Understanding the Effect of Grant Aid on College Enrollment in the Presence of Close Credit Substitutes: Evidence from Pell Grant Formula Generosity

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Abstract

Recent studies find mixed or minimal effects of the Pell Grants, a key source of financial aid in higher education, on college outcomes despite $31 billion in annual spending on 6.5 million students in the U.S. and broader evidence that grant aid increases enrollment. I use national administrative data from the US Department of Education for 17.6 million first-year, dependent students to study how differential changes across income groups to the Pell grant formula's generosity from 2008 to 2022 affect persistence in college and student loan borrowing. My research design uses a formula instrument to isolate the effect of idiosyncratic formula changes that provide students with a larger or smaller grant than is typical for their income group or award year, allowing me to study a wider population than previous approaches focused on students near eligibility thresholds. I find that $1,000 in additional Pell grant aid increases persistence in college by 1.3-1.5 percentage points, while decreasing borrowing by $0.19-$0.22 for each dollar of available grant aid. These results are much more pronounced for students pursuing Associate's degrees and those at open-access institutions, where persistence increases by 2.5-3.2 percentage points and there is no crowd out of borrowing. Cross-state comparisons of treatment effects suggest that the degree of substitution between loan and grant aid is a key mechanism underpinning enrollment effects, a key fact for interpreting previous estimates of the program's effectiveness.

1. Introduction

The Pell Grant is the most important need-based grant that the United States federal government provides to help students of limited financial means pay for college while limiting their reliance on loans. Despite its role as a pillar of federal financial aid policy, recent studies of the program with credible, causal research designs have largely (with one notable exception) found minimal effect of the Pell grant on students' enrollment, persistence, completion, and earnings. Given the well-documented positive effect of other forms of grant aid on enrollment and completion, the general pattern of small to zero effects of the Pell grant has proven puzzling.

With this study, I aim to provide new evidence about the financial aid dynamics at play in the patterns we have seen in the existing literature in order to help resolve its apparent puzzles. I use aggregated administrative data from the US Department of Education from Award Years 2008–2022 for over 17 million first year college students to construct a formula instrument and study idiosyncratic differences in Pell Grant eligibility across students of different income levels and award years. Using a fixed population of students to calculate the expected Pell eligibility of income groups over time, I am able to use a difference-in-differences approach to study whether formula-driven changes to the grant's generosity result in enrollment or borrowing changes from students who benefit from an idiosyncratically generous award in their first year in college.

This research design differs from the recent literature in that it does not use a regression discontinuity (RDD) or regression kink (RK) design. The RDD approach is appealing for its strong identification, but has the drawback of limiting the population of study to two areas of the income distribution where the formula creates discontinuous changes in Pell eligibility. My approach depends on comparisons between students across the parents' income distribution. In addition to allowing me to study the full Pell eligible population, this approach also includes larger differences in Pell between students over time than are typically studied in the RD context.

With a wider population and different set of treatment contrasts, my study concludes that each $1,000 of additional Pell grant aid available to students raises students' probability of re-enrolling in the following award year by 1.3–1.5 percentage points. At the same time, students reduce their borrowing by a (marginally significant) \$0.20 for each additional dollar of Pell they have access to. The scope of my data, which covers a national population and select subgroups for over a decade, allows me to analyze how these effects differ for key populations in my study. From these heterogeneity analyses emerges a clear story: for students at less selective or open access institutions, especially those pursuing Associate's degrees, the impact of the Pell grant on re-enrollment is much larger, at between 2.5–3.2 percentage points. These students are also less likely to reduce their borrowing in response to gaining access to more Pell aid, and in some cases they may even increase their borrowing as a result. Finally, state-level estimates show that in states where students' borrowing decrease less in response to increased Pell eligibility are also states where Pell seems to increase persistence the most.

This analysis contributes to the existing Pell grant literature by shining a light on the financial aid substitution dynamics that underpin the apparent ineffectiveness of the Pell grant in raising college enrollment or completion rates. While I find Pell to be more effective at raising persistence rates than past studies, these effects are concentrated in students in less selective or open access institutions, where other forms of student financial support are scarcer. This reveals that even as the Pell grant serves as an alternative to borrowing for students at more selective institutions who will attend and remain in college regardless of how they finance their education, another sizable group of students experiences greater Pell generosity as an expansion to the money available to them to pay for college.

2. Pell Grant History & Literature

The Pell Grant is the main source of federal grant aid that undergraduates use to pay for college in the United States, with \$31 billion in total grant money disbursed to 6.5 million recipients in Award Year 2023–24. This grant program dates to 1973, when it was known as Basic Education Opportunity Grants, and serves as the backbone of a financial aid system, awarding up to \$7,395 to help students to pay for college. Unlike loans, grants do not need to be repaid, and Pell is therefore targeted to lower-income students, with generosity decreasing roughly in proportion to students' household income for students who are qualified for less than the maximum award and phasing out completely around a family AGI level of $80,000. For the period I study, Pell Grant eligibility was determined by a federal calculation of financial need known as the Expected Family Contribution (EFC). This formula depends on extensive financial information from students and their families and, as its name implies, attempts to quantify what students can afford to pay for college. Students submit this financial aid information through the Free Application for Federal Student Aid (FAFSA) and it is processed by the US Department of Education's Office of Federal Student Aid (FSA).

Despite the considerable federal investment made in the Pell grant program, credible causal research on the effect of the grant on students' college-going and completion behavior remains mixed in its conclusions. Early studies of Pell disagree about the effect of the Grant on initial college entry and persistence, with Kane (1995) finding no evidence of an increase in enrollment resulting from the program's 1973 introduction, while Seftor and Turner (2002) report positive impacts of 1–2 percentage points from the introduction of the program and that a reduction of Pell benefits for independent students reduced adult student enrollment 3.9 to 4.2 percentage points. Bettinger (2004) uses an increase in the generosity of the grant between the 1999–00 and 2000–01 award years to conclude that an additional $1,000 in 8–9 percentage point reduction in stopping out, though his cross-sectional analysis comparison of students with different family sizes finds a 1–4 percentage point reduction in stopping out in response to $1,000 of Pell.

This pattern of conflicting results remains in the more recent literature that features regression discontinuity designs (RDD), though the balance of findings is more clearly on the side of minimal or no effects on enrollment, persistence, and completion. These RDD studies focus on two peculiarities of the EFC formula that create discontinuous jumps in the Pell award for income groups just on each side of important income thresholds. The first is the threshold that determines whether students are eligible for the Pell grant at all, with studies finding that becoming eligible nets students between $250 and $480 of grant money compared to those barely ineligible (Carruthers and Welch, 2019; Chan and

Heller, 2025; Marx and Turner, 2018; Park and Scott-Clayton, 2018; Rubin, 2011). The other margin of study is the "automatic zero" threshold, which sets the income level where students are exempts from virtually all of the aid application because their incomes are low enough that additional financial information is not likely to change their final eligibility determination. Students below this point are automatically presumed to be unable to contribute any financial resources to paying for college (thus "zero") and therefore receive the maximum Pell grant. For students right at the margin of qualifying, the automatic maximum award can often mean several hundred dollars in extra Pell eligibility. Denning et al. (2019) study Pell recipients this margin in Texas, while Eng and Matsudaira (2021) analyze the universe of U.S. federal aid recipients near this threshold. Across these studies, the authors report discontinuities in Pell receipt between \$140 and \$850 for those on the margin of automatic zero eligibility.

With the exception of Denning et al. (2019) these seven studies tend to find minimal impacts of the Pell grant on important academic and labor outcomes. For initial enrollment, Carruthers and Welch (2019) and Rubin (2011) study high school graduates in Tennessee and a sample of the US population respectively, and find no effect of gaining Pell eligibility on college enrollment within the first year following their completion of secondary school. Relatedly, Marx and Turner (2018) estimate no effect of Pell on initial enrollment for students who apply to CUNY institutions. The three studies that find an impact on initial enrollment report differently signed effects; Park and Scott-Clayton (2018) observe lower enrollment of barely Pell-eligible students Ohio community colleges compared to their ineligible peers, while Denning et al. (2019) find positive effects on initial community college enrollment for Texas students of 3–10%, though no effect for students at four year colleges. Using national data, Eng and Matsudaira (2021) report a positive effect of Pell on enrollment at the automatic zero threshold for all dependent students and independent students with children, ranging between 1.7% and 6.5% depending on the year.

On the persistence margin, the focus of my study, the recent literature is more unanimous in finding no effect of Pell. Park and Scott-Clayton (2018) see minimal, opposite signed, and not significant effects of any enrollment in spring of the first and second years following initial enrollment. Chan and Heller (2025) report insignificant reductions in enrollment between 1.4 and 3.3 percentage points across students' time in college for all Kentucky students in their sample. Marx and Turner (2018) find no statistically significant persistence effect for their CUNY sample, while Denning et al. (2019) see no enrollment effect for first-time students in Texas, and only a marginally significant 1 percentage point positive enrollment effect for returning students. The largest persistence effects in the Pell literature come from outside of the RDD studies, where Mabel (2016) using a sample of fifth year college students from the Current Population Survey, finds a 4 percentage point drop in persistence per $1,000 of Pell eligibility lost as a result of a policy change that reduced the lifetime limit for receiving a Pell grant from 18 semesters to 12. The lack of persistence effects also corresponds with mostly null findings on completion; Eng and Matsudaira (2021) find small positive completion effects, while neither Park and Scott-Clayton (2018) nor Chan and Heller (2025) find any significant completion effects. Here, Denning et al. (2019) stand out from the pack in finding a sizable five percentage point increase in completion rates at 6 years per $1,000 in additional Pell aid for first-time-in-college students, though they find no significant effect for returning students.

Taken together, these results have left the authors looking for mechanisms that would explain why the results diverge from the broader literature on grant aid, where effects are more consistently positive.[1] Across the studies, a few theories have emerged. First, many find significant substitution across aid sources, which means that even as students gain or lose access to Pell awards, the total amount of financial support they have access to may not change. For example, Chan and Heller (2025) show that while students just barely eligible for Pell in Kentucky receive an additional $351 in the federal grant, which is supplemented by an additional $582 in non-Pell, need-based grant aid. However, these additional grants

[1]The evidence on grant aid generally is quite clear that many types of transfers earmarked to pay for college (e.g., scholarships, state grants, tax-based aid, etc.) have positive impacts on attendance and persistence, concluding that $1,000 increase in grant aid (or decrease in net price) increases college enrollment by 3 to 5 percentage points and improves the chances of persisting in or completing college by 1.5 to 2 percentage points (see Dynarski & Scott-Clayton, 2013; Page & Scott-Clayton, 2016; Nguyen, Kramer, & Evans, 2019; Dynarski, Page, & Scott-Clayton, 2023 for reviews of this literature).

are virtually completely offset by a $734 reduction in borrowing. Denning et al. (2019) also show that automatic zero eligibility crowds in other grant aid in Texas, but they too see a corresponding reduction in borrowing about 30–50% the size of the total grant aid boost. Similarly, Park and Scott-Clayton (2018) find that while marginal eligibility increases Pell and state grant receipt for Ohio students by $400-$600, those students also reduce their borrowing roughly equivalently. Marx and Turner (2018) report reductions in borrowing that average between $0.42 and $0.51 for every additional dollar in Pell aid, though among likely borrowers they estimate loan reductions of $1.82-$1.89 for every additional dollar of Pell. Finally, Eng and Matsudaira (2021) report a 2–2.5 percentage point reduction in the borrowing rate at the automatic zero threshold.

Overall, the finding of loan aid responding to increases in grant aid is quite robust across the studies, even as the effect on outcomes varies. My study will use the scope of its data coverage to look systematically at the relationship between the aid displacement and the enrollment effect and show that this is an important mechanism underpinning the patterns we observe and one that varies across different institutional contexts where different types of aid are used.

3. Data and Sample

The data for this study comes from publicly available aggregated administrative data released by the US Department of Education's Office of the Chief Economist. The underlying microdata were drawn from the Office of Federal Student Aid (FSA)'s databases and combine records from the Central Processing System (CPS), which is responsible for processing FAFSAs to calculate official aid eligibility and stores all of the formula's inputs and outputs, with data elements from the National Student Loan Data System (NSLDS), which contains information about all federal aid disbursements. The data is aggregated to the income-group by award year level, and the study period spans from award years 2007–08 through 2021–22 and covers the universe of first-year, federally-aided, dependent student FAFSA filers in the United States who were entering freshman in a Bachelor's or Associate's program and enrolled for at least 30 days in that award year.

The income-group bins are defined by parents' total income, which is an EFC formula construct that sums parents' responses to questions about their income. Each bin is on average $2,500 in width, and the income levels range from a lowest bin containing parents' with incomes of $0 or below to the highest with parents' income of $450,000 or above.[2] The bins are fixed in real terms (of 2023 dollars), so that they represent students coming from families with incomes of comparable purchasing power over time. The data contains aggregates of two key outcomes of interest: 1) the average amount of all federal subsidized and unsubsidized (FFEL and Direct) loans borrowed and 2) the share of students enrolling in college for at least 90 days at any institution in the following award year, my measure of persistence. The dataset also contains two key measures of the Pell grant available to these groups: average eligibility (the amount a student is eligible for according to the CPS calculations) and average disbursements (the amount actually provided to the student), which mostly differ for enrollment intensity reasons.[3]

Also provided in the data is the income-group average Pell amount for a fixed population of all aid applicants from the 2015–16 award year would have received had they faced the Pell eligibility formula rules in each award year. Discussed in greater detail below, this fixed-population measure of formula generosity allows me to measure for a stable group of students how formula changes affect the amount of Pell available to each income group over

[2]The number of bins are set by the automatic binning procedure created by the Stata command binsreg, which produces a binned scatterplot with an optimal binning procedure designed to show an aggregated approximation of the relationship between two variables from a large number of underlying datapoints. The goal is to balance the risk of over-smoothing (missing relevant changes in the relationship by using too few bins) and overfitting (mistaking noise for true changes in how the variables relate) by minimizing the integrated mean square error for the bins. In my case, I used the procedure to create bins of total parents' income from the FAFSA data that best grouped students to preserve relationships between observed Pell amounts (in real 2023 dollars) and the parent income groups across award years. I constrained the procedure to use the same bins across award years and restricted the candidate number of bins to between 100 and 160. The procedure chose 154 bins of roughly equal density (i.e., bins with the same amount of data rather than the same width) for each year, producing 2,307 cells in total across the award years, after accounting from cell suppression for cells with fewer than 100 individuals.

[3]On average, in student-weighted terms, the amount students are eligible for exceed the amount disbursed to them by approximately $500.

time. Using a fixed population means that any changes in the average Pell amounts across years are driven by formula changes and not population differences over time. For this reason I use it to instrument for observed Pell eligibility and receipt to isolate the variation from potentially exogenous formula changes.

Finally, the data also contains subgroup aggregates of all of this information. That is, the data contains each of the above measures of re-enrollment and aid receipt, for the same set of award year and income group cells, but also subset by additional characteristic, to permit heterogeneity analysis. These additional subgroups are: the degree a student is pursuing, the Barron's selectivity category of the institution they attend, and the student's state of residence as recorded at the time of their FAFSA. Table 1 shows how the sample varies over time on each of these characteristics. First we can see that the number of first-year Dependent student FAFSA filers in my sample ranges from 790,000 in Award Year 2007–08 to a peak of 1.34 million in 2014. Among these students, between 70% and 81% were pursuing BA degrees, with the remainder pursuing an Associate's degree. Over the sample period, the average Pell award was around \$3,000-\$3,500 and between two-thirds and three quarters of such applicants received a Pell grant.

The table also shows an important fact about the data source: until 2012–13, institutions were not required to report to the Department of Education the enrollment status of students who received only a Pell Grant but no loans because the Department only needed to track the continuous enrollment of borrowers so that it could know when their repayment period would begin. Beginning with AY2013, ED began requiring institutions to report enrollment information for all students receiving federal aid so that it could enforced accountability regulations. For this reason, there is a sizable shift in the share of students who are borrowing, dropping from about 92% to 68.5% in award year 2013 and continuously declining to about 61% by 2023. This pattern is also reflected in the number of students in each year, which jumps from around a million in AYs 2010–2012 to 1.3 million in 2013 and hovering around that level through the end of the series, with a notable pandemic decline.

The prevalence of students who only receive a Pell grant is much higher at community colleges, a fact reflected in the change in the percentage distribution of the predominant

Table 1: Summary Statistics by Award Year

	Award Year															
	2008	**2009**	**2010**	**2011**	**2012**	**2013**	**2014**	**2015**	**2016**	**2017**	**2018**	**2019**	**2020**	**2021**	**2022**	**2023**
Share receiving Pell (%)	51.4%	51.5%	58.6%	59.3%	60.9%	61.8%	62.6%	63.1%	63.2%	62.8%	62.1%	62.5%	62.8%	61.3%	60.0%	59.3%
Avg. Pell grant (2023)	2,157	2,473	3,454	3,417	3,317	3,098	3,138	3,236	3,260	3,184	3,241	3,328	3,352	3,160	2,907	2,932
Share borrowing (%)	92.9%	92.9%	92.0%	91.4%	84.0%	68.5%	66.4%	65.5%	64.8%	64.9%	64.6%	63.6%	61.4%	60.7%	61.2%	61.2%
Avg. loans disbursed (2023)	7,934	9,238	9,656	10,011	8,968	7,080	6,801	6,836	6,941	6,949	6,797	6,635	6,297	5,710	5,747	5,776
Persisted to next AY (%)	86.4%	87.0%	84.0%	84.4%	85.1%	79.8%	79.3%	80.6%	80.9%	81.2%	80.7%	81.8%	79.7%	81.9%	83.6%	85.4%
Student Degree Type																
1st Bachelor's Degree	80.9%	78.8%	77.8%	78.7%	77.6%	71.9%	70.9%	71.6%	72.4%	72.9%	74.9%	75.7%	74.7%	77.5%	77.4%	76.7%
Assoc degree (occup/techn prog)	11.0%	11.9%	13.1%	12.3%	11.5%	12.6%	12.6%	12.0%	11.4%	10.9%	10.2%	9.8%	9.9%	8.9%	9.0%	9.3%
Assoc degree (gen ed / transfer)	8.1%	9.3%	9.1%	9.0%	10.9%	15.5%	16.5%	16.4%	16.3%	16.2%	14.9%	14.5%	15.3%	13.6%	13.6%	14.0%
Predominant Degree Level																
Other/Unknown	8.4%	8.1%	9.3%	8.5%	6.6%	6.0%	5.6%	5.2%	4.1%	3.6%	3.3%	3.0%	3.0%	2.3%	2.2%	2.5%
Predominantly certificate	3.8%	4.6%	5.4%	5.3%	6.5%	9.2%	9.5%	8.9%	8.8%	8.5%	8.5%	8.2%	8.7%	7.6%	7.3%	7.4%
Predominantly associate's	12.9%	14.6%	16.0%	16.6%	20.1%	28.4%	29.2%	28.5%	28.4%	28.2%	28.1%	27.1%	28.1%	24.2%	23.4%	24.0%
Predominantly bachelor's	75.0%	72.6%	69.3%	69.6%	66.7%	56.5%	55.7%	57.3%	58.7%	59.7%	60.1%	61.6%	60.2%	65.9%	67.0%	66.2%
Institution Selectivity																
Most competitive (%)	3.6%	3.2%	3.3%	3.4%	3.2%	2.5%	2.5%	2.6%	2.6%	2.6%	2.6%	2.6%	2.5%	2.8%	3.2%	3.0%
Specialized (%)	0.8%	0.8%	0.8%	0.8%	0.7%	0.6%	0.5%	0.5%	0.5%	0.6%	0.5%	0.6%	0.6%	0.5%	0.6%	0.6%
Highly competitive + (%)	3.2%	2.9%	2.8%	2.7%	2.5%	2.0%	1.9%	2.0%	2.0%	2.0%	2.0%	2.0%	1.9%	2.3%	2.5%	2.3%
Highly competitive (%)	5.7%	5.4%	5.2%	5.3%	5.1%	4.1%	4.0%	4.1%	4.2%	4.3%	4.3%	4.4%	4.3%	4.9%	5.4%	5.2%
Very competitive + (%)	3.4%	3.2%	3.0%	3.2%	3.0%	2.5%	2.4%	2.5%	2.5%	2.6%	2.6%	2.8%	2.8%	3.1%	3.5%	3.4%
Very competitive (%)	14.5%	13.9%	12.8%	12.7%	12.2%	10.2%	9.9%	10.2%	10.4%	10.6%	11.0%	11.5%	11.1%	12.4%	13.0%	13.0%
Competitive + (%)	3.5%	3.4%	3.1%	3.1%	3.0%	2.6%	2.6%	2.7%	2.7%	2.7%	2.9%	3.0%	3.0%	3.4%	3.3%	3.3%
Competitive (%)	28.9%	28.3%	26.7%	26.9%	25.9%	21.9%	21.8%	22.6%	23.2%	23.5%	23.6%	23.9%	23.4%	24.9%	24.8%	24.8%
Less competitive (%)	6.1%	6.0%	5.8%	6.0%	5.6%	4.8%	4.8%	4.7%	4.8%	4.9%	5.0%	5.1%	5.0%	5.3%	5.1%	5.0%
Non-competitive (%)	2.5%	2.5%	2.5%	2.5%	2.5%	2.3%	2.3%	2.2%	2.2%	2.2%	2.2%	2.1%	2.1%	2.1%	1.9%	1.9%
No category (%)	27.7%	30.3%	33.8%	33.4%	36.3%	46.6%	47.4%	45.9%	45.0%	44.1%	43.4%	42.0%	43.4%	38.1%	36.8%	37.6%
Millions of Students	0.79	0.87	1.00	1.00	1.08	1.33	1.34	1.32	1.29	1.29	1.32	1.28	1.31	1.14	1.24	1.26

Note: Sample includes all first-year, federally-aided, dependent student FAFSA filers entering Bachelor's or Associate's degree programs who enrolled at least 30 days in each award year. All dollar amounts in constant 2023 dollars. Share receiving Pell and Share borrowing represent the percentage of students receiving Pell grants or federal subsidized/unsubsidized loans, respectively. Persisted to next AY indicates enrollment for at least 90 days at any institution in the following award year. Student Degree Type, Predominant Degree Level, and Institution Selectivity show the percentage distribution of students across categories within each award year. Beginning in AY 2012-13, institutions were required to report enrollment status for Pell-only recipients, causing a structural break in borrowing rates and sample composition. Data source: U.S. Department of Education aggregated administrative files combining Central Processing System (FAFSA) and National Student Loan Data System (NSLDS) records.

degree level of institutions attended by students in the sample, which drops from around 70% in the years just preceding the data change to 55%-60% through most of the rest of the series. This decline is also present in the Barron's selectivity distribution across the period, where we see a jump in students at institutions with no Barron's category, and a decline in the other largest category "Competitive." While unfortunate, this type of change to our sample characteristics will not fundamentally compromise our ability to identify effects of the Pell grant, as these changes to the sample will be netted out by award year fixed effects. Nevertheless, given the mixed composition of the sample across the period, I will also present my estimates for a sample restricted to just the award years 2013 and later to facilitate interpretation of the estimates for a fixed population of students that contains both students whose aid packages contain Pell grants only and those who have Pell grants and loans.

4. The Pell Grant Formula

The Pell Grant is a need-based grant, meaning that is awarded to students according to their financial circumstances. In general, the rule for awarding Pell amounts is that eligible students receive the maximum award, net of their expected family contribution (EFC), a measure of their ability to pay.[4] The Pell award amount then depends on the EFC in the following way:

$$\text{Pell}_{i,t} = \max\{0,\ \text{Max Pell}_t - \text{Expected Family Contribution}_{i,t}\} \tag{1}$$

Because it drives Pell award levels, we will focus our attention on components of the EFC formula and how its patchwork annual updating process makes it possible to extract credible causal estimates of the Pell grant on college persistence and student loan borrowing. Equation 2 below shows roughly how income and assets from parent and student sources

[4] As a result of the 2021 FAFSA Simplification Act, there was a substantial overhaul of the needs analysis process, including a number of changes to how the calculations work. Another consequence of that law is that the EFC is now known as the Student Aid Index (SAI). These changes occurred after my sample period, so for simplicity I will refer to the previous approach using its concepts and phrasing for its various constructs. (Congressional Research Service, 2022)

contribute to the final EFC calculation, though many questions and calculations feed into each broad category of the equation:

$$\begin{aligned} EFC = & \frac{(\text{ParentsÍncome} - \text{Income Allow.}) + 0.12 \times (\text{ParentsÁssets} - \text{Asset Allow.})}{\#\text{ children in college}} \\ & + 0.5 \times (\text{Student's Income} - \text{Allowances Against Student Income}) \\ & + 0.2 \times (\text{Student's Assets} - \text{Allowances Against Student Assets}) \end{aligned} \tag{2}$$

The income components are derived both from the family's AGI and any untaxed income sources, while assets include cash and savings, investment accounts other than retirement, the value of a business or farm with over 100 full time employees, and any real estate owned other than the family's primary residence. However, as Equation 2 shows, only a portion of the value of the assets is considered available to help pay for college, and only half of a dependent students' income is allowed to contribute.

Researchers and policymakers have long lamented the FAFSAs complexity, which arises from the many inputs to the EFC formula. The key to my identification strategy is the way this complexity interacts with automatic annual updating and periodic statutory changes to the EFC calculation. Over my period of study, award years 2008 to 2022, the formula changes each year, but in different ways for different income groups. To demonstrate this, Figure 1 shows the amount of Pell that different income groups from the fixed population of award year 2015–16 dependent student FAFSA filers would have been eligible for under each award year's formula, in constant 2023 dollars.

Each marker represents the Pell amount for a different income group under each set of formula rules. For dependent students, parents' total income is the biggest driver of Pell eligibility, so students are broken into 154 income groups ranging from parents income levels of \$0 to more than \$450,000 in bins of an average width of about \$2,500 (though the specific ranges are suppressed for privacy reasons). The plot then shows Pell as a function of parents' total income and is revealing in a few ways. First, it shows the basic progressivity of the formula, with the maximum award going to those whose income qualifies them for a zero EFC and then decreasing with income above that point until reaching the income levels where students are not eligible for Pell.

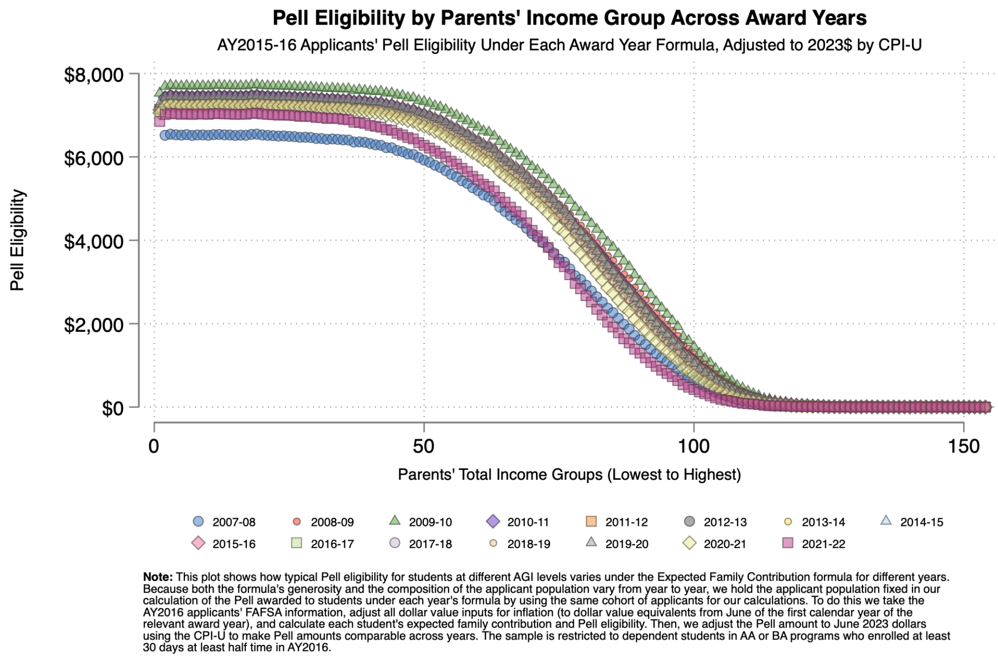


Figure 1: Simulated Pell eligibility by income group

Second, it shows that students in the same income groups can receive quite different Pell amounts in different years and that the year-to-year differences in Pell eligibility are not consistent across groups. This is easiest to see when comparing the award levels under the AY 2007–08 (blue circles) and 2021–22 (pink squares) formulas. At the bottom of the of income distribution, students who filed their FAFSA in the later period were eligible for a few hundred dollars more than their peers in the same income bin filing under the 2007–08 formula. But the difference in award levels shrinks, and the award schedules cross around the middle of the income distribution. This fact make it possible to difference out year-specific shocks, which I describe in greater detail below.

What drives these differences in Pell eligibility over time? This question is key for understanding the plausibility of the exogeneity of the variation I study. The biggest drivers are the terms of the program itself. Federal rules also determine both what the maximum and minimum award is and who is automatically eligible for the maximum award without needing to fill out the FAFSA, who can file the simple form without asset information, and who is ineligible for any award. The formula also contains a series of rules that govern how different income and asset sources are treated for the purposes of calculating a student's ability to pay. In all cases, only a portion of the student or their family's resources are considered eligible to contribute to paying for college.

By and large, these rules are all governed by parameters that determine who gets treated a certain way and the amount of their family's resources are required to contribute to the final calculation of the EFC. The changes in these parameters drive the differences we see in the relationship between parents' income and Pell eligibility over time in Figure 1. Many of these parameters are changed automatically each year according to the statutorily determined updating rules. Most of these rules apply an inflation adjustment (based on the CPI-U) to increase parameter values, but some are handled through other calculations, such as the annuity method that governs the annual changes to the Asset Protection Allowance (APA) schedule or the calculations based on the Treasury Department's Statistics of Income file that were used to calculate allowances for state taxes.[5]

Many of the biggest drivers of Pell eligibility are frequent targets for legislative change. While some are also subject to some form of automatic indexing to inflation, others are not, and in all cases the biggest changes we see across my period of study come from Congressional action. Table 2 shows each of the major parameters and how it has changed (in nominal terms) over time. The most important of these is the total maximum Pell Grant, which is largely set through the annual appropriations process and determines the amount received by the large share of recipients who qualify for the maximum award.[6] But two

[5]See 20 U.S.C. § 1087rr, requiring the Secretary to publish annual updated tables for the need-analysis formula, including income-protection allowances, adjusted net worth of a farm or business, the asset-protection allowance computed via an annuity method, assessment schedules, and employment-expense allowances. This section also defines the rules for the updating process in each case.

[6]The Pell Grant is funded by a combination of mandatory and discretionary appropriations, where the total maximum award is the sum of the discretionary base amount and an add-on that comes from mandatory funds. While in some periods, such as from 2013–14 through 2017–18, the mandatory portion of the funding was indexed to CPI, there is no permanent Higher Education Act authorization of indexing to inflation or other automatic updating process, as with portions of the EFC formula. Additionally, the discretionary base of the full maximum Pell level must be set Congress through the appropriations process each year (Dortch, 2024).

other key parameters are also set by Congress: the automatic zero and simplified needs test thresholds. The former determines which students are automatically determined to have a $0 EFC (and thus eligible to receive the maximum Pell award without needing to fill out the FAFSA beyond qualification questions), while the latter determines which students can skip just the asset portions of the form.

Table 2: Changes to Key Pell Formula Parameters Over Time

	Pell Grant		Autozero Threshold	Simplified Needs Test Threshold	Income Protection Allowance	
Award Year	Maximum	Minimum	All Students	All Students	Student's Income	Parents' Income (HH=3, College=1)
2005–2006	4,050	400	15,000	49,999	2,440	17,270
2006–2007	4,050	400	20,000	49,999	2,550	17,970
2007–2008	4,310	400	20,000	49,999	3,000	18,680
2008–2009	4,731	523	20,000	49,999	3,080	19,150
2009–2010	5,350	609	30,000	49,999	3,750	19,730
2010–2011	5,550	555	30,000	49,999	4,500	20,210
2011–2012	5,550	555	31,000	49,999	5,250	20,210
2012–2013	5,550	577	23,000	49,999	6,000	20,410
2013–2014	5,645	582	24,000	49,999	6,130	21,290
2014–2015	5,730	587	24,000	49,999	6,260	21,720
2015–2016	5,775	581	24,000	49,999	6,310	21,890
2016–2017	5,815	591	25,000	49,999	6,400	22,220
2017–2018	5,920	593	25,000	49,999	6,420	22,300
2018–2019	6,095	650	25,000	49,999	6,570	22,810
2019–2020	6,195	650	26,000	49,999	6,660	23,140
2020–2021	6,345	639	26,000	49,999	6,840	23,760
2021–2022	6,495	650	27,000	49,999	6,970	24,200
2022–2023	6,895	692	27,000	49,999	7,040	24,440

Notes: All dollar values are expressed in nominal terms. The Pell Grant Maximum is the maximum award a student can receive in that award year, while the Minimum is the effective minimum award for eligible students. The Autozero Threshold is the income level below which, conditional on meeting categorical requirements such as simplified tax filing eligibility or receipt of a federal means-tested benefit, a student's Expected Family Contribution (EFC) is automatically set to zero. The Simplified Needs Test Threshold is the income level below which, for filers meeting similar categorical criteria, assets are excluded from the federal aid calculation. Income Protection Allowance (IPA) values represent the portion of income excluded from the EFC calculation to allow for basic living expenses. For dependent students, there is an IPA for both the student's own income (shown in the "Student's Income" column, which is the same for all dependent students regardless of household composition) and the parents' income (shown in the "Parents' Income" column for a family of three with one student in college), though the latter varies by household size and number of students enrolled in college.
Sources: Award year-specific Expected Family Contribution Formula Guides (2005–06 through 2022–23), U.S. Department of Education, Office of Federal Student Aid. Pell Grant maximum and minimum award amounts from Congressional Research Service, "Federal Pell Grant Program of the Higher Education Act: How the Program Works, Recent Legislative Changes, and Current Issues" (R45418), Appendix B, available at `https://crsreports.congress.gov/product/pdf/R/R45418`.

In this table we can see that the maximum Pell Grant sometimes stays flat (in nominal terms) and sometimes grows by several hundred dollars. Over our sample period, the income cutoff for automatically receiving the maximum Pell Grant has jumped from $20,000 to

$30,000 in one year, but dropped from $31,000 to $23,000 in another; the threshold for the Simplified Needs Test has never changed (in nominal terms). The maximum Pell award increased in nominal terms by 6.4% between Award Years 2006–07 and 2007–08 (from $4,050 to $4,310), 9.7% the next year (from $4,310 to $4,731), and 13.1% the year after that ($4,731 to $5,350). This Great Recession-era boost to Pell is not matched anywhere else in the series, and is an important source of variation to pay attention to, both for its size and because it coincides with such a consequential economic downturn. There are even larger changes to the IPA values for dependent students across the years I study.[7] Between AYs 2007 and 2008, students went from having $2,550 to $3,000 of income protected, a 17.6% increase. Between AYs 2010 and 2011, the change was 21.8%, while the next year it increased 20% and the years after that 16.7% and 14.3% respectively.

The net effect of all of these changing parts of the formula is clear in Figure 1, where we can see the final total average Pell eligibility for each income groups across formula structures. Figure 2 shows the year-to-year difference in Pell eligibility for a randomly chosen set of award year comparisons. In this plot we can see that the change between any two award years varies greatly across the parents' income distribution. While changes are roughly flat across groups toward the bottom of the distribution, the level of change across different year comparisons is quite different, sometimes positive, sometimes negative, and of quite different magnitudes. In the middle of the distribution, we see that these income groups have year to year changes that are quite different from their peers in the lower and income groups in the same year and from themselves across years. The size of these changes we see is often notable as well, as it is often at or above $1,000. This is a difference in eligibility that exceeds most differences across common regression discontinuity cutoffs.

[7]Though they are bigger in the year-to-year dollar and percentage differences, it is worth noting that, because dependent students' income gets assessed at a rate of 50%, changes in the IPA for this group do not result in dollar-for-dollar reductions to the EFC in the way changes to the maximum award do. Note also that it is only student IPA values that have such disparate changes from year to year across the period of study; updates to the IPA tables that govern the protected income for parents of dependent students and independent students changed by between 0% and 4% over these same years. In general, dependent student income is not a major source of available resources for their households, and so protecting this income from consideration may not be terribly consequential.

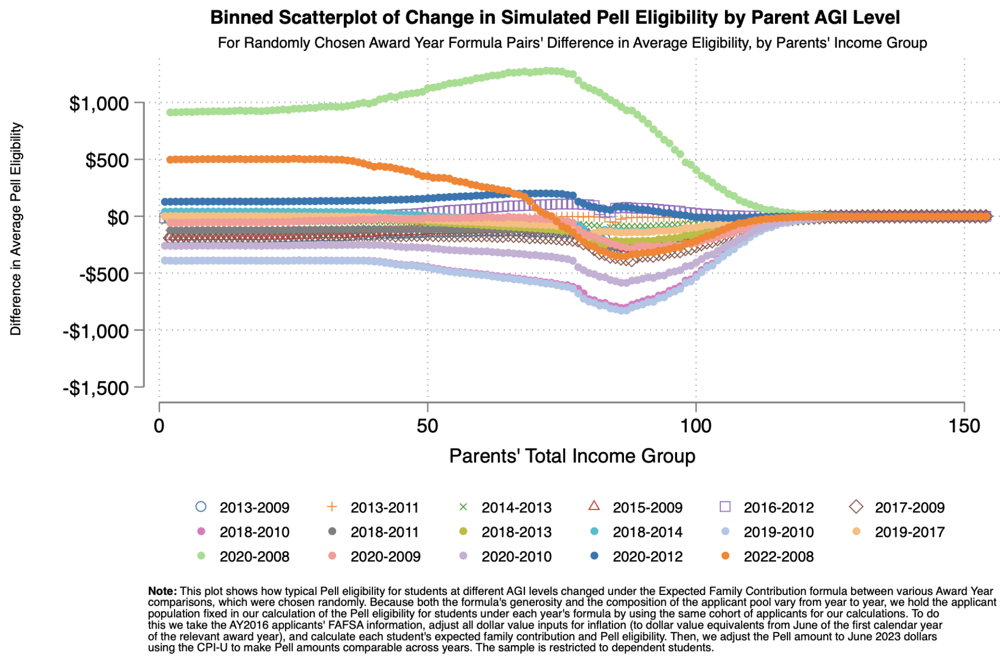
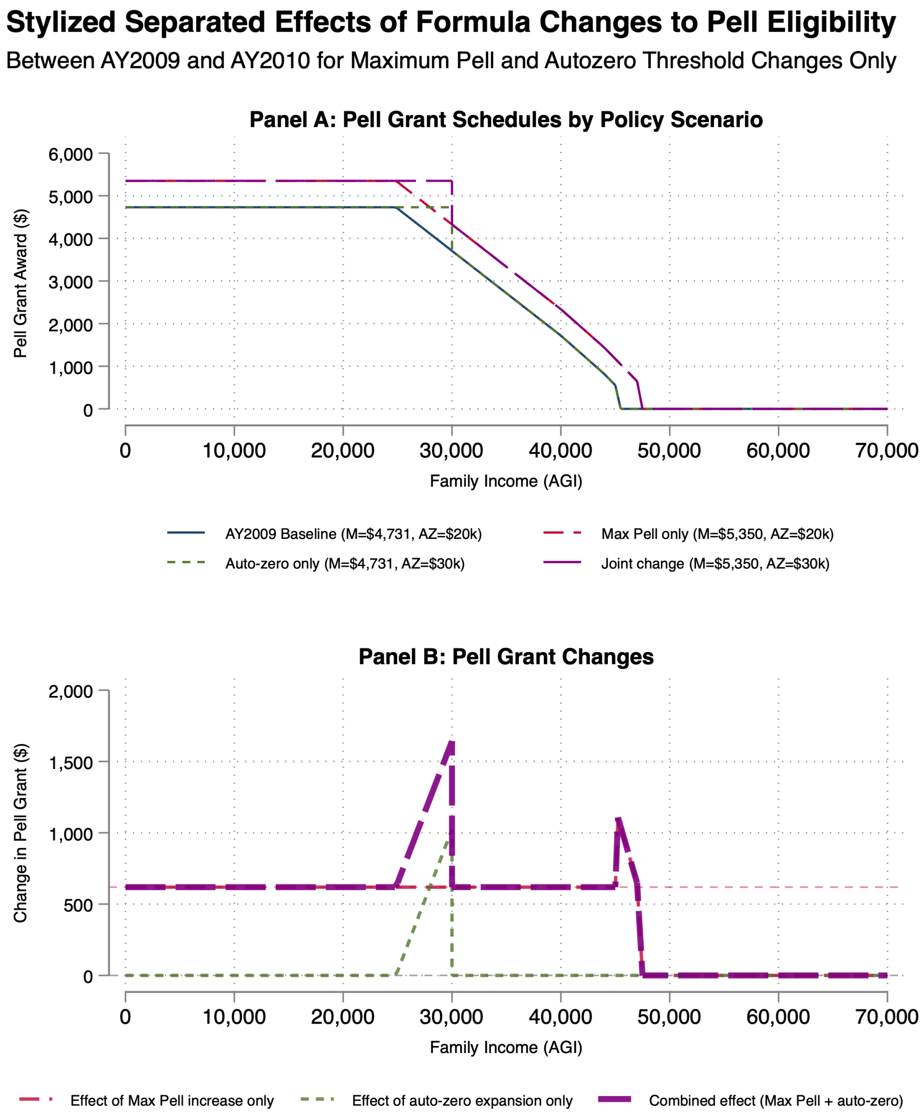


Figure 2: Differences in Simulated Pell eligibility by Parents Income Group Across Award Years

This differential exposure across groups and years reflects the fact that especially consequential changes to the formula come from Congress, which is often focused on the levels of the various input parameters — increasing the maximum Pell grant, for example, is a perpetual policy goal for many in Washington — but from the perspective of changes to eligibility, the middle of the income distribution is where the biggest swings happen. Additionally, modifications to formula inputs often happen in isolation and in ways that offset or amplify each other. Consider the changes made to the maximum Pell grant and the automatic zero threshold between award years 2008-09 and 2009-10. The maximum award jumped $691, while the automatic zero threshold moved $10,000 up the income spectrum to include students whose parents had income below $30,000.

Figure 3 shows, for a stylized version of the true formula, how each of these changes, separately and then in combination, affects Pell eligibility for students at different income



Note: All scenarios apply AY2009 EFC tables (Texas state tax allowance, income protection allowances, employment expense cap, and Social Security wage base). The purple line isolates the joint effect of raising the maximum Pell award to $5,350 and expanding the auto-zero threshold to $30,000. Family assumptions: Texas resident, household size 3, one student in college, single earner, categorical eligibility for auto-zero EFC. Simplified Needs Test assumptions applied: assets excluded from EFC calculation. Discontinuities appear at auto-zero thresholds ($20,000 and $30,000) and at the maximum eligible EFC (where Pell drops to $0). Maximum and minimum Pell awards from Dortch (2024): AY2009 maximum $4,731, minimum $523; AY2010 maximum $5,350, minimum $609.

Figure 3: Stylized Separated Effects of Formula Changes on Pell Eligibility

levels.[8] This simplified version of the true formula is meant to show the basic dynamics at play with just one of the major changes that can happen to the formula over time and the unusual consequences they can have in combination. The first thing to see is that moving the auto-zero threshold does not affect the bottom of the income distribution, only students with parental income between $20,000 and $30,000, who were not already automatically eligible under the previous regime. Second, we can see the increase in the maximum Pell grant translates dollar for dollar to increases in Pell eligibility for students with low EFCs, but as they rise, students are limited to only a portion of the maximum Pell increase that they are eligible to receive.

These changes are shown both separately and in combination on the plot in Panel A, and the way they affect different income groups' change in eligibility is shown in Panel B. Here it is clear that the middle of the income distribution, where students experience most or all of the maximum Pell increase and also benefit from the automatic zero change (moving them from something less than a full Pell grant to the maximum). It also provides a disproportionate bump to students at the top end of the eligibility distribution, for whom the increase in the maximum award makes them newly eligible for the award and so they experience a bump of both the previous minimum award plus the increase amount.

5. Research Design

The idiosyncratic combination of formula elements apparent in Figure 3, when incorporating all of the parameters, results in the varied year-to-year changes apparent in Figure 2. These year-to-year changes, purged of the components that are common to all income groups in an award year and to all award years for each income group, are the core of my identifying variation. The research design I use to take advantage of these eligibility shifts begins with a difference-in-differences estimation strategy. The comparisons we hope to make are between the enrollment and borrowing outcomes of students who receive higher and lower

[8] This plot holds all other elements of the formula at AY2009 rules and assumes a family with a household size of 3 and one child in college. The family is also assumed to live in Texas, have one working parent, and meet all of the categorical eligibility requirements for an automatic zero EFC.

Pell Grant awards. A naïve approach to these regressions would clearly be biased; Pell Grants are awarded predominantly on the basis of income and lower-income students are going to be less likely to persist in college for many reasons besides the size of their Pell grant. Similarly, their borrowing patterns are likely to be systematically different from those of their higher income peers. To accurately estimate the effect of Pell itself, we need to find reasons students get more or less Pell grant aid that will not themselves affect a student's likelihood of persisting other than through the amount of Pell those students receive.

The difference-in-differences strategy takes advantage of income-group and award year fixed effects to isolate the variation in Pell eligibility that is atypical for that award year (relative to other income groups) and unusual for that income group (compared to other other award years). The two-way fixed effects approach compares deviations from typical Pell levels to deviations from typical outcome levels (re-enrollment rates or borrowing). That is, rather than comparing changes in the level of Pell to the level of re-enrollment across income groups over time, the estimator examines how different a group's Pell level is in a given year to how different its' re-enrollment rates are.

To see the importance of this design, consider the types of bias that could arise from using just cross-year variation in Pell levels. The timing of the large changes to both the maximum Pell award and autozero threshold discussed in Section 4 was not coincidental; the Great Recession motivated Congress to increase the award levels as part of the recovery legislation.[9] However, that severe economic downturn also induced many students to go back to college as appealing labor market opportunities became scarce. This common cause of both increased Pell levels and enrollment rates could bias an analysis that depended only on the year-over-year changes in award amounts resulting from the legislative change.

My approach differences out common shocks like these, both across years and across

[9] The 2009–10 appropriations in the American Recovery and Reinvestment Act (ARRA) set the appropriated (base) Pell maximum at \$4,860; combined with the pre-existing mandatory add-on, this yielded a total maximum of \$5,350 for AY 2009–10. See Pub. L. No. 111–5, div. A, tit. VIII ("Student Financial Assistance"), 123 Stat. 183–84 (2009). The increase in the automatic-zero EFC threshold from \$20,000 to \$30,000 (effective beginning with AY 2009–10), and the requirement that this threshold be annually indexed to CPI-U, were enacted earlier in the College Cost Reduction and Access Act (CCRAA) of 2007. See Pub. L. No. 110–84, § 602(a)(2)

income groups. Instead of asking whether students who attend college under a more generous Pell formula regime re-enroll in college at a higher rate, the TWFE estimator identifies effects from atypical variation—income groups receiving amounts that deviate from what would be predicted based on both that group's historical pattern and that year's overall formula generosity. This means that any variation in award amounts that comes from factors that are a persistent feature of how the EFC formula treats an income group (e.g. that lower income groups are eligible for large amounts of Pell) or variation that is specific to a given award year (e.g. that the award years immediately following the great recession were especially generous) is removed from the comparisons I make between groups. The goal is isolate only the variation that comes from quirks in eligibility differences that arise from formula changes.

To execute this estimation approach, I use Equation 3

$$Y_{g,t} = \alpha_g + \lambda_t + \beta\, D_{g,t} + \varepsilon_{g,t} \tag{3}$$

where $Y_{g,t}$ is the outcome of interest (re-enrollment or borrowing) for income group g in award year t; α_g are income-group fixed effects; λ_t are award-year fixed effects; $D_{g,t}$ is the Pell grant amount (in hundreds of dollars) for group g in year t; and $\varepsilon_{g,t}$ is the error term. At this stage we rely on the assumption that after removing income-group and award-year averages, the residual variation in Pell eligibility is not correlated with determinants of $Y_{g,t}$ not captured by our estimating equation. Under this assumption, the coefficient β captures the effect of a \$100 increase in Pell grant eligibility on re-enrollment and loans.

In this context, this assumption means that there are no factors that vary with our outcome measures in the same way that changes to Pell grant eligibility do, after accounting for year and income-group fixed effects. The highly varied residual Pell variation visible in Figure 2 makes such stories hard to conjure. One might be concerned, say, that low-income students are especially responsive to labor market downturns and also atypically targeted for changes to their Pell eligibility. But for such a pattern to be a threat to our identification, both the targeting of low-income students and their responsiveness to the labor market conditions would need to be 1) different than it was for that group in previous years 2) different from other groups in the same year, and 3) would need to vary in the same way as the formula.

Using the potential outcomes framework, we define $Y_{g,t}(d)$ as the outcome for income group g in year t if its Pell eligibility were d (in hundreds of dollars). We observe $Y_{g,t} = Y_{g,t}(D_{g,t})$. Our TWFE estimator identifies the effect of changes in Pell eligibility by comparing groups experiencing different changes to their Pell eligibility. Since Pell is a continuous treatment, our target parameter is the marginal effect of a one-unit (\$100) increase in Pell eligibility:

$$\text{ATE} = \mathbb{E}\left[\frac{\partial Y_{g,t}(d)}{\partial d} \,\middle|\, d = D_{g,t}\right] \tag{4}$$

which our linear specification estimates as the coefficient β from Equation 3.

Since we cannot observe the same group-year under both treatment levels, identification requires what Callaway et al. (2024) label “strong“ parallel trends:

$$\mathbb{E}\big[Y_{g,t}(d) - Y_{g,s}(d)\big] = \mathbb{E}\big[Y_{h,t}(d) - Y_{h,s}(d)\big] \tag{5}$$

This assumption states that, conditional on income-group and award-year fixed effects, the residual trends in enrollment and borrowing for income groups g and h, when observed from period s to period t, would have evolved in parallel had Pell generosity not changed due to formula updates. The “strong“ version of parallel trends requires that groups’ potential outcomes would evolve in parallel across all values of d, meaning that treatment effects are homogeneous across levels of Pell. With this assumption, our TWFE coefficient β from Equation 3 identifies a weighted average of treatment effects across all pairwise comparisons in the data.

One potential remaining source of a spurious relationship between Pell and enrollment or borrowing is the potential for compositional changes in our income groups over time. To see the challenge of compositional changes, consider that the TWFE estimator fundamentally relies on differencing cell means. Our goal is to use the variation created by idiosyncrasies in the formula updating process, but if we use observed Pell amounts in our estimation of Equation 3, β will rely on more than just formula variation. In any year, Pell (d_i) is a function of both applicant characteristics (W_i) and eligibility rules ($\boldsymbol{\theta}_t$), resulting in the individual award function: $d(w; \boldsymbol{\theta}_t)$. The aggregated observed Pell values we would use in our estimation would therefore depend both on changes to the rules and changes to the people the rules are applied to. As a result, any changes to the composition of the groups that form the basis of our aggregates could change both the Pell amounts and the outcomes we observe in a way that is not captured by our income-group fixed effects. For example, if dependent students' own earnings (which are not part of the way we define the parents' total income groups g) increased across income groups between years, this would decrease Pell amounts and could change re-enrollment rates, perhaps because students with higher earnings have a higher opportunity cost of attending college.

This is a common problem in analyses that attempt to use formula variation, and, starting with Currie and Gruber (1996), analysts have approached this challenge by calculating eligibility across formulas for a fixed population of individuals, dividing them into the same groups used by the estimating equation, and instrumenting for observed eligibility with the fixed population's eligibility. Formally, this instrument (which I will follow Cohodes et al. (2016) in calling "fixed simulated eligibility") is defined as: $z_i = \frac{1}{N}\sum_j F(w_j; \boldsymbol{\theta}_t)$, where again $\boldsymbol{\theta}_t$ represents the rules to which each individual i is subject, though not based on those individuals' own set of formula inputs w_i, but rather the formula inputs w_j of members of a fixed population j who share some of individual i's characteristics. Currie and Gruber characterize this instrument as a "convenient parameterization of legislative differences" that allows them to isolate formula-based variation from compositional changes to the population they study.

In my context, to remove the risk of compositional changes across first-year student cohorts, I use the FAFSA inputs for a fixed population of first-year, dependent student FAFSA filers in Associate's or Bachelor's degree programs from the 2015-16 award year to calculate average eligibility amounts for each parents' income group in each award year:

$$Z_{g,t} = \frac{1}{N_g^{2016}} \sum_{j=1}^{N_g^{2016}} d\big(w_j^{2016};\, \boldsymbol{\theta}_t\big) \tag{6}$$

where $Z_{g,t}$ is the fixed-population simulated average Pell for income group g under award-year t rules; N_g^{2016} is the number of base-year (AY 2015–16) individuals in group g; w_j^{2016} are those individuals' FAFSA inputs, deflated where necessary to the appropriate dollars for the relevant award year; $\boldsymbol{\theta}_t$ is the vector of Pell/EFC formula parameters in year t; and $P(\cdot;\cdot)$ maps FAFSA inputs and rules into an eligibility amount. This leads to two estimation approaches, each using the instrument to isolate the clean, formula-based variation. The first is a simple reduced form regression of the outcomes of interest on the instrument:

$$Y_{g,t} = \alpha_g + \lambda_t + \beta^{\mathrm{RF}} Z_{g,t} + \varepsilon^{\mathrm{RF}}_{g,t}. \tag{7}$$

where $Y_{g,t}$ is the outcome for income group g in award year t (re-enrollment share or average student loans); $Z_{g,t}$ is the fixed-population simulated average Pell defined in (6); α_g and λ_t are income-group and award-year fixed effects; and β^{RF} is the reduced-form (ITT) effect of a \$100 increase in $Z_{g,t}$ on $Y_{g,t}$.

The next approach is a two-stage least squares (2SLS) regression where we use the instrument in the first stage to predict either Pell eligibility or Pell disbursements for each income group-by-award year cell:

$$D_{g,t} = \alpha_g + \lambda_t + \pi Z_{g,t} + u_{g,t} \tag{8}$$

$$Y_{g,t} = \alpha_g + \lambda_t + \beta \widehat{D}_{g,t} + \varepsilon_{g,t} \tag{9}$$

where $D_{g,t}$ is the observed Pell measure for group g in year t (eligibility or disbursement; measured in \$100s for interpretation), π is the first-stage coefficient linking the simulated instrument to observed Pell, and β is the 2SLS estimate of the causal effect of a \$100 increase in Pell on $Y_{g,t}$. Fixed effects α_g and λ_t absorb group and year averages. Across specifications, I cluster the standard errors at the income-group-by-award year level.

In both the reduced form and 2SLS approaches, the fixed simulated eligibility instrument allows us to isolate the variation that comes from formula changes. The TWFE estimator calculates how much fixed simulated eligibility for each income group in the 2015-16 population differs in each award year from both the mean award across all income groups in their award year and from the average amount that income group received across all award years. Then, if this fixed population of students would have received an especially large or small Pell grant relative to their historical average or the overall average for that award year, we examine if their enrollment or borrowing behavior was also atypical relative to their own historical average and the award-year wide outcome mean. In the case of the 2SLS approach, this analysis happens for the predicted values of observed Pell eligibility and disbursements that come from the first stage regression of those values on the instrument rather than from de-meaning the instrument itself, as in the reduced form case. As we will see in the results section, because parents' income is far and away the most important input to the Pell formula, the instrument very strongly predicts observed Pell amounts so the results are quite similar across the three specifications that use the instrument, regardless of its exact implementation.

6. Results

The first order outcome of interest for Pell Grants, which are meant to facilitate college attendance by making it possible for low-income students to afford to pay tuition bills, is enrollment. The administrative data available from the Office of Federal Student Aid's databases is limited to FAFSA filers, and so I would be limited in my ability to study the margin of college entry, since federal data would have no record of students who failed to ever file a FAFSA in the first place. As a result, my analysis focuses on persistence, which measures continued enrollment after an initial enrollment spell. My sample is restricted to students who enrolled for at least 30 days in an award year and measured in the following award year to see if they are enrolled at least half time for at least 90 days. This allows me to measure meaningful enrollment spells that occur in both the year in which a student receives their Pell grant and in the following year. The question is whether a large Pell grant in a given award year promotes continuation in a program such that a student is more likely to be enrolled in their second year. This is also why it is important that our sample be restricted to first year students at Associate's and Bachelor's program, where students would be expected to continue attending in their second year if they wish to complete their degree.

Table 3 shows the results from regressions that measure the relationship between the rate of re-enrollment for each income-group in each award year and the Pell grants those students receive. As a benchmark for our instrumental variables estimates, the results begin in column 1 with the results from the two-way fixed effects estimation from Equation 3, which uses observed Pell eligibility as its measure of the treatment rather than fixed simulated eligibility. Because the Pell grant variable here is measured in hundreds of dollars, each coefficient can be interpreted as the effect of a \$100 increase in Pell eligibility in an award year on the re-enrollment rate in the following award year for a given income group. Here, without adjusting for possible confounding from shifting samples of students, we do not observe any statistically or economically significant effect of Pell on re-enrollment. However, once we take advantage of our instrument to measure formula generosity, which we see in columns 2–4, a consistent story emerges.

Table 3: Re-enrollment Effects of Pell Grants (AY2008–2022)

	(1) OLS	**(2) RF**	**(3) IV-Elig**	**(4) IV-Disb**
Pell Eligibility (100s, 2023\$)	-.0000127		.00144***	
	(.0000895)		(.000217)	
Simulated Pell (100s, 2023\$)		.00128***		
		(.000153)		
Pell Disb. (100s, 2023\$)				.00148***
				(.000207)
Intercept	.817***	.768***		
	(.00326)	(.00582)		
N	17,589,261	17,589,261	17,589,261	17,589,261
Adj. R2	0.953	0.954	-0.075	0.058
1st Stage F			301.444	371.950
Inc. group FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

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

Note: All regressions use Department of Education administrative data for award years 2008-2022. The dependent variable is an indicator for re-enrolling at least half-time for 90 days in the subsequent award year (percentage points). All regressions include award year and parents' income bin fixed effects, weight observations by the number of students in each state-year-income cell, and cluster standard errors at the award year × parents' income bin level. Column (1): Ordinary least squares regression of re-enrollment on observed Pell eligibility. Column (2): Imputation approach replacing observed Pell eligibility with simulated Pell eligibility for the student's income group in that award year. Column (3): Two-stage least squares with observed Pell eligibility instrumented by simulated eligibility. Column (4): IV-Disbursement specification instrumenting actual Pell disbursement with simulated Pell eligibility computed by applying each award year's Pell Grant formula to a fixed cohort of students from award year 2015-16. All Pell amounts in hundreds of 2023 dollars.

Column 2 shows the reduced form version of the regression, where re-enrollment rates are regressed directly on the instrument value as outlined in Equation 7 , while columns 3 and 4 show the value for β from Equation 9, each instrumenting for a different observed value. In column 3 we use fixed simulated eligibility to predict calculated eligibility for Pell, while in in column 4 we predict actual Pell disbursements. Across all three specifications, I estimate that an additional \$100 of Pell grants increases re-enrollment by a statistically significant 0.128 - 0.148 percentage points. If we assume these effects scale linearly, this translates to a 1.3 - 1.5 percentage point enrollment effect per $1,000, largely in line with the consensus persistence effects found by Nguyen, Kramer, & Evans (2019) in their meta-analysis of persistence effects. As I describe in Section 3, FSA began requiring colleges to report enrollment information for student who received only a Pell grant but no federal loans in award year 2012–13. For this reason, I also report in Appendix Table A.1 the results for a sample that is restricted only to award years 2012–13 through 2021–22. These estimates are a bit more than half the size, showing an effect between 0.7 and 0.9 percentage points per $1,000.

Despite the fact that this magnitude of effect is in line with past meta-analytic estimates of the effect of grant aid and exceeds the size of the regression discontinuity studies showing little to no effect of the grant on initial enrollment or persistence, the 1.5–1.9% increase in re-enrollment (off of the baseline of 80–87% shown in Table 1) may still leave some question as to why such a large federal investment does not have a larger effect. As I discussed in my overview of the previous studies on this topic, several studies have postulated that other aid often fills the gap that a student might otherwise use Pell to fill. Indeed, one of the more robust findings of this literature is that students reduce their borrowing in response to receiving additional grant aid. Several of the regression discontinuity studies demonstrate this dynamic directly, showing reductions in borrowing that offset the increase in grant aid. The degree of offset varies across studies, ranging from 30% of the additional Pell students receive to more than 100%.

Does this same dynamic hold when we examine a sample of students that includes students other than those around the automatic zero and Pell eligibility cutoffs? Table 4 shows the loan displacement dynamics in my sample of first year dependent students FAFSA filers pursuing Associate's and Bachelor's degrees. It shows the same set of specifications as Table 3, but with the average federal loan amount (in hundreds of dollars) as the outcome. This value includes all Federal unsubsidized and subsidized Stafford or FFEL loans awarded to students in each income group in each award year. Here we see a similar, if less pronounced, dynamic of substitution between Pell grants and federal loan borrowing. Across our three specifications that use the instrument to measure Pell eligibility, we see that each additional dollar of Pell eligibility or receipt reduces borrowing by about 20 cents.

Table 4: Total Loans Disbursed for Award Year and Pell Grant Amounts (AY2008-22)

	(1) OLS	**(2) RF**	**(3) IV-Elig**	**(4) IV-Disb**
Pell Eligibility (100s, 2023$)	-1.1***		-.211*	
	(.0384)		(.122)	
Simulated Pell (100s, 2023$)		-.187		
		(.115)		
Pell Disb. (100s, 2023$)				-.217*
				(.128)
Intercept	112***	78.7***		
	(1.39)	(4.38)		
N	17,589,261	17,589,261	17,589,261	17,589,261
Adj. R2	0.977	0.971	0.077	0.031
1st Stage F			301.444	371.950
Inc. group FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

*** p<.01, ** p<.05, * p<.1

Note: All regressions use Department of Education administrative data for award years 2008-2022. The dependent variable is total federal student loans disbursed (excluding Parent PLUS) in hundreds of 2023 dollars. All regressions include award year and parents' income bin fixed effects, weight observations by the number of students in each state-year-income cell, and cluster standard errors at the award year × parents' income bin level. Column (1): Ordinary least squares regression of loan disbursement on observed Pell eligibility. Column (2): Imputation approach replacing observed Pell eligibility with simulated Pell eligibility for the student's income group in that award year. Column (3): Two-stage least squares with observed Pell eligibility instrumented by simulated eligibility. Column (4): IV-Disbursement specification instrumenting actual Pell disbursement with simulated Pell eligibility computed by applying each award year's Pell Grant formula to a fixed cohort of students from award year 2015-16. All Pell amounts in hundreds of 2023 dollars.

Though these estimates are only marginally significant, the dynamic is considerably more pronounced in Appendix Table 5, which excludes the years in our sample period where institutions were not required to report enrollment for students who only received Pell grants. In just the AY 2013 - 2022 period we see that each additional dollar of Pell grants reduces borrowing by $0.38 - $0.53. The difference between these two sets of results underscores the fact that my main coefficients represent an average over two samples, one where the extensive margin of borrowing is not available to observe (AY 2008 - 2012) and the other where we know the enrollment behavior of non-borrowers (AY 2013 - 2022). The fact that the estimates nearly double suggests that a considerable portion of the effect of borrowing reductions comes from students who stop borrowing completely in response to Pell increases.

Still, even at these higher levels of estimated borrowing displacement, my estimates are at the lower end of estimates from other studies. A key hypothesis of the existing literature has been that the level of substitution between aid types means that marginal additional Pell

Table 5: Total Loans Disbursed for Award Year and Pell Grant Amounts (2013+)

	(1) OLS	(2) RF	(3) IV-Elig	(4) IV-Disb
Pell Eligibility (100s, 2023$)	-.483***		-.525***	
	(.0609)		(.0692)	
Simulated Pell (100s, 2023$)		-.381***		
		(.0514)		
Pell Disb. (100s, 2023$)				-.527***
				(.0728)
Intercept	81.9***	77.9***		
	(2.44)	(2.07)		
N	12,852,641	12,852,641	12,852,641	12,852,641
Adj. R2	0.998	0.998	0.098	0.026
1st Stage F			755.009	1670.368
Inc. group FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

*** p<.01, ** p<.05, * p<.1

Note: All regressions use Department of Education administrative data for award years 2013-2022. The dependent variable is total federal student loans disbursed (excluding Parent PLUS) in hundreds of 2023 dollars. All regressions include award year and parents' income bin fixed effects, weight observations by the number of students in each state-year-income cell, and cluster standard errors at the award year × parents' income bin level. Column (1): Ordinary least squares regression of loan disbursement on observed Pell eligibility. Column (2): Imputation approach replacing observed Pell eligibility with simulated Pell eligibility for the student's income group in that award year. Column (3): Two-stage least squares with observed Pell eligibility instrumented by simulated eligibility. Column (4): IV-Disbursement specification instrumenting actual Pell disbursement with simulated Pell eligibility computed by applying each award year's Pell Grant formula to a fixed cohort of students from award year 2015-16. All Pell amounts in hundreds of 2023 dollars.

money does not increase the total aid available to students. This hypothesis suggests that in the absence of these substitution patterns, we would see larger effects of the Pell grant on enrollment outcomes because Pell does not crowd out other aid. This raises the question of whether the larger persistence effects I find compared to the existing literature are related to the lower substitution rates I observe. Certainly, this impressionistic comparison of my two main estimates to the existing literature does not tell us much. However, we can use the scale of my administrative data to ask whether we observe a relationship between the loan substitution effects and enrollment effects across subsamples. A simple starting place for this analysis is to look at students pursuing different types of credentials. The main differences in degree types available to me in the data are a split between Bachelor's degrees and two types of Associate's degrees, one focused on occupational or technical subjects and the other focused on general education or transfer students.[10]

Table 6 shows how the treatment effects we measure vary by these categories. Here

[10] These descriptions come directly from the FAFSA, where students select the degree they are pursuing and the available options distinguish between "occupational or technical" and "general education or transfer" Associate's programs, though no official definition exists.

we can see the stark differences in enrollment effects between BA matriculants and those pursuing an AA degree. BA recipients are the dominant student type in my sample and therefore have a treatment effect that most resembles the overall treatment effect, AA recipients remain a sizable portion of our sample and show much larger effects, with an additional \$1,000 in Pell increasing re-enrollment in occupational and technical AA programs by 2.5 percentage points and by 3.2 percentage points in general education and transfer AAs. In our first hint that enrollment effects could be related to the substitution dynamics we observe in our loan regressions, we can see that for BA degree aspirants, an increase of \$1 in Pell reduces borrowing by 32 cents, while for AA degree recipients, there is no apparent effect on borrowing for career oriented fields, but a statistically significant effect of Pell crowding in loans at a rate of \$0.49 per dollar of Pell received (though this latter effect is not apparent when restricting the sample just to award year 2013 and later).

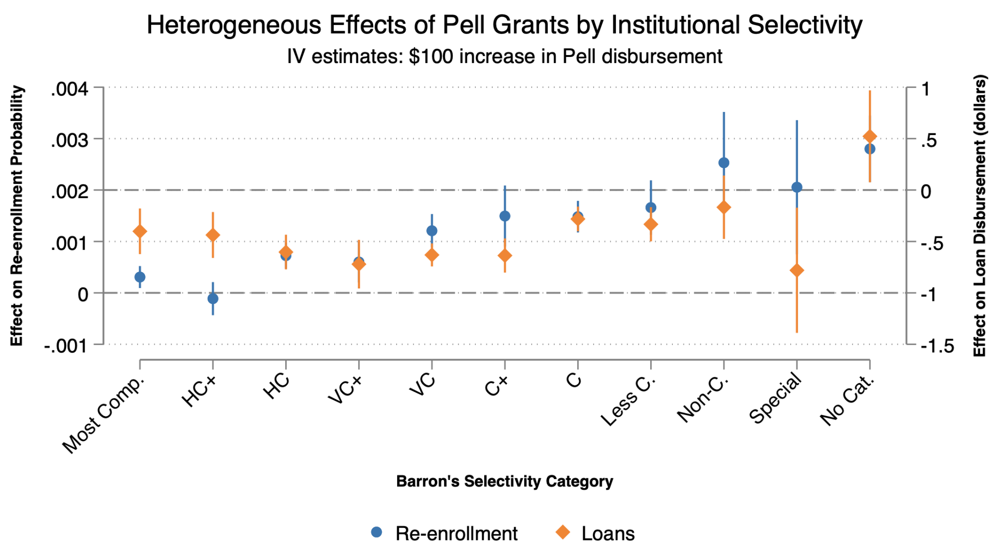
Table 6: Re-enrollment Rates and Pell Grant Amounts - Disaggregated (AY2008-22)

	(1) BA	**(2) AA-Occ**	**(3) AA-Gen**
Pell Disb. (100s, 2023\$)	.00114***	.00248***	.00324***
	(.000158)	(.000418)	(.000554)
N	13,225,104	1,951,661	2,412,496
Adj. R2	.0187	.134	.164
1st Stage F	439.727	196.496	140.265
Inc. group FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes

*** p<.01, ** p<.05, * p<.1

Note: All regressions use Department of Education administrative data for award years 2008-2022 and estimate the IV-Disbursement specification separately by degree type. The dependent variable is an indicator for re-enrolling at least half-time for 90 days in the subsequent award year (percentage points). All regressions instrument actual Pell disbursement with simulated Pell eligibility computed by applying each award year's Pell Grant formula to a fixed cohort of students from award year 2015-16, include award year and parents' income bin fixed effects, weight observations by the number of students in each state-year-income cell, and cluster standard errors at the award year × parents' income bin level. Column (1): Students pursuing a first bachelor's degree. Column (2): Students pursuing associate's degrees in occupational/technical programs (FAFSA degree code 3). Column (3): Students pursuing associate's degrees in general education/transfer programs (FAFSA degree code 4). All Pell amounts in hundreds of 2023 dollars.

Another dimension of heterogeneity we have available to us is to examine effects by the selectivity level of the institution that students are attending. In Figure 2 I present treatment effects of an additional \$100 in Pell on both enrollment and loans side by side, to show how they both move in tandem, with effects decreasing in selectivity. This coefficient plot first shows the coefficients from separate regressions for each Barron's selectivity category, using the same method described above where I use fixed simulated eligibility to instrument for observed Pell disbursements. First in the circular markers we can see the persistence effects, which range from near-zero at the high end of the selectivity distribution to between 1.7 and 2.8 percentage point bump to re-enrollment rates per $1,000 of additional Pell grants. The dynamics for loans are similar in that these effects are also decreasing in selectivity, with the exception of the two most competitive categories ("Most Competitive" and "Highly Competitive +"), where students decrease their borrowing a bit less in response to Pell than students at "Competitive+" through "Highly Competitive" institutions. Still, in the non-competitive schools and for institutions not ranked by Barrons, the estimates are a (statistically insignificant) $0.17 reduction in borrowing and $0.52 increase in borrowing per extra $1 of Pell received, respectively.



Notes: Points show IV-disbursement estimates with 95% confidence intervals. Left axis: Re-enrollment probability (0-1 scale). Right axis: Loan amounts in $100s of 2023 dollars. All regressions instrument actual Pell disbursement with simulated eligibility computed by applying each award year's Pell formula to a fixed cohort of students from award year 2015-2016, include award-year and parents' income-bin fixed , effects weight observations by the number of students in each cell, and cluster standard errors at the award-year × income-bin level. Categories with fewer than 50 eligible cells are omitted. Sample: First-year, federally-aided, dependent student FAFSA filers in Bachelor's or Associate's programs enrolled for at least 30 days, award years 2008-2022.

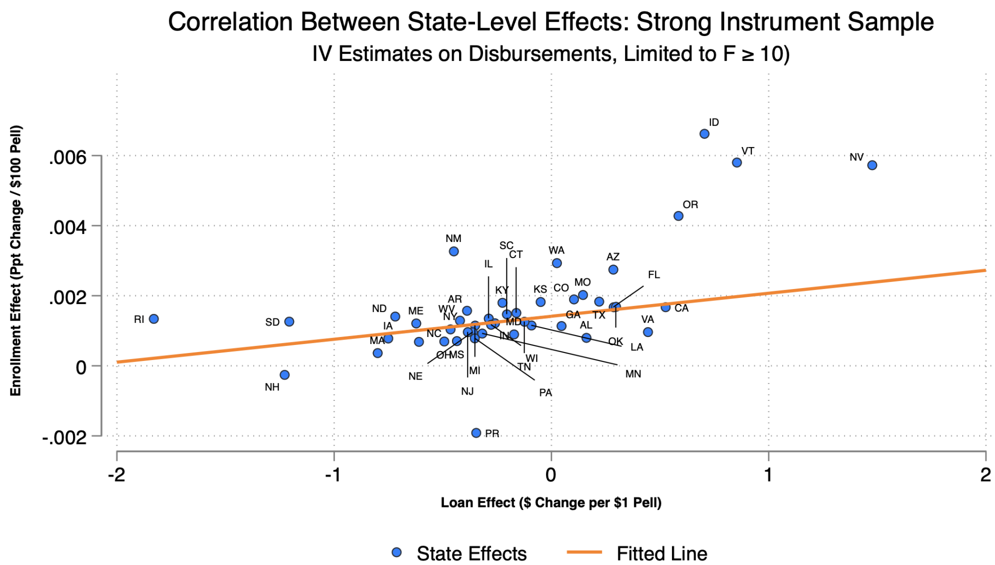
Figure 4: Heterogenous Effects of Pell Grants on Enrollments and Loans by Institutional Selectivity

This pattern is consistent with my findings for Associate's and Bachelor's degrees, as students pursuing AAs are almost exclusively doing so at institutions not ranked by Barron's. However, the additional variation in aid dynamics provides us with more evidence of how tightly loan responsiveness and enrollment behavior track each other. Figure 4 shows that the relationship is tightest for "Highly Competitive" institutions and below, where changes to the coefficients across these outcomes seems to rise proportionately as we move down the selectivity groupings. The jump is especially notable in the institutions not categorized by Barron's, where the enrollment effect is nearly 3 percentage points per $1,000 in Pell aid and loan effects are also very different from the other categories, suggesting $0.52 cent crowd-in of loan aid per $1,000 of Pell aid provided.

These dynamics are consistent with the different dependence on both loans and Pell across the Barron's groupings. In my data for the AY 2008–22 period, 35–43% of students receive Pell at the high end of the competitiveness distribution ("Very Competitive" or more selective). This is more than 10 percentage points lower than the Competitive and Competitive+ institutions, where the rate is 52–53% and over 20 percentage points lower than Less Competitive (62%), Non-Competitive (66%) and uncategorized (77%) institutions. Across these groups, borrowing rates also differ, though the biggest difference lies between Barron's institutions that are at least somewhat competitive ("Less Competitive" or higher), where borrowing rates are between 80–88%, and non-competitive and uncategorized institutions, where 72% an 48% of students borrow, respectively. Together these facts mean that students at open access institutions are much more reliant on Pell grants and, at least for students at colleges not categorized by Barron's, much less reliant on federal student loans. Given these student aid patterns, we might not be surprised that the enrollment effects of the Pell grant are so much more pronounced for students at these open admission institutions because Pell is often their only source of funding, as students at their institutions are either unlikely or unable to borrow.

TK stacked table

Finally, we can examine the interplay of the Pell and loan effects for an even wider set of subgroups: states. State and local financial aid is an important part of the college funding landscape and can play either an enhancing or offsetting role relative to federal financial aid. For example, Eng & Matsudaira (2021) posit that state aid policies are an important source of variation in how Pell affects the total aid available to students because some state formulas crowd-in other aid as students become eligible for an automatic zero EFC, while others crowd out such aid as more Pell becomes available. In this study, my best proxy for such dynamics are the estimates I have for how responsive loan aid is to additional Pell generosity by state. Figure 5 shows the relationship between these estimates at the state level for our same instrumental variables specification as Figure 4 above.[11] On the X-axis we see the degree to which a state's students change their borrowing in response to an additional $1 in Pell grants, with most states ranging from reducing borrowing by less than $1 per dollar of additional Pell and many increasing their borrowing in response.



Note: Each point represents a U.S. state. The horizontal axis shows the estimated causal effect of a $100 increase in Pell Grant disbursement on total federal student loan disbursement (in hundreds of 2023 dollars); the vertical axis shows the corresponding effect on the probability of re-enrolling for at least 90 days in the subsequent academic year (in percentage points). Both coefficients are estimated via two-stage least squares, instrumenting actual Pell disbursement with simulated Pell eligibility calculated by applying the Pell Grant formula from each award year to a fixed population of students from award year 2015-16. All regressions include academic year and parents' income bin fixed effects, are weighted by the number of students per state-year-income cell, and cluster standard errors at the academic year × parents' income bin level. The sample includes all states with at least 50 state-year-income cells. The fitted line represents a student-weighted linear regression of enrollment effects on loan effects. Sample period: award years 2008-2022.

Figure 5: Heterogenous Effects of Pell Grants on Enrollments and Loans by State

Whatever the sign of the effect, as we move from left to right on the x-axis, states become less likely to reduce their borrowing in response to additional Pell. Though this does not

[11] The scatter plot window of Figure 5 is restricted to loan effects between -$2 and +$2 and enrollment effects between -0.2pp and 0.8pp to optimize visual clarity for the 45 states in this range. Five outliers (DE, HI, MT, UT, and OTHER) with more extreme estimates are omitted from the display but included in computing the fitted line with full student weights.

guarantee that students' total aid packages are affected in the same way, federal aid is an important source of funding for many students, so signs of substitution between Pell and loans in a state means that students in those places would need to receive grant aid from other sources that more than fully compensated for the loss of loans to keep their total aid package from declining in total value. On the Y-axis of this plot we see the corresponding enrollment effects for each state. When the two sets of estimates are combined, we can see that in states where there is less substitution between loans and Pell we also see enrollment impacts that are larger. Here again, the relationship between these dynamics suggests that aid displacement is an important part of the mechanism that determines the enrollment effects we observe.

7. Discussion

This paper set out to provide new estimates of the effect of the Pell grant on a wider set of students than has previously been studied and in the process shed new night on an apparent puzzle: why do well-identified recent studies using regression discontinuity designs find minimal or zero effects on enrollment and persistence, running counter to the intended policy effects of this large federal program and the broader literature on grant aid? Compared to recent regression discontinuity studies of the Pell Grant, which focus on students near the automatic zero EFC and Pell eligibility thresholds, my analysis takes advantage aggregated administrative data from the US Department of Education for the full population of dependent students who receive federal aid over the period from 2008 through 2022. This allows me to study a wider range of students than previous work, including the lowest income students, who do not have enough resources to be on the margin of automatically receiving the maximum Pell grant, and middle income students, who sit in the range between an automatic zero EFC and total phase out of eligibility.

With a more complete view into the population of potentially affected students and a larger range of dosages, I observe a larger effect of the Pell Grant on college persistence than has been observed in the past. I find that $1,000 of additional Pell eligibility or disbursement increases persistence to the following year by 1.3 to 1.5 percentage points. I also document significant loan displacement: each additional dollar of Pell reduces federal loan borrowing by 20 to 53 cents, depending on the sample and specification, indicating that Pell often substitutes for rather than supplements other forms of financial aid.

Additionally, my ability to look at heterogeneity across degree levels, institution types, and state contexts allows me to shine a light on the sizable subpopulation of students for whom the effect of Pell is especially large. This group includes students who are pursuing associates degrees and those enrolled at open access institutions. They experience persistence effects that are twice to three times the size of the main effect for the full population of students, which mostly consists of BA students. In both cases, these more responsive students are also less likely to respond to additional Pell availability by reducing their borrowing. This suggests that the degree of substitution between grants and loans (or other forms of aid) is an important mechanism through which enrollment is affected by Pell grant.

If other forms of aid can provide students with the money they need to cover the cost of enrollment, even if those funds come in a more costly type of credit such as federal student loans, changes to the Pell grant at current margins may not move enrollment rates as much as we might hope. The best evidence of this type of substitution comes from my cross state-analysis, where we can see that in states where students have a lower rate of substitution between Pell and loans, the Pell grant also has a larger enrollment impact. Taken together these findings suggest that in some parts of the US higher education system the Pell grant serves mostly as a debt reduction tool for students who are simply choosing between ways to pay for college, while in others it is the main or only form of student aid and additional Pell dollars provide a real expansion of available funds to pay for college.

This pattern emphasizes the importance of understanding responses of other actors in the complicated college financing system when trying to evaluate the value of a program such as the Pell grant based on impact estimates in the current equilibrium. Partly because the Pell grant does not cover the full cost of attendance for many students, the college financing system has a number of additional supports that fill the remaining gap. From federal loans and state grant programs to institutional aid and private scholarships, students often have a number of financing options available to them. However, these options have been developed in a funding context that has, at least since 1973, featured the availability of a large grant from the federal government. If that program were to suddenly disappear or be dramatically cut, its not at all obvious that the current alternatives to the Pell grants would be able to fully compensate for this pillar of affordability.

Several limitations of this study suggest important directions for future work. First, my analysis focuses on year-to-year persistence (re-enrollment in the following award year) rather than degree completion. While persistence is a meaningful outcome and a necessary precondition for completion, it would be valuable to extend this analysis to longer-term outcomes. Second, while I provide evidence that loan displacement mediates the relationship between Pell increases and enrollment effects, my measures of loan displacement are proxies for the underlying aid packaging dynamics. A more complete understanding would require detailed data on institutional grant aid, state grant aid, and private scholarships for the full population of Pell recipients—data that are not available at a national scale in a way that would allow the type of analysis I do across time and income groups here. Future research could use state-specific administrative data linking FAFSA records to detailed aid packages to characterize more precisely how different states' and institutions' aid systems create "crowd-in" versus "crowd-out" environments. Third, I study dependent students in their first year of college. While this population is large and policy-relevant for the Pell grant, independent students and students in later years of college may respond differently to Pell Grant changes. Extending this analysis to later years and to independent students would provide a more complete picture of Pell's effects across the full college lifecycle.

Nevertheless, my estimates are able to provide new evidence that the Pell grant, at least over the period from AY2008–22, has increased the chance a student will persist from their first into their second year in college by 1.3–1.5 percentage for every additional $1,000 available to students. This estimate is largely in line with metaanalytic estimates of grant aid generally on persistence (Nguyen et al, 2019). Still, estimates for Associate's degree students and those attending open access institutions are much larger, with such enrollees seeing a boost to their chance returning to school for a second year of 2.5–3.2 percentage points per $1,000 of new Pell money. This effect seems to operate through students who do not have recourse to other financial aid funds, suggesting that the highest impact investments in Pell should be targeted to students who are not likely to borrow to pay for college otherwise. In any case, policymakers must be careful about judging the value of the Pell grant program as a whole based on estimates of its impact on enrollment at current margins, since many students can find funding to plug holes in their financial aid packages resulting from small reductions in Pell availability, but would not be able to replace the entirety of this essential funding source.

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Appendix A: 2013–2022 Sample Robustness

This appendix presents results for the restricted sample of award years 2013–2022, the period when enrollment data was available for all federal aid recipients, including students receiving only Pell grants without loans. This restriction addresses the compositional change that occurred in AY2013 when the Department of Education began requiring institutions to report enrollment status for all aid recipients rather than only borrowers. Tables A.1, A.2, and A.3 present the same specifications as their main text counterparts but restrict the sample to award years 2013–2022. Figure A.1 and Figure A.2 similarly present the heterogeneity analyses for the restricted sample period.

Table A.1: Re-enrollment Effects of Pell Grants (2013–2022 Sample)

	(1) OLS	**(2) RF**	**(3) IV-Elig**	**(4) IV-Disb**
Pell Eligibility (100s, 2023$)	.000372**		.000941***	
	(.000149)		(.000252)	
Simulated Pell (100s, 2023$)		.000684***		
		(.000172)		
Pell Disb. (100s, 2023$)				.000944***
				(.000239)
Intercept	.786***	.773***		
	(.00602)	(.0069)		
N	12,852,641	12,852,641	12,852,641	12,852,641
Adj. R2	0.987	0.987	-0.004	0.056
1st Stage F			755.009	1670.368
Inc. group FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

*** p<.01, ** p<.05, * p<.1

Note: All regressions use Department of Education administrative data for award years 2013-2022. The dependent variable is an indicator for re-enrolling at least half-time for 90 days in the subsequent award year (percentage points). All regressions include award year and parents' income bin fixed effects, weight observations by the number of students in each state-year-income cell, and cluster standard errors at the award year × parents' income bin level. Column (1): Ordinary least squares regression of re-enrollment on observed Pell eligibility. Column (2): Imputation approach replacing observed Pell eligibility with simulated Pell eligibility for the student's income group in that award year. Column (3): Two-stage least squares with observed Pell eligibility instrumented by simulated eligibility. Column (4): IV-Disbursement specification instrumenting actual Pell disbursement with simulated Pell eligibility computed by applying each award year's Pell Grant formula to a fixed cohort of students from award year 2015-16. All Pell amounts in hundreds of 2023 dollars.

Table A.2: Total Loans Disbursed and Pell Grant Amounts (2013–2022 Sample)

	(1) OLS	**(2) RF**	**(3) IV-Elig**	**(4) IV-Disb**
Pell Eligibility (100s, 2023$)	-.483***		-.525***	
	(.0609)		(.0692)	
Simulated Pell (100s, 2023$)		-.381***		
		(.0514)		
Pell Disb. (100s, 2023$)				-.527***
				(.0728)
Intercept	81.9***	77.9***		
	(2.44)	(2.07)		
N	12,852,641	12,852,641	12,852,641	12,852,641
Adj. R2	0.998	0.998	0.098	0.026
1st Stage F			755.009	1670.368
Inc. group FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

*** p<.01, ** p<.05, * p<.1

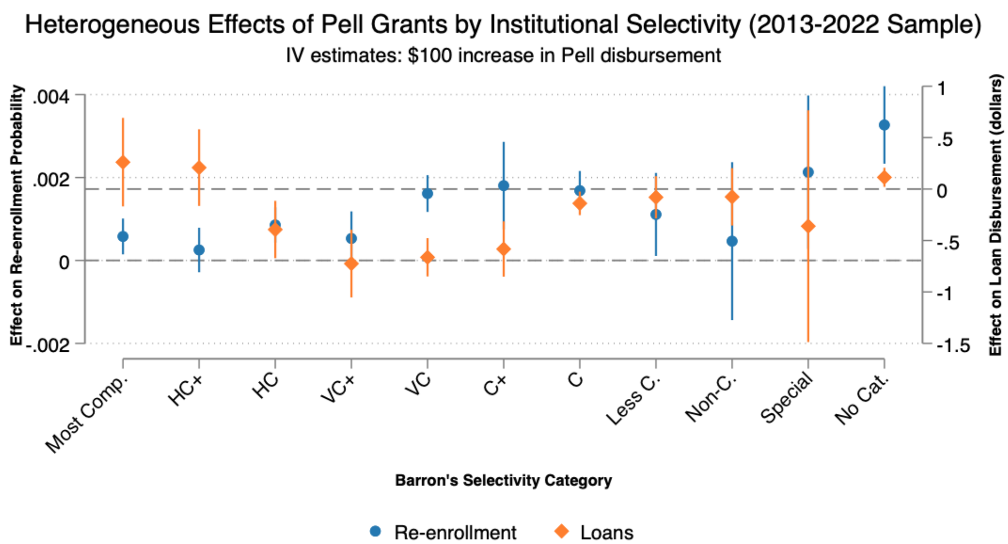
Note: All regressions use Department of Education administrative data for award years 2013-2022. The dependent variable is total federal student loans disbursed (excluding Parent PLUS) in hundreds of 2023 dollars. All regressions include award year and parents' income bin fixed effects, weight observations by the number of students in each state-year-income cell, and cluster standard errors at the award year $\times$ parents' income bin level. Column (1): Ordinary least squares regression of loan disbursement on observed Pell eligibility. Column (2): Imputation approach replacing observed Pell eligibility with simulated Pell eligibility for the student's income group in that award year. Column (3): Two-stage least squares with observed Pell eligibility instrumented by simulated eligibility. Column (4): IV-Disbursement specification instrumenting actual Pell disbursement with simulated Pell eligibility computed by applying each award year's Pell Grant formula to a fixed cohort of students from award year 2015-16. All Pell amounts in hundreds of 2023 dollars.

Table A.3: Re-enrollment Effects by Degree Type (2013–2022 Sample)

	(1) BA	**(2) AA-Occ**	**(3) AA-Gen**
Pell Disb. (100s, 2023$)	.000897***	.00272***	.00308***
	(.00015)	(.000758)	(.000724)
N	9,499,826	1,382,897	1,969,918
Adj. R2	.0398	.0787	.0638
1st Stage F	2043.390	442.275	378.493
Inc. group FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes

*** p<.01, ** p<.05, * p<.1

Note: All regressions use Department of Education administrative data for award years 2013-2022 and estimate the IV-Disbursement specification separately by degree type. The dependent variable is an indicator for re-enrolling at least half-time for 90 days in the subsequent award year (percentage points). All regressions instrument actual Pell disbursement with simulated Pell eligibility computed by applying each award year's Pell Grant formula to a fixed cohort of students from award year 2015-16, include award year and parents' income bin fixed effects, weight observations by the number of students in each state-year-income cell, and cluster standard errors at the award year $\times$ parents' income bin level. Column (1): Students pursuing a first bachelor's degree. Column (2): Students pursuing associate's degrees in occupational/technical programs (FAFSA degree code 3). Column (3): Students pursuing associate's degrees in general education/transfer programs (FAFSA degree code 4). All Pell amounts in hundreds of 2023 dollars.



Notes: Points show IV-disbursement estimates with 95% confidence intervals. Left axis: Re-enrollment probability (0-1 scale). Right axis: Loan amounts in $100s of 2023 dollars. All regressions instrument actual Pell disbursement with simulated eligibility computed by applying each award year's Pell formula to a fixed cohort of students from award year 2015-2016, include award-year and parents' income-bin fixed , effects weight observations by the number of students in each cell, and cluster standard errors at the award-year × income-bin level. Categories with fewer than 50 eligible cells are omitted. Sample: First-year, federally-aided, dependent student FAFSA filers in Bachelor's or Associate's programs enrolled for at least 30 days, award years 2013-2022.

Figure A.1: Heterogeneous Effects of Pell Grants on Enrollments and Loans by Institutional Selectivity (2013–2022 Sample)

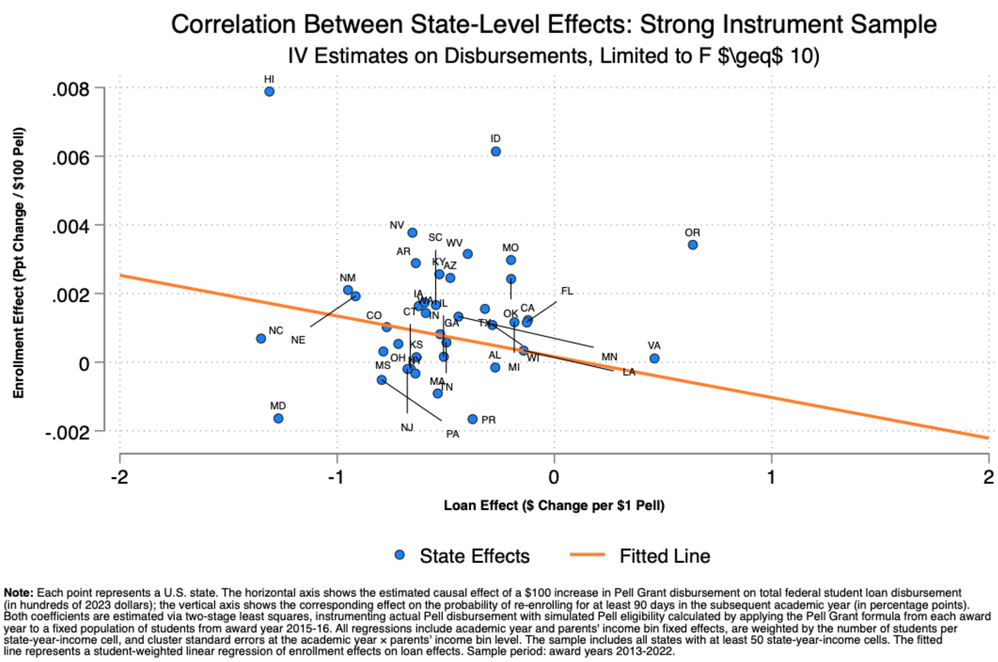


Figure A.2: Heterogeneous Effects of Pell Grants on Enrollments and Loans by State (2013–2022 Sample)

Appendix B: Detailed Coefficient Estimates by Selectivity

This appendix provides detailed coefficient estimates for the heterogeneous treatment effects by institutional selectivity presented in Figure 4 and Appendix Figure A.1. Tables B.1 and B.2 report results for the full sample (AY2008–2022), while Tables B.3 and B.4 report results for the restricted sample (AY2013–2022). Each table presents results from separate instrumental variables regressions for students attending institutions in different Barron's selectivity categories, using the same specification as the main results. Detailed state-by-state coefficient estimates are available upon request.

Table B.1: Re-enrollment Effects by Barron's Selectivity Category (Full Sample, AY2008–2022)

	MC	**HC+**	**HC**	**VC+**	**VC**	**C+**	**C**	**LC**	**NC**	**Spec**	**None**
Pell Disb. (100s, 2023$)	0.0003***	-0.0001	0.0007***	0.0006***	0.0012***	0.0015***	0.0015***	0.0017***	0.0025***	0.0021***	0.0028***
	(0.0001)	(0.0002)	(0.0001)	(0.0002)	(0.0002)	(0.0003)	(0.0002)	(0.0003)	(0.0005)	(0.0007)	(0.0003)
N	496,658	394,968	815,989	495,638	2,034,721	521,056	4,285,330	913,984	397,183	89,838	7,119,562
Adj. R2	-.00403	-.0045	-.00857	.00298	-.0606	.00754	.0243	.0482	.06	-.00266	.184
1st Stage F	378.320	387.803	510.322	513.961	514.375	446.853	466.709	362.232	251.121	252.086	165.308
Inc. group FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

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

Note: All regressions use Department of Education administrative data for award years 2008-2022 and estimate the IV-Disbursement specification separately by institutional selectivity. The dependent variable is an indicator for re-enrolling at least half-time for 90 days in the subsequent award year (percentage points). All regressions instrument actual Pell disbursement with simulated Pell eligibility computed by applying each award year's Pell Grant formula to a fixed cohort of students from award year 2015-16, include award year and parents' income bin fixed effects, weight observations by the number of students in each state-year-income cell, and cluster standard errors at the award year × parents' income bin level. Column headers show Barron's selectivity categories: (1) Most competitive, (2) Highly competitive +, (3) Highly competitive, (4) Very competitive +, (5) Very competitive, (6) Competitive +, (7) Competitive, (8) Less competitive, (9) Non-competitive, Specized, (11) No category. Only categories with N≥50 and sufficient instrument variation included. All Pell amounts in hundreds of 2023 dollars.

Table B.2: Loan Effects by Barron's Selectivity Category (Full Sample, AY2008–2022)

	MC	**HC+**	**HC**	**VC+**	**VC**	**C+**	**C**	**LC**	**NC**	**Spec**	**None**
Pell Disb. (100s, 2023$)	-0.4014***	-0.4371***	-0.6030***	-0.7207***	-0.6312***	-0.6368***	-0.2813***	-0.3327***	-0.1673	-0.7803**	0.5224**
	(0.1132)	(0.1138)	(0.0860)	(0.1206)	(0.0569)	(0.0848)	(0.0613)	(0.0847)	(0.1571)	(0.3104)	(0.2281)
N	496,658	394,998	816,021	495,638	2,034,721	521,148	4,285,330	914,119	397,506	106,399	7,119,562
Adj. R2	.0605	.0639	.156	.0278	.179	.0633	.131	.0745	.029	-.0149	-.0973
1st Stage F	378.320	387.755	510.316	513.961	514.375	446.821	466.709	362.215	251.635	348.798	165.308
Inc. group FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

*** p<.01, ** p<.05, * p<.1

Note: All regressions use Department of Education administrative data for award years 2008-2022 and estimate the IV-Disbursement specification separately by institutional selectivity. The dependent variable is total federal student loans disbursed (excluding Parent PLUS) in hundreds of 2023 dollars. All regressions instrument actual Pell disbursement with simulated Pell eligibility computed by applying each award year's Pell Grant formula to a fixed cohort of students from award year 2015-16, include award year and parents' income bin fixed effects, weight observations by the number of students in each state-year-income cell, and cluster standard errors at the award year $\times$ parents' income bin level. Column headers show Barron's selectivity categories: (1) Most competitive, (2) Highly competitive +, (3) Highly competitive, (4) Very competitive +, (5) Very competitive, (6) Competitive +, (7) Competitive, (8) Less competitive, (9) Non-competitive, Specized, (11) No category. Only categories with sufficient sample sizes and instrument variation are included. All Pell amounts in hundreds of 2023 dollars.

Table B.3: Re-enrollment Effects by Barron's Selectivity Category (2013–2022 Sample)

	MC	**HC+**	**HC**	**VC+**	**VC**	**C+**	**C**	**LC**	**NC**	**Spec**	**None**
Pell Disb. (100s, 2023$)	0.0006***	0.0003	0.0009***	0.0005	0.0016***	0.0018***	0.0017***	0.0011**	0.0005	0.0021**	0.0033***
	(0.0002)	(0.0003)	(0.0002)	(0.0003)	(0.0002)	(0.0005)	(0.0002)	(0.0005)	(0.0010)	(0.0009)	(0.0005)
N	338,258	262,141	564,156	346,937	1,413,401	369,392	2,996,162	634,974	277,675	57,657	5,575,076
Adj. R2	.00669	.00367	.0175	.0116	.0564	.032	.0936	.0291	.00626	.0107	.0751
1st Stage F	874.730	923.831	1665.679	887.990	2290.133	1398.640	1340.025	670.744	402.797	378.555	422.007
Inc. group FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

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

Note: All regressions use Department of Education administrative data for award years 2013-2022 and estimate the IV-Disbursement specification separately by institutional selectivity. The dependent variable is an indicator for re-enrolling at least half-time for 90 days in the subsequent award year (percentage points). All regressions instrument actual Pell disbursement with simulated Pell eligibility computed by applying each award year's Pell Grant formula to a fixed cohort of students from award year 2015-16, include award year and parents' income bin fixed effects, weight observations by the number of students in each state-year-income cell, and cluster standard errors at the award year × parents' income bin level. Column headers show Barron's selectivity categories: (1) Most competitive, (2) Highly competitive +, (3) Highly competitive, (4) Very competitive +, (5) Very competitive, (6) Competitive +, (7) Competitive, (8) Less competitive, (9) Non-competitive, Specized, (11) No category. Only categories with sufficient sample sizes and instrument variation are included. All Pell amounts in hundreds of 2023 dollars.

Table B.4: Loan Effects by Barron's Selectivity Category (2013–2022 Sample)

	MC	**HC+**	**HC**	**VC+**	**VC**	**C+**	**C**	**LC**	**NC**	**Spec**	**None**
Pell Disb. (100s, 2023$)	0.2615	0.2086	-0.3937***	-0.7247***	-0.6632***	-0.5831***	-0.1388**	-0.0799	-0.0756	-0.3606	0.1140**
	(0.2193)	(0.1899)	(0.1421)	(0.1677)	(0.0949)	(0.1370)	(0.0584)	(0.1047)	(0.1421)	(0.5744)	(0.0471)
N	338,258	262,141	564,188	346,937	1,413,401	369,484	2,996,162	635,077	277,836	69,845	5,575,076
Adj. R2	-.00569	-.00922	.0122	.00145	.0022	-.00922	-.012	-.00233	.000145	-.00539	-.0182
1st Stage F	874.730	923.831	1665.609	887.990	2290.133	1398.497	1340.025	670.707	404.428	400.836	422.007
Inc. group FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

*** p<.01, ** p<.05, * p<.1

Note: All regressions use Department of Education administrative data for award years 2013-2022 and estimate the IV-Disbursement specification separately by institutional selectivity. The dependent variable is total federal student loans disbursed (excluding Parent PLUS) in hundreds of 2023 dollars. All regressions instrument actual Pell disbursement with simulated Pell eligibility computed by applying each award year's Pell Grant formula to a fixed cohort of students from award year 2015-16, include award year and parents' income bin fixed effects, weight observations by the number of students in each state-year-income cell, and cluster standard errors at the award year $\times$ parents' income bin level. Column headers show Barron's selectivity categories: (1) Most competitive, (2) Highly competitive +, (3) Highly competitive, (4) Very competitive +, (5) Very competitive, (6) Competitive +, (7) Competitive, (8) Less competitive, (9) Non-competitive, Specized, (11) No category. Only categories with sufficient sample sizes and instrument variation are included. All Pell amounts in hundreds of 2023 dollars.