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PLUS or Minus?
The Effect of Graduate School Loans on Access, Attainment, and Prices*

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Abstract

In 2006, the federal government essentially uncapped student borrowing for graduate programs with the introduction of the Graduate PLUS loan program. We find that access to additional federal loans increased previously constrained students' borrowing and shifted the composition of their loans from private to federal debt. However, the increase in borrowing limits had no effect on graduate student enrollment or the racial and gender composition of entering graduate students. Further, we find no effects on student graduation or earnings. Finally, we document that programs that were more exposed to the policy increased prices.

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1. Introduction

Graduate school is an increasingly common educational choice. The number of adults with an advanced degree more than doubled between 2000 and 2021 and today, 14.4 percent of American adults hold a postbaccalaureate degree.[1] Student loans are the predominant form of financial aid available to post-baccalaureate students and, as more students enroll in graduate programs, graduate student debt makes up an increasing share of outstanding student loans. In the 2021-22 school year, nearly half of all new federal loans disbursed were to graduate students (Ma and Pender 2022). Yet surprisingly little is known about whether graduate students are helped or harmed by federal loans and whether this source of funding has broader spillovers in the market for higher education.

Increased availability of graduate student loans could raise student human capital investment and earnings if students are credit constrained. However, there may be adverse effects of additional graduate loans if students do not increase their human capital or do not see returns on additional human capital investments.[2] The majority of large-balance student loan borrowers accrued loan debt in graduate school.[3] While graduate student borrowers are less likely to fall delinquent or default on their loan payments than those who borrowed as undergraduates, in recent years, loan repayment has fallen among high balance borrowers, suggesting an increasing burden of such debts (Looney and Yannelis 2019).

In this paper, we study the large expansion in federal loan access caused by the 2006 creation of the Graduate PLUS Student Loan Program (hereafter, "Grad PLUS"). Grad PLUS effectively eliminated federal loan limits for a large number of graduate students by allowing them to borrow up to the total cost of attendance.[4] While all graduate students were eligible for the program, some students or programs benefitted more because of the high cost of their program;

[1] See America Counts Staff, 2019 and https://www.census.gov/data/tables/2021/demo/educational-attainment/cps-detailed-tables.html.

[2] A related literature that examines the effect of student loans for undergraduates finds that access to additional loans can improve student outcomes. Prior studies find that undergraduate students who are constrained by the maximum borrowing limit see increases in educational attainment and earnings from access to additional loans (Black et al. 2020). Several studies have documented positive educational attainment effects in community colleges (Dunlop 2013; Wiederspan 2016; Marx and Turner 2019; Barr et al 2021). Others have found low-income students benefit the most at four-year institutions from access to additional loans (Denning 2019).

[3] In 2014, almost two-thirds of borrowers with student loan balances exceeding $50,000 had borrowed to attend graduate school (Looney and Yannelis 2019).

[4] The cost of attendance includes tuition and fees and the estimated cost of books, supplies, and living expenses for a student's graduate program. In 2003-04, 37 percent of graduate students with federal loans were borrowing at the existing limit (Woo and Shaw 2015).

we take advantage of this variation in exposure to the increase in loan limits to estimate effects on graduate borrowing, enrollment, graduation, degree receipt, and prices.

We use administrative data on graduate student programs offered by Texas public and private not-for-profit universities and detailed student-level records of enrollment, educational outcomes, prices, demographics, and graduation, linked to earnings records from the Texas Unemployment Insurance program, to identify the effects of expanded loan access. We use difference-in-differences designs that compare programs (or people) who were more exposed to the increase in loan availability to those who were less exposed, before and after the increase.

We first examine whether Grad PLUS increased access to graduate education. The Grad PLUS program may facilitate access to high return but expensive programs for students who would have otherwise struggled to finance such degrees with private loans. This may be especially relevant for Black students who – due to historic discrimination in housing and other markets – have access to much lower family wealth, on average, than white students.[5] Holding prices constant, students interested in attending programs that had higher pre-Grad PLUS prices experienced larger increases in effective loan limits, conditional on the price exceeding the baseline federal loan limit. Thus, we compare the demographics of students entering programs that had baseline prices exceeding the pre-Grad PLUS federal loan limit to programs in which students would not have experienced an expansion in loan access due to Grad PLUS (i.e., with baseline prices below the limit) before and after the program was created. Estimated effects are small, precise, and statistically insignificant, suggesting that Grad PLUS did not increase enrollment or affect the composition of students attending programs in which effective loan limits rose to a greater extent. We can rule out effects greater than a 1.8 student increase in enrollment (0.4 percent relative to the average program size) per $1000 increase in loan limits and any positive effect on the percentage of entering students who were Black or Hispanic.[6]

We next examine how expanded loan access affected the outcomes of enrolled students by comparing students who are likely to be affected by the increase in loan availability to those who are unlikely to be affected, before and after Grad PLUS was created. Intuitively, students who

[5] More generally, groups that are underrepresented in graduate school may also require a graduate credential to be on equal footing in the labor market. For example, in 2016, Hispanic workers with a graduate degree only earned slightly more than white workers with a bachelor's degree but no graduate education on average ($55,700 versus $50,000, respectively). See https://nces.ed.gov/programs/raceindicators/indicator_RFD.asp

[6] Denning and Turner (forthcoming) show that enrollment in and access to graduate programs that tend to lead to high earnings was similarly unaffected.

borrowed at the statutory federal loan maximum prior to the implementation of the policy should be more likely to take advantage of additional loan availability than those who borrowed less than the statutory limit. We find that students who were constrained by the pre-Grad PLUS limit increased borrowing by a larger amount than borrowers who were unbound by the limit, and, similar to Bhole (2017), that Grad PLUS led to some substitution away from private loans. However, in contrast to prior research finding positive effects of increases in undergraduate loan limits on human capital accumulation (Black et al. 2020), we find no evidence that Grad PLUS affected constrained graduate students' degree receipt.

Expanded access to federal loans may also have unintended consequences if institutions and programs alter their prices to "capture" some portion of the additional funds. This possibility – labeled the Bennett Hypothesis – has been tested in the case of state and federal grants and undergraduate loan limit expansions, but there is limited evidence on whether it is also relevant for graduate programs.[7] Recent work by Kelchen (2019, 2020) tests for the Bennett Hypothesis in the context of law, business, and medical school programs using the introduction of Grad PLUS and does not find significant effects on program prices.[8] Our work benefits from access to individual level data that allows us to create measures of which programs are more affected and less affected by the Grad PLUS expansion based on student borrowing in the years before Grad PLUS and we find evidence in support of the Bennett Hypothesis. Programs with more students constrained by federal loan limits in the years before Grad PLUS see significantly larger increases in both federal borrowing and prices following Grad PLUS implementation. Further, with student-level data on other sources of financial aid, we can test for changes in grant aid and implications for net prices, both overall and for different student groups. On average, some of the price increase due to Grad PLUS was is offset by increases in grant aid. However, we find no

[7] For undergraduates, research has considered the Bennett hypothesis by looking at aid for veterans (Baird et al. 2022), federal need-based grants (Cornwell, Mustard, Sridhar 2006; Singell and Stone 2007; Turner 2016; Cellini and Goldin 2014), and federal loans (Lucca, Nadauld, and Shen 2019; Gordon and Hedlund 2022; Kargar and Mann forthcoming). These papers are somewhat mixed in their findings often finding some scope for price increases in response to increased student aid availability.

[8] The author uses interrupted time series methods as well as difference-in-differences designs comparing changes in graduate tuition and fees to changes in undergraduate tuition and fees, before and after the creation of Grad PLUS. The former research design cannot account for other time-varying shocks, such as changes in economic conditions, which may also affect pricing decisions. The difference-in-differences approach does address this concern if trends in undergraduate prices following the creation of Grad PLUS provide a good counterfactual for how graduate prices would have trended in the absence of Grad PLUS. At the same time, undergraduate loan limits were increased twice following the creation of Grad PLUS and these increases have been shown to have effects on undergraduate prices (Lucca, Nadauld, and Shen 2019).

evidence that the additional grants were directed at Black, Hispanic, or low-socioeconomic status students.

The rest of the paper proceeds as follows: Section 2 discusses the setting and policy environment. We describe our data and empirical methods in Section 3. Sections 4, 5, and 6 discuss our estimates of the effects of Grad PLUS on graduate program access, prices, and graduate student outcomes, respectively, while Section 7 concludes.

2. Setting and policy environment

Before the introduction of Grad PLUS, most graduate students could borrow up to $18,500 per year from the federal government through the Stafford Loan Program.[9] Some health professional programs had higher limits.[10] The Deficit Reduction Act of 2005 established the Grad PLUS Loan Program, effective July 2006, by allowing graduate students to participate in the Parent Loan for Undergraduate Students (PLUS) Loan Program.[11] Stafford loan limits were also increased by $2,000.

Prior to Grad PLUS, graduate students who faced a cost of attendance that exceeded the Stafford Loan limit needed to make up the difference with private loans, savings, or another source of financing. With the creation of Grad PLUS, students could cover these costs completely with Grad PLUS loans. Specifically, the program allowed a student to borrow up to the total cost of attendance (COA) less any other grants and federal loans. A student's cost of attendance equals tuition and fees and an allowance for estimated living expenses and books and supplies. The allowance for estimated living expenses is almost always set at the institution level and does not vary across students who are not living in housing owned by the institution. However, graduate program prices and the estimated allowance for books and supplies can vary

[9] Only $8,500 of this amount could be in the form of subsidized loans. The Budget Control Act of 2011 eliminated subsidized loan eligibility for graduate students after June 30, 2012. Prior to 2007, graduate students faced a lifetime federal borrowing limit of $138,500.

[10] Medical students (including those studying osteopathic medicine, dentistry, veterinary medicine, and optometry) could borrow an additional $20,000 per academic year while students in public health, health administration, pharmacy, clinical psychology, and chiropractic graduate programs could borrow an additional $12,500 per academic year. Students in these programs faced correspondingly higher lifetime federal borrowing limits as well. See Hegji (2021), Appendix C for additional details.

[11] The PLUS loan program was previously limited to parents of dependent undergraduate students, and we distinguish between this and the program we study by always referring to the Graduate PLUS Loan Program as "Grad PLUS" and the parent program as "Parent PLUS".

substantially across graduate programs.[12] As a result, after the creation of Grad PLUS, the only constraint on how much students could borrow from the federal government came from the institution.

3. Data and analysis samples

To examine the effect of expanded access to loans for graduate school, we use de-identified administrative data from the Texas Education Research Center (ERC). These data come from the Texas Higher Education Coordinating Board (THECB), a state agency that oversees post-secondary education in Texas and contain individual-level information on enrollment, graduation, and financial aid for all graduate students enrolled in public and nonprofit higher education institutions and information on program of study for all public and a subset of private nonprofit graduate students.[13] We link student records to data from the Texas Workforce Commission containing quarterly earnings records for all workers covered by the Texas Unemployment Insurance (UI) system.[14] Our data starts in the 2000-2001 (hereafter, 2001) school year, which is the first year that financial aid information is available.[15]

The coverage of the ERC data makes it close to ideal for studying how the Grad PLUS program affected the decisions of students and schools. Nationwide, public and nonprofit institutions produce more than 91 percent of master's degrees and 95 percent of doctoral degrees (de Brey et al. 2022). This coverage allows us to study the impact of expanded access to loans for most graduate students and programs.

A. Defining Graduate Programs

Conceptually, a program of study is a series of courses in the same field of study in which students seek the same degree from a given institution (e.g., master's degree in social work

[12] Universities have a good deal of discretion over estimated living expenses and for undergraduate students, this portion of the COA can far exceed tuition (Kelchen, Goldrick-Rab, and Hosch 2017).

[13] Some information on for-profit institutions and students is available in THECB data in more recent years but we do not observe graduate student enrollment or outcomes in this sector during the years surrounding the creation of Grad PLUS. Nationally, only 8 percent of graduate students attended for-profit institutions in 2004 (authors' analysis of 2004 National Postsecondary Student Aid Study data, via PowerStats). In Texas, only 2 percent of graduate enrollment was in for-profit institutions in 2004 through 2006 (authors' analysis of IPEDS 12-month enrollment data).

[14] UI records cover employers who pay at least $1500 in gross wages to employees or have at least one employee during twenty different weeks in a calendar year. Students employed by their college or university are not included in the but work study funding is available in the THECB data.

[15] Information on enrollment and attainment within nonprofit institutions is available starting in 2003.

degree at the University of Texas, Austin) and face admissions criteria, tuition, and requirements that are relatively similar within broad categories (e.g., in-state versus out-of-state students). Of particular interest is distinguishing between (primarily doctoral) academic degree programs and professional degree programs because of substantial differences in funding, admissions requirements, and prices. Unfortunately, we do not observe specific programs of study directly in the data, only the credential level (master's degree, professional degree, or doctoral degree) and Classification of Instructional Code (CIP) code, which we use to identify the field of study.[16]

For professional degree-seeking students, we define a program as a unique 4-digit CIP code (i.e., field of study) by institution combination. Distinguishing between master's and doctoral degree-seeking students, however, is more challenging. Specifically, for some 4-digit CIP codes, doctoral degree-seeking students who are likely entering a PhD program are initially classified as master's degree-seeking if they do not hold a master's degree when they first enroll in the program. Thus, to distinguish between students in terminal professional master's degree programs and students in PhD programs who are initially misclassified, we use the percentage of terminal degrees awarded at the master's and doctoral levels within a given 4-digit CIP to assign students to professional master's degree programs and academic doctoral programs. Specifically, if more than 85 percent of degrees within a 4-digit CIP by institution cell are terminal master's degrees, we classify the program as a terminal master's degree program. If less than 85 percent of degrees are terminal master's degrees, we classify it as an academic doctoral program.[17] We make a few exceptions to this classification scheme: all medical/allied health and education programs are assumed to be professional degree programs.[18]

[16] A small number of CIP codes are added, deleted, or combined every decade. We use NCES crosswalks to adjust observed CIP codes to ensure that fields of study are measured consistently over the years our data span. We exclude students listed as being in a medical residency as their field of study. The Online Appendix provides additional details.

[17] As an example, if over 85 percent of degrees awarded to graduates with a CIP code of 4506 (Economics) at the University of Texas at Austin were master's degrees, this would be classified as a terminal master's degree in economics, but if less than 85 percent of degrees awarded were doctoral, all students would be classified as academic doctoral students (even if their highest degree obtained is a master's degree). The one exception to this classification scheme is health programs. Students in a program with a 2-digit CIP code of 51 (Health Professions) are considered to be in a professional degree program even if most of the degrees granted are classified as doctoral. The 85 percent cut-off results in around 80 percent of graduate students in our sample being classified as entering a non-academic program. Our results are robust to using alternative cut-offs in our classification of academic and professional programs; details can be found in the Online Appendix.

[18] Medical/allied health programs are those with a 2-digit CIP code of 51, education programs are those with a 2-digit CIP code of 13. This is to match the treatment of these programs in other nationally representative data sets, such as the NPSAS, which classify these programs as being distinct from academic PhD programs.

Additionally, we combine programs that are classified as academic doctoral degree programs into a single broad category of "academic" programs. This is because it is common for students enrolling in an academic PhD program to receive tuition waivers and stipends, which is generally very different than the (much more limited) funding available to professional degree-seeking graduate students.[19] We also create a few broad groupings of terminal master's and professional degree programs within 2-digit CIP codes: engineering, law, theological and ministerial studies, and education. Remaining students are assigned programs based on 4-digit CIP codes. One limitation of the data is that we only observe nonprofit students' program of study in a limited number of cases: theology, education, law, and chiropractic programs.[20] The complete list of fields of study and corresponding student characteristics and graduation rates can be found in Appendix Table 1.

B. Analysis Samples

We use three analysis samples. First, to examine the effects of access to additional loans through Grad PLUS on graduate school enrollment—overall and among specific demographic groups—we construct a program by year dataset with information on the number of entering students, their demographic characteristics, average loan amounts, and baseline (2006) cost of attendance. We restrict the sample to a balanced panel of programs that had enrollment in each of the 2004 through 2010 academic years.[21] We also exclude programs that had fewer than 20 students per year who filed a FAFSA, on average, in the pre-period.[22] Because we are especially interested in examining how expanded federal loan limits affected the enrollment of Black and Hispanic students, in our main analysis sample we also exclude programs in HBCUs, although our estimates are robust to their inclusion.

[19] We also classify multidisciplinary studies, general studies, and area studies programs as academic, even if more than 85 percent of graduates receive a master's degree.

[20] Private schools in Texas are required to submit a different information to the Texas Higher Education Coordinating Board, which limits our nonprofit sample to these four program types. Graduates from these programs made up 22 percent of all graduate degrees granted by nonprofit institutions in 2006 and overall, we can identify programs for 82 percent of all graduate student degree recipients in 2006 (authors' calculations using IPEDS completions data).

[21] We begin in 2004 because data for private schools begins in 2003. We want to consider students who are enrolling in their program for the first time and the way we determine this is by checking for enrollment in the year prior, hence the 2004 is the earliest year where we can determine if someone is enrolling in a program for the first time.

[22] We make this restriction because a program's COA – our best measure of price – is only available in our data for students who filed a FAFSA.

To study how the loan expansion affected graduate students' outcomes conditional on enrollment, we construct a student-level data set. We follow a strategy similar to that of Black et al. (2020) by limiting our sample to first-time, entering graduate students who borrowed, and compare outcomes for "constrained" compared to "unconstrained" borrowers before and after Grad PLUS loans were available. Constrained borrowers are first year entrants whose federal Stafford Loans were equal to the annual limit and unconstrained are first-year borrowers with federal Stafford Loans below this amount.[23]

Our final analysis sample is used to test whether expansions in loan access through Grad PLUS led to higher program prices. To answer this question, we construct another program-by-year level data set but do not limit the underlying sample to first-time students; rather, we include all students enrolled in the program. Our key outcome is the price faced by students in a given program. Unfortunately, we do not observe tuition directly, only the cost of attendance (i.e., tuition and fees and the estimated cost of books and supplies and living expenses) for students who file an application for federal student aid and enroll in college. Thus, we calculate the average cost of attendance for each program (as defined in the previous section).

The tuition component of cost of attendance can depend on the number of credits a student attempts and their semesters of enrollment. While the program-year-level average cost of attendance is likely highly correlated with the tuition of a program, it could also reflect differences across programs in enrollment intensity and the extent to which students enroll in the fall, spring, and/or summer semesters. This motivates the use of a second cost of attendance measure – the predicted cost of attendance as a function of credits attempted in each semester, whether students attended both semesters, and fixed effects for program and academic year. We use these estimates to predict the cost of attendance for a full-time, full-year student.

C. Characteristics of Texas graduate students

Table 1 shows summary statistics for students enrolled in graduate school for the first time over the 2001-02 (2002) through 2010-11 (2011) academic years combined as well as separately by before and after Grad PLUS implementation. Most (58 percent) first-time graduate students are female mirroring similar ratios at the undergraduate level. Slightly over half of the students entering graduate school are white, 6 percent are Asian, 10 percent are Black, 14 percent are

[23] We adjust limits to account for part-year enrollment, but our results are robust to using the full-year limit for all students.

Hispanic, and 12 percent are international students. On average, new graduate students are 30 years old and 77 percent are in-state students. The demographics of new graduate students do not change substantially before and after the start of Grad PLUS.

Approximately 71 percent of first-time graduate enrollees complete a degree within 6 years of entry. The 6-year completion rate grew from 67 percent in the pre-Grad PLUS cohorts to 74 percent for later cohorts, mirroring similar increases in undergraduate graduation rates (Denning et al. 2022). The most common broad areas of study are education (20 percent), business (13 percent), health (13 percent), and engineering (6 percent).

On average, 37 percent of new students borrow in their first year of attending a graduate program and this fraction grew from 32 percent before Grad PLUS to 42 percent after. Total (inflation adjusted, unconditional) first-year student loan debt grew from approximately $5600 for pre-Grad PLUS cohorts to approximately $7700 for later cohorts. Around 10 percent of students borrowed at or above the Stafford Loan annual limit. Entering graduate students faced an average (inflation-adjusted) cost of attendance of just over $26,000 prior to Grad PLUS and just over $32,000 in later years.

4. Effects of increased loan limits on graduate program access

We first examine the effects of higher federal loan limits on access to graduate education. To do so, we leverage variation in program prices prior to the policy change as a measure of treatment intensity in a generalized difference-in-differences approach. Prior to Grad PLUS, prospective students could fully finance attending only a subset programs with only federal loans. Specifically, student could pay for programs that charged prices below the statutory Stafford Loan limit. Grad PLUS did not expand access to these programs (abstracting from direct effects on prices) and thus, these programs will serve as our control group. In contrast, prospective students interested in programs that charged prices higher than the Stafford limit would not have been able to fully finance these programs with existing federal before the creation of Grad PLUS. As a result, students lacking access to other sources of financing, such as private student loans or savings, may have found it difficult to enroll in these programs. These more expensive programs are where we expect the effects of Grad PLUS-driven loan limit expansion to be most pronounced and are our "treated" group.[24] Approximately 44 percent of

[24] To give a specific example, Grad PLUS should have larger effects on access to a program with a $40,000 COA at baseline than a program with a $20,500 COA (the post-Grad PLUS Stafford limit). The composition of students

programs had baseline prices below the Stafford Loan limit and among programs that would experience borrowing limit expansions based on pre-period prices, the average increase was approximately $6,600 with a standard deviation of $6,000.

Although Grad PLUS increased effective loan limits for students enrolling in treated programs by the amount that the cost of attendance exceeded the Stafford limit, we classify programs based on their baseline (2006) cost to abstract from changes in program prices induced by higher loan limit (i.e., Bennett Hypothesis effects).[25] We estimate reduced form models where the variable of interest is the distance between the pre-period price and pre-period Stafford loan limit ("projected limit increase"). We also estimate instrumental variables specifications in which a program's actual loan limit is the endogenous regressor (because it may be affected by program price after 2006) and the excluded instrument is the projected limit increase. Under the key identifying assumption that changes in other factors affecting access after the creation of Grad PLUS are uncorrelated with the projected limit increase, this approach will provide estimates of the causal effects of loan limits on graduate program access.

Let $LimInc_p = \max\{COA_{p,2006} - Limit_p)\ , 0\}$ represent the predicted increase in borrowing limits due to Grad PLUS (in $1000) for program p, based on the program's baseline (2006) COA and the annual Stafford Loan limit. We estimate event-study models of the following form:

$$Y_{pc} = \sum_{c \neq 2006} \gamma^c \big(LimInc_p * \mathbf{1}[Cohort = c]\big) + \theta_c + \theta_p + \varepsilon_{pc} \qquad (1)$$

Where Y_{pc} is an outcome (e.g., the number of students or the fraction of students belonging to a given racial/ethnic group) for program p and entry cohort c, and θ_c and θ_p are entry cohort and program fixed effects, respectively. This approach uses both the extensive margin of the policy change and a continuous measure of exposure to the policy change as identifying variation, but we show that our results are robust to discretizing our measure of treatment to weaken the

enrolling in programs with COAs below the pre-Grad PLUS limit should not be as affected, as there was no increase in loan limits for students who enrolled in these programs.

[25] We later show that there was an increase in program prices at programs where more students were constrained by pre-Grad PLUS federal loan limits relative to programs where fewer students were constrained. This effect would reinforce our motivation for using pre-Grad PLUS prices to create a measure of the potential increase in limits that is not contaminated by endogenous pricing responses.

identifying assumptions required for a continuous difference-in-differences design (Callaway, Goodman-Bacon, and Sant'Anna 2021).[26]

Point estimates of γ^c and corresponding 95 percent confidence intervals from equation (1) are displayed in Figure 1. We find no evidence that the size or composition of programs where Grad PLUS led to larger expansions in federal loan access was changing differentially before the program was created, providing support for our identifying assumptions.

Figure 1 also shows that in the years following Grad PLUS, there is no evidence that overall enrollment or the demographics of entering students in programs with larger projected loan limit increases changed differentially relative to unaffected programs. To provide a summary measure of these effects, we estimate a more parsimonious model that replaces the indicators for specific cohorts in equation (1) with a single indicator for cohorts that entered after the Grad PLUS program was created, $Post_c$:

$$Y_{pc} = \beta\left(LimInc_p * Post_c\right) + \theta_c + \theta_p + \varepsilon_{pc} \qquad (2)$$

Panel A of Table 2 displays estimates of β from equation (2). We again find small and statistically insignificant effects on enrollment in programs where entering students would have experienced a larger increase in effective borrowing limits. Specifically, a $1,000 increase in the difference between pre-period cost of attendance and the pre-period limit led to 1.4 student decrease in the number of entering students. This represents an approximately 1 percentage point decrease relative to average program enrollment in the pre-period (139 students).

Columns 2 through 5 of Panel A display estimated effects on the composition of enrolled students, including the percentage of entering students who are Black, Hispanic, male, or international. We find small albeit statistically significant *decreases* in the percent of entering students who are Black and the percent who are Hispanic (counter to what we expected) and negative insignificant effects on the percent of the entering cohort who is male or an international student.[27] Specifically, a $1000 increase in the projected limit increase resulted in a 0.04 percentage point decrease in the percent of entering students who are Black, a 0.05

[26] Specifically, a stronger "parallel trends assumption" is required: that for all doses of treatment (i.e., magnitudes of the difference between baseline COA and the Stafford Loan limit), the average evolution in outcomes in the post-treatment period across all units if they had been assigned that amount of dose is the same as the average evolution in outcomes over time for all units that actually experienced that dose of treatment.

[27] Absent program capacity constraints, we would not expect changes in international student enrollment because students in the group are not eligible for federal student loans.

percentage point decrease in the share Hispanic, null effects on the percent male, and a 0.04 percentage point decrease in the percent who were international students.

Panel B displays estimates from IV models in which we instrument for a program's realized limit increase with the projected limit increase. The first stage coefficient is approximately 1.08 suggesting that programs for which Grad PLUS would have led to larger limit increases absent price changes did raise their prices more than programs that were less or unaffected. IV estimates are quite similar to OLS estimates and quite precise—we can rule out increases in enrollment greater than 1.8 students (1.3 percent relative to mean baseline enrollment), suggesting that Grad PLUS did not lead to higher enrollment in programs that had larger expansions in effective loan limits. Upper bounds of estimated 95 percent confidence intervals rule out any positive effect of $1000 increase in loan limits on the share of entering students who were Black or Hispanic. Altogether, these estimates suggest that increases in borrowing limits that were caused by Grad PLUS did not expand graduate program access.

Our estimates are robust to alternative specifications and sample definitions (Appendix Table 2). First, to avoid issues with continuous differences in differences designs (Calloway, Goodman-Bacon, Sant'Anna 2022), we use a discrete version of the treatment variable equal to an indicator for having above median pre-period prices (Panel A). Second, we estimate models using an alternative measure of a program's cost of attendance – the average COA across 2004-2006—versus 2006 in our main specification (Panel B). This has the advantage of more data being used to calculate the cost of attendance; however, given trends in tuition earlier years may be less informative about price than later years. Third, we use the predicted program price for full-time, full-year students (Panel C). Since we calculate prices using an average of students we observe, our measure could be biased by differences in enrollment intensity. Using a prediction allow us to address this by predicting the cost of attendance for a full-time, full year student. We also estimate models that are unweighted by program size (Panel D). We also expand the years used in estimation to go until 2013 to allow for results to appear over a longer time frame (Panel E). The longer time frame has the benefit of accommodating a longer time horizon for the effects of Grad PLUS to appear; however, differences in differences is best suited to pick up effects in the short aftermath of the program. We change the requirement to only have an average of 10 students in the pre period to see if our restriction to larger programs changes our results (Panel F). This allows us to use smaller programs but also makes our cost of attendance average more

sensitive to outliers. We show our estimates are robust to including HBCU programs in the analysis sample (Panel G). In all cases, our estimates are very similar to those produced by our main specification. Finally, Denning and Turner (forthcoming) show that access to programs that typically lead to high earnings was not affected by expansions in loan limits due to Grad PLUS.

Our results speak to an often-referenced potential benefit of Grad PLUS—increasing access to programs. We find no such benefit overall. Similarly, we find no effect in the composition of entering graduate students suggesting that binding credit constraints were the determinative factor for graduate students' ability to enroll in graduate school prior to the advent of Grad PLUS, even among students that are traditionally underrepresented in graduate school and even for programs that are most likely to offer the highest earnings gains. An important limitation of these analyses is that our estimates are limited to programs that existed before the start of Grad PLUS and thus, we are unable to test whether access was expanded through the creation of *new* programs.

Our finding that Grad PLUS did not increase enrollment or student composition in programs where students would have experienced the largest loan limit increases is also important for our subsequent analysis. If student enrollment had changed in response to Grad PLUS, the sample of students in graduate school would be endogenously affected by Grad PLUS, thus complicating analysis using students who enrolled after the policy. However, because we find no evidence that enrollment or observable student characteristics changed and as a result, we examine outcomes among both students who were enrolled when Grad PLUS was created and those who entered after Grad PLUS led to higher loan limits.

5. Effects on student academic and labor market success

We next focus on estimating the effect of increased liquidity due to Grad PLUS on students' educational attainment and labor market outcomes. Classic models of credit constraints predict that increased access to loans for human capital investment should increase borrowing, increase human capital investment such as graduate school enrollment and completion, and increase earnings. We test these predictions in our setting by focusing on a set of individuals who are likely to be credit constrained.

We follow a similar approach to Black et al. (2020). We classify students as constrained if they borrowed the maximum amount available from Stafford Loans in their first year of the program as "constrained students" and classify those who borrowed less than the maximum

amount as unconstrained. We compare the outcomes of constrained and unconstrained students who entered in cohorts that were more and less affected by the Grad PLUS increase in borrowing limits and estimate:

$$Y_{ipc} = \beta(Cons_i * Post_c) + \mathbf{X_i}\boldsymbol{\beta_x} + \theta_c + \theta_p + \varepsilon_{ipc} \qquad (4)$$

Where Y_{ipc} is an outcome such as degree completion or annual earnings for student *i* who belonged to entry cohort *c* and first enrolled in program *p,* $Cons_i$ indicates whether a student is classified as constrained (borrowing at Stafford maximum in their first year, accounting for half year enrollment), and $Post_c$ indicates whether a student belonged to an entry cohort that was potentially affected by Grad PLUS and thus would have gained access to higher federal loan limits due to the establishment of the Grad PLUS program. $\mathbf{X_i}$ is a vector of baseline student characteristics (e.g., age, and indicators for race/ethnicity, gender, college educated parents and enrollment in fall and spring, and main effects for constrained status), θ_c and θ_p are entry cohort and entry program fixed effects, respectively. Standard errors are clustered by entry program.

Under the key identifying assumption that the outcomes of constrained and unconstrained students would have evolved similarly in the absence of loan limit increases due to the establishment of Grad PLUS, estimates of β will represent the causal effect of access to additional federal loans for constrained graduate students. While the key identifying assumption of parallel trends in the absence of Grad PLUS is fundamentally untestable, we can provide evidence in its support by (1) using event-study models to test for parallel trends in outcomes for cohorts that entered early enough that they would presumably have left graduate school before Grad PLUS existed and (2) testing for differences in baseline demographic characteristics between constrained and unconstrained students for cohorts that were and were not "treated" by the Grad PLUS program.

Following Black et al. (2020), we restrict the sample to students who borrowed in their first year to enable comparisons between similar students who had demonstrated a need to borrow. As a result, we are precluded from estimating effects of higher loan limits on entry into graduate programs. As discussed in Section 4, we find no evidence that programs where students would have experienced relatively larger increases in loan limits saw increases in enrollment or changes in student characteristics after Grad PLUS.

The cleanest strategy would be to focus on students who made their initial enrollment decisions before Grad PLUS loans were available. Given that many graduate programs are two years and our need to condition on first-year borrowing, this restriction would limit us to one year of "post" data. Instead, we use cohorts up to the 2007-08 (2008) academic year which includes two cohorts of students who entered a graduate program after the implementation of Grad PLUS but still limits the sample to students who enrolled before the start of the Great Recession. Our analysis sample includes the 2002 through 2008 public institution entry cohorts and 2004 through 2008 private nonprofit institution entry cohorts.[28]

Another complication is that the 2005 cohort did not gain access to Grad PLUS Loans in their first two years of graduate school but if they remained enrolled, they would be treated in their third year. The same is true for the 2004 cohort which is untreated for its first through third years of graduate school but potentially would be treated if enrolled in a fourth year. We test the extent to which earlier entry cohorts gained access to Grad PLUS loans by estimating event study models—similar to equation (4) except $Post_c$ is replaced with a set of entry cohort indicators—in which the dependent variable is cumulative Grad PLUS loans. Figure 2 displays point estimates and corresponding 95 percent confidence intervals from models in which the 2004 entry cohort serves as the reference group. These estimates suggest that constrained students in the 2005 through 2008 cohorts saw significant increases in cumulative Grad PLUS loans relative to earlier cohorts. Thus, we classify cohorts that entered before 2005 as untreated and cohorts that entered in 2005 and later as treated.[29]

We find a slightly different pattern when estimating effects on cumulative loans from all sources from event study models. As shown in Figure 3, the increase in cumulative borrowing is smaller than the increase in Grad PLUS loans and we find no effect on this outcome for the 2005 entry cohort. We investigate the potential reasons for this difference by estimating the more parsimonious equation (3) and breaking out total loans into Grad PLUS, total federal loans (PLUS and Stafford), and a combined category of state and private loans, results are shown in

[28] We exclude the students who appear to enter graduate school in the first year of available data (2001 for publics and 2003 for nonprofits) to make sure we identify entering students and need at least one year of data to identify if people have enrolled in graduate school previously. Our main analysis sample excludes cohorts who entered after the start of the Great Recession, although our results are robust to including these later entry cohorts.

[29] In Black et al. (2020), constrained undergraduate students who experienced loan limit increases had significantly higher rates of persistence and graduation and experienced significant increases in post-college earnings. So, while this strategy inherently identifies the effect of additional loans on students who were already enrolled, previous research has shown undergraduates in a similar situation were affected by additional access to credit.

Table 3. We find significant increases in constrained students' cumulative and federal borrowing in treated entry cohorts and significant decreases in cumulative nonfederal loans. Six years after entry, constrained students who gained access to Grad PLUS loans borrowed additional an $8,042 in Grad PLUS loans (Panel A). Effects on total federal loans are slightly smaller with a $7,580 increase in cumulative federal debt. However, some of this increase was offset by a $3,679 reduction in state and private loans. Taken together, constrained students who gained access to Grad PLUS experienced significant increases in cumulative total student loans by approximately $3,900.

Grad PLUS appears to have increased total borrowing while also shifting some existing borrowing from private and state loans to federal loans, a result consistent with Bhole (2017) based on nationally representative credit bureau data. Federal loans, including PLUS loans, may have better terms than private loans, especially for students with low credit scores or those lacking a long credit history. Additionally, federal borrowers generally have access to a wider variety of repayment options compared to what is offered by private lenders.

We next consider the effects on educational attainment outcomes. We find that constrained students were no more likely to persist after they gained access to additional federal loans in any year after entry (Table 4, Panel A). Although estimated effects on cumulative years of enrollment in the sixth through tenth years after entry are marginally significant, the magnitude is quite small – 0.1 additional years of enrollment. Estimated effects on cumulative credits attempted, shown in Panel B, follow a similar pattern: statistically significant but economically small point increases in the sixth through tenth years after entry (1.7 additional credits or a 3 percent increase at the end of our 10-year panel). Finally, we find no evidence of significant increases in graduate degree receipt (Panel Table 4, Panel C and Figure 4). Estimates are small and statistically insignificant – we can rule out effects larger than a 2-percentage point increase. Similarly, we find no evidence of significant effects on specific degrees (Appendix Tables 3 and 4). So, while constrained students borrowed more when they gained access to Grad PLUS Loans, the additional debt did not appear to lead to increased human capital.

Finally, we examine whether access to additional student loans affected constrained students' labor market outcomes. We first examine whether the probability of having any earnings during the academic year (i.e., for AY 2010, we consider earnings between 2009-Q4 and 2010-Q3. As shown in Panel A of Table 5, we find some evidence that constrained students in affected entry

cohorts saw small increases in the probability of having any earnings in UI covered employment in Texas in the second through fifth years after entry. However, by the sixth year after entry, we find no evidence that the probability of working in Texas was affected, and this null result persists through the tenth year after entry. These results simplify the interpretation of earnings results and suggest that Grad PLUS did not differentially affect the probability of working in Texas for constrained versus unconstrained students and we avoid difficulties with differential attrition with respect to earnings outcomes.

Given that we found no effects on human capital accumulation, it is unsurprising that we find little evidence of effects on long-run earnings. One year after entry, constrained students who gained access to Grad PLUS loans have significantly higher earnings but effects on earnings in all subsequent years are insignificant (Panel B). Estimates are negative in the third through fifth years after entry and positive in later years and cohort by cohort estimates from event study models show no clear pattern of earnings gains (Figure 5). However, effects on earnings should be interpreted with some caution as by the end of our panel, we only observe 57 percent of students in the sample with any annual earnings in Texas, whereas the national labor force participation rate for individuals with a post-baccalaureate degree is between 70 and 80 percent.[30] This is likely due to substantial mobility out of the state after leaving the state.

6. Effects on program prices

Finally, we turn to examine effects of the increase in federal loan availability and borrowing on graduate program prices. Universities, recognizing that students have more ability to pay when loan limits are increased, may try to capture some of the additional funding through higher prices. Ideally, to determine the pass-through of federal loan generosity to institutions, we would compare increases in borrowing due to Grad PLUS to increases in prices. However, realized changes in borrowing after Grad PLUS will be a function of the price increase. Thus, to estimate these possible unintended consequences of the Grad PLUS program, we employ an approach in the spirit of Lucca et al. (2019) and estimate reduced form models in which we compare changes in prices for graduate programs that had a high share of students borrowing at the limit before the Grad PLUS program to changes in prices for graduate programs where few students borrowed were constrained by the statutory limit in earlier years. The intuition for this strategy is that

[30] See, for instance, https://www.bls.gov/spotlight/2017/educational-attainment-of-the-labor-force/home.htm.

programs where many students were constrained by federal loans limits should see the largest increase in borrowing after these constraints are lifted and will have the most scope to raise prices and see increases in tuition revenue.[31]

Our estimating equation is

$$Y_{pc} = \beta\left(Pct_{p,pre} * Post_c\right) + \theta_c + \theta_p + \varepsilon_{pc} \qquad (3)$$

Where Y_{pc} are components of program price including cost of attendance, institutional grants, and tuition waivers. $Pct_{p,pre}$ represents the average percentage of students borrowing at the pre-Grad PLUS federal loan limit in the 2003 to 2006 academic years, and the other variables are defined as in equation (2).[32] We require that programs have enrollment in every year from 2003 to 2010 to guarantee we have a balanced panel and weight program observations by the average number of students for whom we observe cost of attendance between 2003 and 2006. Further, we require that the average number of students submitting financial aid information in a given year in the pre-period is at least 20 to avoid relying on variation from small programs, which will be measured with more noise.[33] Standard errors are clustered at the institution level.

We first estimate event study models in which $Post_c$ in equation (3) is replaced with indicators for event time (i.e., year before/after 2006). Figure 6 shows that in the years preceding Grad PLUS, program prices trended similarly for programs with low and high shares of students who were constrained, providing support for our identifying assumptions. After Grad PLUS, however, programs with a higher percentage of students who were constrained at baseline show significantly larger increases in average cost of attendance. Estimated effects are of a similar magnitude to the estimated effects on average Grad PLUS loans received by students in a program.

[31] We only observe a program's cost of attendance, of which tuition is only one component. Our implicit assumption in using COA as a measure of price is that incentives for schools to change the other components of COA were unaffected by changes in loan limits and borrowing because spending on these other components does not go to the school. Specifically, we assume that the living expense allowance in COA is changing similarly for programs that were more and less exposed to Grad PLUS-driven increases and thus is differenced out.

[32] We define the percent constrained by the number of students who are borrowing from the federal government at or above the Stafford maximum accounting for partial year enrollment. However, our results are robust to using a measure that considers both federal and nonfederal borrowing in defining which students are at the limit or not accounting for partial year enrollment.

[33] We later show the results are robust to changing enrollment-based sample restrictions.

Table 5 presents estimates from equation (3) and provides evidence that programs more exposed to Grad PLUS experienced significantly larger increases in average Grad PLUS and federal Stafford loans than programs with a lower share of students who were constrained at baseline. The point estimate for Grad PLUS loans, equal to 79.4, implies that a 1 percentage point increase in the share of students who were constrained by loan limits at baseline resulted in a $0.79 increase in average annual Grad PLUS borrowing per student. In total, annual federal loan aid per student increased by $0.54, suggesting that average Stafford Loans taken out by students may have decreased after Grad PLUS although the point estimates not statistically distinguishable from estimated effects on only Grad PLUS loans.[34] Some of the increase in federal borrowing was offset by the significant $0.33 decrease in average private student loans per student while state loan aid was unaffected.

Next, we estimate price effects, Table 7 contains these results. Programs with a higher share of students who were constrained at baseline see significant increases in the average cost of attendance following the creation of Grad PLUS. A percentage point increase in constrained students at baseline resulted in an approximately $0.60 increase in average cost of attendance after Grad PLUS. Taken together with the results in Table 6, these estimates suggest that prices increased by $0.75 per $1 increase in average per-student Grad PLUS loans and more than dollar for dollar with increases in total federal student loans. When we examine effects on the predicted cost of attendance for a full-time, full-year student (column 2), we find significant effects that are of a similar magnitude.

While Grad PLUS led to relatively larger increases in prices for programs in which students experienced the largest scope for borrowing increases, grant aid was also affected. We find that approximately half of the increase in cost of attendance was offset by a $0.23 increase in institutional grants per 1 percentage point increase in share constrained (Table 7, column 3). This is consistent with colleges engaging in price discrimination–programs' sticker prices increased but students also received more grant aid. Aid received in the form of tuition waivers was unaffected. Taking the offsetting effects on list price and grants into account suggests a more modest degree of crowd-out: a $0.44 increase in net price per $1 of Grad PLUS loan aid (Table 7

[34] Although students are required to exhaust their Stafford Loan eligibility before borrowing through the Grad PLUS program, these two results are not necessarily inconsistent. An increase in Grad PLUS borrowing and a decrease in Stafford borrowing could be explained by a change in the composition of students who borrow, with fewer students taking out federal loans overall but those who borrow taking on larger amounts.

column 5 estimate scaled by Table 6 column 1 estimate) or \$0.64 per \$1 increase in total federal loans (Table 7 column 5 estimate scaled by Table 6 column 2 estimate).

We confirm these back-of-the-envelope calculations by estimating IV models in which total federal loan aid is the endogenous regressor and $Pct_{p,pre} * Post_c$ serves as the excluded instrument.[35] Estimates suggest that \$1 increase in federal loans resulted in a significant \$1.16 increase in a program's list price and a \$0.66 increase in net price (Table 7).

While prices may have gone up in response to the policy change, all students may not have borne the burden of tuition increases equally given institutions' ability to price discriminate by offering discounts through grants and tuition waivers. We investigate this by creating a new data set at the program-by-year-by-group level, where group is defined by baseline student characteristics such as Pell Grant receipt as an undergraduate or race/ethnicity. Ex ante, it is not clear how price discrimination will work in graduate school, and while we observe income and assets for students (and their spouse, if present), this information will only be relevant for need-based (versus merit-based) aid. Groups are defined by student race (white, Black, Hispanic, Asian/Pacific Islander), parental education (parents with a college degree versus first-generation college students), Pell Grant receipt as an undergraduate in Texas, and Expected Family Contribution (above versus below median).

Table 9 presents estimates by student race/ethnicity. We find some evidence of heterogeneous borrowing responses to Grad PLUS, with Black students' federal loans increasing by \$0.80 per 1 percentage point increase in percent constrained at baseline, Hispanic and white students' loans increasing by \$0.48, and Asian and Pacific Islander (API) students seeing a \$0.37 increase. Taking this into account, the implied increase in grant aid per \$1 increase in federal loans for white and API students (approximately \$0.70 per \$1 increase in loans) is over 3 times the size of the implied effect on Black students (\$0.22 increase in grant aid per \$1 increase in federal loans). Hispanic students also see smaller implied increases in grant aid (\$0.49 per \$1 increase in federal loans). These differences in grant aid translate into differences in net price. Black students see their net price increase by \$0.44 per \$1 increase in federal loans and Hispanic

[35] Total loans or Grad PLUS loans could also serve as the endogenous regressor in IV models. We used total federal loans due to the reasoning that it is the most policy relevant measure of treatment – both Stafford and PLUS Loans are determined by the federal government, whereas private loans may adjust endogenously to these policy decisions.

students see a $0.37 increase, while increases for white and API students are $0.17 and $0.25, respectively.

Next, we examine whether there is heterogeneity in borrowing, grant, and price effects by measures of socioeconomic status (SES). Table 10 contains these results. We find some variation, with more advantaged students generally seeing larger implied increases in grant aid per $1 increase in federal loans, but differences are smaller than those implied by the estimates by race/ethnicity. Taken together, the estimates in Tables 9 and 10 suggest that the increase in grant aid was not targeted to underrepresented students or students from less advantaged backgrounds. We do not have measures of pre-graduate school academic preparation (such as GRE scores) which may be the most relevant dimension for price discrimination.[36]

We test the robustness of these results to changing the minimum program size required for sample inclusion to 10 or 30 and find very similar results (Appendix Table 5, Panels A and B). We also show these estimates are robust when we do not weight by the size of the program (Panel C). Finally, we use several alternative measures of constrained borrowing: federal borrowing at the Stafford Loan annual maximum not accounting for partial year enrollment (Panel D), total borrowing at the Stafford maximum not accounting for partial year enrollment (Panel E), and federal borrowing at the Stafford maximum accounting for partial year enrollment (Panel F). Our results are similar across all these measures of treatment intensity. When we replace $Pct_{p,pre}$ – the average percent constrained between 2003 and 2006 – with the percent of students who are constrained in 2006 alone, we find similar effects (Panel G). This measure may have more measurement error in the true fraction constrained because less students are used to calculate it, but it is temporally closer to treatment. Finally, we obtain similar estimated effects on borrowing, COA, and grants (albeit less precise) from a specification that discretizes the continuous measure of the percent of students constrained at baseline in equation (3) into programs with above/below median share constrained at baseline (Panel H) to avoid issues with continuous differences in differences. Overall, our results demonstrate that schools do in fact respond to increased loan access by increasing tuition, and it appears that this burden is not born disproportionately by higher-SES students.

[36] Another relevant factor affecting changes in grant provision could be field of study, but our sample is too small to estimate heterogeneity along this dimension.

7. Discussion and Conclusion

This paper explores the effects of a large expansion in federal student loans for graduate school due to the creation of the federal Grad PLUS Program. Grad PLUS increased student borrowing for graduate school and shifted graduate borrowing from private to federal student loans. However, the increase in federal loan limits did not increase access to graduate programs overall or for underrepresented students.

We find little evidence of short or longer-run effects on the human capital accumulation of students who were or would have been constrained by federal borrowing limits in the absence of Grad PLUS, even though cumulative debt significantly increased for these students when they gained access to Grad PLUS loans. This suggests that access to additional liquidity did not constrain graduate student borrowers' human capital investments prior to the implementation of Grad PLUS. We also find little evidence of an impact on later earnings, consistent with no change in human capital accumulation.

Where we do see effects is on program prices. Grad PLUS-driven increases in federal student loans significantly increased program prices. This provides confirmatory evidence for the Bennett Hypothesis. We also find some evidence that schools engaged in price discrimination more after the increase in student loan availability by increasing grant aid, resulting in smaller increases in net prices than the listed price. Additional grant aid was not disproportionately provided to students who were Black, Hispanic, or from low-SES backgrounds.

Our results are directly relevant for policy. The implementation of Grad PLUS loans seems to have benefitted students very little in terms of human capital accumulation, suggesting that, prior to the implementation of Grad PLUS loans, few students faced binding credit constraints. This contrasts with what has been found for undergraduates (Black et al 2020). Our results raise important questions about the utility of essentially uncapped government-backed loans for graduate school.

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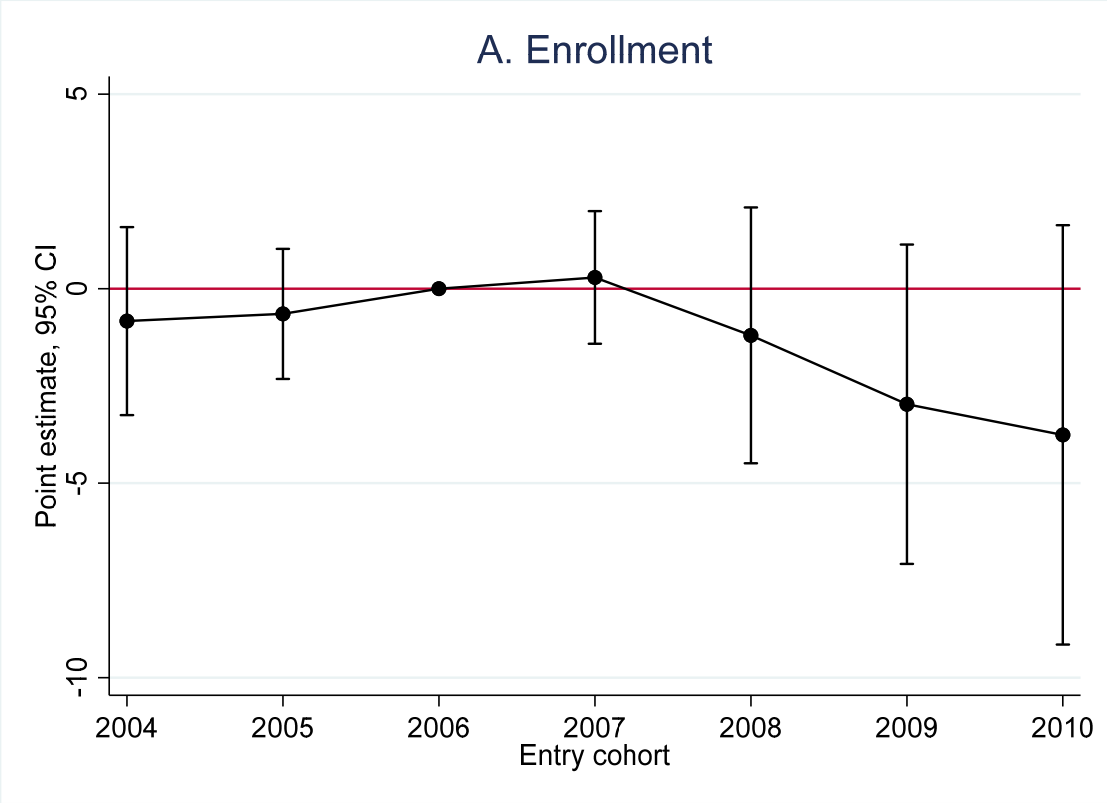
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Figures and Tables

Figure 1: Effect of a $1000 increase in loan limits on graduate program access



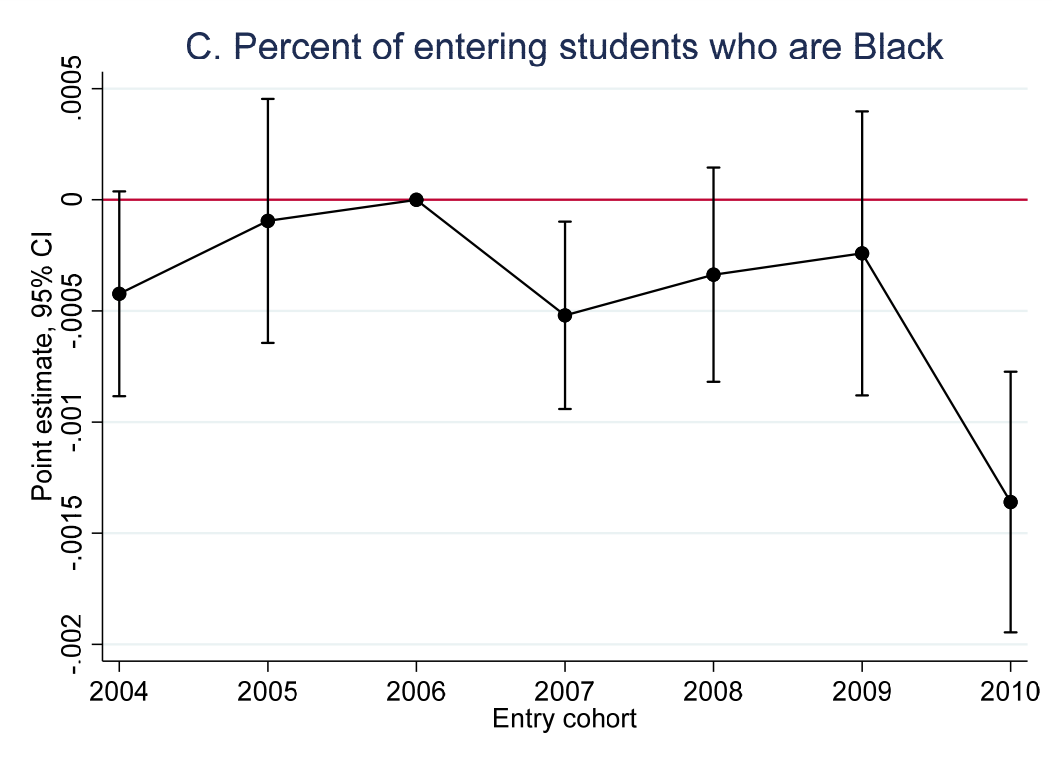
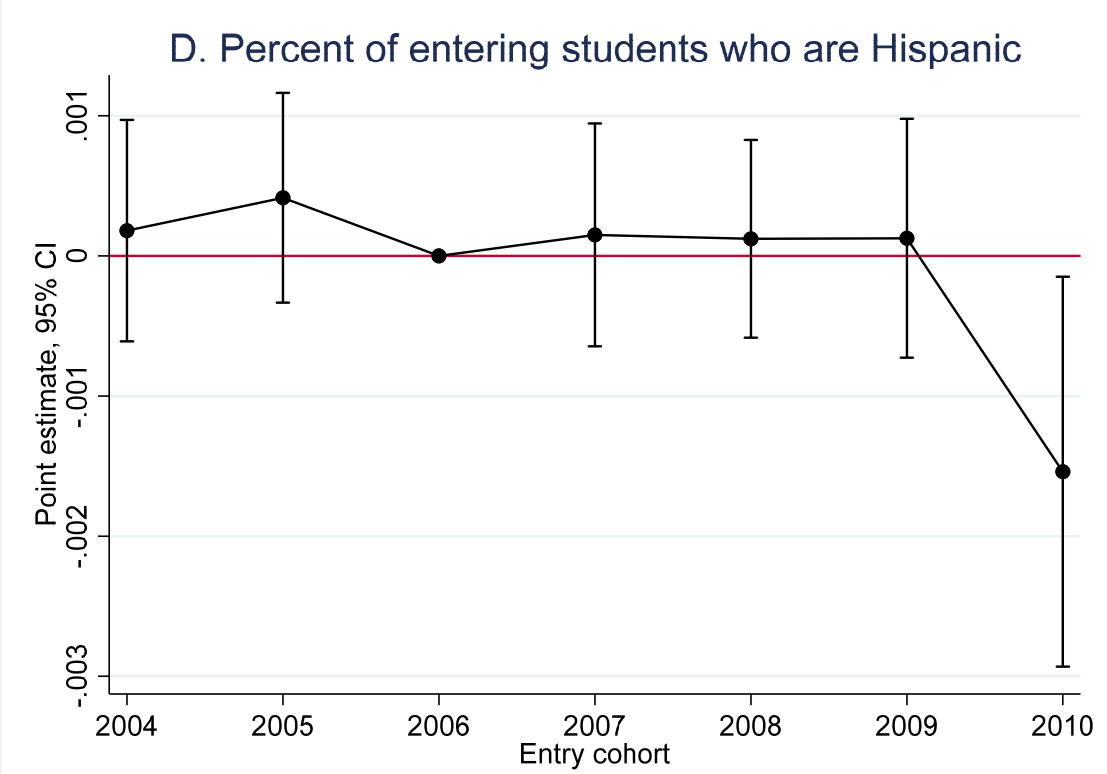


Figure 1, continued


D. Percent of entering students who are Hispanic
Point estimate, 95% CI
.001
0
-.001
-.002
-.003
2004
2005
2006
2007
2008
2009
2010
Entry cohort

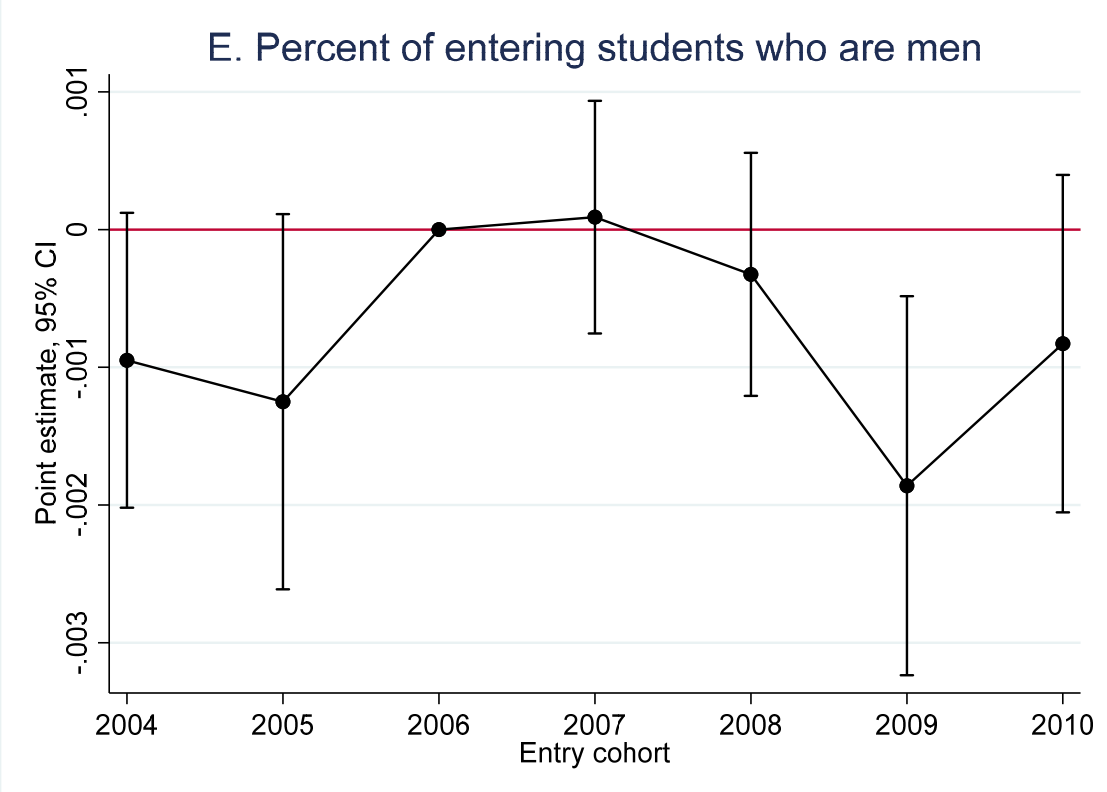
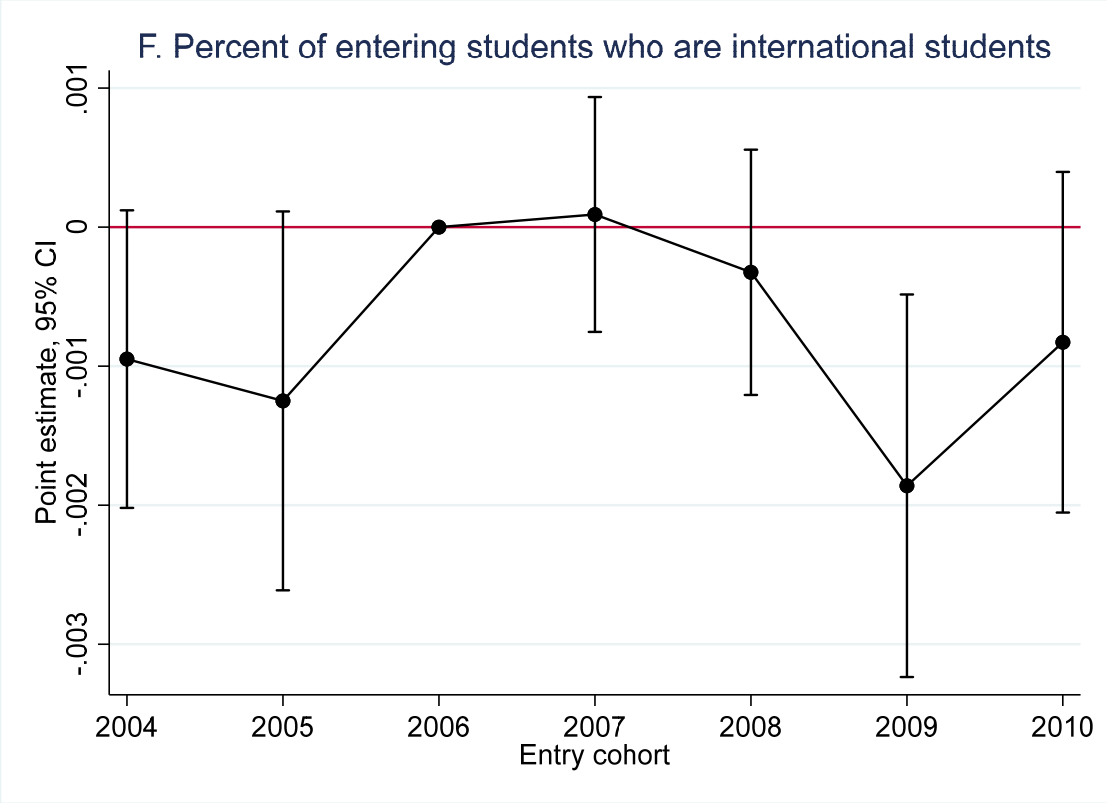
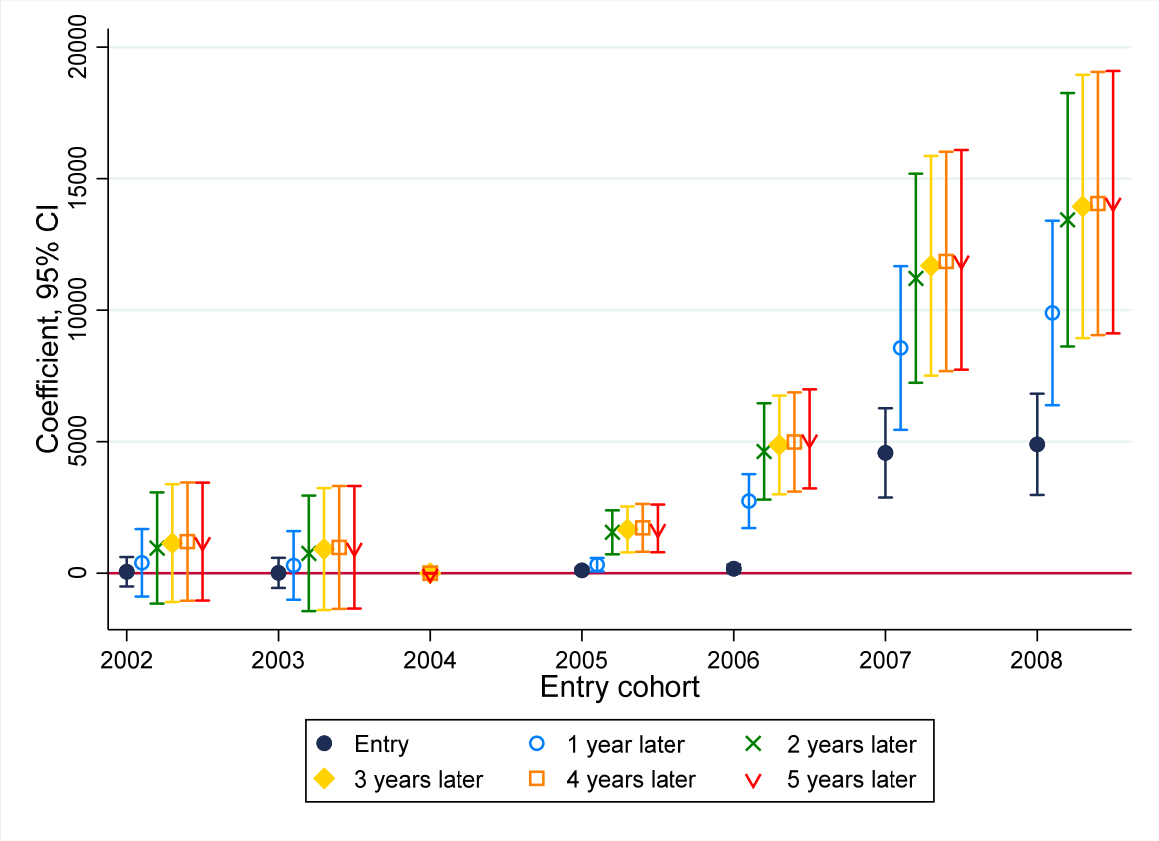

E. Percent of entering students who are men
Point estimate, 95% CI
.001
0
-.001
-.002
-.003
2004
2005
2006
2007
2008
2009
2010
Entry cohort

Figure 1, continued



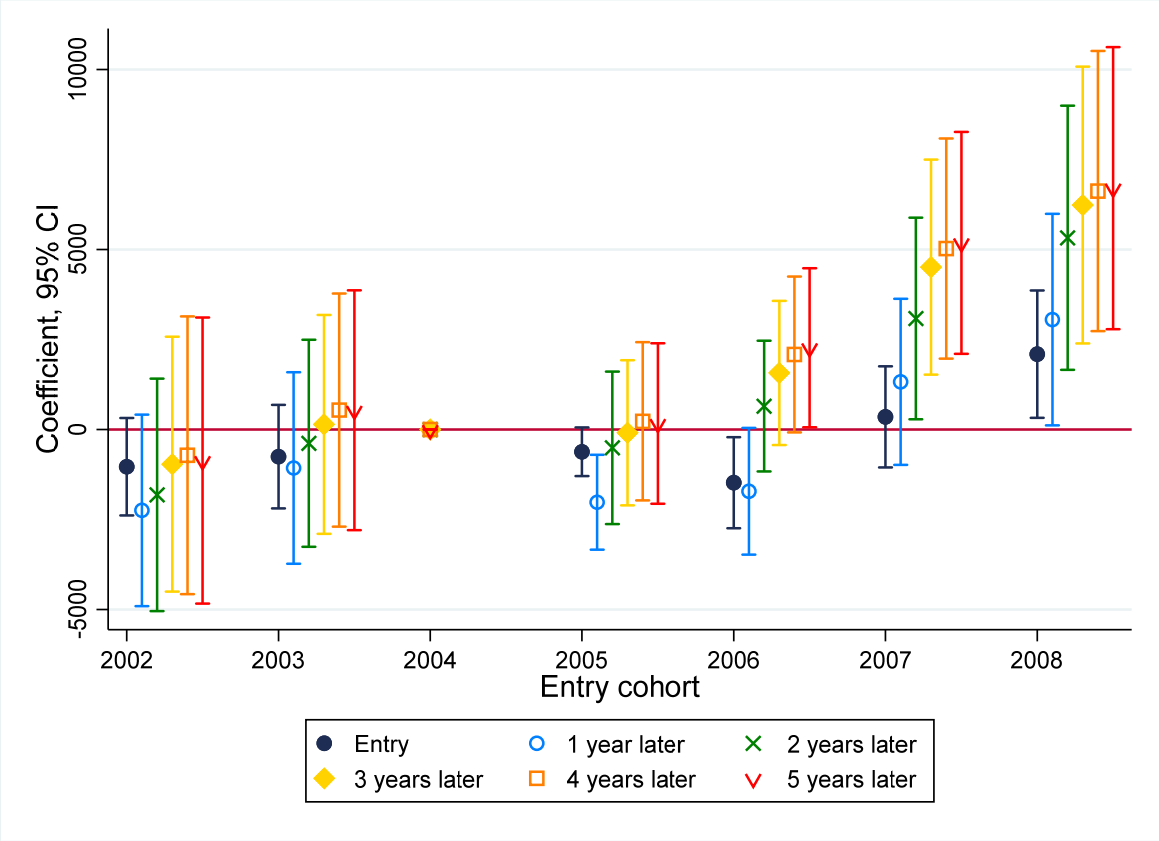
Notes: The sample includes graduate programs that had entering students in each of the 2004 through 2010 academic years and had at least 20 entering students who filed a FAFSA in 2004 through 2006. Point estimates and 95% confidence intervals from regressions of first-year enrollment (Panel A) or share of entering students with the given demographic characteristic (Panels B through F) on an interaction between entry cohort and the projected federal loan limit increases (see text for details). Regressions also include entry cohort and program fixed effects. Standard errors are clustered at the program level.

Figure 2: Effects on constrained students' cumulative Grad PLUS loans by entry cohort



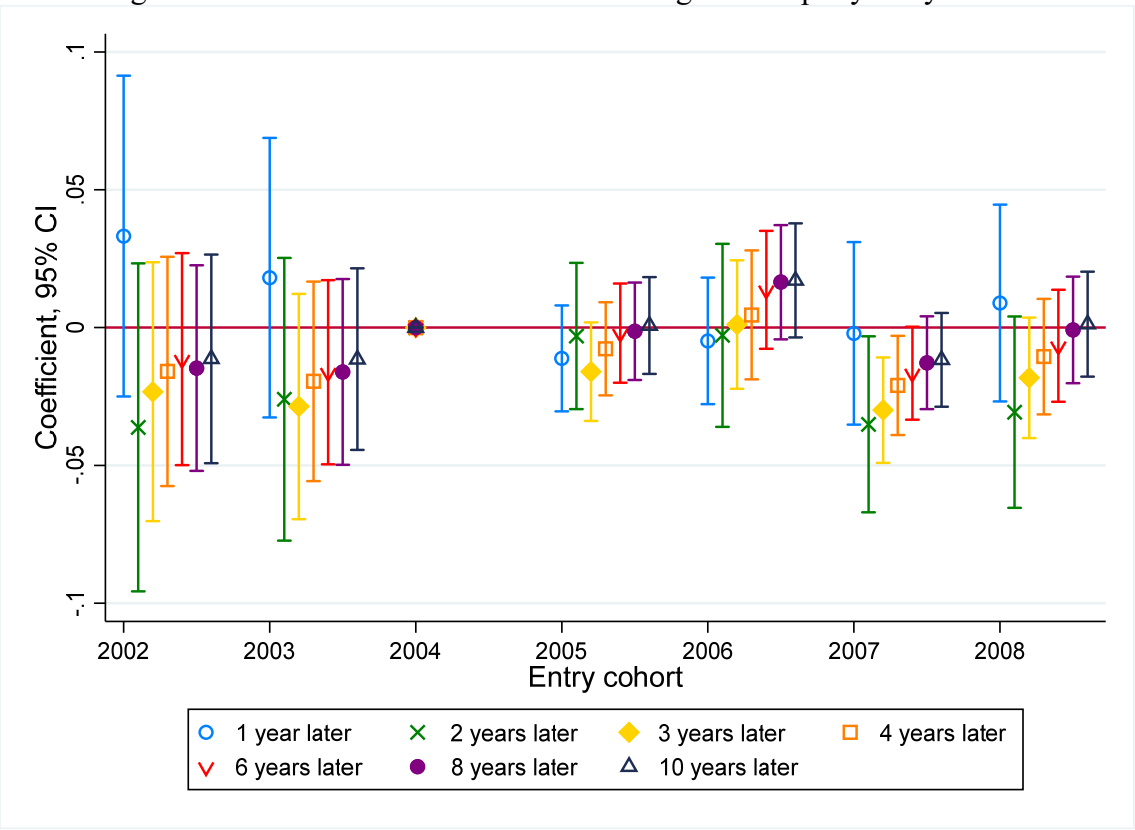
Notes: The sample includes first-time graduate students who borrowed in their first year of enrollment. Point estimates and 95% confidence intervals from regressions of cumulative Grad PLUS loans at the specified number of years since entry on an interaction between entry cohort and an indicator for being constrained (borrowing at the federal Stafford Loan limit). Regressions also include entry cohort and entry program fixed effects and age, indicators for race/ethnicity, gender, college educated parents and enrollment in fall and spring, and constrained. Standard errors are clustered at the program level.

Figure 3: Effects on constrained students' cumulative student loans from all sources



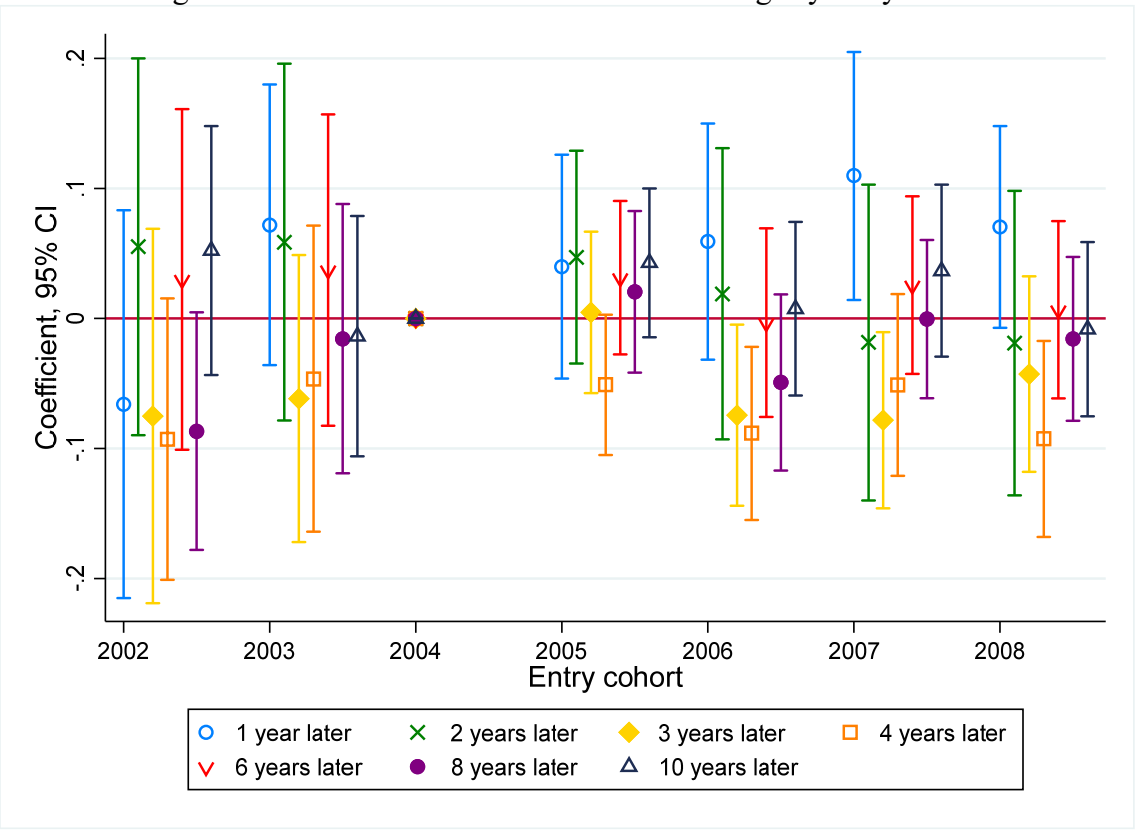
Notes: The sample includes first-time graduate students who borrowed in their first year of enrollment. Point estimates and 95% confidence intervals from regressions of cumulative student loans at the specified number of years since entry on an interaction between entry cohort and an indicator for being constrained (borrowing at the federal Stafford Loan limit). Regressions also include entry cohort and entry program fixed effects and age, indicators for race/ethnicity, gender, college educated parents and enrollment in fall and spring, and constrained. Standard errors are clustered at the program level.

Figure 4: Effects on constrained students' degree receipt by entry cohort



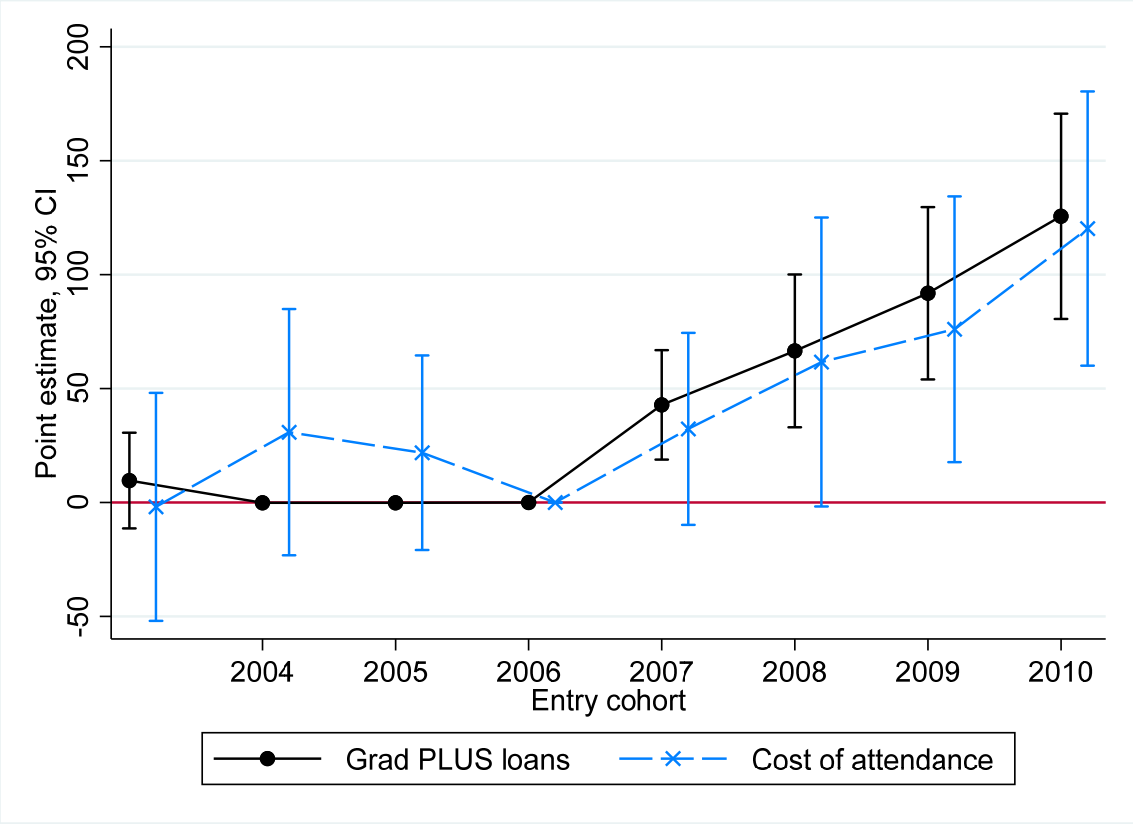
Notes: The sample includes first-time graduate students who borrowed in their first year of enrollment. Point estimates and 95% confidence intervals from regressions of the probability of any graduate credential receipt as of the specified number of years since entry on an interaction between entry cohort and an indicator for being constrained (borrowing at the federal Stafford Loan limit). Regressions also include entry cohort and entry program fixed effects and age, indicators for race/ethnicity, gender, college educated parents and enrollment in fall and spring, and constrained. Standard errors are clustered at the program level.

Figure 5: Effect on constrained students' earnings by entry cohort



Notes: The sample includes first-time graduate students who borrowed in their first year of enrollment. Point estimates and 95% confidence intervals from regressions of ln(annual earnings) at the specified number of years since entry on an interaction between entry cohort and an indicator for being constrained (borrowing at the federal Stafford Loan limit). Regressions also include entry cohort and entry program fixed effects and age, indicators for race/ethnicity, gender, college educated parents and enrollment in fall and spring, and constrained. Standard errors are clustered at the program level.

Figure 6: Effect of a 1 percentage point increase in baseline percent of students who are constrained by Stafford Loan limits on program-level borrowing and prices



Notes: The sample includes a balanced panel graduate programs with enrollment in the 2003 through 2010 academic years and at least 20 federal aid recipients enrolled per year, on average, between 2003 and 2006. Point estimates and 95% confidence intervals from regressions of average Grad PLUS loans per student (dark solid markers) or average cost of attendance (light Xs) on interactions between academic year indicators and the percent of students who were constrained at baseline (see text for definition). Regressions also include academic year and program fixed effects. Standard errors are clustered at the program level.

Table 1: Characteristics of first-time graduate students

	(1) All (2002-2011)	(2) Pre (2002-2006)	(3) Post (2007-2011)
Female	0.58	0.58	0.58
Race/ethnicity/nativity			
Asian	0.06	0.05	0.07
Black	0.10	0.09	0.10
Hispanic	0.14	0.16	0.13
White	0.52	0.54	0.51
International	0.12	0.13	0.12
Age (in yrs.)	30	30	29
Texas Resident	0.77	0.77	0.77
Degree receipt within			
6 years	0.71	0.67	0.74
Broad field of study			
Education	0.20	0.21	0.19
Business	0.13	0.12	0.14
Health	0.13	0.12	0.14
Engineering	0.06	0.06	0.06
Borrowers	0.37	0.32	0.42
Constrained borrowers	0.10	0.07	0.12
Amount borrowed (2018$)			
Federal Stafford	$6,039	$5,237	$6,773
Grad PLUS	$362	$13	$682
Federal Perkins	$62	$82	$44
State	$34	$17	$49
Private and other	$237	$289	$189
Total loans	$6,734	$5,637	$7,738
Total grants (2018$)	$1,261	$734	$1,743
Cost of Attendance (2018$)	$30,014	$26,064	$32,231

Notes: Sample is limited to first-time graduate students who enrolled in the 2002 through 2011 academic years. Constrained borrowers are students who borrowed the maximum available Stafford Loan amount for the academic year.

Table 2: Effect of projected and realized increases in federal loan limits on enrollment and the composition of entering graduate students

	(1)	Percent of entering students who are:			
	Enrollment	(2) Black	(3) Hispanic	(4) Men	(5) Interntl
A. OLS estimates					
Projected limit increase ($1k) x Post	-1.4	-0.0004	-0.0005	0	-0.0004
	(1.8)	(0.0002)*	(0.0002)*	(0.0003)	(0.001)
Observations	2,338	2,338	2,338	2,338	2,338
B. IV estimates					
Federal loan limit ($1k)	-1.3	-0.0004	-0.0005	0	-0.0004
	(1.6)	(0.0002)**	(0.0002)*	(0.0003)	(0.001)
95% CI	[-4.4, 1.8]	[-0.001, -0.00002]	[-0.001, -0.0001]	[-0.001, 0.001]	[-0.001, 0.001]
Observations	2,338	2,338	2,338	2,338	2,338
C. Pre-Grad PLUS mean	139	0.08	0.17	0.41	0.12

Notes: The sample includes graduate programs that had entering students in each of the 2004 through 2010 academic years and had at least 20 entering students who filed a FAFSA in 2004 through 2006. Panel A displays point estimates from regressions of first-year enrollment or share of entering students with the given demographic characteristic on an interaction between post-Grad PLUS entry cohort and the projected federal loan limit increases (see text for details). Panel B displays point estimates from instrumental variables models in which the interaction between the projected limit increase and the indicator for post-Grad PLUS serves as the excluded instrument for the realized federal loan limit. All specifications also include entry cohort and program fixed effects. Robust standard errors, clustered at the program level, in parentheses;
** $p < 0.01$, * $p < 0.05$.

Table 3: The effect of Grad PLUS on constrained students' cumulative borrowing

Years since entry =	0	1	2	3	4	5	6
A. Federal PLUS loans							
Constrained x treated cohort	2531	5438	7608	7880	7970	8020	8042
	(438)**	(938)**	(1,351)**	(1,375)**	(1,371)**	(1,369)**	(1,366)**
Dep var mean (cons, pre-2005 cohorts)	$1	$4	$12	$104	$151	$192	$236
B. Federal loans (Stafford + PLUS)							
Constrained x treated cohort	1347	3378	6265	7085	7385	7493	7580
	(542)*	(1,090)**	(1,451)**	(1,587)**	(1,636)**	(1,673)**	(1,705)**
Dep var mean (cons, pre-2005 cohorts)	$27,144	$49,241	$62,891	$66,173	$67,369	$68,078	$68,562
C. State and private loans							
Constrained x treated cohort	-805	-2,402	-3,519	-3,680	-3,687	-3,682	-3,679
	(335.4)*	(731.8)**	(988.1)**	(1,034)**	(1,029)**	(1,027)**	(1,027)**
Dep var mean (cons, pre-2005 cohorts)	$3,214	$6,382	$7,957	$8,109	$8,156	$8,178	$8,182
D. Total loans							
Constrained x treated cohort	542	976	2746	3405	3697	3811	3901
	(488)	(859)	(1,050)**	(1,099)**	(1,149)**	(1,178)**	(1,209)**
Dep var mean (cons, pre-2005 cohorts)	$30,358	$55,624	$70,847	$74,283	$75,525	$76,257	$76,745

Notes: The sample includes first-time graduate students in the 2002 through 2008 entry cohorts who borrowed in their first year of enrollment. Point estimates from regressions of cumulative student loans at the specified number of years since entry on an interaction between an indicator for being constrained (borrowing at the federal Stafford Loan limit) and an indicator for bellowing to a treated cohort (academic year 2005 and later). Regressions also include entry cohort and entry program fixed effects and age, indicators for race/ethnicity, gender, college educated parents and enrollment in fall and spring, and constrained. Robust standard errors, clustered at the program level, in parentheses; ** $p < 0.01$, * $p < 0.05$.

Table 4: The effect of Grad PLUS on constrained students' educational attainment

Years since entry =	1	2	3	4	5	6	7	8	9	10
A. Cumulative years enrolled										
Constrained x treated cohort	0.01	0.03	0.05	0.07	0.08	0.09	0.10	0.11	0.11	0.11
	(0.01)	(0.02)	(0.03)	(0.04)	(0.05)	(0.05)+	(0.06)+	(0.06)+	(0.06)+	(0.06)+
Dep var mean (const, pre-period)	1.91	2.54	2.70	2.78	2.83	2.88	2.91	2.94	2.96	2.98
B. Cumulative credit hours attempted										
Constrained x treated cohort	-0.29	0.44	0.83	1.12	1.30	1.48	1.59	1.68	1.70	1.73
	(0.55)	(0.54)	(0.63)	(0.68)	(0.74)+	(0.79)+	(0.82)+	(0.84)*	(0.86)*	(0.87)*
Dep var mean (const, pre-period)	36.86	45.85	47.53	48.47	49.13	49.57	49.92	50.2	50.46	50.66
C. Any graduate degree received										
Constrained x treated cohort	-0.012	-0.006	-0.005	-0.002	0.001	0.003	0.004	0.006	0.007	0.006
	(0.020)	(0.013)	(0.009)	(0.009)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
Dep var mean (const, pre-period)	0.309	0.776	0.868	0.89	0.897	0.902	0.905	0.906	0.908	0.909

Notes: The sample includes first-time graduate students in the 2002 through 2008 entry cohorts who borrowed in their first year of enrollment. Point estimates from regressions of cumulative years of enrollment (Panel A), cumulative credit hours attempted (Panel B), or the probability of any degree receipt (Panel C) as of the specified number of years since entry on an interaction between an indicator for being constrained (borrowing at the federal Stafford Loan limit) and an indicator for bellowing to a treated cohort (academic year 2005 and later). Regressions also include entry cohort and entry program fixed effects and age, indicators for race/ethnicity, gender, college educated parents and enrollment in fall and spring, and constrained. Robust standard errors, clustered at the program level, in parentheses; * $p < 0.05$, + $p < 0.1$.

Table 5: The effect of Grad PLUS on constrained students' labor market outcomes

Years since entry =	1	2	3	4	5	6	7	8	9	10
A. Any earnings (N = 117,954)										
Constrained x treated cohort	0.012	0.022	0.020	0.018	0.019	0.016	0.010	0.011	0.012	0.012
	(0.011)	(0.012)+	(0.011)+	(0.009)*	(0.009)*	(0.009)+	(0.009)	(0.010)	(0.011)	(0.011)
Dep var mean (const, pre-period)	0.677	0.657	0.675	0.651	0.630	0.612	0.601	0.587	0.577	0.568
B. ln(earnings)										
Constrained x treated cohort	0.066	-0.017	-0.022	-0.045	-0.015	0.002	0.032	0.006	0.002	0.014
	(0.032)*	(0.047)	(0.029)	(0.030)	(0.028)	(0.027)	(0.024)	(0.026)	(0.022)	(0.028)
Dep var mean (const, pre-period)	9.58	9.91	10.66	10.98	11.04	11.11	11.15	11.21	11.26	11.28
Observations	87,587	85,519	88,277	86,146	84,122	82,424	80,816	79,539	78,309	77,347

Notes: The sample includes first-time graduate students in the 2002 through 2008 entry cohorts who borrowed in their first year of enrollment. Point estimates from regressions of the probability of having earnings in a UI-covered sector in Texas (Panel A) or ln(annual earnings) as of the specified number of years since entry on an interaction between an indicator for being constrained (borrowing at the federal Stafford Loan limit) and an indicator for bellowing to a treated cohort (academic year 2005 and later). Regressions also include entry cohort and entry program fixed effects and age, indicators for race/ethnicity, gender, college educated parents and enrollment in fall and spring, and constrained. Robust standard errors, clustered at the program level, in parentheses; * $p < 0.05$, + $p < 0.1$.

Table 6: Effect of 1 pp increase in baseline percent of students constrained by Stafford Loan limit on average per-student loans

	(1) Grad PLUS	(2) Total Federal	(3) State	(4) Private
%constrained*Post	79.4	54.4	3.3	-33.0
	(17.6)**	(28.8)+	(3.2)	(12.2)**
Observations	2,264	2,264	2,264	2,264

Notes: The sample includes a balanced panel graduate programs with enrollment in the 2003 through 2010 academic years and at least 20 federal aid recipients enrolled per year, on average, between 2003 and 2006. Point estimates from regressions of average loans per student from the specified source on an interaction between post-Grad PLUS and the percent of students who were constrained at baseline (see text for definition). Regressions also include academic year and program fixed effects. Robust standard errors, clustered at the program level, in parentheses; ** $p < 0.01$, + $p < 0.1$.

Table 7: Effect of 1 pp increase in baseline percent of students constrained by Stafford Loan limit on program prices and financial aid

	(1) COA	(2) FTFY COA	(3) Grants	(4) Tuit waivers	(5) Net price
%constrained*Post	59.8	62.9	23.0	1.8	35.0
	(21.1)**	(18.9)**	(13.7)+	(3.1)	(30.2)
Observations	2,264	2,264	2,264	2,264	2,264

Notes: The sample includes a balanced panel graduate programs with enrollment in the 2003 through 2010 academic years and at least 20 federal aid recipients enrolled per year, on average, between 2003 and 2006. Point estimates from regressions of average prices or average aid per student (indicated in the column heading) on an interaction between post-Grad PLUS and the percent of students who were constrained at baseline (see text for definition). COA = cost of attendance. FTFY COA is the predicted program-level cost of attendance for a full-time, full-year student (see text for details). Net price equals COA minus grants and tuition waivers. Regressions also include academic year and program fixed effects. Robust standard errors, clustered at the program level, in parentheses; ** $p < 0.01$, + $p < 0.1$.

Table 8: IV estimates of the effect of federal loans on program price

	(1) COA	(2) Net price
Federal loans	1.155*	0.662+
	(0.526)	(0.367)
Observations	2,264	2,264

Notes: The sample includes a balanced panel graduate programs with enrollment in the 2003 through 2010 academic years and at least 20 federal aid recipients enrolled per year, on average, between 2003 and 2006. Point estimates from instrumental variables models in which average federal student loans per student is the endogenous regressor and an interaction between post-Grad PLUS and the percent of students who were constrained at baseline (see text for definition) is the excluded instrument. COA = cost of attendance. Net price equals COA minus grants and tuition waivers. Regressions also include academic year and program fixed effects. Robust standard errors, clustered at the program level, in parentheses; * $p < 0.05$, + $p < 0.1$.

Table 9: Heterogeneity in the effect of exposure to Grad PLUS by race/ethnicity

	(1) Grad PLUS loans	(2) Total federal loans	(3) Grants	(4) Net price	Grant aid/ fed loans
A. Asian or Pacific Islander					
%constrained*Post	49.8	36.9	26.3	9.2	0.71
	(11.3)**	(21.7)+	(14.4)+	(33.9)	
Observations	2,047	2,047	2,047	1,738	
B. Black					
%constrained*Post	92.8	80.1	17.4	35.3	0.22
	(18.9)**	(29.8)**	(27.5)	(39.6)	
Observations	2,140	2,140	2,140	2,056	
C. Hispanic					
%constrained*Post	80.7	48.3	23.8	17.7	0.49
	(18.2)**	(31.8)	(20.1)	(37.5)	
Observations	2,228	2,228	2,228	2,179	
D. White					
%constrained*Post	67.3	47.7	33.2	8.2	0.70
	(14.5)**	(22.7)*	(17.2)+	(38.9)	
Observations	2,235	2,235	2,235	2,216	

Notes: The sample includes a balanced panel graduate programs with enrollment in the 2003 through 2010 academic years and at least 20 federal aid recipients enrolled per year, on average, between 2003 and 2006. Programs without any students of the specified race/ethnicity are excluded. Point estimates from regressions of average loans, grants, or price per student on an interaction between post-Grad PLUS and the percent of students who were constrained at baseline (see text for definition). Net price equals COA minus grants and tuition waivers. The last column is a simple scaling of the point estimate in column (3) by the point estimate in column (2). Regressions also include academic year and program fixed effects. Robust standard errors, clustered at the program level, in parentheses; ** $p < 0.01$, * $p < 0.05$, + $p < 0.1$.

Table 10: Heterogeneity in the effect of exposure to Grad PLUS by socioeconomic status

	(1) Grad PLUS loans	(2) Total federal loans	(3) Grants	(4) Net price	Grant aid/fed loans
A. Received Pell Grant as undergrad					
%constrained*Post	103.4	66.0	19.9	20.3	0.30
	(22.7)**	(34.9)+	(19.9)	(31.2)	
Observations	2,257	2,257	2,257	2,247	
B. Did not receive Pell as undergrad					
%constrained*Post	86.6	68.2	26.1	32.7	0.38
	(17.9)**	(32.2)*	(15.3)+	(33.0)	
Observations	2,264	2,264	2,264	2,264	
C. First generation college student					
%constrained*Post	95.7	77.2	25.6	34.8	0.33
	(20.5)**	(36.0)*	(19.3)	(34.6)	
Observations	2,264	2,264	2,264	2,264	
D. College educated parent					
%constrained*Post	103.4	74.3	28.4	29.3	0.38
	(20.3)**	(35.4)*	(17.9)	(34.1)	
Observations	2,247	2,247	2,247	2,247	
E. EFC < median					
%constrained*Post	100.9	80.8	27.7	36.1	0.34
	(21.3)**	(35.0)*	(18.2)	(34.3)	
Observations	2,262	2,262	2,262	2,262	
F. EFC ≥ median					
%constrained*Post	91.5	66.4	26.4	15.8	0.40
	(18.9)**	(33.4)+	(17.4)	(28.2)	
Observations	2,255	2,255	2,255	2,255	

Notes: The sample includes a balanced panel graduate programs with enrollment in the 2003 through 2010 academic years and at least 20 federal aid recipients enrolled per year, on average, between 2003 and 2006. Programs without any students of the specified category are excluded. EFC = expected family contribution (limited to students who filed an application for federal student aid). Undergraduate Pell Grant receipt only available for students who received an undergraduate degree from a Texas public higher education institution. Point estimates from regressions of average loans, grants, or price per student on an interaction between post-Grad PLUS and the percent of students who were constrained at baseline (see text for definition). Net price equals COA minus grants and tuition waivers. The last column is a simple scaling of the point estimate in column (3) by the point estimate in column (2). Regressions also include academic year and program fixed effects. Robust standard errors, clustered at the program level, in parentheses; ** $p < 0.01$, * $p < 0.05$, + $p < 0.1$.