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Abstract

Federal financial aid depends on a student's Expected Family Contribution (EFC)—the higher her EFC, the less aid she receives. We estimate the effects of increasing federal aid by leveraging an increase in the income threshold for an "automatic zero EFC," which qualifies students for the most generous federal aid. We find some evidence for crowd out of federal student loans and greater enrollment but no effect on institutional grants, working while in school, or choosing a STEM major. We argue that volatility in federal aid from year to year is blunting the effect of greater aid generosity.

Keywords: Higher education, Federal grants, Pell grants, Financial aid

JEL Codes: I22, I23, I28

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1 Introduction

This paper estimates the effect of federal grants on student outcomes. Federal financial aid is awarded according to a complicated formula that depends on a large number of factors. This formula produces a measure called the Expected Family Contribution (EFC) that represents the federal government's estimate of how much a student's family can be expected to contribute toward her education. The lower a student's EFC, the more financial aid she is eligible for. For students with family income below a certain threshold, the government assigns them an "automatic zero EFC," thereby making them eligible for the most generous federal financial aid package. For the 2007–2008 school year, the auto-zero-EFC threshold was $20,000; thus, EFC's for students below this income threshold were automatically set to zero. The threshold was then raised to $30,000 for the 2009-2010 school year and again to $31,000 for the 2011–2012 school year. Raising the threshold had no effect on aid for students below $20,000 of income, but it substantially increased grants for students above $20,000. We employ a difference-in-differences strategy to exploit this policy change and estimate the effect of increased federal grants on student outcomes. We find some evidence for crowd out of federal student loans.[1] We also find positive effects on short run and longer run enrollment. We estimate relatively precise zero effects on institutional grants, working while in school, or choosing a STEM major. Unfortunately, our estimates of the effect on six-year completion rates are too imprecise to be informative.

We are far from the first to estimate the effect of federal grants on student outcomes. In a review of the literature, Deming and Dynarski (2010) report mixed findings on the effect of Pell grants on student outcomes. Other forms of grant aid such as Social Security benefits (Dynarski, 2003), veterans benefits (Bound and Turner, 2002), and state-based merit grants (Dynarski, 2000; Kane, 2007) show more promising results. Our paper is most closely related to Denning et al. (2019), which leverages the discontinuity in federal aid introduced by the auto-zero-EFC threshold. Using data from Texas, Denning et al. (2019) exploit the discontinuity in Pell grants at the auto-zero-EFC threshold to estimate the effect of Pell grants, and they find positive effects of receiving an automatic zero EFC on

[1] Some of this crowd out may be mechanical, due to the federal financial aid formulas themselves.

college completion and wages. In contrast, Eng and Matsudaira (2021) replicate Denning et al. (2019)'s empirical strategy using administrative data on the universe of students who received federal aid, and they find smaller and less robust effects on completion and wages. Our paper differs from both in several ways. First, like Eng and Matsudaira (2021) but unlike Denning et al. (2019), we use nationally representative data. Second, both papers focus on the discontinuity imposed by the auto-zero-EFC income threshold, whereas we exploit a change in that threshold. Third, Denning et al. (2019) are missing data on several criteria for an auto-zero-EFC, while our data include all the inputs used in federal aid calculations. And fourth, Denning et al. (2019)'s data allow them to look at both enrollment effects and labor market outcomes. Like Eng and Matsudaira (2021), our data sample students who are already enrolled in college, so we are restricted to look at financial and academic outcomes after first enrollment. We also present new evidence on a potential explanation for the conflicting findings in the literature—federal financial aid is surprisingly volatile from year to year which may blunt the effect of a one-time increase in a student's Pell grant award.[2] State policies may mitigate the volatility in federal aid, as happened in the setting studied by Denning et al. (2019), or exacerbate it, as for example if state aid were tied to Pell eligibility. We argue that the stability and predictability of future aid should figure more prominently in policies designed to increase the enrollment and completion rates of low income students.

2 Policy Change

Federal financial aid in the United States is determined by a complicated nonlinear formula that takes into account income, assets, family size, and other factors to calculate a student's EFC. Students who apply for federal aid must complete the Free Application for Federal Student Aid (FAFSA) which contains over 100 questions, many of which request detailed financial information about the student and her parents. To ease the burden of completing the FAFSA for lower income students, students can qualify for a simplified

[2]This volatility comes from volatility in both parental income and/or assets and in the aid formulas themselves.

EFC formula if they pass a "Simplified Needs Test" (SNT), the most important element of which is that income be below $50,000.[3] Among students who pass the SNT, those with incomes below the auto-zero-EFC threshold receive an automatic zero EFC, which qualifies them for the most generous federal aid available. While some students who do not receive an automatic zero EFC do end up receiving a zero EFC anyway, in general their EFC's tend to be higher, thereby reducing the federal aid they receive. Figure 1 plots mean EFC by income for those who passed the SNT. For incomes below the auto-zero-EFC threshold, the mean EFC is zero. But above the threshold, the mean EFC immediately jumps by about $1,000 and rises gradually with income thereafter.

The auto-zero-EFC threshold was raised from $20,000 to $30,000 for the 2009-2010 school year and again to $31,000 two years later. Figure 1 plots mean EFC in $1,000 income bins for the 2007-2008 school year and the 2011-2012 school year. As Figure 1 illustrates, the increase in the auto-zero-EFC threshold shifted the entire EFC schedule to the right, which lowered the mean EFC's for everyone except for those below $20,000 who were already at a zero EFC. Students just above the old threshold of $20,000 saw a drop in their EFC's of about $1,000 while those just below the new threshold of $31,000 saw their EFC's fall by over $2,000.

The change in the auto-zero-EFC threshold translated into a pronounced increase in federal Title IV grants. Figure 2 plots mean Title IV grants received in $1,000 income bins for the 2007-2008 school year and the 2011-2012 school year. For students below $20,000 of income, grants did not rise because those students were already receiving the most generous federal aid available. But for students just above $20,000, grants rose by nearly $1,000 while for students closer to $30,000 grants rose by $1,500. Although they did not receive automatic zero EFC's, students above $31,000 also received more grants due to the overall shift in the EFC schedule.

[3] In addition, the SNT requires that either 1) at least one member of the student's household must have received benefits from any of the designated means tested federal benefit programs, OR 2) the student's parents were eligible to file IRS form 1040A or 1040EZ or were not required to file any income tax return.

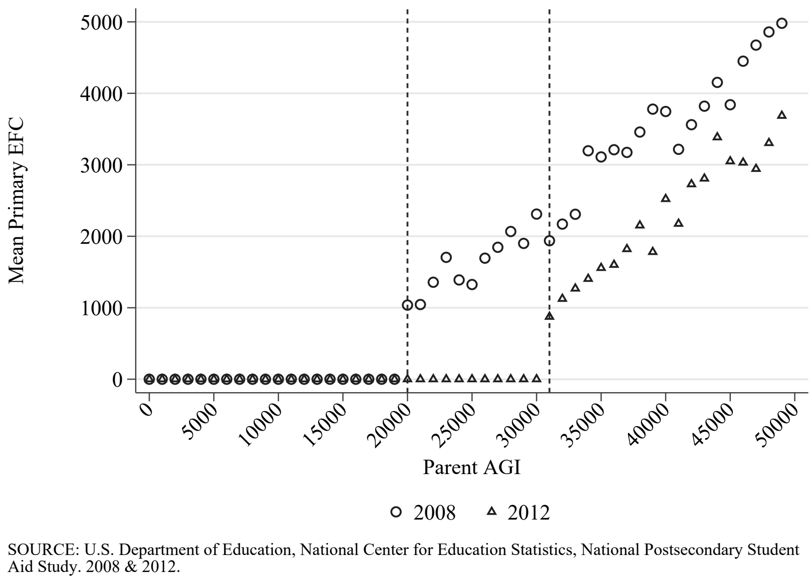


Figure 1: This figure plots mean EFC in $1,000 income bins for the 2007-2008 school year and the 2011-2012 school year. The vertical dashed lines indicate the auto-zero-EFC thresholds for each year.

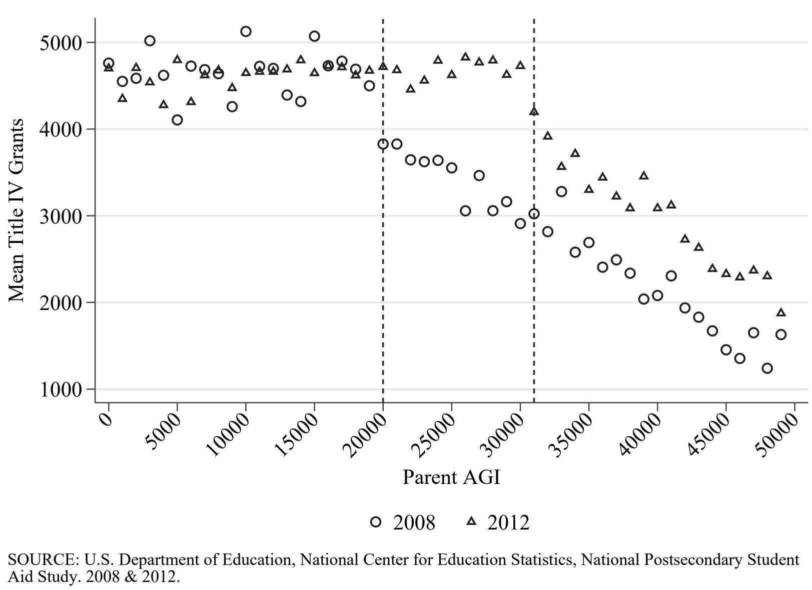


Figure 2: This figure plots mean Title IV grants received in $1,000 income bins for the 2007-2008 school year and the 2011-2012 school year. The vertical dashed lines indicate the auto-zero-EFC thresholds for each year.

3 Data

Our data come from the National Postsecondary Student Aid Study (NPSAS) and Beginning Postsecondary Students (BPS). NPSAS is a large, nationally representative cross-section of college students in the United States. The survey is conducted about every four years, and provides a detailed picture of how students finance their education. BPS is drawn from first-time freshmen in NPSAS and follows them for six years. For this paper, we used the 2008 and 2012 waves of NPSAS (covering the 2007-2008 and 2011-2012 school years) and the 2004-2009 and 2012-2017 panels of BPS. Both NPSAS and BPS contain detailed data on income and financial aid, including a student's EFC. The NPSAS data sample has the advantage of being both larger and closer in proximity to the policy change, while the BPS sample allows us to follow students over time. Thus, we use NPSAS to estimate short run effects and BPS to estimate longer run effects.

Table 1 reports summary statistics for the NPSAS sample. The sample is restricted to dependent students who completed the FAFSA and qualified for the SNT, which means their parents' income is below $50,000. They are predominantly enrolled at public colleges.[4] Due to a change in survey design, NPSAS sampled more students at two-year and for-profit colleges in 2012, so in our regression analyses we use control for institution level and type. EFC's are lower in 2012, and federal grants higher, due to the shift in the EFC schedule documented in Figure 1.

Table 1 also reports summary statistics for the BPS sample. Unlike the NPSAS sample, this sample is restricted to freshmen and is therefore smaller. But it does follow students for six years, which allows us to look at several longer run outcomes such as cumulative student borrowing, cumulative enrollment, and six-year completion. As with NPSAS, a change in survey design increased the number of students at for-profit colleges in 2012, so we control for institution level and type in all regressions.

[4]We dropped students who attended multiple institutions in the same year.

Table 1: Descriptive Statistics

	NPSAS		BPS	
	2008	2012	2004	2012
Number of Observations	7,460	8,180	1,150	2,490
Mean Expected Family Contribution (EFC)	$1,596	$625	$1,524	$536
Percent with $0 EFC	46.2%	73.9%	41.4%	74.8%
Parent Adjusted Gross Income (AGI)				
Less than $10,000	11.7%	11.1%	16.4%	13.6%
$10,000 to $20,000	26.5%	28.2%	25.8%	28.6%
$20,000 to $31,000	28.7%	31.0%	28.7%	28.3%
$31,000 to $50,000	33.2%	29.8%	29.1%	29.5%
Mean Age	20.1	19.2		
Percent Female	58.8%	54.9%	59.4%	60.1%
Mean ACT Score	20.3	19.5	19.5	19.6
Mean Title IV Grant Amount	$3,510	$4,205	$2,672	$4,406
Mean Federal Loan Amount	$3,653	$5,311	$2,525	$4,931
Institution Level				
4-year	72.0%	58.3%	61.2%	59.2%
2-year	28.0%	41.7%	38.8%	40.8%
Institution Type				
Public	68.4%	55.7%	68.8%	59.4%
Private not-for-profit	23.6%	14.2%	26.6%	19.4%
Private for-profit	8.1%	30.1%	4.6%	21.2%

Income is less than $50,000 in our sample, in keeping with the criteria of the Simplified Needs Test. Title IV grant amount is the sum of contributions from Pell grants, Academic Competitiveness Grants (ACGs), National Science and Mathematics Access to Retain Talent (SMART) grants, and Federal Supplemental Education Opportunity Grants (FSEOGs). Sample counts were rounded to the nearest 10. No sample weights were used.

4 Empirical Strategy

We leverage the change in the income threshold for an automatic zero EFC by using a difference-in-differences strategy. We compare students with an income between below \$20,000 (below the original threshold) with students above \$20,000.[5] Students in the first group ("Below") were eligible for an automatic zero EFC in both years while students in the second group ("Above") received a lower EFC in 2012 after the threshold was raised. Our regression specification is

$$y_{it} = \beta_1 \mathbf{1}\{\text{Above}\} + \beta_2 \mathbf{1}\{\text{After Change}\} + \beta_3 \mathbf{1}\{\text{Above}\} \times \mathbf{1}\{\text{After Change}\} + X_i \delta + u_{it} \quad (1)$$

where β_3 is the parameter of interest. We also include the following controls (X_i): student's race, gender, test score, grade level, and household size; parents' education; state fixed effects; and college level and type.[6] Our identification strategy relies on the standard parallel trends assumption. Specifically, we assume that the trends over time for the Above and Below groups would be the same in the absence of the policy change.

We can interpret β_3 from equation (1) in two ways. If we consider treatment to be whether a student receives a lower EFC due to a shift in the threshold, then β_3 represents the average treatment effect on the treated. Instead, if we consider treatment to be the amount of a student's federal grants, then β_3 represents an "intention-to-treat" effect, which would need to be divided by the "first stage" effect of receiving a lower EFC on federal grants in order to arrive at the local average treatment effect of grants on student outcomes. In section 5, we report β_3 for a variety of outcome variables. We do find that β_3 is large and very significant when the outcome variable is Title IV grants, implying a strong "first stage." But our findings for other outcomes are more mixed.

We estimate the regression in equation (1) on both the full sample of students who qualified for the SNT and a smaller sample of students who were near the auto-zero-EFC threshold.[7] The "Above" group in this restricted sample were all eligible for an

[5]The threshold in 2004 was \$15,000, so we use this threshold with the BPS data.

[6]Our findings are not sensitive to including these controls.

[7]For NPSAS, this includes students above \$10,000 but below \$31,000 of income, while for BPS this

automatic zero EFC in 2012, so β_3 represents the average treatment effect on the treated of actually receiving an automatic zero EFC. For the full sample of SNT eligible students, β_3 represents the average treatment effect on the treated of the overall shift in the EFC schedule.

5 Results

5.1 Difference-in-Differences Estimates

Tables 2 and 3 report estimates of the regression specification in (1) for several short run outcomes using the NPSAS data. As we might expect, increasing the automatic zero EFC threshold raised Title IV grants by over \$1,000. Among students near the threshold, this increased grant aid appears to crowd out \$453 of student borrowing, although no such crowd out appears for the full sample. Total Title IV aid (grants, loans, and work-study) rises by \$820 for those near the threshold and \$1,149 for the full sample. This increased aid does not appear to crowd out institutional grants—we can rule out a drop in institutional grants larger than \$194 for those near the threshold and \$109 for the full sample. We estimate relatively precise zeros for the effect on whether a student works, job hours per week, and whether the student chooses a STEM major. For the full sample, we find modest, but statistically significant, effects on enrollment intensity and grades, although these effects are not statistically significant for students near the threshold.[8]

Tables 4 and 5 report estimates for several longer run outcomes using the BPS data. The BPS sample is smaller and provides less precise estimates than NPSAS, but it does allow us to look at longer run outcomes. Just as in the NPSAS data, we estimate a strong effect on grants, some crowd out of borrowing for students near the threshold, and no evidence of crowd out for institutional grants. For the full sample, we find a positive effect on cumulative enrollment at the three-year and six-year marks, with the point

includes students above \$5,000 but below \$31,000.

[8] We do not have data on the number of credits earned.

Table 2: Short Run Effects on Student Outcomes (All SNT Filers)

	Title IV Grants	Title IV Loans	Total Title IV Aid	Institutional Grant Amount	Months Enrolled Full-Time	Whether Student Works	Job-Hours per Week	Grade Point Average	Whether Student is a STEM Major
AGI>$20k * 2012	1192.4	-47.92	1149.3	150.4	0.212	-0.00153	0.0890	5.256	-0.00287
	(61.70)***	(147.2)	(170.6)***	(132.3)	(0.0808)**	(0.0156)	(0.476)	(2.523)*	(0.0125)
R^2	0.273	0.307	0.335	0.447	0.100	0.085	0.072	0.140	0.168
Mean	3917.4	5467.5	9613.0	2173.0	8.496	0.524	13.40	270.9	0.154
Controls	X	X	X	X	X	X	X	X	X
State fixed effects	X	X	X	X	X	X	X	X	X
Obs	15630	15630	15630	15630	15630	15630	15630	15630	15630

The sample includes students with parent income between $0 and $50k. Grade point average is scaled so that 400 corresponds to a 4.00 GPA. The mean of the outcome for students with income between $20k and $50k in 2012 is reported in the lower panel. Heteroskedasticity robust standard errors are in parentheses. No sample weights were used.

SOURCE: U.S. Department of Education, National Center for Education Statistics, National Postsecondary Student Aid Study. 2008 & 2012.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 3: Short Run Effects on Student Outcomes (Near Threshold Only)

	Title IV Grants	Title IV Loans	Total Title IV Aid	Institutional Grant Amount	Months Enrolled Full-Time	Whether Student Works	Job-Hours per Week	Grade Point Average	Whether Student is a STEM Major
AGI>$20k * 2012	1252.5	-452.8	819.6	143.2	0.184	0.00225	-0.0724	1.502	-0.00722
	(76.44)***	(183.8)*	(215.1)***	(171.8)	(0.104)	(0.0202)	(0.614)	(3.247)	(0.0164)
R^2	0.267	0.317	0.343	0.437	0.091	0.081	0.071	0.144	0.165
Mean	4687.2	5103.4	10026.7	2014.1	8.591	0.530	13.65	267.3	0.159
Controls	X	X	X	X	X	X	X	X	X
State fixed effects	X	X	X	X	X	X	X	X	X
Obs	8960	8960	8960	8960	8960	8960	8960	8960	8960

The sample is limited to students with parent income between $10k and $31k. Grade point average is scaled so that 400 corresponds to a 4.00 GPA. The mean of the outcome for students with income between $20k and $31k in 2012 is reported in the lower panel. Heteroskedasticity robust standard errors are in parentheses. No sample weights were used.

SOURCE: U.S. Department of Education, National Center for Education Statistics, National Postsecondary Student Aid Study. 2008 & 2012.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

estimate at six years corresponding to roughly one additional semester of enrollment.[9] However, for students near the threshold the effects on cumulative enrollment are smaller and are not statistically significant at the three-year mark. We find a similar pattern with cumulative borrowing—a statistically significant, positive effect on borrowing at the six-year mark for the full sample, but a smaller and insignificant effect for students near the threshold. We also estimate the effect on degree completion, but the estimates are too imprecise to be informative.

Figures 3 and 4 plot the difference-in-differences estimates across both short run outcomes (from NPSAS) and longer run outcomes (from BPS). To make these estimates comparable, the subfigures on the left scale the estimates by the mean of the outcome for the treated group in 2012. Thus, the estimates are presented as a percentage change relative to the mean. The subfigures on the right divide the estimates by the standard deviation of the outcome for the treated group in 2012. This measure is also known as Cohen's *d*, and it measures a treatment effect in standard deviations of the outcome.[10] The effect on Title IV grants stands out with an effect greater than 25 percent of the mean or more than 0.5 standard deviations. For short run enrollment and GPA, although we find a statistically significant effect we can rule out effects larger than five percent of the mean. And, aside from Title IV grants and total aid, we can rule out effect sizes larger than 0.2 standard deviations for any short run outcome. For longer run outcomes, the confidence intervals are less informative, but the point estimates for cumulative enrollment for the full sample are roughly ten percent of the mean at both the three-and six-year marks, which corresponds to about 0.2 standard deviations. This additional enrollment may explain the 13 percent rise in borrowing that we see for the full sample. But for students near the threshold, the longer run estimates on enrollment and borrowing are closer to zero and, except for enrollment at six years, are no longer significant.

[9] We do not have data on the cumulative number of credits earned.

[10] An effect size of 0.2 is often considered "small," 0.5 is considered "medium," and 0.8 is considered "large."

Table 4: Longer Run Effects on Student Outcomes (All SNT Filers)

	Title IV Grants	Title IV Loans	Total Title IV Aid	Institutional Grant Amount	Cumul. Title IV Loans (6 Years)	Bach. Deg. (6 Years)	FTE Months (3 Years)	FTE Months (6 Years)
AGI>$15k * 2012	1069.9	6.504	989.8	111.5	2031.7	0.0328	1.777	3.931
	(122.5)***	(309.2)	(359.8)**	(326.7)	(1021.3)*	(0.0336)	(0.651)**	(1.205)**
R^2	0.325	0.342	0.399	0.447	0.191	0.263	0.214	0.168
Mean	4251.6	4968.8	9549.8	2977.0	15780.1	0.343	21.73	33.12
Controls	X	X	X	X	X	X	X	X
State fixed effects	X	X	X	X	X	X	X	X
Obs	3640	3640	3640	3640	3640	3640	3640	3640

The sample is limited to students with parent income between $0k and $50k. The first four columns report short run outcomes that are comparable to the first four columns of Table 3, while columns 5–8 report longer run outcomes. The mean of the outcome for students with income between $15k and $50k in 2012 is reported in the lower panel. Heteroskedasticity robust standard errors are in parentheses. No sample weights were used.

SOURCE: U.S. Department of Education, National Center for Education Statistics, Beginning Postsecondary Students. 2004 & 2012.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

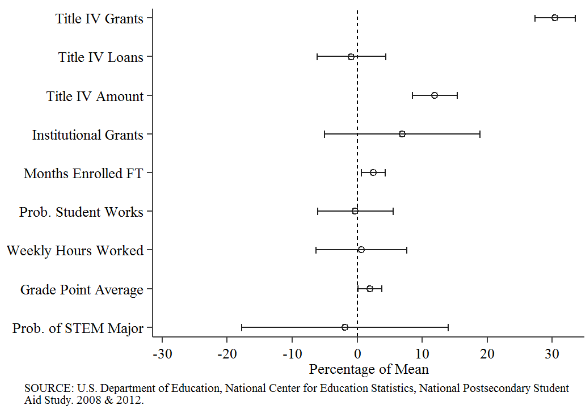
Table 5: Longer Run Effects on Student Outcomes (Near Threshold Only)

	Title IV Grants	Title IV Loans	Total Title IV Aid	Institu-tional Grant Amount	Cumul. Title IV Loans (6 Years)	Bach. Deg. (6 Years)	FTE Months (3 Years)	FTE Months (6 Years)
AGI>$15k * 2012	1061.1	-378.7	624.3	132.4	1030.0	-0.00614	1.390	3.186
	(137.8)***	(352.3)	(412.4)	(396.0)	(1261.1)	(0.0395)	(0.765)	(1.431)*
R^2	0.334	0.352	0.401	0.434	0.185	0.282	0.229	0.183
Mean	4827.3	4740.6	9904.3	2759.8	15276.5	0.316	21.12	32.28
Controls	X	X	X	X	X	X	X	X
State fixed effects	X	X	X	X	X	X	X	X
Obs	2370	2370	2370	2370	2370	2370	2370	2370

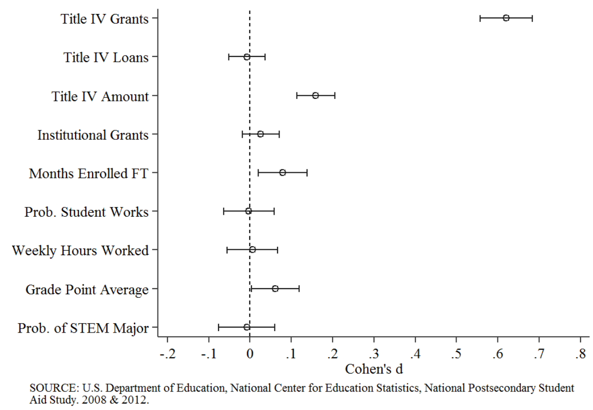
The sample is limited to students with parent income between $5k and $31k. The first four columns report short run outcomes that are comparable to the first four columns of Table 3, while columns 5–8 report longer run outcomes. The mean of the outcome for students with income between $15k and $31k in 2012 is reported in the lower panel. Heteroskedasticity robust standard errors are in parentheses. No sample weights were used.

SOURCE: U.S. Department of Education, National Center for Education Statistics, Beginning Postsecondary Students. 2004 & 2012.

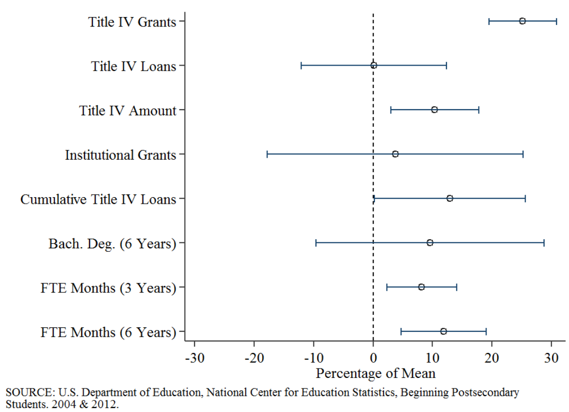
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$



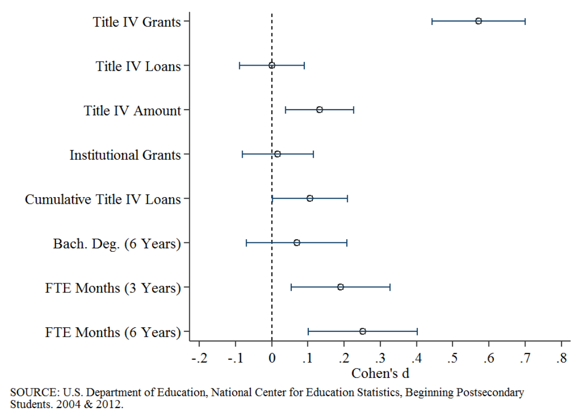
(a) Treatment effect scaled by mean (NPSAS)



(b) Effect size (NPSAS)



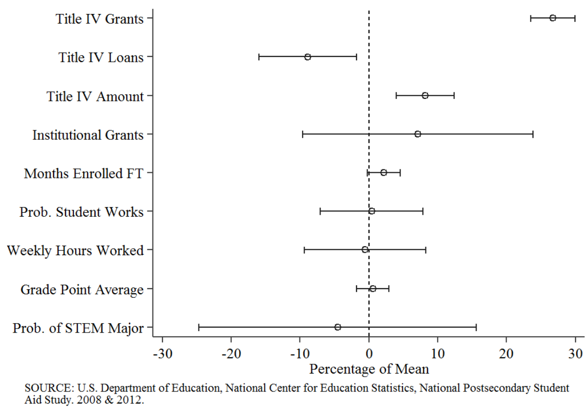
(c) Treatment effect scaled by mean (BPS)



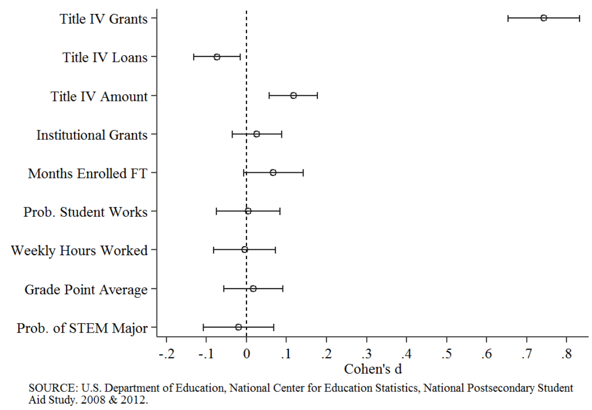
(d) Effect size (BPS)

Figure 3: Treatment effects across multiple outcomes (All SNT Filers)

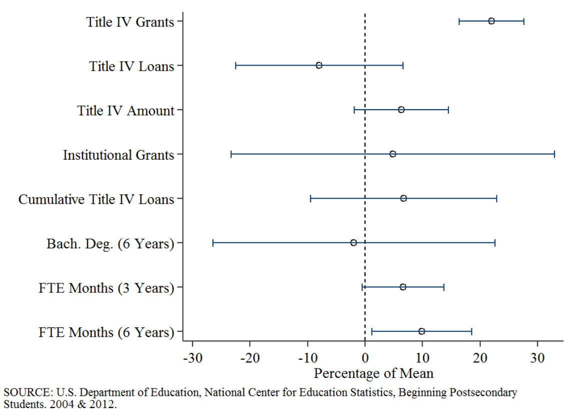
Notes: This figure plots the difference-in-differences estimates from Tables 2 and 4. Figures 3a and 3b plot short run outcomes from NPSAS whereas Figures 3c and 3d plot longer run outcomes from BPS. Circles represent point estimates while bars represent 95 percent confidence intervals. Figure 3a scales the treatment effect by the mean of the outcome for students between \$20k and \$50k of income in 2012 while Figure 3c scales by the mean outcome for students between \$15k and \$50k of income in 2012. Figures 3b and 3d instead scale the treatment effect by the standard deviation of the outcome, a measure of effect size known as Cohen's *d*.



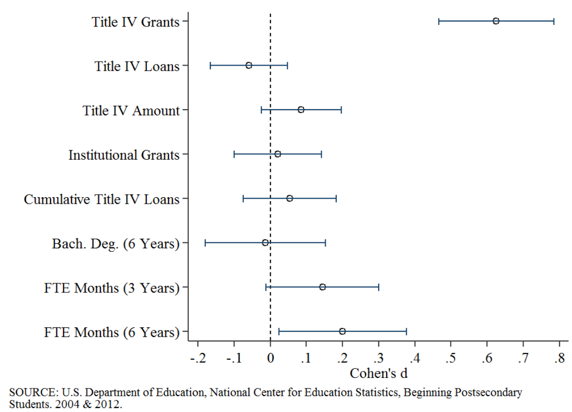
(a) Treatment effect scaled by mean (NPSAS)



(b) Effect size (NPSAS)



(c) Treatment effect scaled by mean (BPS)



(d) Effect size (BPS)

Figure 4: Treatment effects across multiple outcomes (Near Threshold Only)

Notes: This figure plots the difference-in-differences estimates from Tables 3 and 5. Figures 4a and 4b present short run outcomes from NPSAS whereas Figures 4c and 4d present longer run outcomes from BPS. Circles represent point estimates while bars represent 95 percent confidence intervals. Figure 4a scales the treatment effect by the mean of the outcome for students between \$20k and \$31k of income in 2012 while Figure 4c scales by the mean outcome for students between \$15k and \$31k of income in 2012. Figures 4b and 4d instead scale the treatment effect by the standard deviation of the outcome, a measure of effect size known as Cohen's *d*.

5.2 Two-Stage Least Squares Estimates

Tables 6 and 7 report two-stage least squares (2SLS) estimates using the difference-in-differences interaction term as an instrument for federal grants. In essence, the 2SLS estimates rescale the difference-in-differences estimates from Tables 2 and 3 to reflect the effect of federal grants on student outcomes rather than the effect of the shift in the auto-zero-EFC threshold. The first stage F-statistics are very large, which is unsurprising given the dramatic effect of the policy change on grants illustrated by Figure 2. For the full sample, one thousand dollars in grants translates into 0.177 additional months enrolled full time and a rise in GPA of 0.04 on a four point scale. For students near the threshold, additional grants crowd out borrowing, causing total Title IV aid to rise by only $654, with no significant effect on other short run outcomes.

Tables 8 and 9 report 2SLS estimates for longer run outcomes. As with the short run outcomes, the first stage F-statistics are large, but the smaller sample size lowers the precision of the estimates. For the full sample, one thousand dollars in grants translates into 1.7 additional months of enrollment after three years and 3.7 additional months after six years. It also translates into $1,899 of additional borrowing over six years, perhaps due to the additional enrollment. For students near the threshold, additional grants crowd out borrowing, causing total Title IV aid to rise by only $588, with smaller effects on enrollment and borrowing. The estimates for degree completion after six years are too imprecise to be informative, with a point estimate of 0.03 and confidence interval $(-0.03, 0.09)$ for the full sample and a point estimate of -0.006 and confidence interval $(-0.08, 0.07)$ for students near the threshold.

6 Comparing Results with Denning et al. (2019)

Our setting is similar to that of Denning et al. (2019), so in this section we compare our findings with theirs. Before doing so, we highlight the differences between the two papers. First, Denning et al. (2019) use data from a single state (Texas) while we use nationally representative data. Second, they focus on the discontinuity imposed by the auto-zero-

Table 6: 2SLS Estimates of the Effect of Federal Grants on Short Run Outcomes (All SNT Filers)

	Title IV Loans	Total Title IV Aid	Institu-tional Grant Amount	Months Enrolled Full-Time	Whether Student Works	Job-Hours per Week	Grade Point Average	Whether Student is a STEM Major
Title IV grants (in thousands)	-40.19	963.8	126.2	0.177	-0.00128	0.0747	4.408	-0.00241
	(123.4)	(126.3)***	(110.0)	(0.0636)**	(0.0130)	(0.399)	(2.092)*	(0.0105)
R^2	0.304	0.471	0.452	0.197	0.086	0.070	0.153	0.163
Mean	5467.5	9613.0	2173.0	8.496	0.524	13.40	270.9	0.154
Controls	X	X	X	X	X	X	X	X
State fixed effects	X	X	X	X	X	X	X	X
Obs	15630	15630	15630	15630	15630	15630	15630	15630
First stage F-stat	373.6	373.6	373.6	373.6	373.6	373.6	373.6	373.6

The sample is limited to students with parent income between $0 and $50k. The table reports estimates of the effect of federal grants on student outcomes, using the increase in the auto-zero-EFC threshold as an instrument. Grade point average is scaled so that 400 corresponds to a 4.00 GPA. The mean of the outcome for students with income between $20k and $50k in 2012 is reported in the lower panel along with the first stage F-statistic. Heteroskedasticity robust standard errors are in parentheses. No sample weights were used.

SOURCE: U.S. Department of Education, National Center for Education Statistics, National Postsecondary Student Aid Study. 2008 & 2012.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 7: 2SLS Estimates of the Effect of Federal Grants on Short Run Outcomes (Near Threshold Only)

	Title IV Loans	Total Title IV Aid	Institutional Grant Amount	Months Enrolled Full-Time	Whether Student Works	Job-Hours per Week	Grade Point Average	Whether Student is a STEM Major
Title IV grants (in thousands)	-361.5	654.3	114.4	0.147	0.00180	-0.0578	1.199	-0.00576
	(150.8)*	(155.0)***	(135.6)	(0.0785)	(0.0160)	(0.488)	(2.571)	(0.0131)
R^2	0.272	0.456	0.444	0.186	0.080	0.072	0.150	0.155
Mean	5103.4	10026.7	2014.1	8.591	0.530	13.65	267.3	0.159
Controls	X	X	X	X	X	X	X	X
State fixed effects	X	X	X	X	X	X	X	X
Obs	8960	8960	8960	8960	8960	8960	8960	8960
First stage F-stat	268.5	268.5	268.5	268.5	268.5	268.5	268.5	268.5

The sample is limited to students with parent income between \$10k and \$31k. The table reports estimates of the effect of federal grants on student outcomes, using the increase in the auto-zero-EFC threshold as an instrument. Grade point average is scaled so that 400 corresponds to a 4.00 GPA. The mean of the outcome for students with income between \$20k and \$31k in 2012 is reported in the lower panel along with the first stage F-statistic. Heteroskedasticity robust standard errors are in parentheses. No sample weights were used.

SOURCE: U.S. Department of Education, National Center for Education Statistics, National Postsecondary Student Aid Study. 2008 & 2012.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 8: 2SLS Estimates of the Effect of Federal Grants on Longer Run Outcomes (All SNT Filers)

	Title IV Loans	Total Title IV Aid	Institutional Grant Amount	Cumul. Title IV Loans (6 Years)	Bach. Deg. (6 Years)	FTE Months (3 Years)	FTE Months (6 Years)
Title IV grants (in thousands)	6.079	925.1	104.2	1898.9	0.0307	1.661	3.674
	(285.9)	(295.2)**	(301.3)	(928.7)*	(0.0306)	(0.557)**	(1.078)***
R^2	0.342	0.492	0.450	0.207	0.281	0.316	0.200
Mean	4968.8	9549.8	2977.0	15780.1	0.343	21.73	33.12
Controls	X	X	X	X	X	X	X
State fixed effects	X	X	X	X	X	X	X
Obs	3640	3640	3640	3640	3640	3640	3640
First stage F-stat	76.30	76.30	76.30	76.30	76.30	76.30	76.30

The sample includes students with parent income between \$0 and \$50k. The table reports estimates of the effect of federal grants on student outcomes, using the increase in the auto-zero-EFC threshold as an instrument. Grade point average is scaled so that 400 corresponds to a 4.00 GPA. The mean of the outcome for students with income between \$15k and \$50k in 2012 is reported in the lower panel along with the first stage F-statistic. Heteroskedasticity robust standard errors are in parentheses. No sample weights were used.

SOURCE: U.S. Department of Education, National Center for Education Statistics, Beginning Postsecondary Students. 2004 & 2012.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 9: 2SLS Estimates of the Effect of Federal Grants on Longer Run Outcomes (Near Threshold Only)

	Title IV Loans	Total Title IV Aid	Institu- tional Grant Amount	Cumul. Title IV Loans (6 Years)	Bach. Deg. (6 Years)	FTE Months (3 Years)	FTE Months (6 Years)
Title IV grants (in thousands)	-356.8	588.3	124.8	970.7	-0.00579	1.310	3.002
	(337.0)	(350.7)	(365.7)	(1150.4)	(0.0368)	(0.655)*	(1.258)*
R^2	0.317	0.478	0.437	0.208	0.275	0.356	0.278
Mean	4740.6	9904.3	2759.8	15276.5	0.316	21.12	32.28
Controls	X	X	X	X	X	X	X
State fixed effects	X	X	X	X	X	X	X
Obs	2370	2370	2370	2370	2370	2370	2370
First stage F-stat	59.30	59.30	59.30	59.30	59.30	59.30	59.30

The sample is limited to students with parent income between \$5k and \$31k. The table reports estimates of the effect of federal grants on student outcomes, using the increase in the auto-zero-EFC threshold as an instrument. Grade point average is scaled so that 400 corresponds to a 4.00 GPA. The mean of the outcome for students with income between \$15k and \$31k in 2012 is reported in the lower panel along with the first stage F-statistic. Heteroskedasticity robust standard errors are in parentheses. No sample weights were used.

SOURCE: U.S. Department of Education, National Center for Education Statistics, Beginning Postsecondary Students. 2004 & 2012.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

EFC income threshold, whereas we exploit a policy change in that threshold. Third, they are missing data on several criteria for an auto-zero-EFC, which forces them to adopt a fuzzy RD design, while our data include all the inputs used in federal aid calculations. And fourth, Denning et al. (2019)'s data allow them to look at both enrollment effects and labor market outcomes. Because our data sample college freshmen and follow them for only six years, we are restricted to look at financial and academic outcomes within a six year window of first enrollment.

With all of that said, our findings match those of Denning et al. (2019) in several ways. We both find that receiving an auto-zero-EFC 1) raises grant aid in the short run, 2) lowers borrowing somewhat in the short run, 3) has no effect on short run work or earnings, and 4) has a modest effect on short run enrollment intensity.[11] Moreover, we both find statistically significant effects on long run enrollment intensity. Their main finding is that receiving an automatic zero EFC raises completion rates and wages. Unfortunately, our data on long run outcomes is not large enough to provide informative estimates of the effect on completion and we lack good data on wages.

Another significant, albeit subtle, contrast arises from the difference between a regression discontinuity (RD) identification strategy and a difference-in-differences (DD) strategy. By using RD, Denning et al. (2019) (and Eng and Matsudaira (2021)) are estimating the effect of receiving an auto-zero-EFC holding the federal aid schedule fixed. In contrast, we exploit a shift in the aid schedule itself which affected some students but not others. The RD approach thus better captures a purely one-time or transitory increase in federal aid while the shift in the aid schedule that we exploit also has potential effects on expectations about future aid. We might expect real world policy changes to involve shifts in the aid schedule, making our identification strategy more policy relevant. With all of that said, we show in the next section that federal aid is sufficiently volatile from year to year that the shift in the aid schedule between 2008 and 2012 did not substantially affect the future EFC's of those between \$20,000 and \$31,000 of income *relative to those between \$10,000 and \$20,000* (see Table 11). Indeed, we argue that the volatility

[11]Denning et al. (2019) measure enrollment intensity in terms of credits attempted while we measure it in terms of months enrolled full time.

in federal aid is a key factor in reconciling the many conflicting findings on the effects of increased student aid.

7 Persistence of Federal Aid

Prior work has attempted to estimate "the effect" of Pell grants on student outcomes with mixed results (Deming and Dynarski, 2010). Eng and Matsudaira (2021) argue that state aid programs are a likely explanation why Denning et al. (2019) find large effects on completion and wages, and they argue that interactions between federal and state aid programs make it difficult to estimate "the effect" of federal aid.

One unusual feature of the setting studied by Denning et al. (2019) lies in the state of Texas's TEXAS Grant, which awards students in Texas a state grant up to the statewide average of tuition and fees. Denning et al. (2019) find that receiving an auto-zero-EFC raises the probability of receiving a TEXAS Grant. Crucially, once a student receives a TEXAS Grant, she is eligible to continue receiving the grant for up to five years, which adds a great degree of predictability to her financial aid. Thus, not only does a TEXAS grant increase a student's financial aid today, it also greatly reduces her *uncertainty* about aid in the future. In contrast, it turns out that federal financial aid is surprisingly volatile from year to year.

In Table 10 we can see that students above \$20,000 of income had average EFC's of \$1,019 in 2012. But of the 69 percent who completed the FAFSA in the following year, the average EFC rose to \$1,667. For those in the \$20,000–\$31,000 income range, the average EFC rose from \$0 in 2012 to \$875 in 2013. Tables 10 and 11 also report difference-in-differences estimates using EFC as the outcome. The first column reports estimates for the current year's EFC. As suggested by Figure 1, shifting the automatic zero EFC threshold lowered the EFC's of students above \$20,000 of income by roughly \$1,500. But when we look at the following year's EFC (column two) the effect is much smaller. For the full sample, the next year's EFC is lowered by only \$455—and only a statistically insignificant \$225 for students near the threshold. While shifting the automatic zero EFC threshold

Table 10: Effect on This and Next Year's EFC (All SNT Filers)

	EFC This Year	EFC Next Year
AGI>$20k * 2012	-1537.4	-455.3
	(50.87)***	(149.0)**
R^2	0.267	0.090
Mean	1018.5	1666.6
Controls	X	X
State fixed effects	X	X
Obs	15570	10680

The sample includes students with parent income between $0–$50k. The table reports difference-in-differences estimates of the effect of shifting the auto-zero-EFC threshold on a student's EFC this year and the following year. The mean of the outcome for students with income between $20k and$50k in 2012 is reported in the lower panel. Heteroskedasticity robust standard errors are in parentheses. No sample weights were used.

SOURCE: U.S. Department of Education, National Center for Education Statistics, National Postsecondary Student Aid Study. 2008 & 2012.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

mechanically reduced a student's EFC in 2012 to zero, it had a much smaller effect on her EFC in 2013, just one year later. This partly comes from year-to-year volatility in parent income, but it also comes from volatility in federal aid program rules—in 2013, the federal government suddenly lowered the auto-zero-EFC threshold from $31,000 to $23,000, thereby eliminating automatic zero EFC's for students between $23,000 and $31,000. It is not hard to imagine how such volatility in aid from year to year could blunt the effects of a one-time increase in federal aid.

Dynarski et al. (2021) and Burland et al. (2022) find direct evidence of the importance of financial aid uncertainty on college choice. Dynarski et al. (2021) find that a guaranteed financial aid offer increases applications and enrollment relative to an uncertain offer. Burland et al. (2022) find that a pre-application guarantee of zero tuition raises application and enrollment rates relative to an offer of zero tuition that is contingent on demonstrating financial need, even for those students who are very likely to qualify for zero tuition. Although both of these papers focus on initial college enrollment rather than persistence and completion, it is not surprising that a one-time increase in aid would have a much smaller effect on educational investments than a guaranteed flow of aid over several years.

Table 11: Effect on This and Next Year's EFC (Near Threshold Only)

	EFC This Year	EFC Next Year
AGI>$20k * 2012	-1562.3	-224.8
	(52.27)***	(164.6)
R^2	0.254	0.071
Mean	0	875.1
Controls	X	X
State fixed effects	X	X
Obs	8940	6220

The sample is limited to students with parent income between $10k–$31k. The table reports difference-in-differences estimates of the effect of shifting the auto-zero-EFC threshold on a student's EFC this year and the following year. The mean of the outcome for students with income between $20k and$31k in 2012 is reported in the lower panel. Heteroskedasticity robust standard errors are in parentheses. No sample weights were used.

SOURCE: U.S. Department of Education, National Center for Education Statistics, National Postsecondary Student Aid Study. 2008 & 2012.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Indeed, while state policies like the TEXAS grant can mitigate uncertainty in financial aid, other state policies can exacerbate it, such as tying state aid to federal Pell grant eligibility. Thus, our findings highlight the importance of a neglected dimension of financial aid—the stability and predictability of aid over a student's college career.

8 Conclusion

Increasing the auto-zero-EFC threshold from $20,000 to $31,000 significantly increased federal grants received by students with incomes above $20,000 but did not affect federal grants for those below. Using a difference-in-differences strategy, we find evidence for modest immediate effects on enrollment and grades and longer run effects on enrollment and student borrowing. We find no crowd out effects for borrowing on the full sample of SNT-eligible students, but we do find that grants crowd out borrowing among students near the automatic zero EFC threshold. We show that EFC's are surprisingly volatile from year to year, and that this volatility may blunt the effect of aid on students' educational investments. More generally, we should expect that prolonged investments in human capital will be more sensitive to permanent increases in financial aid than to transitory increases. This insight has relevance for the design of policies which are intended to increase access to higher education among low-income students.

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