)	(0.025)
N	4,140	6,290	8,560	10,870	14,780	6,310

Note. Reported coefficients are fuzzy estimates using eligibility as an instrument for receipt with the running variable (EFC) and the interaction between EFC and eligibility along with race, gender, and parental education as regressors with institution and wave fixed effects. Sample sizes vary across outcomes based on missing data for each outcome variable. Persistence is calculated by the authors as having attempted credit hours for both the first and second major terms. The IK bandwidths for each panel by outcome variable are $3811, $3744, $2791, $4235, $2783, $1866, $3363, and $1857 respectively. Heteroskedastic-robust institution-level clustered standard errors are in parentheses. All observations have been rounded to the nearest 10 per IES compliance.

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

Appendix Table A1. Differences in aid amounts between recipients and non-recipients near the eligibility threshold across various bandwidths at four-year institutions

Aid type	(1) $1,000 bw	(2) $1,500 bw	(3) $2,000 bw	(4) $2,500 bw	(5) $3,000 bw	(6) IK bw
Pell	307.83**	270.57**	250.75**	247.59**	228.19**	247.93**
	(9.61)	(9.16)	(9.23)	(9.61)	(10.80)	(9.46)
SEOG	163.75**	132.41**	144.54**	142.56**	115.77**	144.65**
	(20.49)	(17.56)	(15.96)	(14.87)	(14.35)	(16.04)
State grant	109.73	83.70	63.04	61.94	104.99*	76.84
	(92.86)	(72.39)	(56.91)	(50.92)	(45.99)	(56.79)
Institutional grant	351.23+	326.19*	285.87*	128.10	51.58	281.57*
	(190.20)	(150.97)	(130.30)	(108.95)	(99.07)	(125.02)
Total grant	1203.93**	1137.21**	1095.53**	899.76**	829.67**	1092.49**
	(255.86)	(201.72)	(177.12)	(149.23)	(135.42)	(171.38)
N	10,750	16,300	22,000	27,810	36,580	22,780

Note. Reported coefficients are fuzzy estimates of aid using eligibility as an instrument for receipt with the running variable (EFC) and the interaction between EFC and eligibility along with race, gender, and parental education as regressors with institution and wave fixed effects. The IK bandwidth is $2,071. Heteroskedastic-robust institution-level clustered standard errors are in parentheses. All observations have been rounded to the nearest 10 per IES compliance.

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

Appendix Table A2. Impacts of Pell Grant eligibility on work-study at four-year institutions

	(1) $1,000 bw	(2) $1,500 bw	(3) $2,000 bw	(4) $2,500 bw	(5) $3,000 bw	(6) $4,000 bw
Has work-study	-0.002	-0.004	-0.002	0.002	0.001	0.006
	(0.021)	(0.016)	(0.013)	(0.012)	(0.011)	(0.009)
N	8,480	12,890	17,390	22,060	26,780	41,650
Work-study hours	-0.047	0.023	0.018	0.054	0.042	0.087
	(0.316)	(0.254)	(0.211)	(0.188)	(0.174)	(0.144)
N	8,480	12,890	17,390	22,060	26,780	41,650

Note. Reported coefficients are fuzzy treatment estimates using eligibility as an instrument for receipt with the running variable (EFC) and the interaction between EFC and eligibility along with race, gender, and parental education as regressors with institution and wave fixed effects. Heteroskedastic-robust institution-level clustered standard errors are in parentheses. All observations have been rounded to the nearest 10 per IES compliance.
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

Appendix Table A3. Continuity and balance checks of demographics at $2,000 bandwidth for gender

Variable	(1) Par ed: HS or less	(2) Par ed: Some college	(3) Par ed: BA or higher	(4) Black	(5) Hispanic	(6) Asian	(7) Other	(8) White
Male								
Pell	0.004	-0.007	0.002	-0.008	0.012	-0.005	0.012	-0.012
eligibility	(0.018)	(0.016)	(0.017)	(0.011)	(0.011)	(0.007)	(0.007)	(0.015)
N	16,810	16,810	16,810	16,810	16,810	16,810	16,810	16,810
Female								
Pell	0.005	0.011	-0.016	0.014	-0.005	-0.009	0.001	-0.002
eligibility	(0.015)	(0.014)	(0.014)	(0.010)	(0.009)	(0.006)	(0.006)	(0.012)
N	23,000	23,000	23,000	23,000	23,000	23,000	23,000	23,000

Note. Reported values are the coefficient estimates on Pell eligibility after regressing the student observable variable on the running variable (EFC), Pell eligibility, and the interaction term of EFC and Pell eligibility around a bandwidth of $2,000 with institutional and wave fixed effects. Heteroskedastic-robust institution-level clustered standard errors are in parentheses. All observations have been rounded to the nearest 10 per IES compliance.
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

Appendix Table A4. Pooled regression discontinuity estimates of job earning excluding work study and assistantship for women at four-year institutions

Reported job earning	(1) $1,000 bw	(2) $1,500 bw	(3) $2,000 bw	(4) $2,500 bw	(5) $3,000 bw	(6) IK bw
Unrestricted	-2715.73**	-1832.54*	-1676.49**	-1330.19*	-1155.64*	-1341.21*
	(925.09)	(730.15)	(613.76)	(540.17)	(493.82)	(569.94)
N	4,660	7,100	9,600	12,210	14,850	11,370
Less than $60,000	-1892.57*	-1292.67*	-853.99+	-899.40*	-922.04*	-832.49+
	(790.44)	(592.65)	(508.54)	(443.49)	(410.45)	(464.13)
N	4,620	7,030	9,510	12,100	14,710	11,270
Less than $50,000	-2084.31**	-1243.23*	-781.67	-979.24*	-889.33*	-881.75*
	(754.80)	(562.06)	(478.94)	(417.33)	(388.26)	(436.11)
N	4,580	6,980	9,440	12,000	14,590	11,190

Note. Reported coefficients are fuzzy treatment estimates using eligibility as an instrument for receipt with the running variable (EFC) and the interaction between EFC and eligibility along with race and parental education as regressors with institution and wave fixed effects. These estimates do not include the 1999-2000 wave. The IK bandwidth is $2,346. Heteroskedastic-robust institution-level clustered standard errors are in parentheses. All observations have been rounded to the nearest 10 per IES compliance.

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

Appendix Table A5. Placebo treatment on loan and weekly job hours at various EFC cutoffs around $2,000 bw for women at 4-year institutions.

	Placebo treatment at given amount below or above centered EFC							
Outcome	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	$250 below	$250 above	$500 below	$500 above	$750 below	$750 above	$1000 below	$1000 above
Total Grant	216.90	237.12	-244.21	269.74	-328.54	-38.76	-503.28*	-44.15
	(202.07)	(209.70)	(223.43)	(203.50)	(225.47)	(195.74)	(236.43)	(193.70)
N	9,670	10,560	9,240	11,090	8,820	11,580	8,380	13,900
Loan	-322.64	-271.19	233.30	-440.65*	659.94**	-194.25	357.45	-292.20
	(239.62)	(205.54)	(230.53)	(193.86)	(236.61)	(195.95)	(248.47)	(181.94)
N	9,670	10,560	9,240	11,090	8,820	11,580	8,380	13,900
Weekly job hours	0.09	-1.36*	-0.71	-0.80	-0.24	0.19	0.36	0.23
	(0.73)	(0.64)	(0.71)	(0.63)	(0.73)	(0.59)	(0.75)	(0.57)
N	9,190	9,990	8,830	10,480	8,430	10,970	8,070	11,440

Note. Reported coefficients are sharp treatment estimates using eligibility as an instrument for receipt with the running variable (EFC) and the interaction between EFC and eligibility along with race, gender, and parental education as regressors with institution and wave fixed effects. Heteroskedastic-robust institution-level clustered standard errors are in parentheses. All observations have been rounded to the nearest 10 per IES compliance.
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

Appendix Table A6. Parametric results for total grant aid, total loan, job earnings, and job hours

	(1) Total grant aid	(2) Total loan	(3) Job earnings	(4) Weekly job hours
Four-year institutions	1443.81**	-573.53**	-2061.30**	-2.16**
	(188.56)	(159.65)	(286.65)	(0.46)
N	104,600	104,600	104,600	104,600
Male 4-year	1419.15**	-362.05	-1366.71**	-1.44+
	(287.02)	(242.34)	(453.93)	(0.76)
N	45,700	45,700	45,700	45,700
Female 4-year	1473.59**	-767.94**	-2521.86**	-2.49**
	(250.38)	(220.20)	(384.63)	(0.63)
N	58,890	58,890	58,890	58,890

Note. Reported coefficients are fuzzy treatment estimates using eligibility as an instrument for receipt with cubic functional form of the running variable (EFC) and the interaction between EFC and eligibility along with race, gender, and parental education as regressors with institution and wave fixed effects. Job earnings are restricted to those reporting earnings of less than $60,000 while enrolled and sample size restricted to having all four outcomes. Heteroskedastic-robust institution-level clustered standard errors are in parentheses. All observations have been rounded to the nearest 10 per IES compliance.

+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$