Aiding or Dissuading? The Effects of Exhausting Eligibility for Need-based Aid on Bachelor's Degree Attainment and Time to Completion

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ABSTRACT

While studies find that need-based financial aid can increase college access, persistence, and completion, little is known about how tuition subsidies affect students after they have enrolled in college and how losing aid impacts postsecondary attainment. I shed light on these questions by exploiting recent changes to federal Pell Grant eligibility rules that reduced the lifetime availability for aid from 9 to 6 full-time-equivalent years. Using longitudinal data from the University System of Georgia, I compare student outcomes before versus after the rule change for Pell recipients who were affected and unaffected by the new lifetime limit. The results indicate that aid exhaustion required students to borrow more, which in turn accelerated their time to completion. The rule change increased the probability of term-over-term re-enrollment and bachelor's degree completion within 8 years by 2-3 percentage points. Students who had at least one year to adjust to the new rule before exhausting aid eligibility were also much more likely to graduate before experiencing aid losses. Importantly, I find no evidence that the rule change decreased the overall probability of degree attainment. The findings imply that decisions over how to allocate financial aid are consequential. Disbursement policies can affect time to degree and the cost-effectiveness of aid expenditures.

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I. INTRODUCTION

For decades financial aid has been a widely utilized strategy to support access to higher education and postsecondary attainment. In the fifty years since the passage of the federal Higher Education Act of 1965, average aid per student has more than tripled in real dollars, from $3,800 (in 2017 dollars) to $14,500, largely due to the expansion of federal aid programs (Baum, Elliott, & Ma, 2014; Dynarski & Scott-Clayton, 2013). Despite this growth in spending, many students who attend college withdraw before earning a certificate or degree. Less than one-third of degree-seeking students who enter community colleges earn an associate's or bachelor's degree within six years of initial enrollment, and nearly 40 percent of students who begin at four-year institutions exit without a degree.[1] The size of aid expenditures and magnitude of dropout have motivated questions about whether financial aid is effectively helping students progress to graduation.

While causal research finds that generous and simple aid programs can increase access and produce long-term impacts on completion and earnings (Angrist, Autor, Hudson, & Pallais, 2016; Bettinger, Gurantz, Kawano, & Sacerdote, 2016; Castleman & Long, 2016; Dynarski, 2003; Goldrick-Rab, Kelchen, Harris, & Benson, 2016), little is known about how financial aid affects students after they have enrolled in college. In particular, although most graduates take longer than is customary to finish, it is unclear whether aid that gets disbursed beyond the second year of college affects whether students will graduate and how quickly they do so.[2]

[1] Authors' calculations using the U.S. Department of Education's National Center for Education Statistics 2004/2009 Beginning Postsecondary Students Survey.

[2] The majority of students do not graduate on time for several reasons: 1) approximately 35 to 40 percent of them are required to take developmental education courses before they can progress towards degree requirements (Bettinger, Boatman, & Long, 2013); 2) most students also work while attending school, hindering full-time enrollment and slowing progress to completion (Bound, Lovenheim, & Turner, 2012; Scott-Clayton, 2012); and 3) discontinuous enrollment is also widespread. Using administrative data from Florida and Ohio, I calculate that nearly one-third of students take time off for at least one semester before returning to pursue their degree.

Millions of college students also lose financial aid each year (Schudde & Scott-Clayton, 2016; Suggs, 2016), yet evidence on whether aid loss affects attainment is limited and inconclusive. Carruthers & Özek (2016) find that losing merit-based aid in Tennessee accelerated when students left college but had no effect on overall degree completion, while Schudde & Scott-Clayton (2016) find evidence that failure to meet academic requirements for Pell renewal increased dropout in the short-run but also potentially increased long-run transfer and completion for students who persisted. Furthermore, because the studies above have only examined effects of aid loss early in college due to poor academic performance, the findings may not generalize to contexts in which aid is taken away later in college and for non-academic reasons.[3] Knowing whether the effects of aid vary over time and how students respond when aid is taken away thus has important implications for maximizing the allocation of aid dollars to support college student success.

I shed light on these questions by examining the impact of exhausting eligibility for need-based aid on long-term student outcomes. Using matched difference-in-differences (DD) and difference-in-difference-in-differences (DDD) designs, I exploit recent changes to federal Pell Grant eligibility rules that reduced the lifetime cap on aid from 9 to 6 full-time-equivalent (FTE) years beginning in the 2012-13 school year. Because it immediately eliminated a subset of continuing students from receiving need-based aid and reduced award amounts for others, the rule change provides a source of plausibly exogenous variation to estimate the effects of exhausting Pell Grant eligibility on several outcomes, including how much students borrow to pay for college, their investments in academic effort, and the probability of bachelor's degree attainment and time to completion.

[3] For example, in their study of the effects of post-9/11 price shocks on undocumented students in New York City, Conger & Turner (2015) find that tuition increases affected the likelihood of completion for recent entrants but not for students who had already attended college for two or more years when costs increased.

To preview my results, I find that students lost one-third of their grant aid after the new lifetime limit took effect. Students compensated for this loss by borrowing 15 percent more per year to pay for college and increasing their academic effort. The eligibility change increased the probability of term-over-term re-enrollment and bachelor's degree completion within 8 years by 2-3 percentage points, and students who had at least one year to adjust to the new rule before exhausting their eligibility were much more likely to graduate before experiencing any loss of aid. Importantly, I find no evidence that the rule change decreased the probability of degree completion overall. These findings indicate need-based aid has heterogeneous effects along the path to graduation. For students who have demonstrated a sustained commitment to finishing college, setting limits on the availability of need-based aid can accelerate time to completion.

I structure the remainder of the paper as follows. In Section II, I discuss the theoretical motivation for this study and provide details on the change to Pell Grant eligibility. I describe the data, analytic samples, and research design in section III. In section IV, I present the empirical results. I conclude in section V with a discussion of the findings and directions for future research.

II. BACKGROUND

Competing hypotheses posit that financial aid may help or have little impact on progress to degree completion. On the one hand, grant aid may persuade students on the margin of graduating to persist by reducing out-of-pocket expenses. According to standard human capital theory, students are expected to choose to enroll in an additional year of college if the expected lifetime benefit of attending an extra year exceeds the expected lifetime benefit of dropping out. Because this model assumes that students update their expectations with experience, decisions to persist in college may be influenced by changes to the availability of financial aid. For students

on the margin of graduating, losing aid may alter the cost-benefit evaluation enough to induce departure.

In addition to changing the investment value of attendance, students may become acclimatized to receiving aid, making it difficult to forecast and contingency plan for abrupt changes in funding. Older students may also face stiffer credit constraints than their younger, financially dependent peers, making it more difficult to offset grant losses with more student loans later in college (Gichevu, Ionescu, & Simpson, 2012). Even in the absence of borrowing constraints, loan aversion, which is pervasive among college-goers, may dissuade students from replacing grants with loans (Boatman, Evans, & Soliz, 2017; Goldrick-Rab & Kelchen, 2015). All of these scenarios predict that losing eligibility for need-based aid will decrease the probability of persistence towards the beginning and end of college. Furthermore, because individuals weigh losses more than gains (Kahneman & Tversky, 1979), losing aid may in fact have greater consequences on degree completion than receiving aid of equal value.

On the other hand, the effect of financial aid on attainment may diminish as students progress in school. For instance, students may become less likely to update their schooling decisions over time as their investments in college accumulate and the remaining cost to completion declines. Decisions to persist may stabilize over time for this reason and attenuate the impact of aid on persistence as experience accrues. Furthermore, because grant aid offsets the full cost of attendance to students, offering aid late into college may also delay graduation for some students. Several studies find that students respond to the availability of financial aid by strategically adjusting their enrollment intensity to meet renewal requirements (Cornwell, Lee, & Mustard, 2005; Richburg-Hayes et al., 2009; Scott-Clayton, 2011), and while most studies find that these effects are tied to performance incentives, recent experimental evidence finds that the

amount of time grant aid is offered can delay time to completion even when few strings are attached (Angrist et al., 2016). If time to completion is partly a function of the financial pressure students feel to enter the labor market, then eliminating aid eligibility late into college could increase the efficiency of degree production.[4]

Empirically, whether the duration that need-based aid is offered affects progress to completion remains an open question. By focusing on initial or cumulative aid amounts, prior studies have largely ignored whether enrollment duration moderates the effect of aid on persistence and completion. DesJardins, Ahlburg, & McCall (2002) do find evidence that frontloading grants and scholarships would lead to modest increases in persistence at four-year college; however, the robustness and generalizability of this study is unclear because it is based on simulated policy changes using a dated sample of students enrolled at a single institution.

Extending the Literature by Examining Effects of Reducing Lifetime Pell Eligibility

In this study, I examine how exhausting eligibility for federal Pell Grant aid affects borrowing and enrollment decisions for students who are close to graduating. As my review of the literature reveals, no studies of which I am aware have isolated the effect of aid on persistence as it is disbursed late into college. To the extent that aid increases postsecondary attainment, effects may be driven by early subsidies that set students on a path they would follow in the absence of continued support. Alternatively, financial constraints may pose a formidable barrier to attainment along the entire pathway to completion. The findings in this paper help to tease out whether the effects of need-based aid vary with time spent in college and how students who have already made considerable educational investments respond to losing grant dollars.

[4] The decision to delay completion may also intensify in weak economic cycles when labor market opportunities for recent graduates are less certain. The aftermath of the Great Recession, which coincided with enactment of the new lifetime Pell limit, is one such period when financial aid might have provided a stronger inducement to forego graduation until more promising job opportunities became available.

To identify effects on student outcomes, I exploit recent changes to Pell Grant eligibility rules which took effect in the 2012-13 school year. In 2011, the Pell Grant program faced an $18 billion shortfall as a result of growing enrollments in college and recent program changes that made more students eligible for aid.[5] After infusing the program with $17 billion, Congress addressed the remaining funding gap by implementing the following four eligibility changes which applied to both incoming and continuing students:

1) Eliminating eligibility for students without a high school diploma or GED;
2) Eliminating eligibility for students who qualified for the smallest grant amount, equivalent to 10 percent of the maximum award, or $555;
3) Reducing the family income ceiling from $32,000 to $23,000 that automatically qualified students for the maximum award; and
4) Reducing the lifetime duration of eligibility from 9 to 6 FTE years.[6]

I examine effects on student outcomes caused in particular by reducing the lifetime limit for Pell Grant aid.[7] Estimates suggest that nearly 400,000 undergraduates were affected by this provision alone and that students attending four-year institutions were disproportionately impacted.[8]

The U.S. Department of Education (DOE) first announced these eligibility changes to institutions in January 2012, six months before they went into effect. However, DOE did not

[5] The number of students receiving a Pell Grant increased by 13 and 27 percent in 2008-09 and 2009-10, respectively, whereas the year-over-year increase never exceeded 5 percent between 2004-05 and 2007-08 (Mahan, 2011). While part of this increase is attributable to enrollment growth during the Great Recession, Congress also relaxed income eligibility restrictions for Pell Grant aid and increased the maximum grant amount during this time, both of which contributed to skyrocketing program costs (Alsalam, 2013).

[6] As I describe in more detail in the next section, the FTE provision means that lifetime Pell use is determined by two factors: 1) how many years students have received Pell support, and 2) their enrollment intensity (i.e., full-time, part-time, etc.) during those years.

[7] All of the students in my analytic sample are high school graduates who qualify for Pell awards above the minimum amounts. I examine empirically whether the income eligibility change for auto-zero qualification impacts student outcomes. As I discuss in section IV, effects are concentrated among students who did not qualify for maximum awards. I therefore find no evidence that the income restriction affected students' schooling decisions.

[8] For example, the California State University System predicts that 4 percent of its total undergraduate population lost eligibility as a result of the new lifetime limit (Nelson, 2012). If this percentage is nationally representative, then 374,000 of the 9.35 million students enrolled in four-year degree programs are predicted to have been affected. I find that four percent of students in the USG dataset were also potentially affected by the new lifetime limit. However, the USG dataset excludes students who entered as transfer students, and as a result, the 400,000 estimate may be conservative.

contact students about the changes until April 2012, and the only students directly contacted (via e-mail) were those who had received more than 4.5 FTE years of Pell (U.S. Department of Education, 2012). Also in April, DOE provided institutions with a list of the students who were e-mailed. Beginning in July 2012, students could check their lifetime Pell use by logging into the National Student Loan Data System, and the federal aid processing system began automatically flagging students in excess of 4.5 FTE years of Pell for school financial aid administrators. As a result of this communication strategy, students and institutions had very little time to prepare for the eligibility changes when they first took effect in 2012-13, while in later years more time and resources afforded students greater opportunity to adjust their enrollment decisions.

III. EMPIRICAL ANALYSIS

Data

The data for this study are from the University System of Georgia Enterprise Data Warehouse (USG), which maintains longitudinal student-level records at twenty-eight public, four-year colleges and universities in the State of Georgia.[9] The dataset includes records on 301,423 degree-seeking students who first attended a USG institution in the fall term between 2002 and 2008, inclusive. From this data I observe information on students at the time of application, including their demographics and college entrance examination scores. The data also contain the financial information that students and their families supplied when completing the Free Application for Federal Student Aid (FAFSA), including the Expected Family Contribution (EFC) used to calculate how much federal Pell Grant aid students are eligible to receive, and all financial aid disbursements students actually received while enrolled in college. Lastly, the dataset includes complete records of students' enrollment, course-taking, and degrees received across the USG system through summer 2016, as well as records of transfer into USG from other

[9] Since 2013, the Board of Regents of the University System of Georgia has consolidated eighteen institutions into nine for cost-saving purposes. However, because the GDW data include institutional identifiers prior to consolidation, the dataset in practice covers student enrollments across thirty-seven unique campuses.

postsecondary systems. Taken together, this rich dataset allows me to construct a cumulative measure of lifetime Pell receipt for each student and examine how the threat or actual loss of Pell Grant eligibility affects student borrowing, re-enrollment decisions, and bachelor's degree completion.

Defining Treated Students

Because the dataset does not include a direct measure of cumulative Pell receipt, I used reported EFC and disbursed Pell Grant amounts to construct this measure for each student and identify those affected by the new lifetime rule. Specifically, I used the following algorithm to calculate the amount of lifetime Pell students used over eight years:

(1) $$Total\ Pell\ FTE\ Years_i = \sum_{t=1}^{8} \frac{Pell\ Received_{it}}{Max\ Pell\ Eligible_{it} | EFC_{it}, Cost_{st}}$$, where

FTE years of Pell received for student i in year t was calculated as the amount of Pell aid received relative to the maximum amount the student was eligible to receive to subsidize the cost of full-time enrollment at college s. To determine the maximum eligible amounts, I relied on annual Pell award disbursement schedules published by the U.S. Department of Education (DOE), which identify the grant amounts students qualify for as a function of their EFC and cost of attendance.[10]

In most years in this study, Pell recipients who enrolled full-time over an entire school year received one FTE year of Pell. Recipients who enrolled less than full-time received less than one FTE year of Pell, with the specific amount determined by the student's EFC and enrollment intensity (i.e., whether the student enrolled three-quarters-time, half-time, or less-than-half-time in each term that Pell aid was disbursed). An exception to these rules occurred in the 2009-10 and 2010-11 school years, when Pell recipients could qualify for a second award in the same

[10] While the USG dataset includes the EFC for students who filed the FAFSA, it does not include the cost of attendance charged to each student; however, this does not preclude using the disbursement schedules to identify eligible award amounts because the cost to attend USG institutions is sufficiently high that eligible award amounts in practice are based solely on EFC for the vast majority of students in the system. This holds, with very few exceptions, for students across all school years, institutions, and living arrangements in this study.

year to subsidize the cost of summer attendance. In those two years some students therefore accumulated more than one FTE year of Pell during a single award year. Nationally, 1.2 million students (13 percent of all Pell Grant recipients) received supplemental awards in 2010-11, which increased the average grant per recipient by approximately $200, or 6 percent (Baum et al., 2014; Delisle & Miller, 2015). In my main analytic sample, which I describe in the next section, 26 percent of students received more than one FTE year of Pell in either 2009-10 or 2010-11.

While students remain eligible for Pell Grants until they receive 6 FTE years of Pell under the new lifetime limit, I define treated students as those who enrolled on or after spring 2012 and received 5 or more FTE years of Pell (i.e., 5 Pell FTE students). The rationale for defining treated students more expansively than is set by the statutory limit is twofold. First, under the new eligibility rules, students whose cumulative Pell receipt exceeds 5 FTE years receive proportionally smaller award disbursements in new award years until they reach the 6 FTE year lifetime limit. For example, a student with an EFC of $1,000 who had received exactly 5 FTE years of Pell through 2011-12 would have been eligible for a Pell Grant of $4,600 in 2012-13, whereas the largest grant available to a student who had accumulated 5.5 FTE years of Pell with the same financial need would have been $2,300. Including 5 Pell FTE students in the treated group therefore accounts for effects resulting from declines in aid generosity before complete aid exhaustion.

It is also possible that the new lifetime limit created anticipatory effects which altered the probability of aid exhaustion in the post-policy period. Under the new rules, students may have chosen to withdraw before they experienced award reductions if they knew they would need to complete more than one year of coursework to graduate. Students aware of the new limit may also have increased their enrollment intensity in order to graduate before experiencing aid losses. As a result, including 5 Pell FTE students in the treated group also allows for estimation of both direct and anticipatory policy effects.

Sample

If all Pell recipients made the same enrollment decisions over time, then students affected by the new lifetime limit could be identified by when they first entered college. In practice, which students are affected is more complicated because students' enrollment decisions are not uniform or random. Some students enroll continuously, while others stop out. Some students also alter their enrollment intensity over time. Because these endogenous decisions affect who is at risk of exhausting aid eligibility and they are correlated with academic outcomes, straightforward comparisons of treated and non-treated students will lead to biased estimates of policy effects. A key challenge in this study is therefore identifying an analytic sample that accounts for selection into the treatment group.

I address the selection issue by conditioning the sample in three steps. In step one, I limited the sample to students for whom complete Pell receipt is observed by excluding individuals who transferred into the USG system at any time. Of the 301,423 students in the dataset, this restriction eliminated 18 percent of the sample. In step two, I further conditioned on students who enrolled in college for 5 or more FTE years (5 FTE students) to isolate the subset potentially impacted by the new lifetime Pell limit.[11] 46,766 students (19 percent of the remaining sample) attained 5 FTE status. Lastly, in step 3, I restricted the dataset to a matched sample of 5 FTE students in which non-5 Pell FTE students are observably similar to the group of 5 Pell FTE students. Matching addresses the fact that even after conditioning the sample on 5 FTE students, the majority of 5 Pell FTE students are observably different from other 5 FTE students on key dimensions. Table 1 presents summary statistics by treatment status for the overall 5 FTE sample and the subset of matched students. Compared to the overall sample, 5 Pell FTE students are more likely to be female (62 percent vs. 56 percent) and Black (61 percent vs.

[11] I use a modified version of equation (1) to calculate FTE status for all students, where students' EFC-eligibility status is ignored and FTE status is determined solely by enrollment intensity. For example, a student who attempted 12 credits during fall 2010 and 6 credits during spring 2011 would be assigned an FTE of 0.75 for that year (i.e., 0.5 for full-time enrollment in the fall and 0.25 for part-time enrollment in the spring). An attractive feature of this approach is that Pell FTE years and FTE years are derived from a consistent set of rules.

41 percent), have greater financial need according to their EFCs at entry and in year 5 ($1,092 vs. $14,434 at entry, for example), and entered college with lower average SAT scores (928 vs. 1,012).

I used the coarsened exact matching (CEM) procedure developed by Iacus, King, & Porro (2012) to construct the matched sample. This procedure allows the researcher to identify which characteristics to match on and specify how to coarsen the data for matching (if at all), and then exactly matches treated and non-treated observations using the coarsened data. CEM has a number of attractive properties over more traditional matching methods like propensity score matching. First, it obviates the search for a suitable matching algorithm to achieve ex-post balance. Second, it avoids creating uninformative matches that approximate random matching and can produce more biased inferences than not matching at all (King, Nielsen, Coberley, & Pope, 2011). CEM also ensures that matching on a subset of observed variables has no effect on the imbalance of variables not used in the matching procedure. Checking for balance on non-matched covariates therefore sheds light on the plausibility of the key assumption of matching: that treatment is independent of potential outcomes conditional on the covariates used to match treated and non-treated observations.

I matched 5 Pell FTE students to other 5 FTE students using the following baseline characteristics: entry cohort (not coarsened), sex, race (White, Black, or Other), an indicator of whether the student enrolled continuously or stopped out in the first four years of college, number of years to attain 5 FTE status (not coarsened), quartile of cumulative credits attempted at the start of students' 5 FTE year (not coarsened), EFC in the 5 FTE year ($0-$1,300, $1,300-$2,600, $2,600-$5,200, > $5,200), and an indicator of whether each student enrolled on or after fall 2012.[12] This procedure generated a matched sample of 16,588 students composed of 8,656 5

[12] The upper bounds of the bottom three attempted credit quartiles are: 126, 136, and 146 credits.

Pell FTE and 7,932 other 5 FTE students.[13] Below, I examine the sensitivity of the results to alternative matching decisions as a robustness check.

Characteristics of the matched sample are reported in columns 3-6 of Table 1. By design, treated and comparison students exhibit the same mean characteristics on the variables that were used in the matching procedure. For example, 62 percent of both groups are now female and the mean number of credits attempted at the start of students' 5 FTE year is 136 in both groups. In addition, both groups are now quite similar on non-matched measures of academic performance at entry and in college. The mean difference in SAT achievement between groups in the matched sample is now just 22 points (928 vs. 950 among treated and non-treated students, respectively), and students in both groups completed the same number of credits (121) and earned the same grades (2.65) on average at the start of their 5 FTE year.

The key difference that distinguishes 5 Pell FTE from other 5 FTE students is the number of years of Pell received. On average, 5 Pell FTE students received 5.6 years of Pell compared to 3.2 years for other 5 FTE students. Because I matched students on EFC in the 5 FTE year to ensure treated and comparison students had similar income profiles when the new lifetime limit took effect, there are two major reasons why some students in the matched sample received less than 5 FTE years of Pell. First, 21 percent of other 5 FTE students failed to file the FAFSA at the start of college, whereas only 1 percent of 5 Pell FTE students did not submit the FAFSA in their first year. Second, many of the other 5 FTE students who applied for aid were not initially eligible for a Pell Grant. The average EFC at entry among other 5 FTE students was $6,642 (in 2016 dollars), which exceeds the maximum value for Pell receipt. In summary, whereas 5 Pell FTE students filed the FAFSA consistently and regularly received Pell aid, other 5 FTE students

[13] 88 percent of 5 Pell FTE students and 21.5 percent of other 5 FTE students are included in the matched sample.

did not. As a result, their eligibility for need-based aid remained unaffected when Congress reduced the lifetime limit to 6 FTE years.

My empirical strategy rests on comparing the outcomes of 5 Pell FTE students to other 5 FTE students in the matched sample before versus after the lifetime rule change took effect. To obtain unbiased estimates of policy effects, I must therefore assume that differences between students who enrolled before versus after the rule change are stable across groups in the matched sample. If this holds, then I can obtain unbiased causal estimates by differencing out any selection effects observed within each group over time.

I examine evidence for differential selection in the matched sample in Table 2. Columns 1-4 report group-specific means before versus after the rule change was introduced. In columns 3 and 6, I present estimates of within-group selection effects. Column 7 shows whether those changes differed for 5 Pell FTE and other 5 FTE students. The results in column 3 indicate that 5 Pell FTE students who enrolled post-2011 entered college with more financial resources than previously enrolled students but were also more academically disadvantaged. However, because these differences (and all others reported in Table 2) are also observed in the other 5 FTE group, there is no evidence of differential selection between groups in the matched sample. Again, this holds by definition for the characteristics on which students were matched as well as for all observable dimensions on which students were not matched.

Empirical Strategy

I estimate intent-to-treat effects on students' term-by-term enrollment decisions and financial aid receipt using a matched difference-in-difference-in-differences (DDD) strategy, where the first difference is before versus after the policy change, the second difference is whether or not a student ever received 5 FTE years of Pell, and the third difference is whether or

not a student had received more than 4.5 FTE years of Pell at the start of each academic term. This design is implemented by fitting the following statistical model to a student-by-term dataset:

$$(2)\quad Y_{ijt} = \alpha_1 Treat_i * Post2011_t * Post4.5FTE_{it} + \alpha_2 Post2011_t * Post4.5FTE_{it} + \alpha_3 Treat_i * Post2011_t + \alpha_4 Treat_i * Post4.5FTE_{it} + \alpha_5 Treat_i + \alpha_6 Post2011_t + \alpha_7 Post4.5FTE_{it} + \phi X_i + \omega_j + \eta_t + \gamma Treat_i * t + \varepsilon_{ijt}$$

In equation (2), Y_{ijt} is a measure of financial aid receipt, re-enrollment, term credits attempted or earned, or term GPA for student i in term t at college j. $Treat_i$ is an indicator for students who received 5 or more FTE years of Pell. $Post2011_t$ is an indicator for terms on or after fall 2012 when the new lifetime limit took effect. $Post4.5FTE_{it}$ is an indicator set to one in terms after students attained 4.5 FTE status and is zero otherwise. Because it is unlikely that treated students far from the eligibility limit in 2012-13 would have immediately changed their enrollment behavior, this indicator is used to identify when treated students would have been likely to respond to the policy, if at all, based on their proximity to the lifetime limit. I define terms post-4.5 FTE as treated terms given that the DOE initially only notified this group of students about the rule change and later developed warning alerts in the aid processing system specifically for them.[14] α_1 captures the effect estimate of interest. It represents the average difference in outcomes of 5 Pell FTE versus other 5 FTE students in terms after versus before exceeding 4.5 FTE years and after versus before the policy change. In theory, one might expect policy effects to vary by the size of the Pell Grants students received. Unfortunately, award amounts vary too little in the matched sample to model effects continuously.[15] I therefore

[14] In equation (2), The $Post4.5FTE_{it}$ indicator also accounts for secular enrollment trends that arise as students spend more time in college (e.g. if students naturally tend to re-enroll at higher rates or take larger courseloads as they get closer to graduation).

[15] For example, the 25th percentile of the eligible award amount in the matched sample is $5,000 (in 2016 dollars) and the maximum eligible amount is $6,050.

estimate average treatment effects, but I examine if effects vary for students eligible for awards less than versus more than $5,000.

To increase the precision of the estimates and reduce bias, I also include a vector of individual-level covariates (X_i) not used in the matching procedure. This set is comprised of the following controls: race dummy variables (Black, Hispanic, Asian, Other, and Missing race/ethnicity), indicators for U.S. citizenship status and Georgia residency status, an indicator of whether each student initially pursued a bachelor's degree at entry, and an indicator of whether each student was assigned to remedial coursework at entry, as well as continuous measures of age at entry, SAT math and verbal scores (imputed where missing), and the student's Expected Family Contribution at entry (imputed where missing).[16] The model also includes school (ω_j) and term (η_t) fixed effects, as well as a linear term trend (γ) that allows enrollment patterns to vary by treatment status. Finally, assuming that the differences in outcomes between 5 Pell FTE and other 5 FTE students would not have changed over time in the absence of the eligibility change, ε_{ijt} represents a mean-zero random error term. In all estimates, I report standard errors that account for the potential clustering of schooling behavior within USG campuses.[17]

Because degree attainment is a singular event for most students, effects on the probability of bachelor's degree completion and time to degree cannot be estimated using the same student-by-term framework as in equation (2). Instead, I estimate degree effects at the student level using a difference-in-differences (DD) design, where the first difference is before versus after the policy change and the second difference is whether or not a student ever received 5 FTE years of Pell. This model takes the following form:

[16] Missing SAT scores and EFC at entry are predicted using the full set of non-missing baseline characteristics. In all results, I present estimates from multiple imputation regressions that account for uncertainty in the imputed values for students with missing data.

[17] I cluster standard errors by the 37 unique campuses prior to USG consolidation activities.

$$(3) \qquad Y_{ij} = \beta_1 Treat_i * Post2011_i + \beta_2 Post2011_i + \beta_3 Treat_i + \phi\, X_i + \omega_j + \varepsilon_{ijt}.$$

In equation (3), Y_{ij} is a measure of bachelor's degree attainment overall or within a specific time interval (e.g., before 6 FTE years) for student i at college j. All other terms are defined as above, and the coefficient on the interaction term (β_1) is the parameter of interest.

IV. RESULTS

Preliminary Graphical Evidence

Comparing the re-enrollment and degree attainment trends of 5 Pell FTE and other 5 FTE students suggests the lifetime rule change caused treated students to increase their effort and graduate more quickly. To illustrate this, I plot re-enrollment rates by treatment status and enrollment status relative to the policy change in Figure 1. In the figure, the solid lines show re-enrollment rates for students enrolled in the post-policy period and the dashed lines show the same for students who last enrolled before the new lifetime limit took effect. 5 Pell FTE students and other 5 FTE students are denoted by white and black circles, respectively. Differences between the solid and dashed lines before versus after students attained 5 FTE status approximate the DDD estimates from equation (2). In terms before 5 FTE attainment, treated students re-enrolled at slightly higher rates than comparison students; however, the relative difference between the two groups remained constant over time. By comparison, treated students in the post-policy period were more likely to re-enroll after receiving 5 FTE years of Pell compared to 5 Pell FTE students who last enrolled in the pre-policy period, whereas the re-enrollment rate among other 5 FTE students did not change before versus after the new lifetime limit took effect.

Importantly, in each time period 5 Pell FTE and other 5 FTE students made nearly identical enrollment decisions in the first four years of college. Those results, presented in Figure

2, provide additional evidence that the spike in re-enrollment observed among treated students is unlikely to be a random artifact of the data. Descriptive patterns of time to bachelor's degree completion also provide visual evidence of an acceleration effect. In Figure 3, I plot rates of bachelor's degree completion before 6 FTE years for 5 Pell FTE (dashed line) and other 5 FTE (solid line) students by year of 5 FTE attainment. The trends show that both groups of students followed similar attainment trajectories prior to 2012, yet after the policy change, treated students were much more likely to graduate before reaching the point of aid exhaustion.

First-Stage Effects on Financial Aid Receipt

Table 3 shows estimated effects of the rule change on financial aid receipt. In column 1, I report estimates from a student-by-year panel restricted to four terms preceding and three terms following 5 FTE attainment, with terms after 4.5 FTE years defined as treated terms. After the rule change took effect, 5 Pell FTE students received $443 less Pell aid per term on average, which represents a 33 percent reduction in grant aid. As discussed above, treated students did not lose their Pell aid in full because students who received 5 FTE years of Pell at the beginning of an award year remained eligible for full awards. The results in panel B indicate that students did not replace lost Pell Grant dollars with other sources of grant aid.[18] However, they did compensate by borrowing more – $408 on average per term – which amounts to a 15 percent increase in loan receipt. The share of aid that 5 Pell FTE students received in the form of loans thereby increased from 66 percent to 76 percent after the new eligibility regime took effect. In column 2 of Table 3, I show that the results are similar if I condition the data on only terms in which students attended college. In Appendix Table A1, I further show that the estimates are robust to additional sample restrictions and to the inclusion of student fixed effects, which

[18] This in part reflects the fact that Georgia does not offer state need-based aid to students.

account for any estimation bias due to time-invariant, unobserved student characteristics that are correlated with aid receipt and exposure to the rule change. In summary, the net effect of the policy change on total aid dollars appears to be zero, but treated students received more of their aid in the form of loans after Congress reduced the lifetime eligibility limit.

Effects on Re-enrollment, Credits, and GPA

In Table 4, I report results from estimation of equation (1) on short-term academic outcomes to examine whether increased borrowing altered students' enrollment behavior. As in table 3, I report results using the full student-by-term sample in column 1 and an enrollment-conditioned sample in column 2. Consistent with the graphical evidence presented earlier, the coefficient in panel A indicates that after attaining 4.5 FTE status, term-over-term re-enrollment increased by 2.8 percentage points (3 percent) for treated students in the post-policy period. The estimates in panels B and C indicate the new lifetime limit also increased the number of credits treated students attempted and earned per term by approximately 0.5 credits.

The enrollment-conditioned estimates in column 2 shed light on whether the overall impacts in column 1 are driven by effects on the extensive margin (i.e., by increasing re-enrollment) or on the intensive margin (i.e., by inducing enrolled students to increase their academic effort). The point estimates in panels B and C of column 2 are roughly one-half the magnitude of the estimates in column 1, and the coefficient on credits completed remains statistically significant. This provides some evidence that the lifetime limit affected not only whether students re-enrolled in college but the intensity with which they did so. In panel D of column 2, I find no evidence that losing aid affected the grades students earned. The coefficient on term GPA is small (0.026) and effects larger than .095 points can be ruled out. As in the case of the estimates on financial aid receipt, the effects on short-term academic outcomes appear

robust to alternative sample restrictions and the inclusion of student fixed effects. Results of these robustness checks are presented in Appendix Table A2.

Effects on Bachelor's Degree Attainment and Time to Completion

To examine effects on the probability of degree completion and time to degree, I turn to results from estimation of equation (3). I present the main results in Table 5 with and without the inclusion of student-level controls in even- and odd-numbered columns, respectively. In the first two columns, the estimates on the interaction term indicate that losing aid did not affect the overall likelihood that students graduated before exhausting their aid eligibility. The coefficients with and without controls are both near zero and non-significant. However, this null effect is not surprising given that more than 80 percent of treated students in the study sample attained 5 Pell FTE status on or before 2012-13 and therefore had little time to graduate before reaching the lifetime limit.

In the next four columns, the estimates indicate that the new lifetime rule increased the probability that treated students graduated in years 6-8 following entry. The coefficient in column 4 is suggestive of a 2.4 percentage point (3 percent) increase in bachelor's degree attainment within 8 years, although it is only marginally significant at the 10 percent level. In column 6, the coefficient on degree completion in years 6-8 is larger in magnitude and more precisely estimated. Reducing the lifetime Pell limit increased the probability of bachelor's degree completion within this timeframe by 3.1 percentage points, or 7 percent. The last two columns of Table 5 report effects on bachelor's degree attainment overall. The coefficients are positive, but one-third the size of the effects on completion in years 6-8 and not distinguishable from zero. Taken together, this evidence suggests that treated students were no more likely to

graduate overall or before reaching the eligibility limit, but nevertheless accelerated their time to completion after losing access to Pell aid.

The graphical evidence in Figure 3 suggests that pooled analyses may conceal dynamic effects on completion according to how much time students had to react to the new lifetime limit. In Table 6, I therefore examine degree effects by year of 5 FTE attainment. As expected, the results in column 1 indicate that treated students who attained 5 FTE status before or in the first year of the new regime were no more likely to graduate before exhausting their eligibility for aid. However, students who attained 5 FTE status after 2012-13 had more opportunity to adjust and were increasingly likely to graduate before their 6 FTE year. The effect estimates for students who attained 5 FTE status in 2013-14 and 2014-15 are 6.7 percentage points and 17.5 percentage points, respectively, which represent relative impacts of 15 percent and 53 percent over comparison group students who attained 5 FTE status in the same years.

I also reject that the impacts on graduating before aid exhaustion are time invariant (the p-value on an F-test of equal effects is less than .001). By contrast, I find no evidence of dynamic effects on bachelor's degree attainment within 8 years or overall in columns 2 and 3 of Table 6.[19] Thus, treated students appear to have reacted to the new lifetime limit consistently (i.e., by accelerating time to completion), but only treated students who maintained more than one year of eligibility when the new limit took effect increased their likelihood of graduation before exhausting Pell aid.

I also find evidence that effects on time to completion vary by the amount of Pell Grant aid students were eligible to receive, although it is unclear a priori whether effects should rise or

[19] Although the coefficient on overall degree attainment for students who attained 5 FTE status in 2014-15 is substantively large (0.044), this is likely an overestimate since most students in this group are members of the 2008 entry cohort and I only observe degree completion through 8 years for this group. Because the point estimates on overall attainment are near zero for students I observe over a longer time horizon, I expect the estimate for the 2014-15 group would attenuate over time.

fall with financial need. On the one hand, students who are eligible for larger awards stand to lose more and might increase their effort more as a result. However, if students with greater financial need also juggle more commitments outside of school, they may have less flexibility to increase their enrollment intensity. In panel A of Table 7, I disaggregate the effects for students who were eligible for Pell Grants of less than versus more than $5,000 to examine this question. All of the acceleration effects are driven by students eligible for less than $5,000 of aid. Degree completion prior to 6 FTE years increased by 7.6 percentage points for this group, whereas the point estimate for students eligible for larger awards is slightly negative (-0.019) and not significant, and I reject that the effects are equal for both groups (the p-value from the joint F-test is .009).

In panel B of Table 7, I examine variation in effects by total credit completion at the start of students' 5 FTE year. Students further from degree completion should be most responsive to the rule change since students close to degree completion may not need to alter their enrollment at all to graduate. For this analysis, I also divide students into two mutually exclusive groups: those who completed fewer than versus greater than 120 credits, with 120 credits serving as a proxy for proximity to graduation.[20] As predicted, all of the acceleration effect is driven by students who completed fewer than 120 credits at the start of their 5 FTE year. In column 1, the estimated effect on completion before the point of aid exhaustion for this group is 5 percentage points. The effect on degree completion within 8 years in column 2 is 6.6 percentage points. The analogous point estimates for students who completed at least 120 credits at the start of their 5 FTE year are both negative (-0.032 and -0.012, respectively) and not significant. Once again, I

[20] Bachelor's degree programs in the USG system require completion of 120 credits at minimum, but graduates on average earn 13 excess credits (Complete College America, 2011).

reject that effects by total credit attainment are homogenous (the p-values from the joint F-tests are 0.041 and 0.027, respectively).

Robustness Check 1: Did Concurrent Policy Changes Drive Acceleration Effects?

The new lifetime limit was enacted in the aftermath of the Great Recession when federal and state governments cut many programs to save costs. In this section, I explore whether the acceleration effects can be attributed to changes to other financial aid programs made around the same time. I examine two concurrent changes to financial aid policy which took effect in 2011-12: 1) elimination of the summer Pell Grant provision, which in 2009-10 and 2010-11 allowed eligible students to receive more than one Pell Grant in an award year, and 2) changes to eligibility requirements for Georgia's merit-aid HOPE Scholarship, which reduced award amounts for some students already enrolled in college who previously received grants covering full tuition costs.[21] If the effects are partly a function of eligibility changes to the state merit aid program, then the point estimates should be larger for merit award recipients. Likewise, if the acceleration effects are due to the year-round Pell rather than the new lifetime limit, the effects should be larger for students who received two Pell awards in the same year.

I present the results of these analyses in Table 8. Panel A reports effects on degree completion separately for students who did and did not receive HOPE funding in their 5 FTE year. Panel B reports analogous results for students who did and did not receive two Pell awards during the 2009-10 or 2010-11 award years. Across all degree outcomes, I fail to reject that effects for HOPE and non-HOPE recipients and for dual Pell versus non-dual Pell recipients are equal (none of the p-values from joint F-tests are below 0.25). Furthermore, the magnitudes of the point estimates are suggestive of larger effects for non-HOPE recipients and for non-dual Pell

[21] In 2010-11, approximately one-third of USG students received HOPE Scholarships. On average, award amounts the following year declined by $300 per semester for students who no longer qualified for full HOPE scholarships (Suggs, 2016). Details about the summer Pell provision are provided in the text on p.9.

recipients. For instance, in panel A of column 1, the estimated effect on completion before exhausting Pell eligibility is 1.3 percentage points for non-HOPE recipients and -1.5 percentage points for HOPE recipients. In panel B of column 1, the point estimates for non-dual and dual Pell recipients are 2.1 and -1.8 percentage points, respectively. The acceleration effects thus do not appear to be driven by coinciding changes to either the federal Pell Grant or Georgia's merit-based aid program.

Robustness Check 2: Stability of Effect Estimates to Alternative Matching Solutions

In addition to concurrent policy changes, another possible concern is that the results may be sensitive to the choice of covariates used in matching or the matching procedure. I therefore examine the stability of the estimates on degree attainment to alternative estimation and matching solutions in Table 9. Column 1 reports the main results from Table 5 for purposes of comparison. To examine whether matching is necessary in the first place, column 2 presents estimates from the main matched sample, but ignoring the matching solution and instead controlling for the full set of covariates used in the matching procedure. The results in columns 1 and 2 differ only with respect to the weights used in estimation. In column 1, the weights depend on the fraction of 5 Pell FTE students in each matched cell. Strata with more treated students are weighted more heavily to obtain the estimand of interest: the average treatment effect on the treated. In column 2, strata with equal shares of 5 Pell FTE and other 5 FTE students receive greatest weight to minimize the variance of the effect estimate. As a result, the estimates in column 2 will only approximate the average treatment effect on the treated if effects are homogenous over students. Consistent with the evidence of heterogeneous effects in Table 7, the estimated effect on degree completion in years 6-8 in column 2 of Table 9 is 3 times larger than the estimate in column 1 (9.2 versus 3.1 percentage points, respectively) because treatment

effects vary over strata. Ignoring the matched solution thus yields upward-biased estimates of the new lifetime limit on time to completion.

Columns 3-6 of Table 9 report estimates of degree effects from four alternative samples that match 5 Pell FTE to other FTE students using fewer observable characteristics. In column 3, students are only matched according to their enrollment status post-2011 and cohort of entry. In column 4, I add the enrollment trajectory measures (i.e., whether students enrolled continuously in years 1-4, total credits attempted at the start of students' 5 FTE year, and years to 5 FTE attainment) to the matching procedure. In column 5, gender and race are also included. In column 6, I include EFC status in students' 5 FTE year but exclude gender and race. The results indicate that the effect estimates are upward-biased when the enrollment trajectory measures are excluded from the matching procedure. For example, the point estimate on degree completion in years 6-8 is 6.3 percentage points in column 3 compared to 2.5 percentage points in column 4. However, once the enrollment trajectory measures are included, the estimates on degree completion within 8 years and in years 6-8 *increase* when imbalance is reduced further by also matching on sex, race, and EFC. The effects in the fully matched sample in column 1 are also more precisely estimated than in columns 4-6, and therefore do not suffer from a bias-variance tradeoff.

In column 7 of Table 9, I add cumulative credits earned and cumulative GPA at the start of students' 5 FTE year to the matching solution to examine if the main results are robust to the inclusion of additional controls. While matching on these additional measures reduces the size of the estimation sample by half and leads to less precise estimates, the magnitude of the coefficients are substantively similar to the main results shown in column 1. Lastly, in column 8 I examine the stability of the estimates to using propensity score matching instead of CEM to

match other 5 FTE students to 5 Pell FTE students.[22] The results are also similar in magnitude to the main results, and even slightly larger. Taken together, the results in Table 9 suggest that matching is necessary to obtain unbiased causal estimates and estimates from the main CEM-matched sample are robust to alternative matching solutions.

V. CONCLUSION

Despite large public investments in financial aid and concern that too many college students are not finishing college or taking too long to do so, little is known about whether aid disbursement policies influence student decisions over whether and when to graduate. Leveraging a recent eligibility change to the lifetime availability of federal Pell Grants, my findings reveal that students are responsive to lifetime eligibility limits for need-based aid and that these limits can be designed to accelerate time to completion. Students who exhausted full or partial eligibility for Pell Grant aid made up for the loss by borrowing 15 percent more on average per term to pay for college. This in turn affected their subsequent enrollment decisions. Reducing the lifetime Pell limit from 9 to 6 FTE years increased the probability of term-over-term re-enrollment and degree completion in years 6-8 since entry by 3 percentage points. Assuming that 400,000 students were initially affected by the new lifetime limit, this translates into 12,000 students who accelerated their time to completion in response to the eligibility change.[23] Degree completion before 6 FTE years also increased 7-18 percentage points (15-53 percent) for students who had at least one year to adjust to the new lifetime limit before

[22] For this procedure, I used the same covariates as in the main CEM matching solution to estimate the probability of 5 Pell FTE attainment after running logit regressions separately by students' post-2011 enrollment status. I then matched 5 Pell FTE to other 5 FTE students with the same enrollment status and similar predicted probability of 5 Pell FTE attainment (i.e., within +/- 0.05 percentage points).

[23] Fifty-one percent of 5 Pell FTE students not enrolled post-2011 graduated in years 6-8 versus 54 percent of treated students enrolled post-2011. As a result, 12,000 students [(400,000*0.54)-(400,000*.51)] are estimated to have graduated more quickly. See footnote 8 for how the estimated number of affected students is derived.

exhausting their eligibility for aid. Importantly, reducing the lifetime availability for Pell aid did not affect the overall probability of bachelor's degree completion.

An obvious question is whether the results would persist if aid eligibility limits affected more students. I believe the findings from this study should be extrapolated cautiously, as the answer to this question is unclear. On the one hand, Congress restricted Pell Grant eligibility to 6 FTE years on the basis of fiscal necessity, not because this limit was known to benefit students. It is therefore possible that more stringent lifetime limits could produce acceleration effects of a similar magnitude. However, students impacted by the current Pell lifetime limit are also distinct with respect to their commitment to degree completion. At some point, excessively stringent aid limits would certainly introduce costs (e.g., increased dropout) that outweigh the benefits realized from the current policy. The tipping point at which lifetime aid limits begin to do more harm than good remains an important question left for further research.

A related question is whether reallocating how aid is disbursed along the path to completion would produce greater benefits than simply eliminating aid that is disbursed late into college. Several recent studies have found that disbursing more generous need-based aid to students in the first two years of college can increase postsecondary attainment and generate positive returns on investment (Angrist et al., 2016; Castleman & Long, 2016; Goldrick-Rab et al., 2016). It may therefore prove more cost-effective to maintain spending on need-based aid but frontload those dollars to help students establish their footing in college. My estimates imply that Pell awards to the approximately 2.1 million first-year recipients per year could have been increased by $705 per year on average if the cost savings from the new limit ($1.48 billion if 400,000 students lost eligibility) were redistributed instead of eliminated. Based on evidence that offering first-year students an additional $1,300 of need-based aid increased bachelor's degree

attainment within six years in Florida by 4.6 percentage points (Castleman & Long, 2016), this implies that reallocating instead of eliminating aid could have achieved the same impacts on time to completion while also increasing overall graduation rates by 2-3 percentage points.

Finally, this study arrives at a time when free college plans are widely popular. Legislation making college tuition-free for certain groups of students has passed in six states as of 2016, and 17 additional states are actively considering free college legislation (Pingel, Parker, & Sisneros, 2016). Despite their intention of making college more affordable, free college plans may have adverse effects, such as incentivizing attendance at resource-constrained institutions where students are less likely to graduate (Svrluga, 2015). The results in this study offer another note of caution: by relinquishing students and their families from having to pay some costs to attend college, free college plans may also hamper efforts to accelerate time to completion.

In closing, the findings from this study indicate that students who have demonstrated a sustained commitment to earning a degree can be encouraged to graduate more quickly by setting limits on the availability of need-based aid. This implies that the effect of need-based aid on attainment is time-varying, and as a result, decisions over how to allocate financial aid are consequential. Disbursement policies can be designed with attention towards accelerating time to degree and can impact the cost-effectiveness of financial aid expenditures.

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Table 1. Summary statistics of full 5 FTE year sample and matched sample by treatment status

	(1)	(2)	(3)	(4)	(5)	(6)
			Matched Sample			
	All students enrolled for 5+ FTE years		Treated: Enrolled for 5+ FTE years and received 5+ years of Pell		Comparison: Enrolled for 5+ FTE years and received < 5 years of Pell	
	Mean	SD	Mean	SD	Mean	SD
Pre-entry characteristics						
Female	0.561	0.496	0.624	0.484	0.624	0.484
Black	0.405	0.491	0.613	0.487	0.613	0.487
Asian	0.063	0.243	0.063	0.244	0.060	0.237
Latino	0.045	0.208	0.039	0.193	0.042	0.201
White	0.447	0.497	0.251	0.434	0.251	0.434
Race Other	0.029	0.167	0.024	0.153	0.023	0.150
Missing Race	0.011	0.103	0.010	0.101	0.011	0.104
U.S. Citizen	0.942	0.235	0.936	0.245	0.932	0.252
GA Resident	0.970	0.171	0.976	0.154	0.961	0.195
BA Degree Program at Entry	0.978	0.148	0.970	0.170	0.978	0.147
SAT Math + Verbal Score	1012	159	928	146	950	147
Missing SAT	0.05	0.217	0.064	0.245	0.046	0.21
Assigned to Remedial Coursework	0.189	0.392	0.244	0.430	0.194	0.395
Age at Entry	18.63	0.91	18.72	1.27	18.69	1.03
EFC at Entry	$14,434	$20,344	$1,092	$2,975	$6,642	$6,702
Missing EFC at Entry	0.155	0.361	0.014	0.119	0.206	0.405
Post-entry characteristics						
Age 5 FTE Year	23.55	1.27	23.51	1.56	23.52	1.31
EFC in 5 FTE Year	$8,201	$14,735	$476	$1,029	$534	$1,244
Cum Credits Attempted at Start of 5 FTE Year	135.30	13.58	136.20	15.74	136.28	13.32
Cum Credits Earned at Start of 5 FTE Year	118.83	15.61	120.46	15.92	120.78	15.09
Cum GPA at Start of 5 FTE Year	2.70	0.52	2.64	0.50	2.65	0.52
Terms to 5 FTE Status	15.71	2.47	15.34	2.33	15.42	2.33
Total Pell FTE Years	2.07	2.26	5.57	0.50	3.20	1.35
Observations	46,766		8,656		7,932	

Source: University System of Georgia administrative records.

Table 2. Matched sample characteristics of treated and comparison students by enrollment status relative to the Pell eligibility rule change

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Treated Students			Comparison Students			
	Did Not Enroll After Fall 2011	Enrolled On or After Spring 2012	Post - Pre	Did Not Enroll After Fall 2011	Enrolled On or After Spring 2012	Post - Pre	DID
Pre-entry characteristics							
Female	0.627	0.622	-0.005	0.627	0.622	-0.005	0.000
			(0.008)			(0.023)	(0.024)
Black	0.631	0.600	-0.030	0.631	0.600	-0.030	-0.000
			(0.018)			(0.022)	(0.023)
Asian	0.063	0.063	0.000	0.055	0.063	0.008*	-0.008
			(0.006)			(0.005)	(0.006)
Latino	0.020	0.052	0.032***	0.027	0.053	0.025***	0.007
			(0.005)			(0.006)	(0.008)
White	0.262	0.243	-0.018	0.262	0.243	-0.018	0.000
			(0.018)			(0.019)	(0.020)
Race Other	0.021	0.026	0.005	0.020	0.025	0.005	-0.000
			(0.003)			(0.005)	(0.005)
Missing Race	0.003	0.015	0.012***	0.005	0.015	0.010***	0.001
			(0.002)			(0.004)	(0.003)
SAT Math + Verbal Score	943	917	-26.232***	958	945	-12.665*	-13.568
			(5.264)			(6.772)	(8.249)
Assigned to Remedial Coursework	0.219	0.262	0.042***	0.180	0.203	0.023	0.019
			(0.015)			(0.019)	(0.021)
Age at Entry	18.72	18.71	-0.009	18.67	18.71	0.047	-0.056
			(0.030)			(0.035)	(0.045)
EFC at Entry	$967	$1,178	206.392***	$6,349	$6,843	539.191*	-332.798
			(70.486)			(268.983)	(280.382)
Post-entry characteristics							
Age 5 FTE Year	23.50	23.52	0.016	23.46	23.55	0.090**	-0.074

			(0.046)			(0.044)	(0.051)
EFC in 5 FTE Year	$500	$460	-39.207	$565	$513	-52.932*	13.725
			(25.528)			(30.448)	(36.492)
Cum Credits Attempted at Start of 5 FTE Year	137.50	135.30	-2.195***	137.87	135.18	-2.681***	0.486
			(0.730)			(0.659)	(0.743)
Cum Credits Earned at Start of 5 FTE Year	119.47	121.15	1.688***	120.11	121.23	1.122**	0.566
			(0.559)			(0.543)	(0.500)
Cum GPA at Start of 5 FTE Year	2.67	2.62	-0.046***	2.66	2.64	-0.017	-0.029
			(0.015)			(0.025)	(0.023)
Observations	3,532	5,124	8,656	3,446	4,486	7,932	16,588

*** p<0.01 ** p<0.05 * p<0.10

Notes: Means are reported in columns (1), (2), (4), and (5). Estimates of pre-post compositional differences are reported in columns (3) and (6). Estimates of the difference in pre-post differences between treated and comparison students are reported in column (7). Standard errors are clustered by institution and reported in parentheses.

Source: University System of Georgia administrative records.

Table 3. Estimates of the effect of the Pell Grant eligibility change on financial aid receipt in terms following 5 FTE attainment

	(1)	(2)
	All student-by-term observations	Restricted to enrolled terms only
A. Pell Grant Aid	-443.288***	-582.163***
	(58.116)	(57.032)
R^2	0.220	0.202
Baseline mean	$1,327	$1,826
B. Other grant aid	30.150	49.772
	(25.108)	(32.955)
R^2	0.092	0.098
Baseline mean	$53	$74
C. Loans	408.054***	454.952**
	(145.994)	(195.708)
R^2	0.131	0.158
Baseline mean	$2,714	$3,769
D. Total financial aid	7.540	-60.062
	(166.901)	(210.731)
R^2	0.180	0.196
Baseline mean	$4,103	$5,679
Student-by-term observations	104,596	92,545

*** p<0.01 ** p<0.05 * p<0.10

Note: The analytic window is restricted to four terms preceding and three terms following 5 FTE attainment, with terms after 5 FTE years defined as treated terms. Student-by-term observations following bachelor's degree receipt and summer terms are excluded. Results are estimated with least squares models that include the full set of controls. See table 5 for details. All models also include linear time trends allowed to vary by treatment status and before versus after the policy change. Robust standard errors, clustered by institution, are shown in parentheses.

Source: University System of Georgia administrative records.

Table 4. Estimates of the effect of the Pell Grant eligibility change on the probability of re-enrollment, credits attempted and earned, and GPA in terms following 5 FTE attainment

	(1) All student-by-term observations	(2) Restricted to enrolled terms only
A. Re-enrolled	0.028***	
	(0.010)	
R^2	0.154	
Baseline mean	0.835	
B. Term credits attempted	0.473**	0.225
	(0.184)	(0.140)
R^2	0.161	0.070
Baseline mean	10.39	12.41
C. Term credits earned	0.470***	0.295**
	(0.163)	(0.144)
R^2	0.158	0.073
Baseline mean	9.13	10.90
D. Term GPA		0.026
		(0.035)
R^2		0.080
Baseline mean		2.55
Student-by-term observations	104,596	92,545

*** p<0.01 ** p<0.05 * p<0.10

Note: The analytic window is restricted to four terms preceding and three terms following 5 FTE attainment, with terms on or after 5 FTE years defined as treated terms. Student-by-term observations following bachelor's degree receipt and summer terms are excluded. Results are estimated with least squares models that include the full set of controls. See table 5 for details. All models also include linear time trends allowed to vary by treatment status and before versus after the policy change. Robust standard errors, clustered by institution, are shown in parentheses.

Source: University System of Georgia administrative records.

Table 5. Estimates of the effect of the Pell Grant eligibility change on bachelor's degree attainment overall and time to degree completion

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	BA before 6 FTE year		BA w/in 8 years of entry		BA in year 6-8 since entry		BA ever	
Post-2011 x Treated	0.004	0.006	0.025*	0.024*	0.036**	0.031**	0.012	0.010
	(0.017)	(0.018)	(0.014)	(0.013)	(0.016)	(0.014)	(0.016)	(0.016)
Treated	-0.031**	-0.030**	-0.002	0.006	0.030**	0.054***	0.006	0.020*
	(0.013)	(0.013)	(0.014)	(0.012)	(0.014)	(0.013)	(0.012)	(0.011)
Post-2011	-0.169***	-0.159***	-0.096***	-0.085***	0.001	-0.003	-0.090***	-0.081***
	(0.017)	(0.016)	(0.015)	(0.016)	(0.017)	(0.016)	(0.017)	(0.018)
R^2	0.029	0.088	0.010	0.091	0.003	0.047	0.011	0.090
Controls	N	Y	N	Y	N	Y	N	Y
Comparison mean post-2011	0.544		0.728		0.454		0.769	
Observations	16,588							

*** p<0.01 ** p<0.05 * p<0.10

Note: Results are estimated with linear probability models. Models with controls are from multiple imputation specifications that include the following covariates: race dummy variables (Black, Hispanic, Asian, Other, and Missing race/ethnicity); U.S. citizen dummy variable; Georgia resident dummy variable; pursued bachelor's degree at entry dummy variable; assigned to remedial coursework at entry dummy variable; SAT math and verbal scores (imputed where missing); age at entry; and the student's Expected Family Contribution at entry (imputed where missing). All models with controls also include institution fixed effects and a constant. Standard errors are clustered by institution and reported in parentheses.

Source: University System of Georgia administrative records.

Table 6. Estimates of the effect of the Pell Grant eligibility change on bachelor's degree attainment by year of 5 FTE attainment

	(1)	(2)	(3)
		Completion outcomes	
	BA before 6 FTE year	BA w/in 8 years of entry	BA ever
5 FTE in 2012-13 or earlier	-0.013	0.020	0.007
	(0.019)	(0.015)	(0.018)
5 FTE in 2013-14	0.067*	0.032	0.006
	(0.038)	(0.036)	(0.031)
5 FTE in 2014-15	0.175***	0.038	0.044
	(0.052)	(0.054)	(0.064)
Tests of equal effects (p-values)	<.001	0.357	0.494
R^2	0.092	0.105	0.108
Comparison mean in 2012-13 or earlier	0.569	0.760	0.801
Comparison mean in 2013-14	0.462	0.639	0.683
Comparison mean in 2014-15	0.328	0.415	0.442
Observations		16,588	

*** p<0.01 ** p<0.05 * p<0.10

Note: Results are estimated with linear probability models that include the full set of controls. See table 5 for details. Standard errors are clustered by institution and reported in parentheses.

Source: University System of Georgia administrative records.

Table 7. Estimates of the effect of the Pell Grant eligibility change on bachelor's degree attainment by eligible Pell award amount and cumulative credit attainment at start of 5 FTE year

	(1) BA before 6 FTE year	(2) BA w/in 8 years of entry	(3) BA ever
A. Eligible annual Pell award amount (in 2016 dollars)			
Less than $5,000	0.076**	0.061**	0.043
	(0.028)	(0.024)	(0.026)
$5,000 to $6,050	-0.019	0.008	-0.004
	(0.020)	(0.017)	(0.020)
Tests of equal effects (p-values)	0.009	0.119	0.165
R^2	0.634	0.795	0.83
Comparison mean post-2011: Pell award < $5,000	0.611	0.782	0.813
Comparison mean post-2011: Pell award of $5,000 to $6,050	0.529	0.715	0.758
Observations		16,588	
B. Credits completed at start of 5 FTE term			
Less than 120 credits	0.050*	0.066***	0.035
	(0.027)	(0.023)	(0.026)
120 credits or more	-0.032	-0.012	-0.012
	(0.021)	(0.018)	(0.019)
Tests of equal effects (p-values)	0.041	0.027	0.177
R^2	0.668	0.804	0.836
Comparison mean post-2011: Completed < 120 credits	0.349	0.596	0.663
Comparison mean post-2011: Completed 120 credits or more	0.701	0.834	0.854
Observations		16,588	

*** p<0.01 ** p<0.05 * p<0.10

Note: Results are estimated with linear probability models that include the full set of controls. See table 5 for details. Standard errors are clustered by institution and reported in parentheses.

Source: University System of Georgia administrative records.

Table 8. Estimates of the effect of the Pell Grant eligibility change on bachelor's degree attainment by HOPE scholarship receipt in 5 FTE year and receipt of two Pell awards in 2009-10/2010-11

	(1)	(2)	(3)
	BA before 6 FTE year	BA w/in 8 years of entry	BA ever
A. Received Georgia HOPE Scholarship in 5 FTE year			
No	0.013	0.031**	0.014
	(0.021)	(0.015)	(0.017)
Yes	-0.015	-0.002	0.002
	(0.037)	(0.025)	(0.023)
Tests of equal effects (p-values)	0.563	0.265	0.636
R^2	0.638	0.797	0.831
Comparison mean post-2011: Did not receive HOPE	0.509	0.702	0.747
Comparison mean post-2011: Received HOPE	0.770	0.897	0.909
Observations		16,588	
B. Received two Pell awards in 2009-10/2010-11			
No	0.021	0.024	0.010
	(0.025)	(0.018)	(0.021)
Yes	-0.018	0.019	0.006
	(0.030)	(0.023)	(0.026)
Tests of equal effects (p-values)	0.359	0.880	0.901
R^2	0.633	0.795	0.829
Comparison mean post-2011: Did not receive two Pell awards	0.539	0.704	0.753
Comparison mean post-2011: Received two Pell awards	0.550	0.764	0.792
Observations		16,588	

*** p<0.01 ** p<0.05 * p<0.10

Note: Results are estimated with linear probability models that include the full set of controls. See table 5 for details. Standard errors are clustered by institution and reported in parentheses.

Source: University System of Georgia administrative records.

Table 9. Robustness of estimates of effects on bachelor's degree attainment overall and time to degree completion to alternative matching solutions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Main Sample		Alternative Matched Samples					
	Matched (CEM)	Unmatched (OLS)	CEM	CEM	CEM	CEM	CEM	PSM
A. BA before 6 FTE year	0.006	-0.011	-0.005	0.004	0.005	0.018	-0.003	0.006
	(0.018)	(0.015)	(0.011)	(0.013)	(0.016)	(0.015)	(0.031)	(0.017)
B. BA w/in 8 years of entry	0.024*	0.019*	0.004	0.008	0.012	0.022	0.035	0.031**
	(0.013)	(0.011)	(0.013)	(0.015)	(0.016)	(0.015)	(0.022)	(0.013)
C. BA in years 6-8 since entry	0.031**	0.092***	0.063***	0.025	0.025	0.031	0.037*	0.054***
	(0.014)	(0.013)	(0.015)	(0.018)	(0.019)	(0.018)	(0.021)	(0.010)
D. BA ever	0.010	0.009	-0.006	-0.001	-0.010	0.015	0.019	0.017
	(0.016)	(0.012)	(0.013)	(0.015)	(0.014)	(0.015)	(0.022)	(0.013)
Covariates used in matching solution								
Cohort		✓	✓	✓	✓	✓	✓	✓
Post-2011 Enrollment Status		✓	✓	✓	✓	✓	✓	✓
Enrolled Continuously in Years 1-4		✓		✓	✓	✓	✓	✓
Credits Attempted at Start of 5 FTE Year		✓		✓	✓	✓	✓	✓
Years to Attain 5 FTE Status		✓		✓	✓	✓	✓	✓
Gender		✓			✓		✓	✓
Race		✓			✓		✓	✓
EFC in 5 FTE Year		✓				✓	✓	✓
Credits Earned at Start of 5 FTE Year							✓	
Cumulative GPA at Start of 5 FTE Year							✓	
Observations	16,588		26,031	25,995	24,844	20,148	8,678	16,588

*** p<0.01 ** p<0.05 * p<0.10

Note: Results are estimated with linear probability models that include the full set of controls. See table 5 for details. Results in column 2 control for the covariates used in matching instead of weighting by the matched stratum. Results in column 8 use propensity score matching to estimate the probability of 5 Pell FTE attainment separately by post-2011 enrollment status and then match students with the same enrollment status and similar predicted probabilities (i.e., within +/- 0.05 percentage points). Standard errors are clustered by institution and reported in parentheses.

Figure 1. Re-enrollment by term within three years of the new lifetime Pell limit, by treatment and enrollment status relative to the policy change

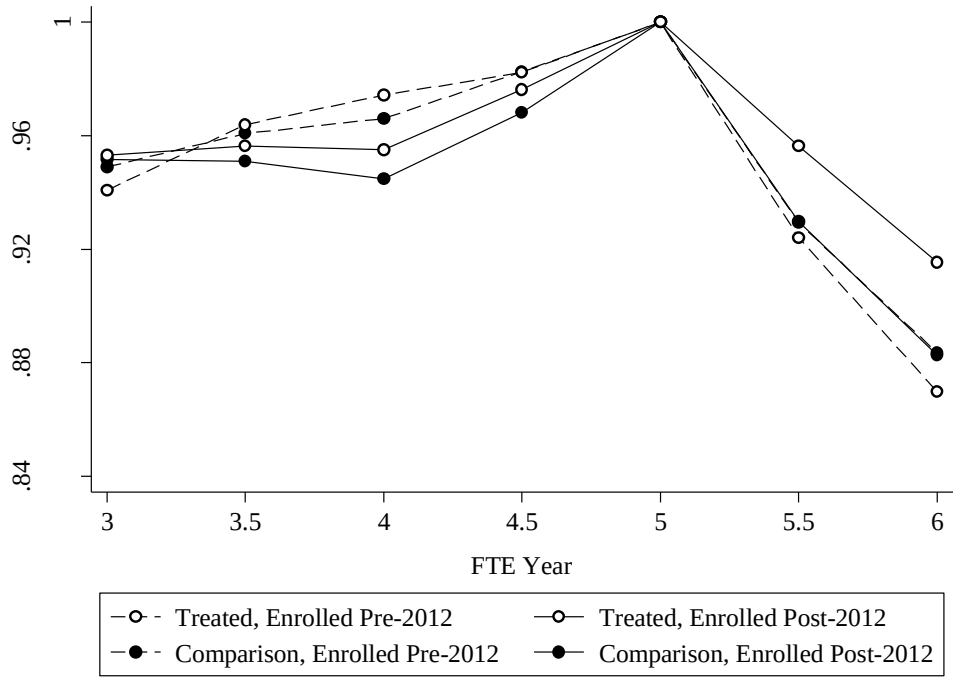
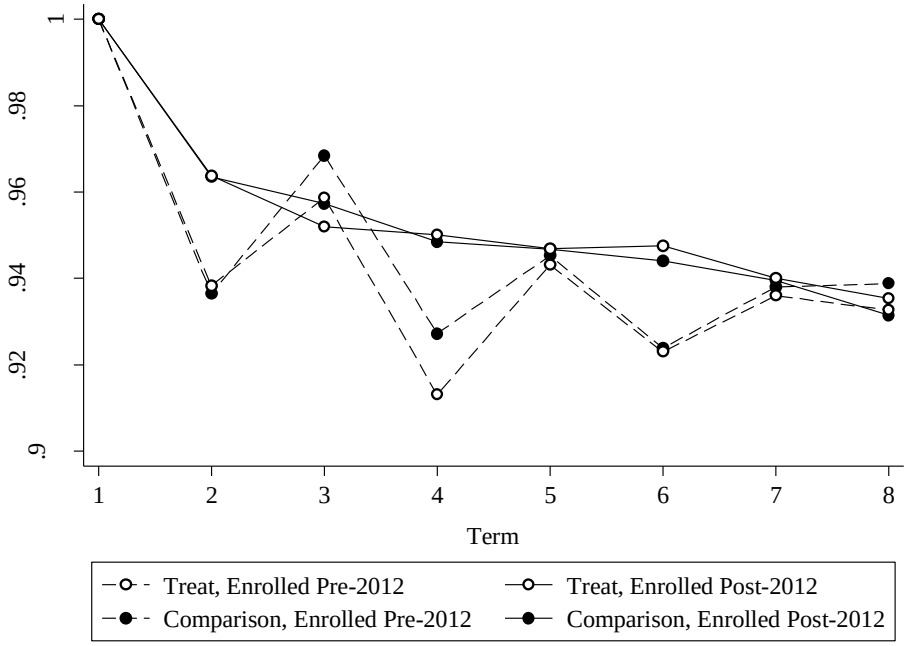


Figure 2. Enrollment and credit attainment in years 1-4 of college, by treatment and enrollment status relative to the policy change

A. Enrollment by term



B. Total credits attempted by term

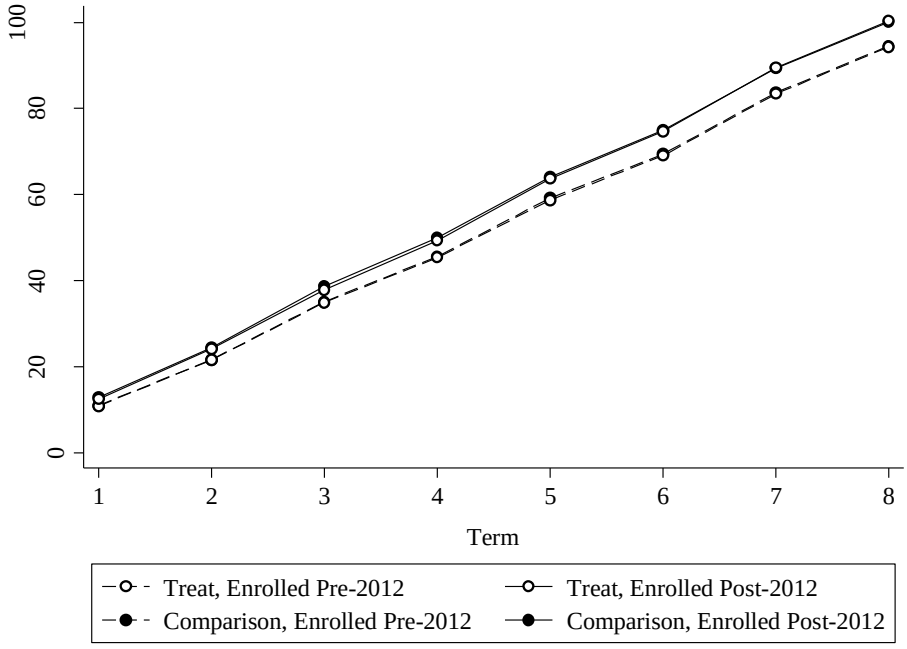
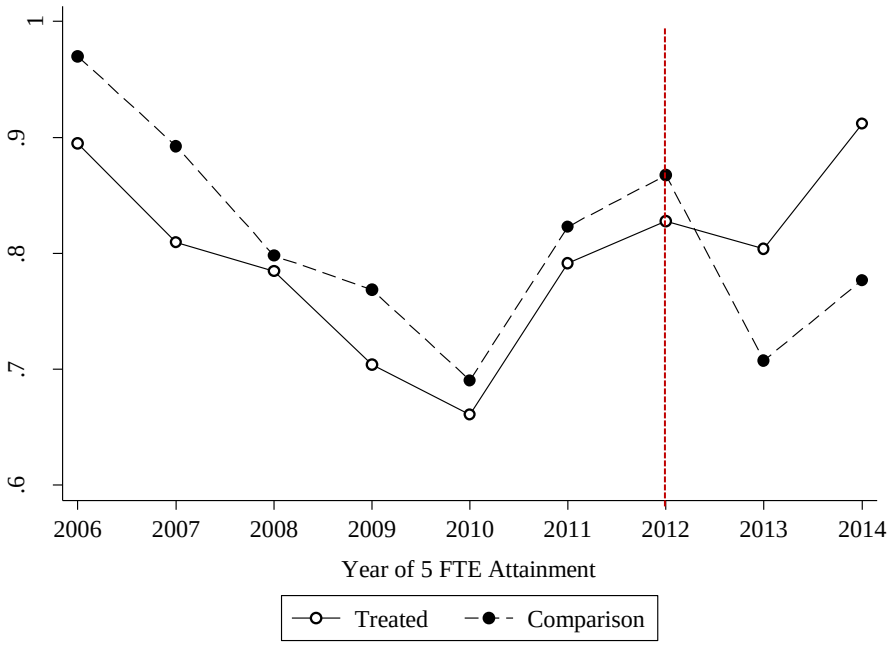


Figure 3. Bachelor's degree completion rates before the attainment of 6 FTE status, by treatment status and year of 5 FTE attainment



Note: Reference line denotes the first year in which the 6 FTE year lifetime Pell limit took effect.

Table A1. Robustness of estimates of the effect of the Pell Grant eligibility change on financial aid receipt in terms following 5 FTE attainment

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Not conditioned on enrollment				Restricted to enrolled terms only			
	Baseline model and sample	+ Student fixed effects	Sample restricted to balanced panel	Sample restricted to graduates	Baseline model and sample	+ Student fixed effects	Sample restricted to balanced panel	Sample restricted to graduates
A. Pell Grant Aid	-443.288***	-412.163***	-491.647***	-498.142***	-582.163***	-524.798***	-720.492***	-630.904***
	(58.116)	(55.273)	(85.981)	(54.962)	(57.032)	(55.361)	(96.253)	(57.253)
R^2	0.220	0.521	0.279	0.188	0.202	0.563	0.251	0.200
Baseline mean	$1,327	$1,327	$1,020	$1,822	$1,826	$1,826	$1,751	$2,019
B. Other grant aid	30.150	29.744	69.569*	35.896	49.772	33.844	73.787	42.139
	(25.108)	(36.514)	(38.657)	(31.685)	(32.955)	(46.925)	(50.875)	(40.983)
R^2	0.092	0.663	0.076	0.095	0.098	0.700	0.084	0.099
Baseline mean	$53	$53	$22	$81	$74	$74	$35	$88
C. Loans	408.054***	376.561**	616.808**	495.069**	454.952**	328.274*	766.637**	324.885
	(145.994)	(183.285)	(286.449)	(187.356)	(195.708)	(181.517)	(319.471)	(202.715)
R^2	0.131	0.571	0.134	0.151	0.158	0.665	0.172	0.170
Baseline mean	$2,714	$2,714	$2,194	$3,381	$3,769	$3,769	$3,787	$3,751
D. Total financial aid	7.540	18.613	218.855	42.834	-60.062	-132.788	153.354	-255.511
	(166.901)	(216.315)	(331.253)	(203.457)	(210.731)	(230.819)	(357.189)	(215.072)
R^2	0.180	0.541	0.183	0.185	0.196	0.641	0.185	0.211
Baseline mean	$4,103	$4,103	$3,239	$5,292	$5,679	$5,679	$5,577	$5,868
Student-by-term observations	104,596	104,596	33,512	80,398	92,545	94,731	27,402	76,427

*** p<0.01 ** p<0.05 * p<0.10

Note: The analytic window is restricted to four terms preceding and two terms following 5 FTE attainment, with terms on or after 5 FTE years defined as treated terms. Student-by-term observations following bachelor's degree receipt and summer terms are excluded. Results are estimated with least squares models that include the full set of controls. See Table 5 for details. All models also include linear time trends allowed to vary by treatment status and before versus after the policy change. In columns (3) and (7), the sample is restricted to students who did not graduate before two terms following 5 FTE attainment. Robust standard errors, clustered by institution, are shown in parentheses.

Source: University System of Georgia administrative records.

Table A2. Robustness of effect estimates on the probability of re-enrollment, credits attempted and earned, and GPA in terms following 5 FTE attainment

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Not conditioned on enrollment				Restricted to enrolled terms only			
	Baseline model and sample	+ Student fixed effects	Sample restricted to balanced panel	Sample restricted to graduates	Baseline model and sample	+ Student fixed effects	Sample restricted to balanced panel	Sample restricted to graduates
A. Re-enrolled	0.028***	0.026**	0.056**	0.027***				
	(0.010)	(0.011)	(0.023)	(0.008)				
R^2	0.154	0.371	0.212	0.0315				
Baseline mean	0.835	0.835	0.704	0.947				
B. Term credits attempted	0.473**	0.413*	0.636**	0.632***	0.225	0.165	-0.047	0.306*
	(0.184)	(0.207)	(0.308)	(0.210)	(0.140)	(0.162)	(0.228)	(0.167)
R^2	0.161	0.393	0.202	0.0701	0.0701	0.350	0.0714	0.0637
Baseline mean	10.39	10.39	8.616	11.98	12.41	12.41	12.25	12.64
C. Term credits earned	0.470***	0.348*	0.481	0.678***	0.295**	0.110	0.124	0.388**
	(0.163)	(0.186)	(0.311)	(0.187)	(0.144)	(0.154)	(0.300)	(0.152)
R^2	0.158	0.411	0.200	0.0677	0.0734	0.362	0.0857	0.0567
Baseline mean	9.13	9.134	6.947	11.07	10.9	10.90	9.802	11.69
D. Term GPA					0.026	-0.000	0.098*	0.035
					(0.035)	(0.038)	(0.056)	(0.036)
R^2					0.0804	0.595	0.0779	0.0803
Baseline mean					2.545	2.545	2.112	2.752
Student-by-term observations	104,596	104,596	33,512	80,398	94,731	94,731	27,402	76,427

*** p<0.01 ** p<0.05 * p<0.10

Note: The analytic window is restricted to four terms preceding and three terms following 5 FTE attainment, with terms on or after 5 FTE years defined as treated terms. Student-by-term observations following bachelor's degree receipt and summer terms are excluded. Results are estimated with least squares models that include the full set of controls. See Table 5 for details. All models also include linear time trends allowed to vary by treatment status and before versus after the policy change. In columns (3) and (7), the sample is restricted to students who did not graduate before two terms following 5 FTE attainment. Robust standard errors, clustered by institution, are shown in parentheses.