The Effects of Losing Pell Grant Eligibility on Student Outcomes

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This paper examines the effects of Pell Grant eligibility on student outcomes. Using a regression discontinuity (RD) design and a partial identification approach, the study provides bounds on the treatment effects that account for selection bias arising from the loss of grant eligibility. While initial eligibility is determined by financial need alone, students must achieve Satisfactory Academic Progress (SAP) to retain the grant. Students eligible for the maximum grant aid are 26 percentage points less likely to persist in the year they lose grant eligibility than those with less aid. This negative effect on persistence extends to graduation; these students are 8 percentage points less likely to graduate within 4 years. Furthermore, these two groups of students differ in their underlying characteristics, which introduces attrition bias into the estimates. Finally, to address this selection bias, I provide bounds on the effects of Pell Grant on student outcomes. While naive RD estimates find no effect on completion rates, bounding estimates reveal that students eligible for the maximum grant aid are up to 4, 2, and 2 percentage points more likely to graduate from a 4-year institution within 4, 5, and 6 years compared to those with less aid, respectively. Furthermore, these eligible students graduate with a higher GPA than previously estimated. These positive effects are larger than those found in earlier studies.

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I. Introduction

Since the 1970s, the Federal Pell Grant program has been a major source of federally funded, need-based financial aid. As of the 2021-2022 academic year, about 6.1 million undergraduate students in the U.S., approximately 32 percent, received a Pell Grant (U.S. Department of Education, 2022c).[1] Aimed at making higher education more accessible for low-income students, eligibility for the Pell Grant is determined solely by financial need; however, to maintain grant eligibility, students must also demonstrate Satisfactory Academic Progress (SAP) alongside financial necessity. The general requirements for federal SAP include maintaining a cumulative grade point average (GPA) of 2.0 or above and completing at least two-thirds of the cumulative credit hours attempted.[2] While linking academic requirements to financial aid may encourage improved student performance, unintended consequences have been observed, most notably on student dropouts (Scott-Clayton and Schudde, 2020; Montalbán, 2023).

To explore this issue, this paper first examines how students eligible for the maximum grant aid and those with adjusted grant aid respond differently to the loss of their grants. I find that students who qualify for more aid are less likely to persist in college when they lose their Pell compared to those with less aid. Additionally, students with higher and lower levels of aid who do not persist in college differ in their underlying characteristics. If students make the decisions not to persist in college based on their grant eligibility status, this introduces attrition bias. To address this selection issue, I provide bounds on the effects of maximum grant eligibility on student outcomes. Specifically, I assess how the treatment effects vary under different assumptions about the counterfactual outcomes for students who lose grant eligibility.

Existing studies provide mixed evidence on whether educational subsidies affect student outcomes. Some have concluded that need-based financial aid positively affects student outcomes (Seftor and Turner, 2002; Bettinger, 2004; Deming and Dynarski, 2009; Park and Scott-Clayton, 2018; Anderson et al., 2020; Liu, 2020), while others have found little to no impact (Hansen, 1983; Kane, 1995; Angrist et al., 2017; Turner, 2017; Marx and Turner, 2018; Carruthers and Welch, 2019; Rattini, 2023).[3]

[1]For more information on the Federal Pell Grant program, refer to Title IV of the Higher Education Act of 1965, which authorizes federal student financial aid programs.

[2]While institutions may have different policies, their SAP policy must be as strict as their graduation requirements, which typically requires students to maintain a GPA of 2.0 or above (Schudde and Scott-Clayton, 2016). Students who fail to meet these academic standards may receive the grant for one additional semester; however, failing to achieve SAP in that term will result in the loss of eligibility (U.S. Department of Education, 2011). Due to the insufficient number of students in the sample not meeting the SAP credit requirements, this paper only focuses on the GPA requirement.

[3]Suggested explanations for the diminished impact of Pell Grants include the complexity and administrative burden of applying for financial aid (Bettinger et al., 2012), a lack of awareness regarding eligibility and benefits, and the interaction—whether implicit or explicit—with other forms of financial aid. Dynarski and Scott-Clayton (2013) note that students often do not realize they qualify for aid and do not apply for financial assistance. Additionally, an increase in Pell Grant amounts is associated

Several recent studies acknowledge that eligibility for need-based financial aid is intertwined with various factors influencing college decisions. To identify the causal effect of financial aid on college outcomes, several studies have leveraged several sources of exogenous variation, such as the sharp cutoffs in grant amounts: students whose family income falls below a year-specific threshold qualify for the maximum grant amount, while those above the threshold are eligible for adjusted amounts. This eligibility is determined by financial need, measured by Expected Family Contribution (EFC).[4] Students with a \$0 EFC are eligible for the maximum grant amount, while those with a positive EFC qualify for adjusted amounts based on their EFC. This discontinuity in grant amounts generates an ideal setup to use the Regression Discontinuity (RD) approach. Exploiting this cutoff in grant eligibility, Castleman and Long (2016) find that eligibility had a positive effect on attendance, credit accumulation, and completion rates for students at 4-year institutions. Similarly, Denning et al. (2019) show that eligibility for more grant aid positively affects both college outcomes and post-college earnings, marking the first study to investigate the effect of US federal grant programs on post-college earnings. Eng and Matsudaira (2021), employing discontinuities and kinks, suggest that Pell Grant eligibility contributes positively to degree completion rates for up to six years, yet it has no impact on earnings.

Despite significant research on the effect of financial aid on student outcomes, the impact of losing aid due to academic requirements has not been explicitly discussed. Schudde and Scott-Clayton (2016) was the first paper to address the impact of losing Pell Grants on student outcomes, with Scott-Clayton and Schudde (2020) as its follow-up. Their results show that Pell recipients with low GPAs are more likely to leave college compared to academically similar non-Pell recipients among students in 2-year programs. Expanding on previous work, I examine the effect of losing grant eligibility on persistence and graduation among students eligible for the maximum Pell aid and those with less aid in 4-year programs. I use data from the Beginning Postsecondary Students study (BPS:12/17) which surveyed a nationally representative sample of undergraduates enrolled in 4-year institutions in 2011-2012, tracking the cohort three times over six years after college entry. Consistent with previous findings, I find that eligible students are less likely to persist in college after losing their grant compared to those eligible for adjusted amounts. More specifically, among students who lost eligibility in the second year, those below the income cutoff are 26 percentage points

with reductions in federal loans, state grants, or institutional aid, thereby offsetting the intended impact of Pell Grants (Turner, 2000, 2017; Bettinger and Williams, 2013; Park and Scott-Clayton, 2018; Eng and Matsudaira, 2021). Lastly, the variability of state and institutional aid programs may explain the inconsistent evidence regarding Pell Grants across different studies (Park and Scott-Clayton, 2018).

[4]On December 27, 2020, Congress passed the Consolidated Appropriations Act, amending the FUTURE Act and including the FAFSA Simplification Act to improve federal student aid distribution. One key change is replacing the Expected Family Contribution (EFC) with the Student Aid Index (SAI) for the 2024–25 award year.

less likely to persist into the second year than those above the cutoff. This negative effect on persistence extends to graduation; these students are 8, 10, and 12 percentage points less likely to graduate within 4, 5, and 6 years, respectively, compared to those above the income cutoff. Furthermore, students who were eligible for the maximum grant, lost their aid, and did not complete college have significantly higher SAT math scores than their counterparts with less aid, suggesting that the two groups may differ in their underlying characteristics.

Two selection issues arise due to the non-random nature of non-persistence/completion behavior. First, the RD estimates may be confounded by attrition bias, which occurs when non-persistence/completion is affected by the treatment, potentially altering the composition of the remaining observations (Lee and Lemieux, 2010). Kapelyuk (2018) explains that attrition bias could arise due to selection effects. For example, Pell recipients with higher aid may be more likely to drop out if they lose their grants, possibly because the higher aid initially induced them to enroll in college. Second, this selection results in biased estimates for certain academic outcomes, such as cumulative GPA or credits, as researchers can no longer observe the outcomes of students who did not persist and graduate from college, which is elaborated on in Section II(3).

To address potential selection bias, I derive bounds on the treatment effect at the income threshold. The observed effects from the Pell Grant could stem from three distinct groups of students: the *always-students*, those who persist in college regardless of their eligibility for full or partial grant amounts; the *induced-students*, those induced to persist by their grant; and the *never-students*, those who leave college regardless of the grant.[5] I impose the following monotonicity assumption: the potential outcomes of the never-students are weakly dominated by those of the induced-students and that the outcomes of the induced-students are weakly dominated by those of the always-students, conditional on treatment.[6] This set of assumptions is similar to the Monotone Treatment Selection (MTS) framework introduced by Manski and Pepper (2000), which assumes that expected potential outcomes move in a specific direction when individuals are treated. This bounding exercise estimates the effect of eligibility for the maximum grant aid on academic outcomes if the students who lost their grant retained it.

Building on previous literature using an RD approach, I first show that within 4-year programs, students eligible for the maximum Pell Grant attempt 19 more credits and graduate with a GPA 0.44 points higher than those eligible for less aid. Additionally, the effects on completion rates are insignificant. The bounding results suggest that

[5] This paper focuses on students who entered college in the 2011-2012 academic year. As these students are already enrolled, the impact of financial aid on enrollment is not covered in this paper.

[6] In the most extreme case, the observed positive effects in outcomes may be solely attributed to the always-students, who would have pursued college education irrespective of financial aid. In another instance, if the induced-students, potentially with lower outcomes, receive a grant but do not persist after losing eligibility, the observed positive effects among degree recipients might overestimate the impact of additional grant aid.

the previously estimated positive impact of maximum Pell aid on student outcomes may be underestimated if selection effects are not considered. In the most extreme case, eligible students are up to 4 percentage points more likely to graduate from a 4-year institution within 4 years compared to those eligible for less aid. Similarly, these students are 2 percentage points more likely to graduate within 5 and 6 years, respectively. Furthermore, these eligible students are estimated to graduate with a GPA up to 1.27 higher and attempt up to 9.38 more credits. Overall, these results suggest that the previously estimated positive impact of maximum grant eligibility may be underestimated when not accounting for selection effects. These positive effects are larger than those reported in previous studies.

Need-based financial aid is designed to attract and support low-income students, for whom the benefits of completing college outweigh the costs, especially since the completion gap is more significant than the enrollment gap (Bound et al., 2010).[7] One may argue that this academic requirement enhances aid efficiency by pushing out students whose costs of finishing college may outweigh the benefits of staying. However, evidence suggests that it also discourages students capable of completing and potentially benefiting from college. I find that, although non-Pell recipients who graduate from college have higher average SAT math scores than Pell recipients who lost their grant and did not persist, a substantial number of these Pell-eligible students have SAT math scores above the average of non-Pell completers. Thus, the question of whether academic requirements exclude students who are not capable of completing college, and thereby increase aid efficiency, should be carefully evaluated.

My results suggest important policy considerations for institutions. Institutions may benefit from implementing more targeted interventions aimed at Pell recipients who are at high risk of losing their grants but demonstrate potential for completing college, such as based on their SAT math scores. As highlighted by Scott-Clayton and Schudde (2020), students are often unaware of SAP requirements until they lose their aid. Similarly, Baum and Scott-Clayton (2013) argue that the Pell program needs more tailored support services, beyond increasing grant amounts. Institutions may need to provide timely guidance or notifications of SAP failure to students at risk of losing their grants. The sooner students realize they may lose their grant aid, the easier it will be for them to take action to maintain their eligibility, making the SAP policy informative rather than restrictive.

This paper contributes to the extensive literature on the effects of educational subsidies on student outcomes. Specifically, it adds to the literature on the consequences of linking academic requirements to need-based financial aid, a topic that has received little attention despite its significance (Scott-Clayton and Schudde, 2020). Bettinger (2004) highlights that students eligible for aid —often those who enter college but

[7] On average, students from high-income families are six times more likely to complete a bachelor's degree compared to their low-income counterparts (Bailey and Dynarski, 2011; Goldrick-Rab et al., 2016).

leave without a degree —are a key demographic for educational attainment initiatives aimed at improving educational attainment. Furthermore, Bettinger (2004) argues that policymakers have traditionally focused more on increasing college enrollment, often overlooking college completion rates. However, as the gap in completion rates persists while enrollment continues to rise, both policymakers and institutions may need to shift their focus toward promoting student persistence and degree completion.

The remainder of the paper is organized as follows: Section II describes the Pell Grant program design and data. Section III outlines the empirical methods. Section IV presents the results, and Section V concludes.

II. The Pell Grant Program Design and Data

1. The Pell Grant Program

Students must file a Free Application for Federal Student Aid (FAFSA) before the beginning of the academic year to be eligible for a Pell Grant. In order to determine a student's eligibility, the Central Process System calculates the Expected Family Contribution (EFC). The EFC is calculated based on information provided in the FAFSA, primarily considering the family's income and their financial capacity for college expenses.[8] Pell Grant eligibility is then determined by the positive difference between the annual maximum Pell Grant amount set by Congress and the EFC. Students are classified into three groups: dependent, independent with dependents, and independent without dependents.[9] The first two groups can qualify for an automatic zero (AZ) EFC if their (or their family's) income falls below the eligibility threshold, whereas the last group of students is subject to a different threshold. Due to the small sample size of independent students without dependents, this study primarily focuses on financially dependent students and independent students with dependents.

The identification leverages a discontinuity in the federal formula to calculate Pell Grant aid. Students with a \$0 EFC receive the maximum Pell Grant amount, while those with family incomes above the AZ income threshold have their Pell Grant amounts adjusted according to their EFC. Students whose income exceeds the AZ threshold may still qualify for a \$0 EFC under certain conditions, such as if someone in their household received means-tested benefits two to three years prior to college entry. Additionally, students must file the FAFSA each year to maintain their federal

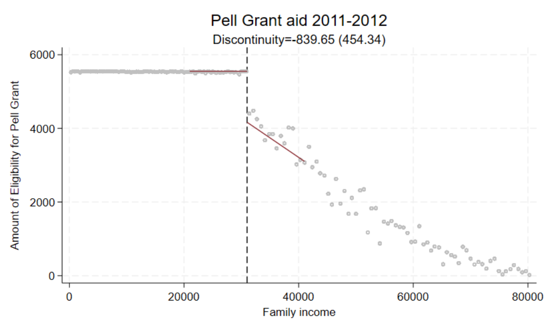
[8] The student's EFC is calculated by the Department of Education based on the family's taxed and untaxed income, assets, benefits, and other factors. It includes parents' contributions as well as the student's contributions from income and assets (U.S. Department of Education, 2022a).

[9] An individual can qualify as an independent student on the FAFSA if they are either 24 years of age or older, married or divorced, have been on active duty in the U.S. Armed Forces, provide primary support for dependents living with them, are classified as an orphan (with both parents deceased), or are enrolling in a graduate program (U.S. Department of Education, 2022b).

financial aid. Failing to file the FAFSA or missing a federal deadline results in zero Pell Grant aid, and late submission can also reduce the amount of aid received.

Although the Pell Grant program is an entitlement —meaning eligible students receive the grant regardless of when they submit the FAFSA, provided they do not miss a federal deadline —many state and institutional aid programs operate on a first-come, first-served basis (McKinney and Novak, 2015). As a result, students who file their FAFSA later (meaning they miss school and state deadlines but not the federal deadline) may receive less aid than students with equal financial need. McKinney and Novak (2015) find that those who did not file FAFSA or filed FAFSA late were systematically different from those who filed it or filed it early. For instance, students who file the FAFSA late or do not file at all are more likely to enroll as part-time students, less likely to complete their majors, and tend to have lower SAT scores. Although these findings are limited to FAFSA filing in the first year of college, they suggest that failing to account for non-filing or delayed filing could introduce confounding effects into the analysis. Unfortunately, the BPS data do not provide information on federal benefits received in 2008 or 2009, nor do they track FAFSA filings after the first year. Consequently, this paper focuses on the discontinuity in Pell Grant eligibility based on income in the first year of college, as continued eligibility in subsequent years is not observable in this dataset.

Figure 1: Discontinuity in Pell Grant Eligibility at the Automatic-Zero (AZ) Threshold in 2011-2012



Notes: This figure displays the Pell Grant schedule for first-time undergraduate students who entered college in the 2011-2012 academic year and were either dependent or independent students with dependents. A dashed line represents the automatic-zero (AZ) income threshold, set at \$31,000 for the 2011-2012 academic year. On average, students with an EFC just below the AZ threshold receive \$840 more than those just above it. Pell grant amounts are in 2012 dollars.
Source: 2012/2017 Beginning Postsecondary Students Longitudinal Study

Figure 1 illustrates the difference in Pell Grant eligibility at the AZ income threshold. In 2011-2012, the threshold was \$31,000. Students whose family income fell below \$31,000 met one criterion to receive a \$0 EFC. Those with a \$0 EFC qualify for the maximum grant aid, which was set at \$5,500 for the 2011-2012 academic year. However, the Pell Grant eligible amount may not equal the realized grant amounts received by students, as the final amount considers other factors beyond the EFC, such as the cost of attendance and enrollment intensity. For example, students eligible for \$5,550 will receive only \$2,780 if they enroll as part-time students. Appendix Figure B1 shows the discontinuity in actual grant amounts received by students from the first to the fourth year of college. While measuring the effects of discontinuity in actual grant amounts is important, estimating the discontinuity in eligible grant amounts is also crucial, as policymakers can only control the eligible amount (further explained in Section II(2)).

2. Data

The Beginning Postsecondary Students (BPS) cohorts were drawn from the National Postsecondary Student Aid Study (NPSAS), which provides a nationally representative sample of undergraduate students enrolled in post-secondary institutions participating in Title IV federal financial aid programs. The BPS study collected data on the types and amounts of federal financial aid from the U.S. Department of Education Central Processing System (CPS) and the National Student Loan Data System (NSLDS). The study compiled federal financial aid records for the entire undergraduate period, enabling detailed observations of the relationship between financial aid and student outcomes for each academic year. This study focuses on the cohort who entered college in 2011-2012 (hereafter BPS:12/17) whose progress was tracked over the first, third, and sixth years after college entry.[10] Students missing the necessary information to determine their type of financial aid were excluded, reducing the sample size from 10,360 to 8,920 for four-year degrees.

Table 1 presents descriptive statistics for the analyzed sample of first-time students in four-year degree programs. The sample in column (1) is restricted to first-time undergraduate students who enrolled in a 4-year institution during the 2011-2012 academic year. The sample in columns (2) and (3) is narrowed down to those who qualify for AZ EFC and whose income falls within \$10,000 below or above the AZ income threshold. The sample in columns (4) and (5) is limited to students who have not completed their degrees 6 years after entry.

Students eligible for the maximum grant aid come from lower-income backgrounds, measured by adjusted gross income (AGI), and are less likely to have parents who hold at least a bachelor's degree compared to those eligible for less aid. Students below and above the income threshold are equally likely to enroll as full-time students in their first year. However, fewer students maintained full-time enrollment in subsequent years, which partially explains the reduction in Pell Grant amounts over time as the grants are determined by students' enrollment intensity (Park and Scott-Clayton, 2018). Students below the cutoff received about \$4,561 in Pell Grants for 2011-2012, covering roughly 36

[10] To avoid unacceptably high rates of misclassification, the BPS administrations employ "excessive oversampling" for the NPSAS study. The first follow-up BPS cohort has a 68 percent response rate, and the second follow-up BPS cohort has a 67 percent response rate. While it is not feasible to analyze the characteristics of non-respondents, BPS sampling weights are used to account for non-response and ensure the analysis is representative (Bryan et al., Washington, DC: U.S. Government Printing Office.).

percent of their tuition, while those above the cutoff received around \$2,731, covering about 20 percent of their tuition.

Students below the income cutoff took out more loans in their first, second, and third years, and they received more state or institutional grants. Figure B2 shows a discontinuity in total federal loans (including both subsidized and unsubsidized Stafford loans) from the first to the third year.[11] In the first and second years, students below the cutoff borrowed \$877 and \$762 more than those above the cutoff; however, these gaps were not significant. In the third year, students below the cutoff borrowed significantly more (\$1,121) compared to those above the cutoff. Additionally, students above the cutoff received \$2,480 more in total institutional grants. All analyses include all types of grants in the model to account for these differences.

III. Empirical Methods

1. Identification

I use a regression discontinuity (RD) approach, which exploits the discontinuity in Pell-eligible amounts generated by the AZ policy to estimate the effect of Pell Grant eligibility on student outcomes. The identification of the RD parameter relies on the assumption that the conditional expectations of potential outcomes are continuous at the threshold. Intuitively, that is, students just below and just above the income threshold should not differ significantly in their characteristics. This reasoning allows one to attribute any observed discontinuous jump in the outcome at the threshold to the causal effect of the Pell Grant (Lee and Lemieux, 2010). If students on either side of the cutoff have similar distributions of all predetermined characteristics, they are assumed to be similar in their underlying characteristics. Then, there should be no reason—other than the Pell eligibility amount—for a discontinuous change in student outcomes at the cutoff. This discontinuity yields the following regression discontinuity (RD) estimand:

$$\Gamma(c) = \lim_{\epsilon \uparrow 0}[Y_i | X_i = c + \epsilon] - \lim_{\epsilon \downarrow 0}[Y_i | X_i = c + \epsilon] \tag{1}$$

where Y_i denotes the observed outcome of student i, X_i is the family income, measured by AGI, and c is the AZ income threshold. The parameter of interest, $\Gamma(c)$, estimates the causal effect of eligibility for the maximum grant aid on student outcomes, such as completion rates, cumulative GPA, and total credits attempted. Under the continuity assumption, which is further discussed in the next section, $\Gamma(c)$ would equal $E[Y(1) -$

[11]The Federal Family Education Loan Program (FFELP) previously offered Federal Stafford Loans (subsidized and unsubsidized); however, the FFEL Program ended in July 2010, and all federal loans are now issued through the Federal Direct Loan Program (FDLP), replacing Stafford Loans with Direct Subsidized Loans.

Table 1: Summary Statistics of the Data

	All	Enrolled in 2011-12		Non-completers	
		Below cutoff -\$10,000	Above cutoff +\$10,000	Below cutoff -\$10,000	Above cutoff +\$10,000
	(1)	(2)	(3)	(4)	(5)
Panel A. Demographics					
Female	.57	.62	.56	.72	.49
Age	18.99	19.33	19.03	20.34	20.44
White	.75	.55	.64	.61	.56
Parental education					
father: bachelor's	.44	.18	.28	.04	.14
mother: bachelor's	.43	.21	.24	.09	.14
Full-time					
2011-2012	.82 (6,549)	.82 (444)	.87 (433)	.76 (134)	.84 (124)
2012-2013	.70 (4,981)	.71 (333)	.78 (345)		
2013-2014	.64 (4,222)	.64 (262)	.67 (269)		
2014-2015	.66 (4,350)	.62 (267)	.68 (267)		
2015-2016	.48 (1,740)	.43 (127)	.49 (118)		
2016-2017	.36 (793)	.43 (81)	.28 (44)		
Time to degree	4.42	4.62	4.54		
Completion	.51	.32	.42		
Grade Average Point					
first-year	2.88	2.73	2.54	2.31	1.40
second-year	2.97	2.67	2.84	2.11	2.12
cumulative	2.96	2.75	2.71		
SAT					
math	540	502	527	481	526
verbal	540	495	519	486	501
Panel B. Labor Market Outcomes					
Earnings	36,442	30,797	33,935	27,619	30,623
Employment status	.64	.59	.54	.53	.47
Panel C. Financial aid					
Adjusted Gross Income (AGI)	86,303	25,838	36,065	25,129	35,095
Tuition (2011-2012)	14,896	12,793	13,957	9,943	9,470
Stafford loans					
2011-2012	3,156	3,212	3,474	4,706	4,564
2012-2013	2,888	3,688	3,214	1,946	2,490
2013-2014	2,868	3,711	2,856	259	1,154
State/Institutional grants					
2011-2012	5,160	6,576	6,077	4,553	2,972
Work-study					
2011-2012	304	690	385	200	231
Pell Grant received					
2011-2012	1,468	4,561	2,731	3,825	2,817
2012-2013	1,290	3,963	2,513	3,322	2,366
2013-2014	1,192	3,379	2,446	872	2,284
2014-2015	1,168	3,171	2,258	603	719
2015-2016	1,087	1,986	1,368	566	635
N (unweighted)	8,920	550	500	180	150

Notes: Column (1) includes first-time undergraduates who enrolled in a 4-year institution in the 2011-2012 academic year. Columns (2) and (3) narrow down to those qualifying for auto-zero EFC with incomes within \$10,000 of the \$31,000 threshold. Columns (4) and (5) are limited to those who have not completed their degrees. Full-time status, first- and second-year GPAs are conditional on enrollment status, with the number of students enrolled each academic year shown in parentheses. Tuition refers to tuition and fees at the NPSAS institution for students who attended only one institution in 2011-2012. Students who attended more than one institution during this period were excluded from this variable. Loans measure the amount of direct subsidized and unsubsidized Stafford loans received by students during the 2011-2012 academic year. State and institutional grants measure the total amount of state and institutional grants received by students in 2011-2012. The variable cumulative GPA is conditional on degree completion. All dollar amounts are adjusted to 2017 values. The sample size is rounded to the nearest 10.
Source: 2012/2017 Beginning Postsecondary Students Longitudinal Study

$Y(0)|X = c]$, which is the average treatment effect at the cutoff c, where $Y(1)$ is the potential treated outcome and $Y(0)$ is the potential untreated outcome.

The model estimates the effect of eligibility for the maximum Pell Grant amounts, rather than the actual awards on student outcomes (i.e., Intent-to-Treat (ITT) effects). This is for two reasons: first, enrollment decisions can be influenced by the award amount; second, policymakers can regulate only aid eligibility, rather than the actual aid amounts students receive. While examining the effects of actual award amounts on student outcomes is important, researchers often look into the ITT to evaluate the effect of an aid policy. I estimate local linear regression with a triangle kernel and the coverage error (CE)-optimal bandwidth of $10,000 for each outcome variable. For constructing the confidence intervals, I also use CE-optimal bandwidths.

2. Validity of the identification assumptions

As with other RD design studies, two potential concerns regarding the identification assumption arise. First, estimates may be biased if students underreport their EFC to receive additional Pell Grant awards (the direction of bias is unclear). Misreporting or manipulating family income is possible; however, this poses a minimal threat to identification for two reasons: first, students are generally unaware of the AZ threshold (Eng and Matsudaira, 2021), and second, over half of Pell Grant-eligible students are subject to FAFSA verification, where income is one of the primary factors reviewed (Denning et al., 2019). Nevertheless, to address this concern, I conducted a density test introduced by McCrary (2008), which provides a test for detecting manipulation of EFC. Figure B6 shows the results of the density test using samples within ±$10,000 of the AZ eligibility cutoff. Any spikes in the density of observations would indicate endogenous sorting. The results show that the density of observations is smooth around the threshold, suggesting that students do not manipulate their EFC to receive additional grants. More specifically, the test yields a discontinuity estimate of -0.11, with a standard error of 0.14. Additional test results for observable predetermined characteristics can be found in Figures B4 and B5.

Another concern involves eligibility-induced enrollment. Marginal students—those who might not have enrolled in college without financial aid—could be systematically different from those who would enroll regardless of financial aid. The BPS study focuses on undergraduate students who have already filed FAFSA and enrolled in college, meaning the dataset does not include students who filed FAFSA but did not enroll; thus, assessing the responsiveness of student enrollment to AZ policies is not directly testable. Although the key identification assumption cannot be tested directly, I employ the following ordinary least squares (OLS) to examine whether students below and above the income threshold respond differently to the loss of a grant due to SAP failure:

$$Y_{y,i} = \beta_0 + \beta_1 maxPell_i + \beta_2 FailSAP_{y,i} + \beta_3 maxPell_i * FailSAP_{y,i} + Z_i\delta + \epsilon_i \quad (2)$$

where $Y_{y,i}$ denotes student persistence and graduation rates for a specific academic year, y, $maxPell_i$ is a binary variable indicating eligibility for the maximum Pell Grant amount, $FailSAP_{y,i}$ indicates whether a student fails to meet GPA requirements in the academic year, y, and Z_i is a vector of student characteristics. To examine the immediate effect of losing Pell on persistence, I use the cumulative GPA from the first and second years, as well as students' persistence rates in the second and third years. The differences in the effect of losing Pell Grant eligibility between students just below and above the income threshold are captured by $\beta_1 + \beta_3$.

Table 2: The Average Effect of Losing Pell on Persistence Rates Among Pell Grant Recipients

	Persistence			Graduation		
	Second	Third	Fourth	4 years	5 years	6 years
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Second year						
maxPell_{y_1}	-.07***	-.05**	.01	-.08***	-.10***	-.12***
	(.02)	(.03)	(.03)	(.03)	(.03)	(.03)
Fail SAP_{y_1}	-.22***	-.35***	-.30***	-.18***	-.37***	-.41***
	(.04)	(.04)	(.06)	(.05)	(.06)	(.06)
maxPell_{y_1} * fail SAP_{y_1}	-.19***	-.09	-.01	.02	.06	.06
	(.05)	(.06)	(.08)	(.07)	(.08)	(.08)
P-value on joint F-test	.00	.01	.00	.03	.01	.00
N (unweighted)	1,040[a]	1,040[a]	1,040[a]	1,040[a]	1,040[a]	1,040[a]
Panel B. Second and third years						
maxPell_{y_1}		.03	.06	-.05*	-.05	-.05
		(.02)	(.04)	(.03)	(.03)	(.03)
Fail $\text{SAP}_{y_{1,2}}$		.04	.13	-.22**	-.40**	-.42***
		(.07)	(.12)	(.11)	(.11)	(.11)
maxPell_{y_1} * fail $\text{SAP}_{y_{1,2}}$		-.12	-.35*	.11	.10	.06
		(.12)	(.14)	(.19)	(.18)	(.17)
P-value on joint F-test		.23	.54	.21	.29	.29
N (unweighted)		910[b]	910[b]	910[b]	910[b]	910[b]

Notes: This table shows the immediate effect of SAP failure in the second year (2012-2013) and third year (2013-2014) on persistence and completion rates for students both below and above the AZ threshold. Panel A presents the effect of SAP failure in the second year on persistence and completion rates, while Panel B shows the impact in both the second and third years. To determine whether students meet SAP requirements for each year, first-year and second-year cumulative GPAs are used, which both are retrieved from transcripts. All models control for age, gender, enrollment status, tuition and fees in 2011-2012, total institutional aid in 2011-2012 (including institutional grants), total Stafford loans borrowed, parental receipt of federal benefits in 2011-2012, and parental education level. The results of an F-test of the 'maxPell' and 'fail SAP' coefficients are shown. All analyses use BPS sampling weights. The sample size is rounded to the nearest 10.
* significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level
Source: 2012/2017 Beginning Postsecondary Students Longitudinal Study

[a]Students who transferred to the 2-year institution are included.

[b]Students who transferred to the 2-year institution are included. Those who are not enrolled in their second year are excluded.

The results in Table 2 suggest that students below the income threshold are less likely to persist in the year they lose eligibility compared to those above the threshold on average.[12] Panel A examines the average effect of second-year SAP failure and Panel B shows the average effect of second- and third-year SAP failure, all on persistence and graduation rates.[13] While SAP failure discourages overall student persistence, this effect is more pronounced for students below the income threshold. Specifically, among students who lost their grant eligibility in their second year, those below the income threshold are 26 percentage points less likely to persist into their second year compared to those above the income threshold. This negative effect on persistence continues until graduation; these students are 8, 10, and 12 percentage points less likely to graduate within 4, 5, and 6 years, respectively, compared to those above the income threshold. Similarly, students below the threshold who lost their grant eligibility in the second year and did not regain it in the third year are 5 percentage points less likely to graduate within 4 years compared to those above the threshold. Overall, the results suggest that eligible students are less likely to persist in college after losing their grant compared to those with adjusted grant amounts.

I further examine how these two groups respond differently to the loss of Pell by comparing students at the GPA cutoff separately for those below and above the income cutoff in Table 3.[14] The conventional RD results indicate that among students below the cutoff, those with a first-year GPA just below 2.0 are 21 percentage points less likely

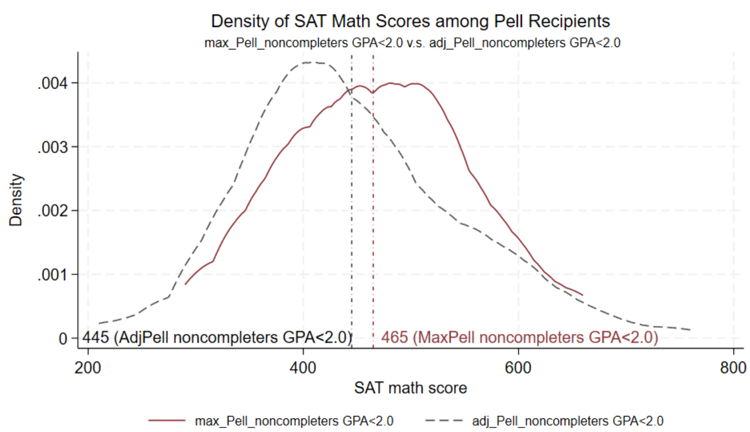
[12]Students who fail to meet the SAP GPA requirement may not lose their grants for the next academic year if they can demonstrate extenuating circumstances and successfully appeal to the school. Alternatively, they may be granted an additional semester to improve their GPA; however, failing to meet the SAP during this period will result in the loss of the grant. In the sample, among students eligible for the maximum Pell aid, 49 percent of those who lost their grant in the first year received Pell Grants in the following academic year, though the amounts were significantly lower than what they originally qualified for. This is partially because students switched from being full-time students to part-time students. Similarly, among those with adjusted amounts, 52 percent of students who failed to satisfy SAP in the first year received Pell Grants in the next academic year.

[13]Students may have transferred from a 4-year institution to a 2-year institution. The BPS studies lack detailed information on which institutions students transferred to and when these transfers occurred, providing only the overall transfer rates; thus, all samples in Table 2 may include students who transferred and persisted in a 2-year institution. The overall transfer rates to 2-year institutions were similar for students below and above the income cutoff. Specifically, 27 percent of students below the income threshold and 27 percent of students above it transferred to a 2-year institution. Among those who transferred, about 67 percent of students below the income threshold did so within the first three years of college, compared to 75 percent of students above the income threshold. The second-year retention rate, defined as the percentage of students who continue at their initial institution in their second year of college, is 59 percent among the analyzed sample.

[14]GPA may be subject to manipulation, which could invalidate the RD approach. Figure B7 shows the results of the McCrary density test on first-year GPA using the samples within $\pm\$10{,}000$ of the AZ eligibility cutoff. The test yields a discontinuity estimate of 2.52, with a standard error of 0.87. Similarly, the test on second-year GPA yields a discontinuity estimate of 0.97, with a standard error of 0.41. Although the presence of discontinuity suggests that GPA may be subject to manipulation, students have imprecise control over their GPAs. According to Lee and Lemieux (2010), when the assignment variable cannot be precisely manipulated, even if individuals have some influence over it, RD designs remain valid.

to graduate within 4 years compared to those with a GPA of 2.0 or above. For students above the income cutoff, the loss of the Pell Grant in the second year has a negligible effect. The results are consistent with the findings in Lindo et al. (2010). Using an RD, they find that students just below the minimum GPA cutoff, receiving academic probation, are significantly less likely to enroll the following year compared to students just above the GPA cutoff. Additionally, Figure 2 shows the distribution of SAT math scores for students who lost their grant eligibility and did not persist in college; this group is further decomposed into those who qualified for the maximum Pell Grant to those with less aid. The results indicate that students who were eligible for the maximum grant, lost their aid, and did not complete college have significantly higher SAT math scores than their counterparts with less aid, with the difference significant at the 10 percent level.

Figure 2: Distribution of SAT Math Scores Among MaxPell and AdjustedPell Non-Completers with a GPA Below 2.0



Notes: This figure shows the distribution of SAT math scores among students eligible for the maximum Pell Grant and those eligible for adjusted amounts, all of whom have a GPA below 2.0 and did not complete college. The results of the Kolmogorov-Smirnov test indicate that, on average, students eligible for the maximum Pell Grant who did not complete college with a GPA below 2.0 have higher SAT math scores than academically similar students eligible for less aid. This difference is statistically significant at the 10 percent level, suggesting that these two groups may differ in their underlying characteristics.
Source: 2012/2017 Beginning Postsecondary Students Longitudinal Study

Table 3: The Effect of Losing Pell on 4-year Graduation Rates Among Pell Grant Recipients at the GPA Threshold

	Fail SAP in the 2nd year		Fail SAP in the 3rd year	
	Below	Above	Below	Above
	(1)	(2)	(3)	(4)
Conventional	.21*	.04	-.11	-.02
	(.11)	(.10)	(.17)	(.07)
Robust	.39*	.01	-.09	.01
	(.22)	(.13)	(.26)	(.09)
CI	[-.04, .82]	[-.24, .26]	[-.60, .41]	[-.16, .18]

Notes: This table shows the effect of losing Pell Grant eligibility in the second and third years on 4-year completion rates. 'Below' represents students below the income threshold, while 'Above' represents students above the income threshold. Among students below the cutoff, 16 percent received a GPA below 2.0 in the first year, compared to 18 percent of students above the cutoff. In the second year, 17 percent of students below the cutoff received a GPA below 2.0, compared to 17 percent of students above the cutoff (this number is similar to findings from Schudde and Scott-Clayton (2016)). All models control for age, gender, enrollment status, tuition and fees in 2011-2012, total institutional aid in 2011-2012 (including institutional grants), total Stafford loans borrowed, parental receipt of federal benefits in 2011-2012, and parental education level. All analyses use BPS sampling weights.
* significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level
Source: 2012/2017 Beginning Postsecondary Students Longitudinal Study

Earlier studies offer several explanations for why reductions or forfeiture of need-based financial aid likely discourage student persistence and why the discouragement effect is more pronounced among students with higher levels of aid. Bettinger (2004) explains after completing the first year, students must decide whether to continue into the next year. Since the initial investment for Pell Grant recipients comes from the federal government, these students may not be as motivated to put in the same effort as those who made more personal investments. Denning (2019) shows that additional aid leads students to reduce their work efforts; thus, students who receive higher financial aid may perform worse, be more vulnerable, and be less likely to persist in college without the aid compared to those with less aid. Similarly, Rattini (2023) argues that lower-aid recipients are more driven to persist and finish college due to the higher initial cost of college compared to those eligible for higher aid.

3. Selection

In this section, I demonstrate how the RD estimand can be confounded by attrition bias, referred to hereafter as selection effects. Selection can result in different covariate distributions on both sides of the threshold, which may fail the continuity assumption on $E[Y(p^{\max})|X = x]$ where $p^{\max} \in \{0, 1\}$. To see this, let $Y_i(P^{\max})$ denote the potential outcome for student i, where $P^{\max} = 1$ if the student is eligible for the maximum Pell Grant amount and $P^{\max} = 0$ indicates otherwise. For simplicity, the subscript i is omitted in the following discussion. The potential outcome is given by:

$$Y = Y(0)(1 - P^{\max}) + Y(1)P^{\max} \tag{3}$$

The parameter of interest is the effect of being eligible for the maximum Pell Grant on student outcomes, denoted as $\Gamma(c) = E[Y(1) - Y(0)|X = c]$, where c is a known income threshold. Let $N(P, P^{\max})$ denote the potential non-persistence outcome, where P indicates whether a student retains a Pell Grant, with $P = 1$ indicating that the student retains the grant and $P = 0$ indicates otherwise. Different persistence behaviors among students who lost their grant eligibility suggest that the conditional mean of $N(0, P^{\max})$ given X may not be equivalent for these students, i.e. $E[N(0, 1)|X = c] \neq E[N(0, 0)|X = c]$, which implies $\lim_{\epsilon\uparrow 0} E[N(0, P^{\max})|X = c+\epsilon] \neq \lim_{\epsilon\downarrow 0} E[N(0, P^{\max})|X = c + \epsilon]$.

Let the potential outcomes depend on both observed covariates, X, and potential non-persistence. Assuming additive separability between observable characteristics and non-persistence, the general form of the treatment effect model can be written as:

$$Y(P^{\max}) = G_{P^{\max}}(X) - F(N(P, P^{\max})) + \epsilon \tag{4}$$

By maintaining the exogeneity assumption throughout, we have $E[Y(P^{\max})|X, N(P, P^{\max})] = G_{P^{\max}}(X) - F(N(P, P^{\max}))$. $G_{P^{\max}}(X)$ represents the return for students with characteristics X, and $F(N(P, P^{\max}))$ is a function summarizing how persistence is related to student outcomes. I assume that if Pell recipients with a GPA below 2.0 had not lost their grant eligibility, their expected value of non-persistence rates would have been continuous at the cutoff, that is, $E[N(0, P^{\max})|X = c] = E[N(0, 1 - P^{\max})|X = c]$. I further assume that if the expected value of non-persistence is the same, then, the expected relationship between non-persistence and student outcomes is also the same across these groups, that is, $E[F(N(P, P^{\max}))|X = c] = E[F(N(P, 1-P^{\max}))|X = c] = E[F(N)|X = c]$. Given this general specification, the RD estimand, $\Gamma_{RD}(c)$, can be decomposed into two terms as follows:

$$\begin{aligned}\Gamma_{RD}(c) = &\big(E[G_1(X)|X = c] - E[G_0(X)|X = c]\big)\\ &+ \big(E[F(N(0, 0))|X = c]E[N(0, 0)|X = c] + E[F(N(1, 0))|X = c]E[N(1, 0)|X = c]\big)\\ &- \big(E[F(N(0, 1))|X = c]E[N(0, 1)|X = c] + E[F(N(1, 1))|X = c]E[N(1, 1)|X = c]\big)\end{aligned} \tag{5}$$

In the absence of selection effects, the RD estimand equals the treatment effect, $E[G_1(X)|X = c] - E[G_0(X)|X = c]$. The difference in persistence behavior among students who lost their grant yields a biased estimate of the treatment effect, shown as $-E[F(N(0, 1))|X = c]E[N(0, 1)|X = c] + E[F(N(0, 0))|X = c]E[N(0, 0)|X = c]$. Thus, the RD estimates are confounded by selection effects, as shown in the following equation:

$$\hat{\Gamma}(c) = \lim_{\epsilon\uparrow 0} E[Y_i|X_i = c + \epsilon] - \lim_{\epsilon\downarrow 0} E[Y_i|X_i = c + \epsilon] + S \tag{6}$$

where $S = -E[F(N)|X = c^-, P = 0]E[P = 0|X = c^-] + E[F(N)|X = c^+, P = 0]E[P = 0|X = c^+]$ denotes the selection effect.

Another issue arises from selection in non-persistence behavior. Certain academic outcomes, such as cumulative GPA or total credits attempted, are only observable for students who graduate. For students who do not persist and complete college, these outcomes remain unobserved, and if this behavior is not random, this selection can introduce bias into the estimates. To see this, consider the following decomposition of the treatment effects:

$$\begin{aligned}
\Gamma(c) &= E[Y(1)|X = c] - E[Y(0)|X = c] \\
&= E[Y(1)|X = c, N = 1]E[N = 1|X = c] + E[Y(1)|X = c, N = 0]E[N = 0|X = c] \\
&- (E[Y(0)|X = c, N = 1]E[N = 1|X = c] + E[Y(0)|X = c, N = 0]E[N = 0|X = c])
\end{aligned} \tag{7}$$

where N denotes the observed persistence outcomes, with $N = 1$ if a student does not persist in college and $N = 0$ indicates otherwise. If a student does not persist in completing college, their outcomes become unobservable, as shown below:

$$\hat{\Gamma}(c) = E[Y|X = c, N = 0]E[N = 0|X = c] - E[Y|X = c, N = 0]E[N = 0|X = c] \tag{8}$$

Identifying the treatment effect requires knowledge of the counterfactual outcomes for students who did not persist in college due to their grant loss, had they completed college. Disentangling selection effects from treatment effects is infeasible, as the true relationship between student persistence and the outcome variable cannot be directly estimated; therefore, the treatment effect can only be bounded. I propose the following thought experiment: *What would the completion rates be if students who lost their grant had retained it?*

To carry out the above thought experiment, the unknown counterfactual outcomes for students who lost their grant, had they retained it, need to be estimated. I divide students into three groups: the *always-students*, *induced-students*, and *never-students*. The always-students are those who persist in college regardless of their eligibility for full or partial grant amounts, i.e., $P(N(0, P^{\max}) = 0, N(1, P^{\max}) = 0)$; the induced-students are those who are induced to persist in college due to their grant, $P(N(0, P^{\max}) = 1, N(1, P^{\max}) = 0)$, and the never-students are those who never finish college regardless of grant, $P(N(0, P^{\max}) = 1, N(1, P^{\max}) = 1)$. Each of these groups can be further divided into 12 subgroups based on their GPAs: substantially below 2.0, just below 2.0, just above 2.0, and substantially above 2.0. Students with a GPA of 2.0 or above are not subject to selection, whereas those with a GPA below 2.0 are.

I first estimate the probability of a student with a GPA just below 2.0 being an induced-student. The conditional probability of not persisting in college ($N = 1$), given an income below the threshold and a GPA just below 2.0, can be decomposed as follows:

$$P(N=1|X=c^-,G=2.0^-) = P(N(0,1)=1,N(1,1)=0|X=c^-,G=2.0^-) \\ + P(N(0,1)=1,N(1,1)=1|X=c^-,G=2.0^-) \quad (9)$$

Those who leave college despite having a GPA just above 2.0 are the never-students:

$$P(N=1|X=c^-,G=2.0^+) = P(N(0,1)=1,N(1,1)=1|X=c^-,G=2.0^+) \quad (10)$$

Assuming that the probability of being never-students is continuous in GPA, one can estimate $P(N(0,1)=1,N(1,1)=0|X=c^-,G=2.0^-)$ by subtracting $P(N=1|X=c^-,G=2.0^+)$ from $P(N=1|X=c^-,G=2.0^-)$. The probability of a student with a GPA significantly above 2.0 being a never-student can be estimated as $P(N(0,1)=1,N(1,1)=1|X=c^-,G>2.0^+) = P(N=1|X=c^-,G>2.0^+)$. The probability of a student with a GPA just below 2.0 being a never-student can be estimated as follows: $P(N(0,1)=1,N(1,1)=1|X=c^-,G=2.0^-) = P(N=1|X=c^-,G=2.0^+)$. Similarly, the probability of a student with a GPA just above 2.0 being a never-student can be estimated as follows: $P(N(0,1)=1,N(1,1)=1|X=c^-,G=2.0^+) = P(N=1|X=c^-,G=2.0^+)$. Lastly, the probabilities of being a never-students and an induced-students with a GPA significantly below 2.0 cannot be distinguished from one another and can only be estimated together : $P(N(0,1)=1,N(1,1)=1|X=c^-,G<2.0^-) + P(N(0,1)=1,N(1,1)=0|X=c^-,G<2.0^-) = P(N=1|X=c^-,G<2.0^-)$. The remaining groups —always-students and induced-students with a GPA above 2.0 as well as those above the income threshold —can be estimated using the same approach. A detailed explanation can be found in Appendix A.

Consider completion rates where induced-students who lost their grants take a value of 0 for their completion rates as a result of losing their grant. The bias arises from these induced-students who made non-persistence decisions based on their treatment status. Never-students will not persist in college regardless of whether they receive a grant; thus, their potential completion rates are 0, whether treated or untreated. To identify the treatment effect, the counterfactual outcomes for induced-students who lost their grants need to be known. To provide bounds on these outcomes, I impose the following monotonicity assumption: the potential outcomes for the always-students are at least as good as those for the induced-students, and the outcomes for the induced-students are at least as good as those for the never-students, conditional on treatment. This assumption is similar to the Monotone Treatment Selection (MTS) assumption introduced by Manski and Pepper (2000), which posits that expected potential outcomes move in a specific direction when comparing individuals in the treatment and control groups. In other words, the potential outcomes for the induced-students are weakly stochastically dominated by those for the always-students, and similarly, outcomes for the induced-students are weakly stochastically dominated by those for the never-students, i.e., $E[Y(P^{\max})|N(0,P^{\max})=0,N(1,P^{\max})=0] \geq E[Y(P^{\max})|N(0,P^{\max})=1,N(1,P^{\max})=0] \geq E[Y(P^{\max})|N(0,P^{\max})=1,N(1,P^{\max})=1]$ where $P^{\max}=\{0,1\}$.

Under the MTS assumption, the observed outcomes of the always-students with GPAs below 2.0 can serve as an upper bound for the counterfactual outcomes of the induced-students with GPAs below 2.0. By imposing the MTS assumption, the upper bound of $E[Y(1)|X = c^-]$ can be obtained as:

$$
\begin{aligned}
&E_{\max}[Y(1)|X = c^-] \\
&\quad = \Big(E[Y|N = 0, X = c^-, G < 2.0^-](P(N = 0|X = c^-, G < 2.0^-) \\
&\quad + P(N = 1|X = c^-, G < 2.0^-))\Big)P(G < 2.0^-|X = c^-) \\
&\quad + \Big(E[Y|N = 0, X = c^-, G = 2.0^-]\big(P(N = 0|X = c^-, G = 2.0^-) \\
&\quad + P(N = 1|X = c^-, G = 2.0^-) - P(N = 1|X = c^-, G = 2.0^+))\Big)P(G = 2.0^-|X = c^-) \\
&\quad + E[Y|N = 0, X = c^-, G = 2.0^+]P(N = 0|X = c^-, G = 2.0^+)P(G = 2.0^+|X = c^-) \\
&\quad + E[Y|N = 0, X = c^-, G > 2.0^+]P(N = 0|X = c^-, G > 2.0^+)P(G > 2.0^+|X = c^-)
\end{aligned}
\tag{11}
$$

The MTS assumption does not provide lower bounds, thus, the lower bounds for counterfactual completion rates for induced-students who lost their grants need to be conjectured. Completion rates range between 0 and 1, and induced students who lost their grants taking a value of 0 introduces bias because, under the assumption, their non-persistence rate is determined by their treatment status. Thus, the worst-case bound makes an additional assumption: the counterfactual completion rates for these students are similar to those at the 10th percentile of the college completion distribution for observed students with similar GPA and income levels. Finally, the lower bound of $E[Y(1)|X = c^-]$ can be obtained as:

$$
\begin{aligned}
&E_{\min}[Y(1)|X = c^-] \\
&\quad = \big(E[Y|N = 0, X = c^-, G < 2.0^-]P(N = 0|X = c^-, G < 2.0^-)P(G < 2.0^-|X = c^-) \\
&\quad + \Big(E[Y|N = 0, X = c^-, G = 2.0^-]P(N = 0|X = c^-, G = 2.0^-) \\
&\quad + \tau_{Y_{10}|N=0,X=c^-,G=2.0^-}(P(N = 1|X = c^-, G = 2.0^-) \\
&\quad - P(N = 1|X = c^-, G = 2.0^+))\Big)P(G = 2.0^-|X = c^-) \\
&\quad + E[Y|N = 0, X = c^-, G = 2.0^+]P(N = 0|X = c^-, G = 2.0^+)P(G = 2.0^+|X = c^-) \\
&\quad + E[Y|N = 0, X = c^-, G > 2.0^+]P(N = 0|X = c^-, G > 2.0^+)P(G > 2.0^+|X = c^-)
\end{aligned}
\tag{12}
$$

where $\tau_{Y_{10}|N=0,X=c^-,G}$ represents the 10th percentile of the college completion distribution for students with similar GPA and income levels who complete college, and is used to calculate the counterfactual outcomes. For other academic outcomes, such as GPA, the worst value of GPA takes a value of 2.0, which is a graduation requirement for most institutions in the U.S. Similarly, the upper and the lower bound of $E[Y(0)|X = c^+]$ can be obtained as follows in a similar manner.

To obtain the upper bound of the causal effect, $\Gamma(c) = E[Y(1) - Y(0)|X = c]$, one can subtract the lower bound on $E[Y(0)|X = c^+]$ from the upper bound on $E[Y(1)|X = c^-]$. To obtain the lower bound, subtract the upper bound on $E[Y(0)|X = c^+]$ from the lower bound on $E[Y(1)|X = c^-]$:

$$\Gamma^{\mathrm{UB}}(c) = E_{\max}[Y(1)|X = c^-] - E_{\min}[Y(0)|X = c^+] \tag{13a}$$

$$\Gamma^{\mathrm{LB}}(c) = E_{\min}[Y(1)|X = c^-] - E_{\max}[Y(0)|X = c^+] \tag{13b}$$

IV. Results

1. The Effects on Degree Completion and Academic Outcomes

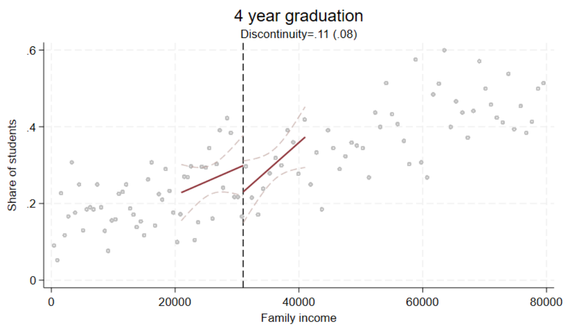
1..1 Naive RD Estimates

Figures 3 and 4 (see also Tables 4 and 5) present conventional and robust RD estimates on academic outcomes, along with robust bias-corrected (RBC) confidence intervals. The naive RD results show that students eligible for the maximum grant aid in their first year graduate with a GPA 0.44 points higher and attempt 18.80 more credits compared to those with adjusted amounts. These students also attempt more credits and achieve higher grades in their first year. However, no significant effect is observed on the completion rate.

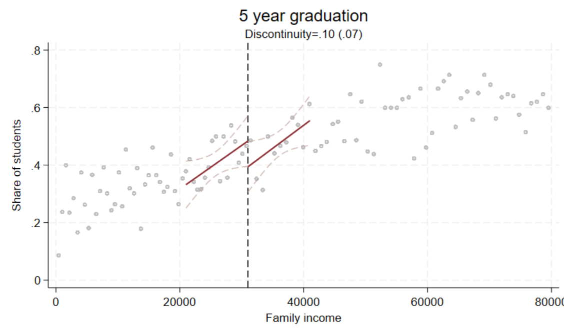
To test the robustness of the results, I repeat the RD analyses using different bandwidths. Table B1 shows the effects of AZ eligibility on the outcomes of interest using various bandwidths. Columns (1) and (2) use bandwidths that are \$1,000 and \$2,000 narrower, while Columns (4) and (5) apply bandwidths that are \$1,000 and \$2,000 wider. Column (3) reports results using the optimal bandwidth. The estimates remain relatively stable, suggesting that the results are robust to changes in bandwidth length. In Column (6), I test whether the results are sensitive to the functional form of the relationship between the outcomes of interest and the EFC by adding a quadratic term of the EFC and its interaction with the quadratic term. The polynomial specification is not significant, indicating that the relationship between the outcomes and the EFC is locally linear.

Figure 3: The Effect of AZ Eligibility on Completion Rates

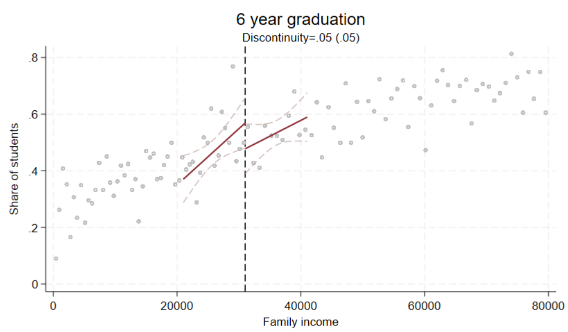
(a) 4-year graduation



(b) 5-year graduation



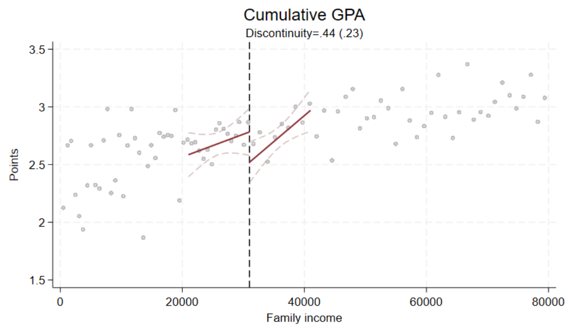
(c) 6-year graduation



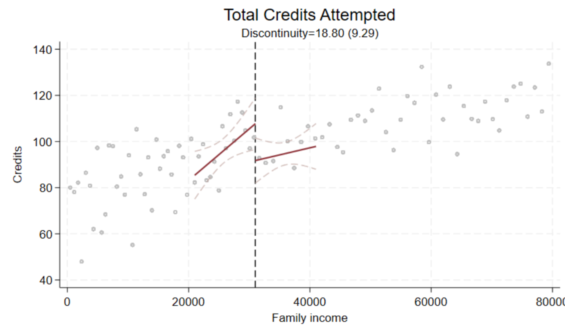
Notes: Each figure shows bias-corrected RD estimates from a local linear regression of each outcome variable on AGI, noted at the top of each figure, with the standard error shown in parentheses. Solid red lines indicate estimates from local linear regression, and light rose dotted lines represent the robust biased-corrected (RBC) confidence intervals.
Source: 2012/2017 Beginning Postsecondary Students Longitudinal Study

Figure 4: The Effect of AZ Eligibility on Academic Outcomes

(a) Cumulative GPA



(b) Total credits



Notes: Each figure shows bias-corrected RD estimates from a local linear regression of each outcome variable on AGI, noted at the top of each figure, with the standard error shown in parentheses. Solid red lines indicate estimates from local linear regression, and light rose dotted lines represent the robust biased-corrected (RBC) confidence intervals.

Source: 2012/2017 Beginning Postsecondary Students Longitudinal Study

Table 4: The Effect of AZ Eligibility on Completion Rates

	Completion		
	4 years (1)	5 years (2)	6 years (3)
AZ eligibility			
Conventional	.11	.10	.05
	(.08)	(.07)	(.05)
Robust	.11	.11	.05
	(.09)	(.08)	(.06)
CI	[-.06, .28]	[-.26, .05]	[-.07, .17]

Notes: The table shows conventional and bias-corrected RD estimates of the effect of AZ eligibility on 4-, 5-, and 6-year completion rates, along with robust bias-corrected (RBC) confidence intervals. All models control for age, gender, enrollment status, tuition and fees in 2011-2012, total institutional aid in 2011-2012 (including institutional grants), total Stafford loans borrowed, parental receipt of federal benefits in 2011-2012, and parental education level. All analyses use BPS sampling weights.
Source: 2012/2017 Beginning Postsecondary Students Longitudinal Study

Table 5: The Effect of AZ Eligibility on Academic Outcomes

	Grade Point Average			Credits	
	First (1)	Second (2)	Cumulative (3)	First (4)	Cumulative (5)
AZ eligibility					
Conventional	.76***	.21	.44*	22.05**	18.80**
	(.28)	(.21)	(.23)	(11.08)	(9.29)
Robust	.79***	.24	.45*	23.46**	20.51**
	(.30)	(.22)	(.25)	(11.74)	(9.82)
CI	[.21, 1.37]	[-.20, .68]	[-.03, .93]	[.45, 46.47]	[-1.25, 39.76]

Notes: The table shows conventional and bias-corrected RD estimates of the effect of AZ eligibility on GPA and total credits attempted, along with robust bias-corrected (RBC) confidence intervals. All models control for age, gender, enrollment status, tuition and fees in 2011-2012, total institutional aid in 2011-2012 (including institutional grants), total Stafford loans borrowed, parental receipt of federal benefits in 2011-2012, and parental education level. All analyses use BPS sampling weights. 'First' GPA refers to a student's cumulative GPA during their first year of enrollment (2011-2012). 'Second' GPA refers to a student's cumulative GPA during their second year of enrollment (2012-2013). 'Cumulative' GPA refers to a student's cumulative GPA at the last known institution they attended. 'Total credits' refers to the total amount of credits attempted.
Source: 2012/2017 Beginning Postsecondary Students Longitudinal Study

While the naive RD found insignificant effects of Pell grant on completion rates, bounding results suggest that previous estimates may be underestimated if selection effects are not considered. Table 6 shows the conventional RD estimates, adjusted for selection effects, along with corresponding 95 percent confidence intervals, obtained using the percentile method.[15]

Table 6: The Boundings on the Effect of AZ Eligibility on Student Outcomes

	Completion			Grade Point Average	Credits
	4 years (1)	5 years (2)	6 years (3)	(4)	(5)
Bounds	[.003, .035]	[-.018, .023]	[.013, .015]	[-1.16, 1.27]	[-4.39, 9.38]
CI	[-.011, .041]	[-.020, .032]	[.009, .021]	[-1.26, 1.34]	[-8.94, 11.04]

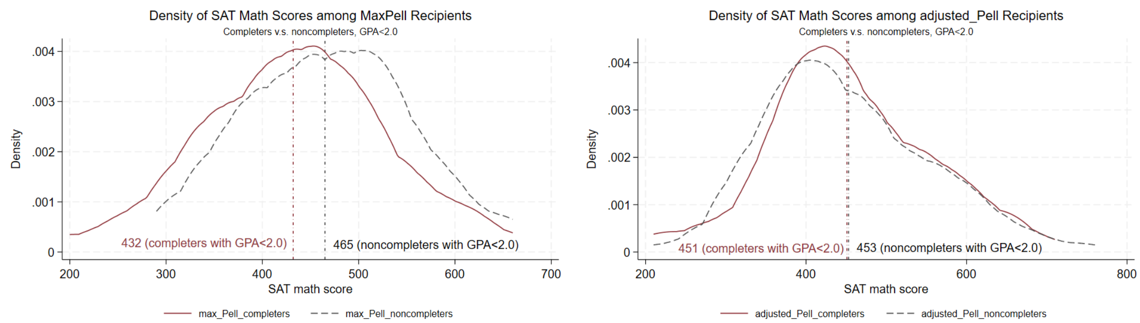
Notes: The table presents conventional RD estimates, adjusted for selection effects. The first row presents the range of the effect of eligibility for the maximum Pell Grant on 4-, 5-, and 6-year graduation rates, GPA, and total credits attempted, with confidence intervals shown in the second row. For instance, in column (1), the effect on 4-year graduation rates ranges from 0.003 to 0.035.
Source: 2012/2017 Beginning Postsecondary Students Longitudinal Study

Assuming that students who did not complete college due to losing their grants would have completed college and performed as well as those who did complete despite losing their grants, with similar income and GPA, the bounding results show that students eligible for the maximum Pell Grant are up to 4 percentage points more

[15] Students with GPAs just below and just above the threshold include those with GPAs between 1.6-2.0 and 2.0-2.4, respectively. This bandwidth is based on the results from Table 3.

Figure 5: Distribution of SAT Math Scores Between Completers and Non-Completers with a GPA Below 2.0

(a) Density SAT Math MaxPell (b) Density SAT Math AdjustedPell



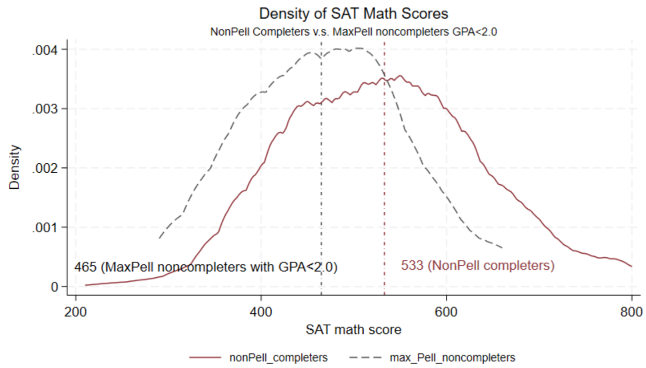
Notes: These figures display the density of SAT math scores for completers and non-completers, separated by eligibility for the maximum Pell Grant and those with less aid, among students who failed to meet Satisfactory Academic Progress (SAP) requirements. The Kolmogorov-Smirnov test results indicate that, among students below the income threshold, those who did not persist and complete college had, on average, higher SAT math scores than those who completed college, with this difference being significant at the 5 percent level. In contrast, no significant difference in SAT math scores was observed among students receiving less aid. These findings suggest that the SAP policy may be pushing out students who are capable of completing college.

Source: 2012/2017 Beginning Postsecondary Students Longitudinal Study

likely to complete a 4-year degree within 4 years compared to those eligible for less aid. Similarly, students eligible for the maximum Pell Grant are up to 2 percentage points more likely to complete a degree within 5 and 6 years, respectively. Under the same assumption, eligible students are estimated to graduate with a GPA up to 1.27 higher and attempted up to 9.38 credits more than those eligible for less aid. Overall, these results suggest that the previously estimated positive impact of maximum grant eligibility may be underestimated when not accounting for selection effects caused by the loss of Pell.

One could argue that linking academic requirements to need-based financial aid eligibility may improve aid efficiency by filtering out students whose costs of finishing college may outweigh the benefits. However, evidence suggests that the policy also discourages students who may be capable of completing college. Figure 5 (a) shows the distribution of SAT math scores for maximum-award Pell recipients who failed to meet SAP requirements in their first year, further broken down by students who completed college to those who did not. Figure 5 (b) presents similar distributions for Pell recipients with adjusted awards who did not meet SAP criteria. The results indicate that eligible students who lost their grant and did not persist in college had, on average, higher SAT math scores than those who lost their grant but completed college. The Kolmogorov-Smirnov test results reveal that the differences between the distributions of these two groups are significant at the 5 percent level. However, no such difference was found among students receiving less aid. Furthermore, Figure 6 compares the SAT score distribution between non-Pell recipients who completed college

Figure 6: Distribution of SAT Math Scores Between Non-Pell Recipients and Max-Pell Recipients with a GPA Below 2.0



Notes: This figure displays the density of SAT math scores for completers and non-completers, separated by eligibility for the maximum Pell Grant and those with less aid, among students who failed to meet Satisfactory Academic Progress (SAP) requirements. The Kolmogorov-Smirnov test results indicate that Non-Pell recipients who completed college have higher SAT math scores, on average, with a difference significant at the 1 percent level compared to Max-Pell recipients who did not complete college and lost their grants. However, a substantial portion of the Max-Pell recipients have SAT math scores above the average of Non-Pell recipients. These findings suggest that the SAP policy may be pushing out students who are capable of completing college.
Source: 2012/2017 Beginning Postsecondary Students Longitudinal Study

and maximum-award Pell recipients, particularly those who lost their grant and did not complete college. While non-Pell recipients have a higher average SAT score, a substantial number of eligible students who lost their aid and did not complete college have SAT math scores that exceed the average of non-Pell recipients. This suggests that students who lost grant eligibility but were capable of completing college may have been pushed out due to the SAP policy. If they had continued, it could have led to larger positive effects on student outcomes than previously estimated.

V. Conclusion

Using nationally representative student-level data and a regression discontinuity (RD) design, this paper argues that previous estimates of the effects of eligibility for the maximum grant aid on student outcomes may be underestimated if the selection effects from Pell Grant loss are not considered. While naive RD estimates show no effect on graduation rates, the bounding results indicate that eligible students are up to 2, 4, and 4 percentage points more likely to graduate within 4, 5, and 6 years, respectively. Similar results are found for cumulative GPA and credits, with a GPA up to 1.27 points higher and attempting up to 9.83 more credits. I also find that a substantial portion of Pell recipients who lost their grant and did not persist in college have higher SAT math scores than the average of non-Pell recipients who graduated. These findings suggest that students capable of graduating college may be pushed out prematurely due to the loss of grant eligibility. As Schudde and Scott-Clayton

(2016) discuss, financial factors may cause students to drop out too early. Furthermore, Scott-Clayton and Schudde (2020) note that students are often unaware of Satisfactory Academic Progress (SAP) requirements until they lose their aid. These students could benefit from timely guidance on both academic and SAP progress. Providing earlier warnings to at-risk students might allow them to retain their grants, potentially leading to greater positive effects of aid on student outcomes than previously estimated.

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A Appendix: Bounding

In this section, I show how selection can lead to biased estimates. To see this, let $Y_i(P^{\max})$ denote the potential outcome for student i, where $P^{\max} = 1$ if the student is eligible for the maximum Pell Grant amount and $P^{\max} = 0$ indicates otherwise. For simplicity, the subscript i is omitted in the following discussion. The potential outcome is given by:

$$Y = Y(0)(1 - P^{\max}) + Y(1)P^{\max} \tag{14}$$

The parameter of interest is the causal effect of maximum grant aid eligibility on student outcomes, denoted as $\Gamma(c) = E[Y(1) - Y(0)|X = c]$, where c is a known income threshold. Let $N(P, P^{\max})$ denote the potential non-persistence outcome, where P indicates whether a student retains a Pell Grant, with $P = 1$ indicating that the student retains the grant and $P = 0$ indicates otherwise. Different persistence behaviors among students who lost their grant eligibility suggest that the conditional mean of N given X may not be equivalent, i.e. $\lim_{\epsilon\uparrow 0} E[N(0, P^{\max})|X = c + \epsilon] \neq \lim_{\epsilon\downarrow 0} E[N(0, P^{\max})|X = c + \epsilon]$. This implies $E[N(0, P^{\max})|X = c] \neq E[N(0, 1 - P^{\max})|X = c]$. Consider the following treatment effect model

$$E[Y(P^{\max})|X = c, N(P, P^{\max}) = n] = G_{P^{\max}}(X) - E[F(N(P, P^{\max}))|X = c] \tag{15}$$

where $F(\cdot)$ is a function summarizing how persistence is related to the student outcomes. I assume that if the probability of not persisting in college is continuous in grant amounts, the expected relationship between non-persistence and student outcomes is the same across students with different grant amounts. That is, if $P(N(P, P^{\max})|X = c) = P(N(P, 1 - P^{\max})|X = c)$ then, $E[F(N(P, P^{\max}))|X = c] = E[F(N(P, 1-P^{\max}))|X = c]$. Using Equation (15), the treatment effect, $\Gamma(c)$, is obtained as $E[G_1(X)|X = c] - E[G_0(X)|X = c]$.

The difference in persistence behavior among students who lost their grant introduces bias into the estimates for two reasons: first, the decision not to persist is related to the treatment, and second, students who do not persist below and above the threshold differ in their underlying characteristics. The RD estimates, thus, can be decomposed into the treatment effect and selection effects, as shown in the following equation:

$$\hat{\Gamma}(c) = \lim_{\epsilon\uparrow 0}[G(X)|X_i = c + \epsilon] - \lim_{\epsilon\downarrow 0}[G(X)|X_i = c + \epsilon] + S \tag{16}$$

where $S = -E[F(N)|X = c^-, P = 0]E[P = 0|X = c^-] + E[F(N)|X = c^+, P = 0]E[P = 0|X = c^+]$ shows the selection effect.

Another issue arises from selection in non-persistence behavior. Certain academic outcomes, such as cumulative GPA or total credits attempted, are only observable for students who graduate. For students who do not persist and complete college, these outcomes remain unobserved. If persistence and completion are not random, this selection can introduce bias into the estimates. To see this, consider the following decomposition of the treatment effect:

$$\begin{aligned}\Gamma(c) &= E[Y(1)|X = c] - E[Y(0)|X = c] \\ &= E[Y(1)|X = c, N = 1]P(N = 1|X = c) + E[Y(1)|X = c, N = 0]P(N = 0|X = c) \\ &- \big(E[Y(0)|X = c, N = 1]P(N = 1|X = c) + E[Y(0)|X = c, N = 0]P(N = 0|X = c)\big)\end{aligned} \tag{17}$$

The first and third terms become unobservable if a student does not persist and complete college.

Identifying the treatment effect requires knowledge of the counterfactual outcomes for students who did not persist in college due to their grant loss, had they completed college. Disentangling selection effects from treatment effects is infeasible, as the true relationship between student persistence and the outcome variable cannot be directly estimated; therefore, the treatment effect can only be bounded. I propose the following thought experiment: *What would the completion rates be if students who lost their grant had retained it?*

To carry out the above thought experiment, the unknown counterfactual outcomes for students who lost their grant, had they retained it, need to be estimated. I divide students into three groups: the *always-students*, *induced-students*, and *never-students*. The always-students are those who persist in college regardless of their eligibility for full or partial grant amounts, i.e., $P(N(0, P^{\max}) = 0, N(1, P^{\max}) = 0)$; the induced-students are those who are induced to persist in college due to their grant, $P(N(0, P^{\max}) = 1, N(1, P^{\max}) = 0)$, and the never-students are those who never finish college regardless of grant, $P(N(0, P^{\max}) = 1, N(1, P^{\max}) = 1)$. Each of these groups can be further divided into 12 subgroups based on their GPAs: substantially below 2.0, just below 2.0, just above 2.0, and substantially above 2.0. Students with a GPA of 2.0 or above are not subject to selection, whereas those with a GPA below 2.0 are.

I first estimate the probability of a student with a GPA just below 2.0 being an induced-student. The conditional probability of not persisting in college ($N = 1$), given an income below the threshold and a GPA just below 2.0, can be decomposed as follows:

$$\begin{aligned} P(N = 1|X = c^-, G = 2.0^-) &= P(N(0,1) = 1, N(1,1) = 0|X = c^-, G = 2.0^-) \\ &\quad + P(N(0,1) = 1, N(1,1) = 1|X = c^-, G = 2.0^-) \end{aligned} \tag{18}$$

Those who leave college despite having a GPA just above 2.0 are the never-students:

$$P(N = 1|X = c^-, G = 2.0^+) = P(N(0,1) = 1, N(1,1) = 1|X = c^-, G = 2.0^+) \tag{19}$$

Assuming that the probability of being never-students is continuous in GPA, one can estimate $P(N(0,1) = 1, N(1,1) = 0|X = c^-, G = 2.0^-)$ by subtracting $P(N = 1|X = c^-, G = 2.0^+)$ from $P(N = 1|X = c^-, G = 2.0^-)$. The probability of a student with a GPA significantly above 2.0 being a never-student can be estimated as $P(N(0,1) = 1, N(1,1) = 1|X = c^-, G > 2.0^+) = P(N = 1|X = c^-, G > 2.0^+)$. The probability of a student with a GPA just below 2.0 being a never-student can be estimated as follows: $P(N(0,1) = 1, N(1,1) = 1|X = c^-, G = 2.0^-) = P(N = 1|X = c^-, G = 2.0^+)$. Similarly, the probability of a student with a GPA just above 2.0 being a never-student can be estimated as follows: $P(N(0,1) = 1, N(1,1) = 1|X = c^-, G = 2.0^+) = P(N = 1|X = c^-, G = 2.0^+)$. Lastly, the probabilities of being a never-students and an induced-students with a GPA significantly below 2.0 cannot be distinguished from one another and can only be estimated together: $P(N(0,1) = 1, N(1,1) = 1|X = c^-, G < 2.0^-) + P(N(0,1) = 1, N(1,1) = 0|X = c^-, G < 2.0^-) = P(N = 1|X = c^-, G < 2.0^-)$.

I next estimate the probability of a student who persists in college and has an income below the income threshold. Assuming that the probability of being always-students is continuous in GPA, one can estimate $P(N(0,1) = 1, N(1,1) = 0|X = c^-, G = 2.0^+)$. The conditional probability of staying in college (N=0), given an income below the threshold and a GPA just above 2.0, can be decomposed as follows:

$$\begin{aligned} P(N=0|X=c^-,G=2.0^+) &= P(N(0,1)=0,N(1,1)=0|X=c^-,G=2.0^+) \\ &+ P(N(0,1)=1,N(1,1)=0|X=c^-,G=2.0^+) \end{aligned} \tag{20}$$

Those who persist in college despite having a GPA just below 2.0 are the always-students:

$$P(N=0|X=c^-,G=2.0^-) = P(N(0,1)=0,N(1,1)=0|X=c^-,G=2.0^-) \tag{21}$$

Then, the probability of a student with a GPA just above 2.0 being an induced-student, $P(N(0,1)=1,N(1,1)=0|X=c^-,G=2.0^+)$, can be estimated by subtracting $P(N=0|X=c^-,G=2.0^-)$ from $P(N=0|X=c^-,G=2.0^+)$. The probability of a student with a GPA significantly below 2.0 being an always-student can be estimated as $P(P(0,1)=0,N(1,1)=0|X=c^-,G<2.0^-)=P(N=0|X=c^-,G<2.0^-)$. Similarly, the probability of being a student with a GPA just below 2.0 can be estimated as follows: $P(N(0,1)=0,N(1,1)=0|X=c^-,G=2.0^-)=P(N=0|X=c^-,G=2.0^-)$. The probability of being a student with a GAP just above 2.0 being an always-student can be estimated as follows: $P(N(0,1)=0,N(1,1)=0|X=c^-,G=2.0^+)=P(N=0|X-c^-,G=2.0^-)$. Lastly, the probabilities of being an always-students and an induced-students with a GPA significantly above 2.0 cannot be distinguished from one another and can only be estimated together $P(N(0,1)=0,N(1,1)=0|X=c^-,G>2.0^+)+P(N(0,1)=1,N(1,1)=0|X=c^-,G>2.0^+)=P(N=0|X=c^-,G>2.0^+)$. The probability of a student being always-students, induced-students, or never-students across various GPA levels with incomes above the threshold can be similarly estimated.

Students from the induced-students and the never-students groups with a GPA substantially below 2.0 are indistinguishable; thus, let ϕ_1 represent $P(N(0,1)=1,N(1,1)=0|X=c^-,G<2.0^-)$. Similarly, students from the always-students and the induced-students groups with a GPA substantially above 2.0 are indistinguishable; thus, let ϕ_2 represent $P(N(0,1)=0,N(1,1)=0|X=c^-,G>2.0^+)$. Similarly, let ϕ_3 represent $P(N(0,0)=1,N(1,0)=0|X=c^+,G<2.0^-)$ and let ϕ_4 represent $P(N(0,0)=0,N(1,0)=0|X=c^+,G>2.0^+)$.

The expected value of potential outcomes for students with income below the threshold and a GPA substantially below 2.0 can be rewritten as follows:

$$\begin{aligned} &E[Y(1)|X=c^-,G<2.0^-] \\ &= E[Y|N=0,X=c^-,G<2.0^-]P(N=0|X=c^-,G<2.0^-) \\ &+ E[Y(1)|N(0,1)=1,N(1,1)=0,X=c^-,G<2.0^-]\phi_1 \\ &+ E[Y(1)|N(0,1)=1,N(1,1)=1,X=c^-,G<2.0^-](P(N=1|X=c^-,G<2.0^-)-\phi_1) \end{aligned} \tag{22}$$

Consider completion rates where induced-students who lost their grants take a value of 0 for their completion rates as a result of losing their grant. The bias arises from these induced-students who made non-persistence decisions based on their treatment status. Never-students will not persist in college regardless of whether they receive a grant; thus, their potential completion rates are 0, whether treated or untreated. To identify the treatment effect, the counterfactual outcomes for induced-students who lost their grants need to be known. To provide bounds on the outcomes, I impose the following monotonicity assumption: the potential outcomes for the always-students are at least as good as those for the induced-students, and the outcomes for the induced-students are at least as good as those for the never-students, conditional on treatment. This assumption is similar to the Monotone Treatment Selection (MTS) assumption introduced by Manski and Pepper (2000), which assumes that expected potential outcomes shift in a particular direction when individuals are treated. In other words, the potential outcomes for the induced-students are weakly stochastically dominated by those for the always-students, and similarly, outcomes for the induced-students are weakly stochastically dominated by those for the never-students, i.e., $E[Y(P^{\max})|N(0,P^{\max})=0,N(1,P^{\max})=0] \geq E[Y(P^{\max})|N(0,P^{\max})=1,N(1,P^{\max})=0] \geq E[Y(P^{\max})|N(0,P^{\max})=1,N(1,P^{\max})=1]$ where $P^{\max}=\{0,1\}$. Under the MTS assumption, the observed outcomes of the always-students with GPAs below 2.0 can serve as an upper bound for the counterfactual outcomes of the induced-students with GPAs below 2.0. By imposing the MTS assumption, the upper bound of $E[Y(1)|X=c^-]$ can be obtained as:

$$
\begin{aligned}
&E_{\max}[Y(1)|X=c^-] \\
&\quad= \Big(E[Y|N=0,X=c^-,G<2.0^-](P(N=0|X=c^-,G<2.0^-) \\
&\quad+ P(N=1|X=c^-,G<2.0^-))\Big)P(G<2.0^-|X=c^-) \\
&\quad+ \Big(E[Y|N=0,X=c^-,G=2.0^-]\big(P(N=0|X=c^-,G=2.0^-) \\
&\quad+ P(N=1|X=c^-,G=2.0^-)-P(N=1|X=c^-,G=2.0^+))\Big)P(G=2.0^-|X=c^-) \\
&\quad+ E[Y|N=0,X=c^-,G=2.0^+]P(N=0|X=c^-,G=2.0^+)P(G=2.0^+|X=c^-) \\
&\quad+ E[Y|N=0,X=c^-,G>2.0^+]P(N=0|X=c^-,G>2.0^+)P(G>2.0^+|X=c^-)
\end{aligned}
\tag{23}
$$

The MTS assumption does not provide lower bounds, thus, the lower bounds for counterfactual completion rates for induced-students who lost their grants need to be conjectured. Completion rates range between 0 and 1, and induced students who lost their grants taking a value of 0 introduces bias because, under the assumption, their non-persistence rate is determined by their treatment status. The worst-case bound makes an additional assumption: the counterfactual completion rates for these students are similar to those at the 10th percentile of the college completion distribution for observed students with similar GPA and income levels. Finally, the lower bound of $E[Y(1)|X=c^-]$ can be obtained as:

$$
\begin{aligned}
&E_{\min}[Y(1)|X=c^-] \\
&\quad = \big(E[Y|N=0,X=c^-,G<2.0^-]P(N=0|X=c^-,G<2.0^-)P(G<2.0^-|X=c^-) \\
&\quad + \Big(E[Y|N=0,X=c^-,G=2.0^-]P(N=0|X=c^-,G=2.0^-) \\
&\quad + \tau_{Y_{10}|N=0,X=c^-,G=2.0^-}(P(N=1|X=c^-,G=2.0^-) \\
&\quad - P(N=1|X=c^-,G=2.0^+))\Big)P(G=2.0^-|X=c^-) \\
&\quad + E[Y|N=0,X=c^-,G=2.0^+]P(N=0|X=c^-,G=2.0^+)P(G=2.0^+|X=c^-) \\
&\quad + E[Y|N=0,X=c^-,G>2.0^+]P(N=0|X=c^-,G>2.0^+)P(G>2.0^+|X=c^-)
\end{aligned} \tag{24}
$$

where $\tau_{Y_{10}|N=0,X=c^-,G}$ represents the 10th percentile of the college completion distribution for students with similar GPA and income levels who complete college, and is used to calculate the counterfactual outcomes. For other academic outcomes, such as GPA, the worst value of GPA takes a value of 2.0, which is a graduation requirement for most institutions in the U.S. Similarly, the upper and the lower bound of $E[Y(0)|X=c^+]$ can be obtained as follows in a similar manner.

To obtain the upper bound of the causal effect, $\Gamma(c)=E[Y(1)-Y(0)|X=c]$, one can subtract the lower bound on $E[Y(0)|X=c^+]$ from the upper bound on $E[Y(1)|X=c^-]$. To obtain the lower bound, subtract the upper bound on $E[Y(0)|X=c^+]$ from the lower bound on $E[Y(1)|X=c^-]$:

$$\Gamma^{\mathrm{UB}}(c)=E_{\max}[Y(1)|X=c^-]-E_{\min}[Y(0)|X=c^+] \tag{25a}$$

$$\Gamma^{\mathrm{LB}}(c)=E_{\min}[Y(1)|X=c^-]-E_{\max}[Y(0)|X=c^+] \tag{25b}$$

B Appendix: Additional Tables and Figures

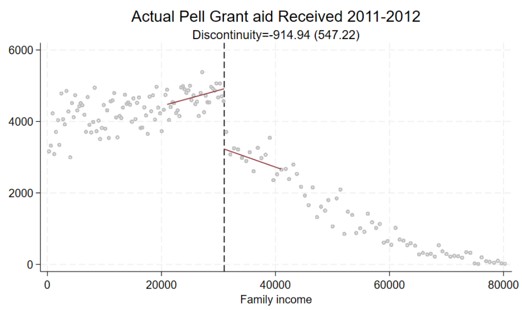
Table B1: Robustness of the Effect of AZ Eligibility on Student Outcomes

	Bandwidth widths					Functional form
	-2,000	-1,000	Optimal	+1,000	+2,000	
	(1)	(2)	(3)	(4)	(5)	(6)
4-year completion rate						
Conventional	.11	.11	.11	.10	.09	.11
	(.09)	(.09)	(.08)	(.08)	(.07)	(.09)
Robust	.01	.03	.11	.09	.10	.11
	(.14)	(.13)	(.09)	(.12)	(.11)	(.09)
CI	[-.28, .26]	[-.23, .28]	[-.06, .28]	[-.14, .31]	[-.12, .31]	[-.07, .29]
5-year completion rate						
Conventional	.11	.10	.10	.10	.10	.10
	(.08)	(.08)	(.07)	(.07)	(.07)	(.08)
Robust	.16	.14	.11	.12	.11	.11
	(.11)	(.11)	(.08)	(.10)	(.10)	(.08)
CI	[-.06, .38]	[-.07, .35]	[-.05, .26]	[-.08, .31]	[-.07, .30]	[-.05, .27]
6-year completion rate						
Conventional	.01	.02	.05	.04	.04	.04
	(.06)	(.06)	(.05)	(.05)	(.05)	(.06)
Robust	-.03	-.02	.05	-.00	.02	.04
	(.10)	(.09)	(.06)	(.07)	(.07)	(.06)
CI	[-.22, .15]	[-.20, .15]	[-.07, .17]	[-.15, .15]	[-.12, .16]	[-.09, .16]
Total credits attempted						
Conventional	23.69***	21.45***	18.80**	17.27*	16.05*	15.91
	(10.49)	(9.71)	(9.29)	(9.17)	(9.08)	(11.01)
Robust	20.66*	25.93*	20.51**	26.02**	23.31**	16.63
	(15.56)	(14.48)	(9.82)	(12.43)	(11.62)	(11.19)
CI	[-.83, 60.15]	[-2.45, 54.31]	[1.25, 39.76]	[1.67, 50.37]	[.54, 46.07]	[-5.31, 38.57]
cumulative GPA						
Conventional	.43*	.45*	.44*	.44*	.43**	.45
	(.26)	(.24)	(.23)	(.22)	(.22)	(.26)
Robust	.34	.36	.45*	.42	.44	.47
	(.34)	(.33)	(.25)	(.31)	(.30)	(.27)
CI	[-.33, 1.01]	[-.29, 1.01]	[-.03, .93]	[-.19, 1.03]	[-.15, 1.04]	[-.05, .99]

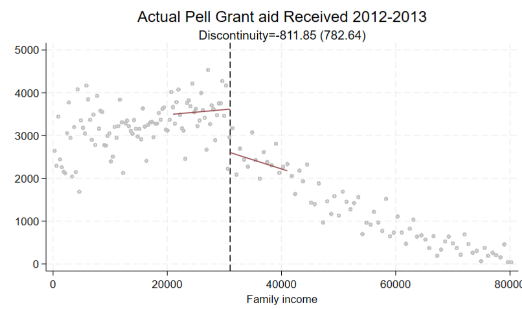
Notes: The table replicates the results from Tables 4 and 5 using different bandwidths. Columns (1) and (2) use bandwidths $2,000 and $1,000 smaller than the optimal, respectively. Column (3) shows the results with the optimal bandwidth, while Columns (4) and (5) use bandwidths $1,000 and $2,000 larger. Column (6) tests the sensitivity of the results to the functional form of the relationship between the outcomes of interest and the EFC by including a quadratic term of the EFC and its interaction with the quadratic term.
Source: 2012/2017 Beginning Postsecondary Students Longitudinal Study

Figure B1: Discontinuity in Actual Pell Grant

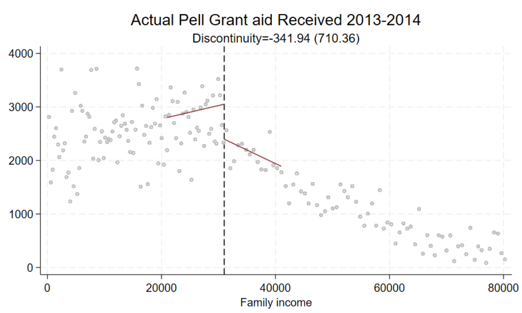
(a) Actual Pell, 2011-2012



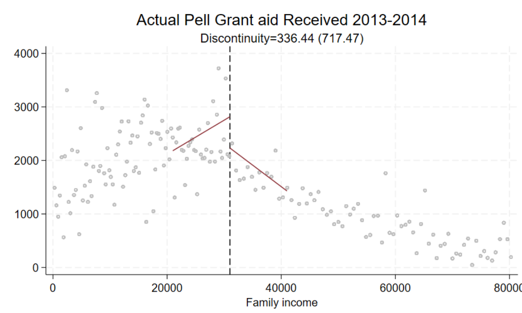
(b) Actual Pell, 2012-2013



(c) Actual Pell, 2013-2014



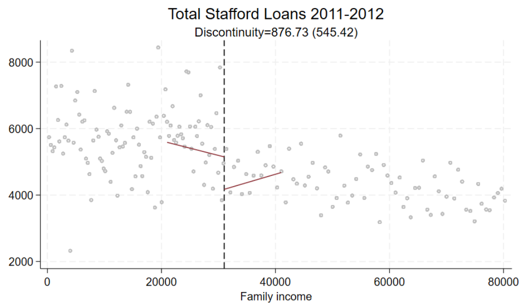
(d) Actual Pell, 2014-2015



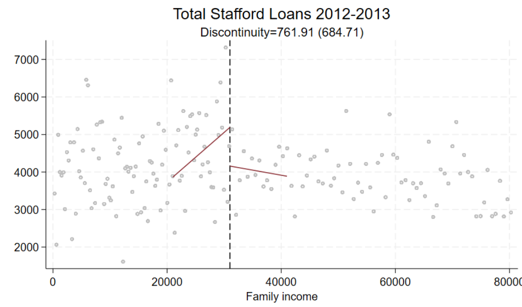
Notes: These figures display the actual Pell Grant received by first-time undergraduate students who entered college in the 2011-2012 academic year, and students who were either dependent and independent students with dependents. A dashed line represents the automatic-zero (AZ) income threshold, set at $31,000 for the 2011-2012 academic year. The Pell-eligible amount may not be equal to the actual Pell Grant received by students, as the final amount considers other factors beyond the EFC, such as the cost of attendance, enrollment intensity, and whether students were enrolled for a full academic year. Each figure shows bias-corrected RD estimates from a local linear regression of each outcome variable on AGI, noted at the top of each figure, with the standard error shown in parentheses. Solid red lines indicate estimates from local linear regression, and light rose dotted lines represent the robust biased-corrected (RBC) confidence intervals. In the first year (2011-2012), students just below the income threshold received an additional $915 in Pell Grant than those just above the threshold. This gap reduced to $812 in the second year (2012-2013) and became insignificant, further decreasing to $342 and $336 in the third and fourth years, respectively. This reduced gap can be partially explained by a decreased share of students enrolled as full-time students and by students who transferred to 2-year institutions.
Source: 2012/2017 Beginning Postsecondary Students Longitudinal Study

Figure B2: Discontinuity in Other Grants

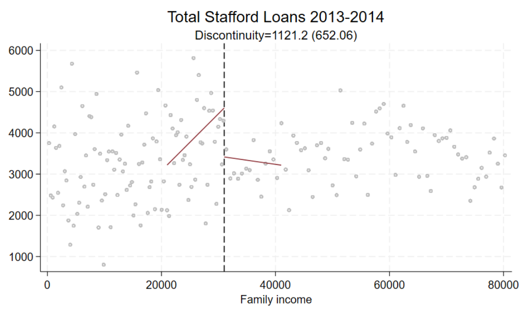
(a) Stafford Loan, 2011-2012



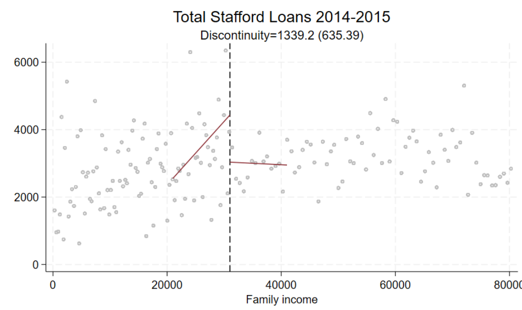
(b) Stafford Loan, 2012-2013



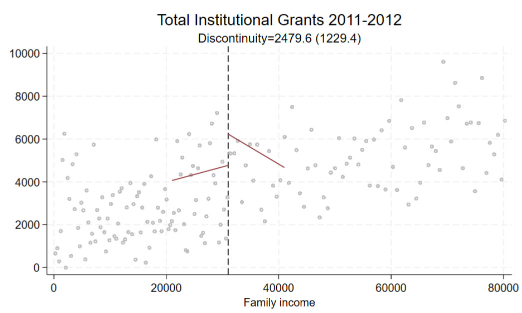
(c) Stafford Loan, 2013-2014



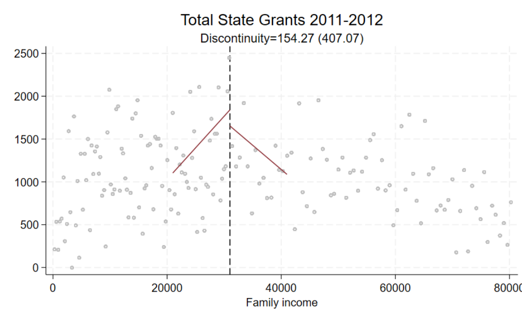
(d) Stafford Loan, 2014-2015



(e) Total Inst Grants, 2013-2014

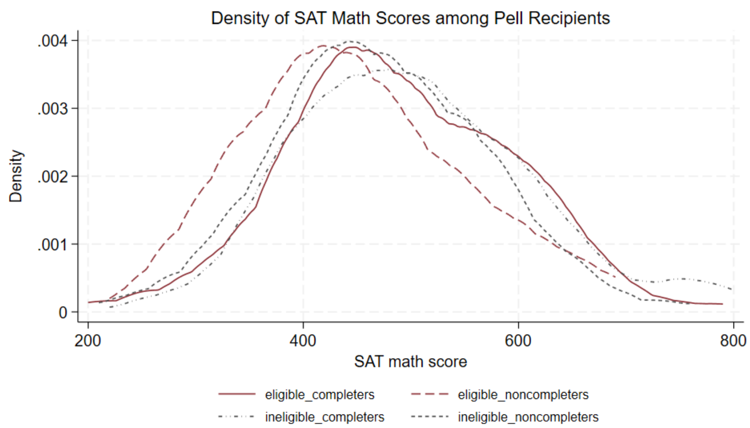


(f) Total State Grants, 2014-2015



Notes: These figures illustrate the discontinuities in Stafford Loans from the first to the fourth years, as well as the discontinuities in total institutional grants and total state grants during the first year. Each figure shows bias-corrected RD estimates from a local linear regression of each outcome variable on AGI, noted at the top of each figure, with the standard error shown in parentheses. Solid red lines indicate estimates from local linear regression, and light rose dotted lines represent the robust biased-corrected (RBC) confidence intervals. In the first year (2011-2012), students just below the income threshold borrowed \$877 more in Stafford loans, including both subsidized and unsubsidized, than those just above the threshold. This gap narrowed to \$762 in the second year (2012-2013) but increased to \$1,121 and \$1,339 in the third and fourth years, respectively. Additionally, students above the threshold received about \$2,480 more in institutional grants in their first year.
Source: 2012/2017 Beginning Postsecondary Students Longitudinal Study

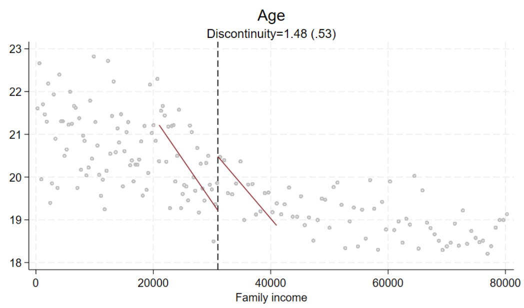
Figure B3: Density of SAT Math Scores Among Pell Recipients



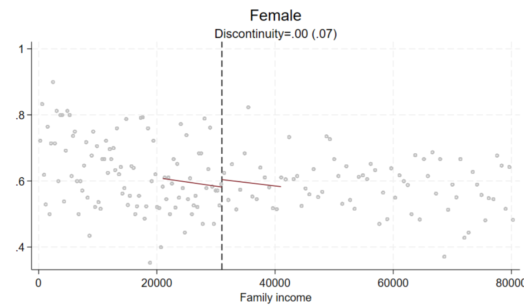
Notes: This figure shows the density of SAT math scores within the analyzed sample. The results of the Kolmogorov-Smirnov test show that among students below the income threshold, those who did not persist in college had significantly lower SAT math scores compared to those who did, with this difference being significant at the 1 percent level. Similar findings were observed among students above the income threshold; those who did not persist in college had lower SAT math scores, with the gap being significant at the 5 percent level.
Source: 2012/2017 Beginning Postsecondary Students Longitudinal Study

Figure B4: Discontinuity in Students' Observable Demographic Characteristics

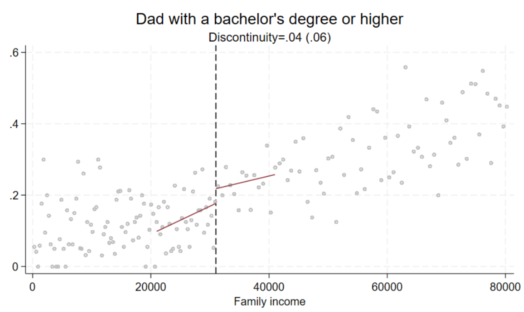
(a) Age



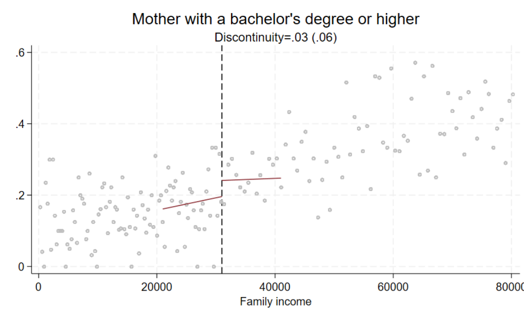
(b) Female



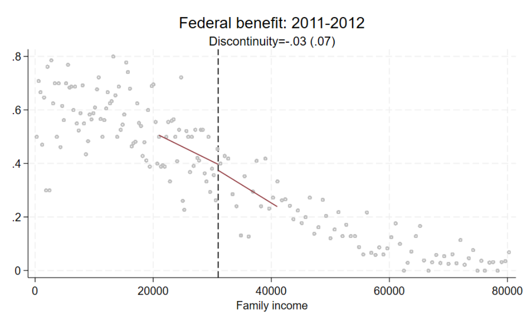
(c) Dad bachelor's degree



(d) Mom bachelor's degree



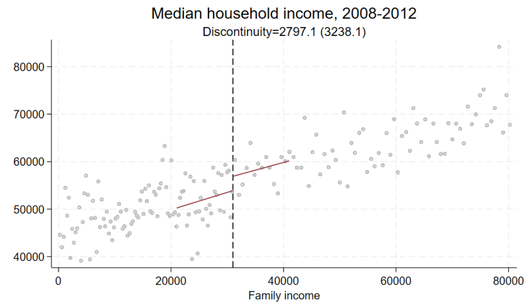
(e) Federal benefits



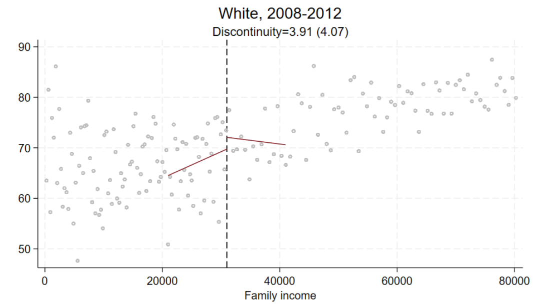
Notes: These figures show the discontinuities in students' observable demographic characteristics at the AZ income threshold. Each figure shows bias-corrected RD estimates from a local linear regression of each outcome variable on AGI, noted at the top of each figure, with the standard error shown in parentheses. Solid red lines indicate estimates from local linear regression.
Source: 2012/2017 Beginning Postsecondary Students Longitudinal Study

Figure B5: Discontinuity in Students' Census Tract Information, 2008-2012

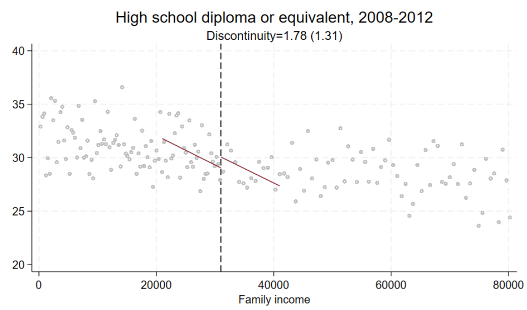
(a) Median household income



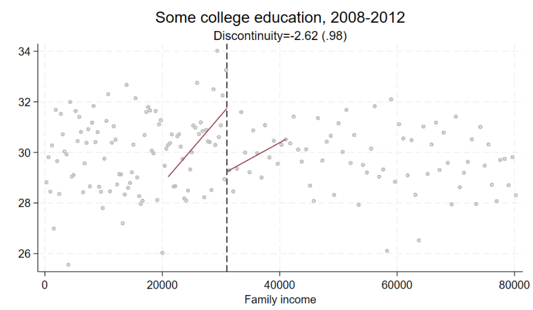
(b) White population



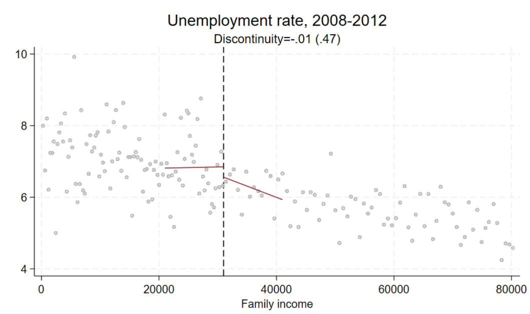
(c) High school diploma or equivalent



(d) Some college education

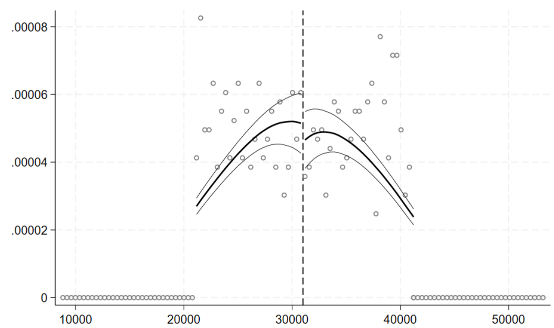


(e) Unemployment rate



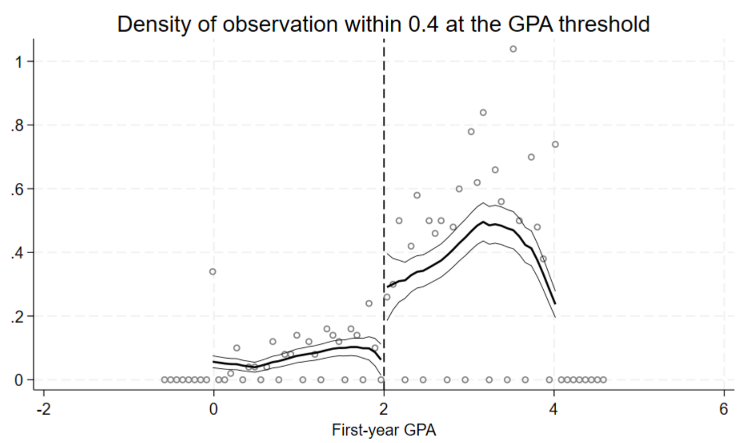
Notes: These figures show the discontinuities in students' census tract information for the period 2008-2012 at the AZ income threshold. Each figure shows bias-corrected RD estimates from a local linear regression of each outcome variable on AGI, noted at the top of each figure, with the standard error shown in parentheses. Solid red lines indicate estimates from local linear regression. The 'Median household income' figure shows the median household income within students' census tracts from 2008-2012. The 'White' figure indicates the percentage of the tract population that was white during this period. The 'High school diploma or equivalent' figure shows the percentage of the population over age 25 within the students' census tracts who held a high school diploma or equivalent. Similarly, the 'Some college education' figure shows the percentage of the population over age 25 with some college education. Finally, the 'Unemployment rate' figure represents the percentage of the population over the age of 16 within the students' census tracts who were unemployed.
Source: 2012/2017 Beginning Postsecondary Students Longitudinal Study

Figure B6: Density of observations withing ± 10,000 at the AZ threshold



Notes: This figure shows the density of observations on family income within approximately ±$ 10,000 around the AZ income threshold for the analyzed sample. Using the density test introduced by McCrary (2008), the analysis reveals a discontinuity estimate of -0.11, with a standard error of 0.14.
Source: 2012/2017 Beginning Postsecondary Students Longitudinal Study

Figure B7: Density of observations within ± 0.4 at the GPA Threshold



Notes: This figure shows the density of observations within ± 0.4 at the 2.0 GPA threshold for the analyzed sample. Using the density test introduced by McCrary (2008), the analysis reveals a discontinuity estimate of 2.52, with a standard error of 0.87.
Source: 2012/2017 Beginning Postsecondary Students Longitudinal Study