NBER WORKING PAPER SERIES

PROPELLED: THE EFFECTS OF GRANTS ON GRADUATION, EARNINGS, AND WELFARE

Jeffrey T. Denning
Benjamin M. Marx
Lesley J. Turner

Working Paper 23860
http://www.nber.org/papers/w23860

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
September 2017, Revised June 2018

We are grateful to Celeste Carruthers, Oded Gurantz, Judy Hellerstein, Lance Lochner, Matthias Schön, and seminar participants at Brigham Young University, Michigan State University, Montana State University, University of Illinois at Urbana Champaign, University of Michigan, University of Utah Department of Finance, West Virginia University, Association for Education and Finance 2017 meeting, Association for Public Policy and Management 2016 conference, International Institute of Public Finance 2017 Annual Congress, and Upjohn Institute for Employment Research for helpful comments on the draft. The conclusions of this research do not necessarily reflect the opinion or official position of the Texas Higher Education Coordinating Board, Texas Workforce Commission, or state of Texas. We gratefully acknowledge support from W.E. Upjohn Institute for Employment Research. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

ProPelled: The Effects of Grants on Graduation, Earnings, and Welfare
Jeffrey T. Denning, Benjamin M. Marx, and Lesley J. Turner
NBER Working Paper No. 23860
September 2017, Revised June 2018
JEL No. D14,D15,D61,H21,H52,I22

ABSTRACT

We estimate effects of the largest U.S. federal grant for college students using administrative data from Texas four-year public colleges and a discontinuity in grant generosity. Eligibility for additional grant aid significantly increases degree receipt and earnings beginning four years after entry. Estimated increases in income tax payments fully recoup government expenditures within ten years. A theoretical model shows that welfare effects of changes in college prices depend on (1) externalities from recipients' behavioral responses and (2) facilitation of intertemporal consumption smoothing. Calibration suggests that increasing grant aid for low-income college students would enhance welfare in many U.S. settings.

Jeffrey T. Denning
Brigham Young University
130 Faculty Office Building
Provo, UT 84604
jeffdenning@byu.edu

Benjamin M. Marx
Department of Economics
University of Illinois at Urbana-Champaign
214 David Kinley Hall, 1407 W. Gregory
Urbana, IL 61801
benmarx@illinois.edu

Lesley J. Turner
Department of Economics
University of Maryland
3115E Tydings Hall
College Park, MD 20742
and NBER
turner@econ.umd.edu

A online appendices is available at http://www.nber.org/data-appendix/w23860

1 Introduction

Federal and state governments provide substantial financial support to college students in the United States. During the 2015-16 academic year alone, low- and middle-income college students received $28 billion in federal Pell Grants, and state governments spent $10.5 billion on student grant aid (Baum et al. 2016). Many studies have examined the effect of grant aid on student outcomes, but less is known about the social return to these expenditures. Many, but not all, grant programs have been shown to increase college enrollment and degree receipt, outcomes that are important but not sufficient for determining whether increasing grant generosity also raises social welfare.

Using a regression discontinuity design, we provide evidence that grant aid targeting disadvantaged college students generates significant attainment and earnings gains. Among low-income bachelor's degree-seeking students in Texas, qualifying for the maximum Pell Grant at college entry generates significant increases in graduation and earnings beginning four years later. Eligibility for the maximum Pell Grant at entry also increases the total amount of grant aid received during college, an added social cost arising from behavioral responses. Nonetheless, effects on tax receipts are sufficiently large that the government should fully recover the increase in grant expenditures within ten years.

We evaluate the welfare implications of changes in grant aid with a theoretical model that allows for a general set of choice variables and potential constraints, including credit constraints. Our model generates sufficient statistics for the welfare effect of changes in grant generosity, and more generally, changes in the cost of attending some set of colleges. Welfare effects depend on net externalities generated by behavioral responses to grant aid and direct welfare gains when grants reduce consumption-smoothing frictions. Estimated fiscal externalities provide a lower bound for net externalities if non-fiscal externalities are positive on net. A simple ratio of marginal utilities fully captures direct welfare effects, regardless of the source of consumption-smoothing frictions. Using consumption data to estimate this ratio suggests that for common choices of utility parameters, a $1 transfer from unconstrained post-college years to college years would generate positive welfare gains of over $0.50 for the average college student. Direct benefits for low-income students are at least 30 percent larger.

Our empirical strategy exploits a fuzzy discontinuity in the Pell Grant formula. Pell Grant aid is a weakly decreasing function of the student's expected family contribution (EFC), and only students with a $0 EFC qualify for the maximum Pell Grant. Students whose family income falls below a year-specific income threshold - ranging from $15,000 to $32,000 in adjusted gross income (AGI) - meet one of the main criteria for receipt of an "automatic zero EFC." In our setting, students with AGIs below this threshold are 52 percentage points more likely to qualify for the maximum Pell Grant and receive an additional $711 in total grant aid (including federal and state) in their first year of college. Consistent with past research on enrollment effects of Pell Grant aid, we can rule out economically meaningful effects on college-going.[1]

We examine effects of automatic zero EFC eligibility on short- and medium-run attainment. Eligibility has no effect on students' first-year grade point averages (GPAs), credits attempted, or reenrollment. In contrast, eligibility generates large gains in later years. Eligible students attempt significantly more credits in each of the following two years and are 2 percentage points (13 percent) more likely to graduate within four years of entry. Estimated impacts on graduation up to seven years after college entry suggest permanent increases in degree receipt. Percentage increases are largest for science, technology, engineering, and math (STEM) majors. Automatic zero eligibility also has significant effects on annual earnings starting four years after college entry, generating a five- to eight-percent increase, corresponding to significant increases in estimated federal income and payroll tax liabilities. We show that these estimates are not driven by potential confounds (such as selection into enrollment, the characteristics of enrollees, or the likelihood of moving out of state) and are robust to a variety of specifications and sample selection criteria.

Our theoretical model offers a general framework for assessing the welfare implications of higher education price changes. We follow the "sufficient statistics" approach described by Chetty (2009), focusing on a small change in grant aid in order to derive welfare implications under relatively weak assumptions. Potential students make discrete choices over whether to enroll, in which college, and at what intensity. These choices may vary with both the size of current subsidies and the extent to which subsidies are affected by the reform under scrutiny. Thus, the model allows for grants to affect a variety of intermediate outcomes, e.g. reducing borrowing (Goldrick-Rab et al. 2016; Marx and Turner forthcoming) and in-school labor supply (Denning 2017), crowding out other sources of grant aid (Turner 2017; Bettinger and Williams 2015), and altering choice of institution (Cohodes and Goodman 2014). Such effects have first-order welfare implications if they alter the amount of taxes paid or educational subsidies received by students.

In our model, constraints are of a general form that allows for lack of information, market-imposed credit limits, or self-imposed constraints. This generality is useful when studying higher education, as past empirical research on traditional credit constraints is mixed (e.g., Lochner and Monge-Naranjo 2012), and recent research provides evidence of less-commonly-modeled barriers, such as transaction costs and debt aversion (Boatman et al. 2017; Marx and Turner 2017; Marx and Turner forthcoming). The ratio of marginal utilities within and after college is a sufficient statistic for the direct welfare effects of grant aid, and thus it is not necessary to know the underlying cause of any failures to smooth consumption. We use the Consumer Expenditure Survey to estimate the relevant ratio of marginal utilities for several values of the real discount

[1] Traditional-aged college students' enrollment decisions are not significantly affected by Pell Grant eligibility or generosity, possibly because students do not learn about their grant aid until they are accepted to college (Kane 1995; Turner 2017; Carruthers and Welch 2015; Marx and Turner forthcoming). Seftor and Turner (2002) provide evidence of increases for older, nontraditional students.

rate and the degree of relative risk aversion. On average, recent college attendees consume more than college students, implying that direct welfare gains should be considered in other settings and may be pivotal in those where grants generate smaller (or negative) net externalities. We present estimated upper and lower bounds of direct welfare effects for high-, average-, and low-income populations to facilitate application to other settings. As an illustration, we apply these direct effects and our welfare formula to effects of federal and state grant aid estimated by Marx and Turner (forthcoming) and Bettinger et al. (2016). Estimated welfare gains are largest when grant aid targets low-income students.

Our findings contribute to the broad literature examining the effects of college costs and financial assistance on student outcomes. Prospective students' college enrollment decisions respond to changes in prices driven by variation in tuition and grant aid provided by simple, easily accessed programs (Deming and Dynarski 2010). In some settings, state grant aid eligibility shifts students into college, across different types of institutions, and into degree receipt (e.g. Scott-Clayton 2011; Castleman and Long 2016; Bettinger et al. 2016; Scott-Clayton and Zafar 2016). We contribute to this literature in several ways. Our model highlights that policies that shift students across sectors may have different welfare implications than policies that increase degree receipt. Second, we are the first to examine the earnings impact of the largest U.S. federal grant program. Further, we exploit a novel source of variation that allows us to examine the effects of grant receipt on low income students in contrast to previous studies that focus on the phase-out of grant aid among higher-income students. Finally, we use administrative education and earnings records from a large state, which gives our estimates statistical power and allows us to observe how grant aid affects students' finances and performance in college years as well as post-college earnings.

Numerous papers that examine the effect of higher education subsidies also consider costs and benefits from the government's perspective. We build upon this literature by considering social welfare effects. Dynamic public finance papers calculate optimal education subsidies accounting for the same types of direct and indirect welfare effects that come out of our model (Findeisen and Sachs 2016; Lawson 2017; Stantcheva forthcoming). This literature focuses on the tax effect of the enrollment decision, a margin that is unaffected in our empirical setting, and we show that other margins and other fiscal externalities can determine whether any particular education subsidy enhances welfare.

2 Setting

We focus on students enrolled in the 39 Texas public four-year institutions, which served over 600,000 undergraduates in fall 2015. Texas four-year public schools are largely representative of the average public four-year institution nationwide. Average annual in-state tuition was $7870 in Texas, while the national average for four-year public institutions was $8543 in fall 2014 (National Center for Education Statistics 2016). The six-year graduation rate among four-year public school students in Texas was 53 percent, while the national average was 60 percent.[2] Texas public four-year schools range from selective research universities (e.g., University of Texas at Austin) to less selective, regional institutions.[3]

2.1 Federal student aid

Our main source of identifying variation comes from a discontinuity generated by the federal need calculation that determines Pell Grant aid. A student's Pell Grant depends on the annual maximum and her EFC, which represents the federal government's estimate of the student and her family's ability to pay for college. All students must complete a free application for federal student aid (FAFSA) to qualify for Pell Grant aid. FAFSA inputs determine EFC through a complicated nonlinear formula that takes into account family income, assets, family size, siblings in college, and a host of other factors.[4] A full-time, full-year student's Pell Grant is $Pell_{it} = \max\left\{\overline{Pell_t} - EFC_{it}, 0\right\}$. $\overline{Pell_t}$ is the maximum Pell Grant in year t and EFC_{it} is student i's expected family contribution. Only students who receive a $0 EFC qualify for the maximum Pell Grant.[5]

Students with family AGI below a year-specific threshold meet one of the requirements to qualify for an automatic zero EFC.[6] Only students with a $0 EFC qualify for the maximum Pell Grant, but it is possible to receive a $0 EFC without having income below the automatic zero threshold as EFC depends on other factors (e.g., family structure). Students whose families received means-tested benefits in the prior year also qualify, regardless of AGI. In addition to meeting the AGI requirement, a student's parents must also be eligible to file a 1040A or 1040EZ tax return, which generally excludes high asset families.

The automatic zero eligibility threshold varies over time. Prior to the 2006-07 academic year (hereafter 2007), the threshold was $15,000. The cut-off increased to $20,000 in 2007, $30,000 in 2010, and $31,000 in 2012, before falling to $23,000 in 2013. Between 2014 and 2016, the threshold remained at $24,000 and increased to $25,000 in 2017.[7]

Students who complete a FAFSA are also eligible for federal loans. Federal loans made up approx-

[2] See http://www.thecb.state.tx.us/reports/PDF/7831.pdf for details.

[3] In fall 2015, 80 Texas public two-year institutions enrolled over 700,000 students and 166 private institutions enrolled 191,000 undergraduates. Data on students in Texas private institutions are not available for research. We exclude students entering community colleges from our analyses due to a lack of a significant effect of eligibility on grant aid (discussed in Section 3).

[4] The Department of Education's 2016-17 EFC guide is 36 pages and includes 6 worksheets and 17 corresponding calculation tables.

[5] Part-year students receive a prorated Pell Grant. Students with less than full-time enrollment are eligible for a lower maximum Pell with a flatter phase-out. Awards are rounded to nearest $100. In a small number of cases when students face a sufficiently low cost of attendance (which includes tuition, fees, room and board, and other expenses), Pell Grants may be reduced so as not to exceed total unmet need.

[6] Only dependent students and independent students with dependents are eligible for an automatic zero EFC. All students under the age of 24 who are unmarried and have no dependents are classified as dependent students.

[7] Online Appendix Figure C.1 displays the annual automatic zero EFC threshold and the maximum Pell Grant award. The latter increased from approximately $4000 to approximately $6000 (in nominal terms) over the period we examine.

imately 90 percent of loan disbursements to undergraduate students in recent years (Baum et al. 2016). Federal subsidized loans do not accrue interest while a student is enrolled in at least 6 credits per semester and are available to students with unmet need (equal to the total cost of attendance less EFC and grant aid from all sources). Students may borrow subsidized loans up to the lesser of the subsidized loan maximum (e.g., $3500 for first year, dependent students) and unmet need. Subsidized loan eligibility could be affected by receipt of an automatic zero EFC, but unsubsidized loans are available to all FAFSA filers regardless of financial need, and federal loans have yearly and lifetime borrowing limits that do not vary with need or family income.[8] Thus, eligibility for an automatic zero EFC does not affect total loan eligibility. Our empirical analysis considers the potential effects of a $0 EFC on subsidized loan eligibility and the amount of state and institutional grant aid.

Private student loans may be available to students who have exhausted their federal loan eligibility. However, private loans entail a credit-worthiness requirement and/or require a cosigner and generally carry higher interest rates than federal loans. Nationwide, only 6 percent of undergraduate students with family AGIs near the automatic zero EFC eligibility threshold received private loans, while 28 percent stated that they would have borrowed more if funds were available, suggesting few undergraduates can access loans that are not publicly provided.[9]

2.2 Texas financial aid programs

Texas' largest financial aid program is the TEXAS (Towards EXcellence, Access and Success) Grant. TEXAS Grant disbursements equaled $200 million in 2008. The maximum TEXAS Grant award for bachelor's degree seeking students in public institutions equals the statewide average of tuition and required fees within the four-year public sector. Eligibility for a TEXAS Grant depends on need and academic performance at college entry.[10] The program is oversubscribed; only 50 percent of eligible students received a TEXAS Grant in recent years (Cohen et al. 2010). Schools have discretion over which students receive a TEXAS Grant and must use institutional funds to cover remaining tuition and fees after federal and TEXAS Grant aid is applied, which may provide an incentive for institutions to give TEXAS Grants to students eligible for large amounts of other grant aid. Smaller Texas grant and loan programs that provide funding for four-year public students are described in Online Appendix A.

[8] Unsubsidized loans have weakly-higher interest rates than subsidized loans and start accruing interest at disbursement. Online Appendix A provides additional information on federal loan options. Our main results are robust to limiting the sample to the subset of students whose eligibility for subsidized loan aid is not affected by automatic zero EFC eligibility.

[9] Authors' calculations using the 2012 NPSAS via PowerStats.

[10] TEXAS Grant recipients must have financial need, a sub-$4000 EFC, and enter public higher education within 16 months of graduating from a Recommended High School Program. To maintain eligibility, recipients must earn at least a 2.5 GPA, complete at least 24 credit hours in an academic year, and not receive grants in excess of their cost of attendance.

3 Data and Sample

Our data come from the Texas Higher Education Coordinating Board (THECB) and Texas Workforce Commission (TWC). The THECB collects administrative data from all public institutions in Texas, including student-level information on enrollment, graduation, college major, GPA, credits attempted, a subset of FAFSA inputs (including AGI), EFC, and financial aid disbursements. TWC data contain quarterly earnings records for all employees in industries covered by unemployment insurance (UI) in Texas.[11] We base our measure of annual earnings on the academic year rather than calendar year and winsorize earnings at the 99 percentile.[12] We impute federal income tax liabilities using NBER's TAXSIM, assuming students are single with no dependents, no deductions, and no income outside of earnings in covered sectors.[13] Texas does not have a state income tax. Since we only observe two of the four quarters of the most recent tax year, we impute tax liabilities by doubling the earnings observed in the first two quarters. All earnings, taxes, and financial aid awards are adjusted for inflation using the CPI-U to represent constant 2013 dollars.

Our analysis sample includes first-time college entrants who enrolled in a public university in Texas in the 2008 through 2011 academic years, which includes the first cohort for which parent AGI is available (2008) and allows us to track students up to seven years following college entry.[14] We observe five years of outcomes for all four of these cohorts, six years for three cohorts, and seven years for two. We focus on students with family AGIs within $12,000 of the automatic zero EFC income eligibility threshold.[15] We limit our sample to four-year college entrants because the "first stage" change in grant aid at the automatic zero threshold is negligible in the two-year sector, limiting our ability to draw conclusions about the effects of additional grant aid for community college students. We further limit our sample to students classified as dependent as most independent students are ineligible for an automatic zero EFC, irrespective of AGI.[16] Because we observe "heaping" at $1000 intervals in AGI and Barreca et al. (2016) demonstrate that regression discontinuity estimates may be biased with non-random heaping at specific values of the run-

[11] 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.

[12] For year t, AY earnings equal the sum of quarterly earnings from Q4 of year $t-1$ through Q3 of year t. We winsorize earnings to deal with a small number of outliers in the earnings data.

[13] We do not observe family structure, potential deductions, and non-UI earnings. Earnings are rounded to the nearest $1000 for the purpose of calculating tax liabilities because TWC earnings data cannot leave a dedicated, non-networked machine. See Feenberg and Coutts (1993) for additional TAXSIM information.

[14] Estimated effects of eligibility on returning college students are available in Online Appendix B. Because we observe a smaller number of returning students, estimates are relatively imprecise, and we can't reject a test of the hypothesis that automatic zero eligibility has equal effects on the outcomes of entering and returning students.

[15] AGI is only observed for FAFSA-filers, thus non-filers are automatically excluded. Among 2012 first-time dependent students entering public four-year institutions with AGIs within $12,000 of the automatic zero threshold, 89 percent filed a FAFSA (2012/14 BPS, authors' calculations via PowerStats).

[16] Only independent students with dependents are eligible for an automatic zero EFC. These students' eligibility is based on their own AGI. Other independent students (those who are 24 or older and those who are married) do not qualify. Nationwide, only 3.6 percent of first-time undergraduates entering public four-year institutions had dependents. Authors' calculations using the 2012/14 Beginning Postsecondary Students (BPS) study via PowerStats.

ning variable, we exclude students with AGIs at \$1000 intervals from our main analysis sample.[17] Our final sample includes 36,697 first-time entrants.

Table 1 displays sample characteristics.[18] On average, students are 19 years old at entry and 97 percent are Texas residents. Racial/ethnic minorities comprise a substantial share of the sample, with 46 percent identifying as Black or Hispanic. Most students do not have parents who attended college: 23 percent report having a father with a college degree and 29 percent report having a college-educated mother. Students receive a substantial amount of grant aid – approximately \$9700 – in their first year of enrollment. On average, students borrow \$2740 through federal and state loan programs and earn \$3844 in their first year. Sample members come from relatively low-income families, especially compared to students near the eligibility threshold for grant aid programs that are commonly studied.[19] Students with AGIs below the automatic zero EFC threshold receive more grant aid, take on less student loan debt, and earn less than to students above the threshold.

4 Identification Strategy

For identification, we exploit the nonlinear relationship between AGI and automatic zero EFC eligibility, which in turn, generates a discontinuous increase in the grant aid. Let agi_t^0 represent the value of the automatic zero EFC cutoff in year t. For student i in entry cohort t, $\widetilde{AGI}_{it} = AGI_{it} - agi_t^0$ is the distance her family's income falls from the year-specific threshold. When a student is ineligible for an automatic zero, her EFC is determined by a complicated nonlinear function of family income, assets, and many other characteristics that are both observable ($\mathbf{X_i}$) and unobservable (U_{it}):

$$EFC_{it} = \mathbf{1}\left(\widetilde{AGI}_{it} > 0\right) \times f\left(AGI_{it}, \mathbf{X_i}, U_{it}\right).$$

Panel A of Figure 1 displays the relationship between $\widetilde{AGI}$ and \$0 EFC receipt. Some students who are income-eligible (hereafter, "eligible") for an automatic zero are disqualified based on other requirements (in most cases, related to family assets and non-earned income). Likewise, students who are ineligible for an

[17]Our AGI measure comes from administrative FAFSA data reported to the THECB. One explanation for the heaping is that students are allowed to submit their FAFSA before their parents have filed their taxes and report an estimated AGI (that will later be updated with a revised submission). Our data only contain information from one FAFSA (not necessarily the final version). Online Appendix Table C.2 compares the characteristics of heapers and nonheapers. Heapers are less likely to be Texas residents, less likely to be white, and more likely to have college educated parents. However, we show that the inclusion of heapers does not substantively alter our results (Section 5.3).

[18]Characteristics are largely similar to those of dependent, first-time, bachelor's degree seeking public college entrants nationwide in 2008 (Online Appendix Table C.1).

[19]Carruthers and Welch (2015) and Marx and Turner (forthcoming) examine the cutoff for the minimum Pell Grant (roughly \$50,000 in family AGI). Castleman and Long (2016) examine a change in Florida state grant aid for students with an EFC of \$1590 corresponding to an income of \$40,000 (in 2011\$). Eligibility for the Cal Grant studied by Bettinger et al. (2016) involves both income and high school GPA thresholds, with the former corresponding to approximately \$60,000 in family income.

automatic zero EFC based on AGI may still qualify based on other FAFSA inputs. Thus, income eligibility (hereafter, "eligibility") imperfectly predicts receipt of a \$0 EFC. Students with AGIs below the eligibility threshold are 51 percentage points more likely to receive a \$0 EFC.

Students with a \$0 EFC qualify for the maximum Pell Grant. No other federal grants are explicitly linked to a \$0 EFC, but many federal and state programs target students with high levels of unmet need (see Online Appendix A). Eligibility generates an approximately \$700 increase in total grant aid (Figure 1, Panel B), driven by an approximately \$500 increase in Pell Grant aid (Panel C). Eligible students also experience a smaller discontinuous increase in TEXAS Grant aid (Panel D). Average grant aid from other sources is continuous through the eligibility threshold (Panel E), while the level of student loan aid discontinuously decreases (Panel F). While the relationship between eligibility and Pell Grant aid is purely mechanical, the relationships in Panels D and E may involve both mechanical effects due to state policies and the endogenous response of institutions to changes in a given student's EFC and financial aid package, and the relationship in Panel F is generated by endogenous responses of students.

We quantify the effect of automatic zero EFC eligibility via ordinary least squares (OLS):

$$Y_{it} = \beta \mathbf{1}\left(\widetilde{AGI}_{it} > 0\right) + f\left(\widetilde{AGI}_{it}\right) + \mathbf{X_i}\gamma + \delta_t + \epsilon_{it} \tag{1}$$

where Y_{it} represents the outcome of student i belonging to entry cohort t, $\mathbf{1}\left(\widetilde{AGI}_{it} > 0\right)$ indicates automatic zero EFC eligibility, $f\left(\widetilde{AGI}_{it}\right)$ is a function of normalized AGI (allowed to vary on either side of the eligibility threshold), $\mathbf{X_i}$ is a vector of observable student characteristics (indicators for parental education, race, gender, and Texas residency, and a continuous term in age at entry), and δ_t is a set of entry-cohort fixed effects. Under the identifying assumption that, in the neighborhood of the eligibility threshold, all unobservable factors that are correlated with both Y and \$0 EFC receipt vary continuously through the threshold, $\hat{\beta}$ represents the causal effect of automatic zero eligibility on student outcomes (Hahn et al. 2001).

In practice, we estimate local linear regression models with a uniform kernel and bandwidth of \$12,000 (approximately the midpoint of the optimal bandwidths chosen across outcomes by the Imbens and Kalyanaraman (2012) procedure). We show that our estimates are robust to larger and smaller bandwidths and higher order polynomials in $\widetilde{AGI}$. Standard errors are clustered at the entry cohort by institution level to account for correlated outcomes within cohort-institution groups.

Assuming that eligibility for an automatic zero EFC only directly affects student outcomes by increasing grant aid allows for instrumental variables (IV) estimates of the effect of additional grant aid on attainment; $\mathbf{1}\left(\widetilde{AGI}_{it} > 0\right)$ serves as the excluded instrument. As long as eligibility weakly increases grant aid for all students, IV estimates represent the weighted average of causal responses to a marginal increase in grants on student outcomes for the set of students induced to receive a $0 EFC by meeting the AGI eligibility requirement (Angrist and Imbens 1995). We scale treatment effects by the change in first-year grant aid because, unlike state grants that offer aid for multiple years, Pell Grant aid is recalculated each year.

4.1 Evaluating the RDD identifying assumptions

Our key identifying assumption is fundamentally untestable but generates testable predictions (Lee and Lemieux 2010). We first test for discontinuities in the density of $\widetilde{AGI}$ by examining the number of students in $100 AGI bins on either side of the eligibility threshold (Figure 2). Panel A excludes students who report an AGI that is a multiple of $1000, while Panel B includes all students. In both cases, there is no observable change in density below the automatic zero EFC eligibility threshold. Barreca et al. (2016) demonstrate that regression discontinuity estimates may be biased with non-random heaping at specific values of the running variable. The cutoff for the automatic zero EFC occurs at a multiple of $1000 in all years. Thus, our main specification excludes students who report EFCs at multiples of $1000. Even so, if parents precisely manipulate or misreport AGI to gain access to a $0 EFC, our key identifying assumption would be violated. This does not appear to be the case in Panel B of Figure 2; there is heaping at all multiples of $1000, and the number of students at the threshold is not substantially larger than heaping at other values.

Other potential concerns for our identification strategy include behavioral responses that exploit the income eligibility rule (e.g., misreporting of income) and enrollment responses. Though income may be under-reported generally, in recent years, over half of all Pell Grant eligible students have been selected for FAFSA verification, and income is one of the main components that is audited. General under-reporting will not threaten identification unless students and their families are more likely to under-report when their income falls just above the threshold, which is unlikely, as the existence of this cutoff is not well known or publicized. Following McCrary (2008), we find an insignificant 0.032 difference in the density of students below the eligibility threshold (with standard error of 0.029), providing evidence against behavioral responses that exploit the income eligibility rule.

We also test for discontinuities in predetermined student characteristics. Point estimates for selected characteristics are presented in Table 2 and Online Appendix Table C.3. To generate a single index of covariates, we predict the probability of graduating within four years of college entry via a logistic regression on all covariates. While two of the 8 regressions shown in Table 2 yields statistically significant differences, there is no significant change in the predicted probability of four-year graduation, and the 95 percent confidence interval rules out effects larger than a 0.007 percentage point difference.[20]

[20]Eligible students are 1.2 percentage points (1 percent) significantly more likely to be Texas residents ($p < 0.05$) and 3.2 percentage points (12 percent) less likely to be Black ($p < 0.1$). When using the Imbens and Kalyanaraman (2012) optimal bandwidths for

5 Empirical Results

We preview our key findings with graphical evidence of the reduced form relationship between $\widetilde{AGI}$ and student outcomes. Eligibility for an automatic zero EFC at entry has only negligible effects on credit hours attempted, GPA, earnings, or the probability of reenrollment in the following year (Figure 3, Panels A through D). Over the longer-run, eligibility increases the probability of graduation starting four years after entry (Figures 4). Increases in graduation rates persist for the duration of our panel, up to 7 years after college entry. Likewise, eligibility appears to generate earnings increases four years after entry, and these gains persist for the remainder of our panel (Figure 5). Summing across all years in which we observe sample members, we calculate total grants, loans, earnings, and tax liabilities. Automatic-zero eligibility is correlated with discrete increases in total grant aid received, total earnings, and total estimated federal income taxes (Figure 6). Despite the reduction in first-year borrowing shown in Figure 1, automatic zero eligibility at entry does not appear to change cumulative debt by the end of our panel.

5.1 Short-run effects on finances and academics

Although endogenous decisions by students and institutions may diminish changes to cash on hand generated by automatic zero EFC eligibility, in our setting, the significant increase in Pell Grant aid crowds in grant aid provided through the TEXAS Grant program (Table 3). Eligibility at entry does not significantly affect other sources of grant aid but leads to a statistically significant $333 decrease in the amount borrowed and a negative but insignificant reduction in earnings from employment.

Effects on first-year academic outcomes are small and insignificant. Eligibility does not significantly affect credits attempted, GPA, or reenrollment in the following academic year (Table 4). Estimated 95-percent confidence intervals exclude an increase greater than 0.5 credits attempted (2 percent), a 0.02 percentage point (3 percent) increase in returning the following academic year, and a 0.098 (5 percent) GPA increase. If we scale these estimates by the change in grant aid received by eligible students (an approximately 9 percent increase), we cannot rule out modest responses of short-run attainment to additional aid, although the effects are smaller than those generated by increases in borrowing due to loan packaging in Marx and Turner (2017). Attainment gains could be modest in the first year but substantial in later years if initial gains increase students' confidence or if the experience in the first year, and the resources remaining after this year, affect subsequent decisions.[21]

Texas residency ($6200) and race ($8300), estimated discontinuities are smaller and statistically insignificant. Given the number of hypotheses tested in Table 2 and Appendix Table C.3 (16) and the lack of significant effects on the composite index, we interpret these estimates as providing evidence in support of continuity in predetermined characteristics through the threshold.

[21] In a randomized controlled trial of privately-funded financial aid on student outcomes, Goldrick-Rab et al. (2016) obtain similar results, with small effects on contemporary outcomes accompanied by significant increases in graduation rates.

5.2 Longer-run effects on graduation and earnings

We first examine effects on academic outcomes including reenrollment, credits attempted, and degree receipt. Panels A through C of Figure 7 display point estimates and corresponding 95 percent confidence intervals from equation (1), which represent reduced-form effects of automatic zero EFC eligibility. Online Appendix Table C.4 contains corresponding point estimates and standard errors. We find no evidence of effects on enrollment in Texas public four-year institutions that are significant at the 5 percent level at any point after college entry (Panel A), though point estimates are positive through the first four years.[22] Eligibility generates significant increases in credits attempted two and three years after entry of 0.6 and 0.8 additional credits attempted, respectively (Panel B).

Automatic zero eligibility significantly increases the probability of graduation within four, five, and six years of college entry (Figure 7, Panel C). Eligible students are approximately 2 percentage points more likely to earn a bachelor's degree within four years (a 13 percent increase relative to the mean completion rate for ineligible students) and 3.6 and 3.4 percentage-points more likely to graduate within five and six years of entry, respectively (representing 12 and 9 percent increases). The estimated impact on the probability of graduation within seven years is similar in magnitude to estimated effects on the five- and six-year graduation rates and larger in magnitude than the increase in the probability of graduating within four years, but because we only observe two entry cohorts for this length of time, the estimate is imprecise.

In the longer-run, increases in graduation rates might persist (as in Bettinger et al. 2016) or fade (as in Scott-Clayton and Zafar 2016). Approximately 7 percent of ineligible students and 8 percent of eligible students are still enrolled 7 years after entry (Online Appendix Table C.4). To close the gap in the overall graduation rate, the difference in the probability of graduation between eligible and ineligible students still enrolled 7 years after entry would have to exceed 50 percentage points (e.g., 100 percent of still-enrolled ineligible students and less than 45 percent of still-enrolled eligible students would eventually graduate). Thus, while there is scope for the the impacts on graduation to ultimately represent retiming of degree receipt rather than increases in new graduates, ineligible students' outcomes would need to dramatically improve for the gains in degree receipt to disappear.

We repeat this exercise for labor market outcomes including annual earnings and estimated federal income and payroll taxes (Figure 7, Panels D though F and Online Appendix Table C.5). Automatic zero eligibility results in significant earnings gains beginning four years after college entry.[23] Estimated impacts on earnings continue to increase for the remainder of our panel and are comparable to estimated effects of

[22] The estimated 2 percentage point increase increase in enrollment two years after entry is significant at the 10 percent level. We find no evidence of significant impacts on the probability of transferring to a Texas community college (Online Appendix Figure C.2).

[23] Estimated impacts on earnings are quite similar when we use non-winsorized earnings except at 7 years post-entry when one automatic zero EFC eligible student earned over $1 million (Online Appendix Table C.6).

increased grant aid and/or degree receipt in other settings.[24] Earnings gains could theoretically increase or decrease over time, depending on the timing of returns to a bachelor's degree receipt and the extent to which ineligible students' degree receipt converges with that of eligible students. Complete convergence in the graduation rates of eligible and ineligible students is unlikely. Given evidence that estimated returns to education are decreasing in time-to-degree, the estimated earnings gains will most likely persist over the long-run (Flores-Lagunes and Light 2010).

Under the assumption that eligibility for an automatic zero EFC at entry only affects students' outcomes by weakly increasing grant aid, we can identify the causal effect of increases in grants on attainment and labor market outcomes. We estimate instrumental variables (IV) models in which the indicator for income below the eligibility cutoff serves as the excluded instrument. Online Appendix Table C.7 displays these estimates. An additional $1000 in first year grant aid generates significant increases in graduation within four through seven years of entry representing increases ranging from 10 to 16 percent relative to mean completion rates of ineligible students. These gains are comparable to the estimated effects of Cal Grant eligibility on the long-run probability of earning a bachelor's degree found by Bettinger et al. (2016).[25]

5.3 Robustness

Interpretation of estimated impacts on earnings is complicated by the fact that we only observe earnings for jobs covered by the Texas UI system. If someone does not work, moves out of state to work, or works for a job not covered by the Texas UI system, they will appear to have no earnings. Automatic zero EFC eligibility could affect the probability that a student remains in state (or in a UI-covered job).[26] In this case, estimated earnings gains will not represent the causal effect of eligibility for additional grant aid. To address this concern, we test whether eligibility affects the probability of being observed "in state," defined as either having earnings or enrollment in a public institution. Eligibility does not significantly alter the probability of being in state. Point estimates only exceed a 1 percentage-point increase at the seven-year

[24]Bettinger et al. (2016) study the effect of eligibility for a grant that is roughly three times larger than the change in grant aid in our setting and find statistically significant increases in earnings that are roughly twice as large. Zimmerman (2014) studies an admissions threshold for a four-year institution and finds receipt of a bachelor's degree is associated with an annual earnings increase of about $30,000 (in 2005$). The corresponding association in our setting is $32,000 (in 2013$).

[25]Mean outcomes for ineligible students at the Cal Grant GPA eligibility threshold in Bettinger et al. (2016) are similar to those in our setting. The change in total grant aid at the GPA eligibility threshold is about $4000, roughly three times the magnitude of the increase in grant aid at the automatic zero EFC threshold. Estimated effects of initial eligibility on degree receipt are roughly proportional: Bettinger et al. (2016) find increases in bachelor's degree receipt that are twice as large as the effects in our setting, and impacts on log labor income due to Cal Grant eligibility translates to a $1630 increase in annual earnings, about 1.5 larger than the effect on annual earnings from automatic zero EFC eligibility five and six years after entry.

[26]College educated young adults are more likely to move between states than those with less education (Malamud and Wozniak 2012). Andrews et al. (2016) rule out systematic differences in earnings for students who leave Texas and those who remain. Furthermore, young college educated adults are relatively unlikely to move out of Texas. Using the IPUMS-CPS (Flood et al. 2015), we estimate an annual interstate migration rate of 3.2 percent for young adults (20-24 years old) with some college in Texas between 2010 and 2016. State grant aid eligibility may also have small effects on interstate migration (e.g., Fitzpatrick and Jones 2016; Bettinger et al. 2016) but it is not clear whether these findings can be generalized to the effects of federal grant aid on the probability remaining in state after attending college.

horizon, when the underlying sample is smallest (Online Appendix Figure C.2).[27]

We estimate bounds on eligible students' earnings gains, following Lee (2009). Specifically, we "trim" the earnings of automatic zero EFC eligible students at the top of the distribution of reported earnings by setting these students' earnings to $0. We use the years-since-entry-specific estimated change in the probability of remaining "in-state" (Online Appendix Figure C.3) to determine the share of eligible students to trim. This exercise produces bounds under the extreme assumption that the entirety of the (insignificant) difference in the probability of being in-state comes from eligible students with the highest earnings who would have otherwise left Texas (rather than, for instance, an increase in the probability of employment). The estimated lower bounds on earnings gains in years 4 through 7 average to approximately one quarter of the effect size in our main specification. If the lower bound of estimated earnings gains for eligible students seven years after entry ($602) persists over a 35-year career, the lifetime earnings effect would be roughly 30 times the size of the initial grant.

Our main findings are robust to several alternative specifications (Online Appendix Table C.8). Models that exclude predetermined student covariates are quite similar to those produced using equation (1) (Panel A). Likewise, including students "heaping" at AGI multiples of $1000 does not alter our findings (Panel B). Our results are not substantially affected by reducing the bandwidth to $6000 (Panel C) or increasing it to $18,000 (Panel D). Finally, Panel E shows that estimates from models that use a bandwidth of $18,000 and a quadratic in $\widetilde{AGI}$ are quite similar to those produced by our preferred specification.

5.4 Evidence of borrowing constraints

Theoretically, grant aid should increase the attainment of students who face liquidity constraints (Lochner and Monge-Naranjo 2012).[28] The importance of credit constraints appears to have grown with increases in costs and returns to college (Lochner and Monge-Naranjo 2011), and a substantial share of U.S. college students and Canadian high school students report a desire to borrow above and beyond their current eligibility (Stinebrickner and Stinebrickner 2008; Belzil et al. 2017). Even students who are not constrained in the traditional sense may not fully smooth consumption due to fixed borrowing costs, concerns over lack of self control, or aversion to taking on debt (e.g., Cadena and Keys 2013; Marx and Turner forthcoming; Boatman et al. 2017).

[27] Automatic zero eligible students are significantly more likely to have UI-covered earnings 4 and 7 years after college entry, and point estimates are of a similar magnitude in years 5 and 6 (Online Appendix Table C.6). Combined with the positive effects of automatic zero eligibility on the probability of graduation for the same time period (Figure 4), this result suggests that automatic zero eligibility leads students to both finish school and enter the labor market sooner than they otherwise would have.

[28] Structural estimates of the importance of credit constraints in the U.S. college students' decisions have generally found that relaxing such constraints would have small effects on educational attainment (Cameron and Taber 2004; Keane and Wolpin 2001; Johnson 2013). As Lochner and Monge-Naranjo (2012) note in their review of the literature, effects on current consumption could be substantial even in the absence of attainment effects.

Few low-income undergraduate students have access to private loans. Thus, we proxy for whether a student faces credit constraints with a dummy indicating that she has borrowed the maximum allowed federal Direct Loan, a necessary (but not sufficient) condition for being constrained.[29] To avoid mechanical reductions in federal loan eligibility generated by the automatic zero EFC-driven increases in grant aid, we focus on the subsample of students who should qualify for the maximum federal loan whether or not they receive an automatic zero EFC.[30] Only 12 percent of ineligible students take-up the maximum allowed Direct Loan (Table 6). Automatic zero eligibility reduces the share of students who have exhausted their federal loan eligibility by 3.3 percentage points (28 percent), suggesting that additional grant aid may relax credit constraints for a number of students in our sample.

Students may also face internal credit constraints. Approximately 27 percent of ineligible students do not borrow, and automatic zero EFC eligibility leads to a statistically significant 4 percentage point reduction in the probability of borrowing. The unconditional amount borrowed in eligible students' first year falls by $620 in response to a $544 increase in total grant aid in the restricted sample, implying that every dollar of grant aid crowds-out over $1 of loans. Crowd-out for "would-be-borrowers" – students who would have borrowed had they not been eligible for an automatic zero EFC – is even larger at $1.57. As described in Marx and Turner (forthcoming), crowd-out of loans in response to grant aid in excess of 100 percent can occur if some students face a (non-monetary) fixed cost of borrowing. This fixed cost can reduce attainment in the same manner as external credit constraints, even though constrained students technically have access to additional loan aid.

These results suggest that grants may have increased attainment by relaxing traditional credit constraints, self-imposed constraints, or a combination of the two. As we show in Section 6, knowing the exact mechanism that prevents students from fully smoothing consumption is not necessary to draw inference about the welfare effects of increasing grant aid.

5.5 Channels for earnings gains

There are several channels through which eligibility for additional grant aid could increase earnings. First, the increased probability of degree receipt within four and up to seven years after entry likely represents a permanent increase in educational attainment. Given the extensive evidence of earnings gains from bache-

[29] Loan amounts reported in the data often exclude small origination fees. As a result we define a student as borrowing the maximum allowed federal loan if she borrows within $100 of the federal Direct Loan limit for first-year dependent students. Changing the definition to be exactly the maximum reduces the coefficients slightly but the results are qualitatively similar. Students are considered to borrow the maximum allowed federal loan if they take-up either the federal maximum for dependent students or the federal maximum for dependent students whose parents are denied a PLUS loan. See Online Appendix A for details.

[30] The sample is defined by students who have unmet need (cost of attendance - EFC - grants) + TEXAS Grants exceeding $13,500. For these students, even if the additional Pell Grant aid were to crowd in $8000 of TEXAS Grant aid (the 99th percentile in the sample), then the student would still have over $5500 of unmet need and be able to borrow the maximum federal Direct Loan. Estimated effects on academic and financial outcomes are quite similar for this restricted sample (Online Appendix Tables C.9 and C.10).

lor's degree receipt (e.g., Barrow and Malamud 2015), the increases in graduation rates are likely a major channel through which eligibility affects earnings. In theory, eligible students may accumulate more student loan debt which could, in turn, increase the likelihood that they choose a higher paying job (e.g., Rothstein and Rouse 2011). However, eligibility for an automatic zero EFC does not significantly affect cumulative student loan debt, allowing us to rule out this channel. Finally, eligibility could induce students to upgrade to institutions or majors that generate higher earnings.

We estimate effects of automatic-zero eligibility on measures of institutional quality to test whether eligibility for additional grant aid leads to upgrading. Institutional quality has been shown to affect attainment and labor market outcomes in many settings (e.g., Hoekstra 2009, Bound et al. 2012, Cohodes and Goodman 2014). We examine effects on inputs (e.g., characteristics of the student body, selectivity), resources (e.g., costs, expenditures, student-faculty ratios), and outputs (e.g, retention and graduation rates). All measures of institutional quality come from the Integrated Postsecondary Education Data System (IPEDS). To create a summary of the numerous measures of institutional quality and to deal with concerns of spurious significant estimates due to the number of hypotheses being tested, we examine effects on the first component from a principal component analysis of these quality measures (Black and Smith 2006). As shown in Online Appendix Table C.11, we find no statistically significant effects of eligibility on any of the institutional quality measures beyond a 0.9 percentage point (2 percent) increase in the average admissions yield of institutions attended by eligible students ($p < 0.1$). The largest percentage change is an 0.8 percentage-point increase in the average four-year graduation rate of the institution attended by eligible students, a 3.7 percent increase relative to the average graduation rate at institutions attended by ineligible students. Estimated effects on the first principle component (FPC) are small and statistically insignificant. The mean difference between consecutively ranked Texas public four-year institutions is 0.46, roughly three times larger than the increase in quality (as measured by the FPC) at the eligibility threshold.

Finally, we test whether eligibility for additional grant aid altered students' field of study by estimating impacts on graduation in STEM (Science Technology Engineering Math) and non-STEM fields (Table 5).[31] Estimated impacts on STEM and non-STEM degree receipt within four and six years of entry are similar in magnitude. However, STEM degree receipt is less common and so a similar percentage point increase represents a larger percent increase over ineligible students' attainment. Eligibility increases STEM degree receipt within four years of entry by 33 percent and by 16 percent within six years of entry. These increases are larger than most of the estimated impacts of federal SMART grants, which explicitly subsidized STEM degree completion (Denning and Turley 2017; Evans 2017). We find a marginally significant ($p < 0.1$)

[31] STEM majors are those with Classification of Instruction Programs (CIP) codes included in the National Science Foundation's list of STEM disciplines (https://www.lsamp.org/help/help_stem_cip_2010.cfm).

increase in the share of students who declare a STEM major during their time in college (Online Appendix Table C.12), which could arise if treatment effects on attainment are larger for STEM majors or if a maximum Pell Grant affects sorting into and/or out of STEM majors.

6 Theoretical Framework

In our setting, eligibility for the maximum Pell Grant increases both cumulative government expenditures on grant aid and eligible students' earnings and federal tax liabilities. To interpret what these findings imply for social welfare, we present a general model and derive sufficient statistics for the welfare implications of changes in the price of schooling. While our model is similar conceptually to that of Chetty (2006a), it differs in key aspects that make it relevant for analyzing the welfare implications of changes in financial aid and higher education pricing. In particular, we generalize from a constant unemployment insurance benefit to a nonlinear college pricing scheme, introduce student choices over which college (if any) to attend, and allow for multiple forms of externalities. The generality of the model allows for application to a variety of policies that alter college prices.[32] We note where a simpler model would apply to our empirical setting.

6.1 The individual's problem

An individual lives for up to $T+1$ periods, indexed by $t \in \{0, 1, ..., T\}$. In each period, she chooses consumption $c_t \geq 0$, a vector $\mathbf{s_t}$ of schooling investments $s_t^k \geq 0$ at colleges $k \in \{1, 2, ..., K\}$, and a length-X vector $\mathbf{x_t}$ of other choices. For example, schooling investment might be denominated in credits attempted and other choices could include the amount of leisure to consume and the number of hours to study or to work. Each college k charges tuition and fees η_t^{km} when schooling investment s_t^k falls within a range μ^{km}, of which there are $M^k \geq 1$. This structure accounts for the fact that many postsecondary institutions have flat pricing within a set range of credits attempted (e.g., 0-6, 6-12, 12 or more) and that some grant programs adjust aid for recipients' courseloads. Heterogeneous individuals i have continuously distributed characteristics; to reduce notation, we omit i subscripts. Choices are made for each value of a state variable ω_t that evolves according to an arbitrary stochastic process and may govern factors such as wages and schooling costs. Expectations $E[\cdot]$ are taken with respect to this state variable (and over individuals in the government's problem). Let $\mathbf{c} = \{c_t(\omega_t)\}_{0 \leq t \leq T}$, $\mathbf{s} = \{\mathbf{s_t}(\omega_t)\}_{0 \leq t \leq T}$, and $\mathbf{x} = \{\mathbf{x_t}(\omega_t)\}_{0 \leq t \leq T}$ denote an individual's full set of state-contingent choices.

[32] While our approach can only provide evidence on the welfare effect of a marginal change in higher education prices, it imposes minimal assumptions regarding the functional form of agents' utility, time preferences, or degree of risk aversion. In contrast, an approach that adds more structure to the individual's optimization problem would allow for estimates (and comparisons) of the magnitude of welfare gains or losses from a variety of counterfactual policies that involve larger changes in prices or other parameters.

The individual chooses consumption, schooling, and any other choices represented by $\mathbf{x_t}$ (e.g., labor supply) to maximizes expected utility $u_t\left(c_t, \mathbf{s_t}, \mathbf{x_t}\right)$, subject to several constraints. Each period's utility function reflects the individual's preferences and information set at time t, is strictly quasi-concave, and we assume standard Inada conditions with respect to consumption to ensure an interior solution. Though we do not find significant impacts of Pell Grant aid on enrollment, the model allows any and all components of $\mathbf{s}$ and $\mathbf{x}$ to take the value of zero; an individual need not ever attend college, and this margin may be affected by a policy change. The budget constraint in each period t is: $c_t = a_t - \sum_{k=1}^{K}\sum_{m=1}^{M^k}\left(\eta_t^{km}\mathbf{1}\left(s_t^k \in \mu^{km}\right)\right) + f_t\left(\mathbf{s}, \mathbf{x}\right) + \theta_t + g_t - \tau_t\left(\mathbf{s}, \mathbf{x}\right) - \frac{1}{R}a_{t+1}$, where a_t is the level of assets at the beginning of period t, η_t^{km} is the student's price of enrollment at college k at intensity μ^{km}, indicated by the dummy $\mathbf{1}\left(s_t^k \in \mu^{km}\right)$, $f_t\left(\mathbf{s}, \mathbf{x}\right)$ is income received in the period (possibly negative), θ_t is a transfer received from the government, g_t is the consumption value of public goods, $\tau_t\left(\mathbf{s}, \mathbf{x}\right)$ is the tax paid to the government, and $R \geq 1$ is the gross interest rate.

The individual faces asset constraints $a_t \geq \underline{a_t}$, which imply that borrowing may be constrained. Such constraints can be imposed by either the market or the individual. In addition to the budget and asset constraints, we allow for $J < X - 2$ generic constraints, $g_{jt}\left(c_t + \sum_{k=1}^{K}\sum_{m=1}^{M^k}\left(\eta_t^{km}\mathbf{1}\left(s_t^k \in \mu^{km}\right)\right) - \theta_t, \mathbf{s}, \mathbf{x}\right) \geq 0$, in each period.[33] As examples, such constraints might include restrictions on the number of hours spent working, rules relating to income-based loan repayment, or the fact that certain grant programs require a minimum number of credits attempted. Lagrange multipliers on all constraints are denoted with λ_{ω_t} and the relevant superscript. Indirect utility depends on the price of schooling across colleges and years, which we denote by $\boldsymbol{\eta}$, defined analogously to the other such vectors.

The indirect utility function at time $t = 0$ is

$$V\left(a_0|\boldsymbol{\eta}\right) = \max_{\mathbf{c},\mathbf{s},\mathbf{x}}\left\{\sum_{t=0}^{T} E\left[\lambda_{\omega_t}^{c}\left(a_t - \sum_{k=1}^{K}\sum_{m=1}^{M^k}\left(\eta_t^{km}\mathbf{1}\left(s_t^k \in \mu^{km}\right)\right) + f_t\left(\mathbf{s}, \mathbf{x}\right) + \theta_t + g_t - \tau_t\left(\mathbf{s}, \mathbf{x}\right) - \frac{1}{R}a_{t+1} - c_t\right)\right]\right.$$

$$\left. + \sum_{t=0}^{T} Eu_t\left(c_t, \mathbf{s_t}, \mathbf{x_t}\right) + \sum_{t=1}^{T} E\left[\lambda_{\omega_t}^{a}\left(\underline{a_t} - a_t\right)\right] + \sum_{t=0}^{T}\sum_{j=1}^{J} E\left[\lambda_{\omega_t}^{g_j} g_{jt}\left(c_t + \sum_{k=1}^{K}\sum_{m=1}^{M^k}\left(\eta_t^{km}\mathbf{1}\left(s_t^k \in \mu^{km}\right)\right) - \theta_t, \mathbf{s}, \mathbf{x}\right)\right]\right\}$$

The first-order condition with respect to consumption in period t' for each value of $\omega_{t'}$ is

[33] This structure requires that schooling expenditures enter into the constraints everywhere that consumption expenditures enter. For example, a tax on consumption would also need to apply to spending on schooling, but spending on schooling may be subject to constraints that do not involve consumption. With this structure, the constraints satisfy the assumptions required by Chetty (2006a).

$$0 = \frac{\partial}{\partial c_{t'}} u_{t'}\left(c_{t'}, \mathbf{s_{t'}}, \mathbf{x_{t'}}\right) - \lambda^{c}_{\omega_{t'}} + \sum_{j=1}^{J} \lambda^{g_j}_{\omega_{t'}} \frac{\partial}{\partial c_{t'}} g_{jt'}. \tag{2}$$

6.2 The government's problem

The government must balance its budget in expectation (i.e., expected government revenue must equal expected tax revenue). The balanced-budget requirement allows us to consider whether a self-funded reform would enhance welfare, rather than comparing the gross benefits to the cost of various financing options, and it avoids the complication of interpersonal comparisons.[34] The government makes transfers θ_t and spends G_t on public goods, the level of which can be affected by educational investment externalities. The government must cover any difference between the social (resource) cost $\nu_t^{km}\mathbf{1}\left(s_t^k \in \mu_t^{km}\right)$ of schooling level s_t^k and the payments made by the student. This requirement is most intuitive for public institutions (as in our empirical setting), and is a reasonable assumption for private institutions. This is because in U.S. higher education, the for-profit sector is relatively small with students' tuition payments disproportionately funded through federal aid, while the nonprofit sector is publicly supported through exemption from corporate income tax and donations that alternatively could purchase other resources. Our formulation follows Andreoni (2006) and Diamond (2006) in that we exclude the utility obtained from making donations and providing nonprofit schooling from welfare analysis.

We are interested in a policy reform that reduces higher education prices through the parameters η_t^{km}. This formulation can be applied to a variety of reforms, including changes to the pricing of public institutions or changes in generosity of student grant aid, and such reforms may apply equally to all enrolled students or vary in generosity with the number of credits attempted. Grants may be portable across all colleges, as is the case for the Pell Grant, or they may only be relevant for certain institutions or sectors, as is often the case with state grants that can only be used in public institutions.

To allow for these variants, we consider a policy change that reduces a student's cost of enrolling at one or more institutions at intensity μ_t^{km}. We parameterize a multi-faceted policy using a scalar p that we define as representing units of the present-value expected cost to the government. We do so by denoting the reductions to the values of μ_t^{km} by $\Delta_t^{km} p$ and requiring that $p = \sum_{t=0}^{T} R^{T-t} \sum_{k=1}^{K} \sum_{m=1}^{M^k} \Delta_t^{km} E\left[\mathbf{1}\left(s_t^k \in \mu_t^{km}\right)\right]$.[35]

[34] Hendren (2016) provides an alternative approach to evaluate welfare that consists of calculating the marginal value of public funds (MVPF) for an uncompensated policy change for a given population and then comparing the MVPF with that of other policy reforms to determine the welfare weights that would need to be applied to different populations to justify one reform over another. For example, to justify increasing Pell Grant aid for higher income students, where estimated effects on student outcomes have been small, versus increasing Pell Grant aid for students with incomes near the automatic zero EFC threshold, Hendren's approach would produce an estimate of how much larger the welfare weights for the high income population would need to be to rationalize such a decision.

[35] As an example, suppose there is a single period, a single college, and students who are evenly split between full-time and part-time enrollment with respective tuition levels of \$2000 and \$1000. A policy to eliminate tuition for all students would have an expected cost of $1500 = .5\,(1000) + .5\,(2000)$ per student. In this example we would have $\Delta^1 = \frac{1000}{1500} \approx 0.67$, and $\Delta^2 = \frac{2000}{1500} \approx 1.33$, and $p \in [0, 1500]$,

We derive a general solution and then show how this simplifies in the case of additional Pell Grant aid received by automatic zero EFC eligible students.[36]

We require that the reform maintain budget neutrality and a fixed level of public goods to capture the full welfare effect of both receiving and paying for any change in schooling expenses. For notational simplicity, we consider a policy that offsets changes to expected education costs by adjusting the final transfer θ_T; we will note how the resulting formula for welfare effects would differ if instead the government spread any offsets to transfers over multiple years.[37]

The government's budget constraint is

$$\theta_T = \sum_{t=0}^{T} R^{T-t} \left(E \left[\tau_t(\mathbf{s}, \mathbf{x}) - \theta_t - G_t - \sum_{k=1}^{K} \sum_{m=1}^{M^k} \left(\nu_t^{km} - \eta_t^{km} \right) \mathbf{1} \left(s_t^k \in \mu_t^{km} \right) \right] \right).$$

Differentiating the budget constraint with respect to the school-price-reducing parameter p yields

$$\frac{d\theta}{dp} = -R^T p + \sum_{t=0}^{T} R^{T-t} E \left[\frac{d}{dp} \tau_t(\mathbf{s}, \mathbf{x}) - \frac{d}{dp_t} G_t - \sum_{k=1}^{K} \sum_{m=1}^{M^k} \left(\nu_t^{km} - \eta_t^{km} \right) \frac{d}{dp} \Pr \left(s_t^k \in \mu_t^{km} \right) \right] \tag{3}$$

Equation (3) represents the changes in future transfers needed to offset any budgetary impacts of changing a student's cost of schooling. The first term captures the direct impact on the government's budget – transfers to inframarginal students – which will depend on the amount of educational investment at affected schools under the status quo. The remaining terms capture the effects of behavioral responses, which may occur in any year. For example, the reform might increase schooling and reduce tax payments in one year but increase tax revenues and the level of public goods in later years. Induced changes in schooling will affect the government's budget if there is a difference between the private and social costs of educational investments. For example, students may be induced to switch between colleges that differ in the degree to which the colleges are subsidized, or students may increase the number of credits they attempt if grant generosity increases with enrollment intensity.[38]

The government maximizes the individual's indirect utility. By invoking the Envelope Theorem (see Online Appendix D), we can ignore behavioral responses other than those that arise through the government budget constraint. The welfare effect of reducing prices through parameter p (and offsetting the budgetary

so that a one-unit increase in p reduces μ^1 by \$0.67 and μ^2 by \$1.33, yielding an expected cost to the government of \$1.

[36] We do not model general equilibrium effects, such as congestion at schools that students are induced to attend. A marginal change in prices should only generate a marginal change in enrollments and so would have limited external effects.

[37] Paying for the policy by adjusting future transfers is akin to providing students with loans. If instead the transfer was funded by increasing income taxes (as in Lawson (2017)), then the distortions caused by these taxes would be reflected in the behavioral responses and in negative multiples of taxed incomes in the derivative of the welfare function with respect to the policy.

[38] At the student level, there may be discrete changes in institution attended, which would create points of nondifferentiability, but continuity of the distribution of student characteristics ensures differentiability and gives the change in the probability of enrollment (over states of the world) as the derivative.

effects through transfers in period T) is $\frac{dV(a_0|\boldsymbol{\eta})}{dp} = -\sum_{t=0}^{T} E\left[\left(\sum_{j=1}^{J} \lambda_{\omega_t}^{g_j} \frac{\partial}{\partial c_0} g_{jt} - \lambda_{\omega_t}^{c}\right) \sum_{k=1}^{K}\sum_{m=1}^{M^k} \Delta_t^{km} \mathbf{1}\left(s_t^k \in \mu^{km}\right)\right] + \frac{d\theta}{dp} E\left[\left(\lambda_{\omega_T}^{c} - \sum_{j=1}^{J} \lambda_{\omega_T}^{g_j} \frac{\partial}{\partial c_0} g_{jT}\right)\right]$. Using the first-order condition for consumption in equation (2), substituting in the expression from the budget constraint in equation (3), and normalizing by expected marginal utility at time T yields

$$\frac{dV(a_0|\boldsymbol{\eta})/dp}{R^T E\left[\frac{\partial}{\partial c_T} u_T\right]} = \left\{ \sum_{t=0}^{T} E\left[\left(\sum_{k=1}^{K}\sum_{m=1}^{M^k} \Delta_t^{km} \mathbf{1}\left(s_t^k \in \mu^{km}\right)\right)\left(\frac{\frac{\partial}{\partial c_0} u_0}{R^T E\left[\frac{\partial}{\partial c_T} u_T\right]} - 1\right)\right] \right.$$
$$\left. + \sum_{t=0}^{T} R^{-t} E\left[\frac{d}{dp}\tau_t\left(\mathbf{s}, \mathbf{x}\right) - \frac{d}{dp_t} G_t + \sum_{k=1}^{K}\sum_{m=1}^{M^k} \left(\eta_t^{km} - \nu_t^{km}\right) \frac{d}{dp} \Pr\left(s_t^k \in \mu^{km}\right)\right] \right\}. \tag{4}$$

Equation (4) illustrates the welfare effects of a marginal change in the price of school. The first expectation captures direct utility effects. The dollars that a student receives in school are transfers that will have to be paid back in the future. Multiplying the current value of the in-school transfer by marginal utility in the year of receipt or in a future period provides the relative utility value of those dollars at each point in time. If $s_t = 0$, then the entire term in period t equals zero: a change in the price of schooling for any particular period only directly affects students who would be enrolled with some probability in that period. Second, if there is no uncertainty, and if the individual's asset and other constraints never bind, then the first-order conditions for future assets imply that $\frac{\partial}{\partial c_t} u_t = R^{T-t} \frac{\partial}{\partial c_T} u_T$, another case in which the entire term is equal to zero. Because the policy is a transfer from one year to another, an unconstrained individual has already adjusted borrowing and saving so as to be indifferent to the timing of a marginal dollar. Thus, lowering the cost of schooling will only have a direct benefit to students who face constraints that elevate their in-school marginal utility of consumption.

The remaining sum in equation (4) captures the value of externalities generated by behavioral responses to the policy change. These include both traditional externalities on the level of public goods and fiscal externalities on taxes and public spending on education. Net externalities theoretically could be either positive or negative and may arise through changes in behavior in years other than the year of policy change (e.g., if changing the cost of schooling in one year affects government-subsidized schooling investment for multiple years). For our expositionally-simple policy, these effects are all incorporated into period-T transfers and therefore valued according to the marginal utility in that period. If instead, the fiscal externalities were spread over multiple periods, then the change in each year's transfer would be multiplied by marginal utility in that year.

7 Welfare Evaluation

In our empirical setting, the general welfare formula in equation (4) can be simplified. First, the amount of grant aid received is affected directly in only one year and does not depend on credits attempted as long as the student attempts at least 12 credits per semester. The vast majority of students in the sample attempt sufficient credits, and we find economically and statistically insignificant effects of the grant on the probability of attempting 12 credits per semester. Second, we consider current students at the automatic zero EFC eligibility threshold. Together, these assumptions imply that $\Delta_t^{km} = 1$ for the student's actual college and enrollment intensity, simplifying the expression for the direct welfare effect. Third, we assume away non-fiscal externalities, for which we do not have estimates. Assuming such externalities are positive on net, as prior literature would suggest (e.g., Lochner 2011), this simplification works against finding that increasing grants would increase welfare. The resulting simplified condition for a welfare gain is:

$$\frac{dV(a_0|\boldsymbol{\eta})/dp}{R^T E\left[\frac{\partial}{\partial c_T} u_T\right]} = \left\{ \sum_{t=0}^{T} E\left[\frac{\frac{\partial}{\partial c_t} u_t}{R^T E\left[\frac{\partial}{\partial c_T} u_T\right]} - R^{-t}\right] + \sum_{t=0}^{T} R^{-t} E\left[\frac{d}{dp}\tau_t\left(\mathbf{s},\mathbf{x}\right) - \sum_{k=1}^{K}\sum_{m=1}^{M^k}\left(\eta_t^{km} - \nu_t^{km}\right)\frac{d}{dp}\Pr\left(s_t^k \in \mu^{km}\right)\right]\right\} \tag{5}$$

This derivative measures the welfare gain or loss from reducing prices for current students. If we assume that $\frac{\partial}{\partial c_{t'}} u_t \geq R^{T-t'} E\left[\frac{\partial}{\partial c_T} u_T\right]$, as would be the case if only intertemporal budget constraints bind, then it is sufficient to show that net externalities of the increase in grant aid are positive. Showing that this holds for fiscal externalities alone will be sufficient if nonfiscal externalities are non-negative.

7.1 Indirect effects

Since equation (5) shows that a revenue-neutral marginal increase in grant aid increases welfare if net externalities are positive and students do not over-consume in college, we assess the net fiscal externalities of the additional grant aid provided to students who qualify for an automatic zero EFC. The change in generosity for these students is not a marginal change but gives a linear approximation of the marginal effect, as is standard in the sufficient statistic literature (Chetty 2009). Estimating the marginal effect of increasing grant aid from any specific baseline level would require parametric assumptions. Our estimate is the slope of a line between the two points we observe on the social welfare function, a more relevant quantity than the slope of the tangent at either of these points, which would give the welfare effects of giving one dollar to compliers who are ineligible or who are eligible, respectively. Moreover, between the two observed levels of generosity there will necessarily be a level at which the marginal welfare effect is equal to the effect we estimate.

To measure program costs, we take into account total cash flows between students and the public sector, abstracting from issues related to transfers between the federal government, state government, and public educational institutions. An advantage of our setting is that we observe all grants that students receive, whether from the Pell Grant program or other sources. As such, we are able to directly estimate the effect of automatic zero eligibility on grant aid received over most students' full college careers. Effects on total grant aid represent one of the two largest fiscal externalities generated by behavioral responses to automatic zero EFC eligibility.

Table 7 shows the effect of eligibility for an automatic zero EFC on total financial aid flows over seven years, a proxy that likely captures most of the lifetime effects on financial aid receipt. The $711 increase in grants received by eligible students at college entry generates a $1310 increase in total grant aid received over the duration of our panel, a period over which the majority of students have completed a degree or dropped out of college. Because automatic zero eligibility only mechanically affects grant aid in a student's first year, the increase in cumulative grant aid beyond the initial $711 is likely generated by increased persistence and the increase in the probability of qualifying for a TEXAS Grant in later years. Impacts on cumulative borrowing are small and statistically insignificant. The significant reduction in first-year borrowing among eligible students is offset by increased borrowing in later years. We also find no evidence of statistically significant effects on years of attendance or expenditures on direct subsidies to public institutions attended by eligible students; point estimates are small, negative, and statistically insignificant.[39]

Public revenue arising from the incremental grant aid exceeds the costs under relatively weak assumptions. Summing over the estimated effects of initial eligibility for an automatic zero EFC on federal income tax liabilities suggests that an additional $707 in federal tax revenue was generated in the seven years following college entry (Table 7).[40] To recover the remaining cost of expenditures generated by initial eligibility requires $603. We cannot say whether the earnings effects (and resulting income tax gains) will continue to grow over the long-run, but if earnings gains remain at the level we observe at the end of our panel, the additional grant will be fully recouped in nine years and will continue to produce additional revenue for many years to come. The effect on subsequent years' earnings could be two-thirds as large, and the grant would pay for itself within ten years. Earnings gains need not even persist if we include the $721 increase in FICA taxes collected.[41]

According to the sufficient condition in equation (5), increasing grant aid generosity would be welfare improving in our setting. Of the $1310 increase in public expenditures, $711 is due to the initial difference in grant aid, which represents a transfer and does not enter directly into equation (5). The behavioral effect

[39] Following Hoxby (forthcoming), we approximate the value of the direct subsidy provided to a given institution using data from the IPEDS and calculating average student-year expenditures on "core expenses" in excess of tuition payments.

[40] Estimated tax liabilities are measured in real terms. No additional discounting is applied.

[41] FICA taxes increase current revenue but also may increase future liabilities, as they are tied to workers' future retirement and health insurance benefits.

on subsidies received by the student is \$0.84 per dollar of initial aid.[42] The behavioral effect on income taxes paid by the student within seven years is \$0.99, giving a net externality of \$0.15 over the first seven years. If students work an additional 30 years and pay an additional \$707 in income taxes every 3 years then the net externality per dollar of initial aid rises to \$9.95. Thus, even if we ignore the private return on the educational investment (which appears to be large) and any nonfiscal externalities (which we expect to be positive on net), our results indicate that such transfers raise welfare if direct effects are positive or reasonably small.

7.2 Direct effects

The first sum in equation (5) captures direct welfare effects that arise when marginal utility in college (when the grant is received) differs from marginal utility in later years (when transfers are adjusted to satisfy the government's budget constraint). Marginal utility should increase after college if constraints prevent students from fully smoothing consumption. Our model shows that we do not need to know the underlying cause of any failures to smooth consumption because the ratio of marginal utilities within and after college is a sufficient statistic for the direct welfare effects of grant aid.

We measure consumption at different ages for current and former college students using data from the Consumer Expenditure Survey (CEX) 2011 through 2015 interview panels. We focus on two groups: current college students aged 18-24 and former students who have completed at least some college that are 25 to 30 years old.[43] By restricting our attention to this relatively young group, instead of individuals at peak earnings ages, we will likely generate an underestimate of the welfare gains of grant aid that moves income from later to earlier periods. Following Meyer and Sullivan (2011), we define consumption to include all expenditures except educational investments, medical expenses, charitable contributions, and retirement savings. We also exclude home production expenditures and use the expected rental value reported by homeowners as a proxy for housing consumption in place of expenditures on mortgage payments, interest, and home maintenance and repairs.[44] We adjust consumption to account for household size.[45]

In the case of a one-time increase in grant aid provided to first-time college freshmen, the direct wel-

[42] Ideally, we would also treat the portion of the increase in TEXAS Grant aid in later years that was due to awarding more TEXAS grants as part of the policy and only include expenditures induced by enrollment responses of these new recipients in the behavioral externality. Automatic zero eligibility leads to a marginally significant ($p<0.10$) 2 percentage point increase in the probability of receiving a TEXAS Grant in a student's first year. Because assignment of the TEXAS Grant for students near the automatic zero EFC threshold does not follow a simple formula, we take the conservative approach of treating all expenses after the first year as externalities.

[43] We exclude students with advanced degrees. Approximately 52 percent of the remaining sample has a bachelor's degree; the remainder have an associate degree, other credential, or left college without receiving a credential.

[44] Mean and median consumption, expenditures, and household AGI for 18-24 year old students and former students within 5-year age groups are reported in Online Appendix Table C.13.

[45] Following Meyer and Sullivan (2011), adjusted household consumption equals annual household consumption divided by $(adults + 0.7 * children)^{0.07}$.

fare effect in equation (5) simplifies to $\left(\frac{\frac{\partial}{\partial c_0}u_0}{R^T E\left[\frac{\partial}{\partial c_T}u_T\right]} - 1\right)$. Calibration is straightforward with the constant relative risk aversion (CRRA) utility function. If utility exhibits CRRA with risk aversion parameter γ and discount rate β, marginal utility is $\beta^t c_t^{-\gamma}$. We allow for uncertainty by calculating the average marginal utility of consumption (rather than the marginal utility of average consumption) for a given level of risk aversion. Online Appendix Table C.14 contains estimated mean marginal utility of consumption in the college student population (Panel A) and the post-college population (Panel B).

Expected future marginal utility may vary across students, and is likely positively correlated with current marginal utility, but this correlation cannot be observed without panel data on consumption. We therefore calculate upper and lower and bounds for this ratio by assuming zero and perfect correlation, respectively. For the upper bound we calculate $\left(\frac{\frac{1}{N_{coll}}\sum c_{coll}^{-\gamma}}{R\beta^t\left(\frac{1}{N_{postcoll}}\sum c_{postcoll}^{-\gamma}\right)}\right) - 1$, where $\frac{1}{N_{coll}}\sum c_{coll}^{-\gamma}$ is the average marginal utility of consumption for the college population, $\frac{1}{N_{postcoll}}\sum c_{postcoll}^{-\gamma}$ is average marginal utility for the college-educated in the next age group (25-30 years old), and t is the difference in average age between the two groups (6 years). To calculate the lower bound, we separate both the student and post-college samples into centiles of consumption, calculate the same statistic for each centile (effectively assuming no mobility or uncertainty), and average over centiles.

We present welfare calculations for multiple values of the CRRA parameter γ, centered around $\gamma = 2$, the baseline chosen by Carroll (1997), Chetty (2006b), and Card et al. (2007). Welfare gains are increasing in the degree of risk aversion; if an individual's utility function is highly curved, then marginal utility will be much greater when consumption is held at a below-optimal value. We also allow the net discount rate $R\beta$ to vary. With $R\beta = 1$, we would expect a constant level of consumption over time, which is consistent with the behavior of the college-educated population in the CEX between the ages of 46 and 65 (Online Appendix Table C.13). When $R\beta > 1$, individuals are patient and prefer to consume more in the future, which reduces the value of additional consumption while in college. The converse applies when $R\beta < 1$.

Table 8 presents estimated upper and lower bounds for the direct effect of grant aid on welfare for $\gamma \in \{1, 2, 3\}$ and $R\beta \in \{0.97, 1, 1.03\}$. We estimate the direct welfare gains for students from low- and high-income families to account for variation in the targeting of grant aid.[46] For low-income students like those in our empirical setting, direct welfare gains are positive and sizable for most parameter values, ranging from \$0.40 to \$1.94 per dollar increase in grant aid when $\gamma = 2$. For most parameter values, average and high-income students also receive fairly substantial, direct welfare gains from additional grant aid. With

[46]We adjust college-age consumption for low-income students to mimic the fact that those in our sample come from families with incomes around \$20,000. Specifically, we scale by the ratio of average consumption among students from families with AGIs between \$10,000 and \$30,000 to average consumption among all students (0.81). Likewise, to approximate the high-income college student population, we use the ratio of average consumption of students from families with AGIs between \$50,000 and \$85,000 to average consumption of all students as the adjustment factor (1.10).

$\gamma = 2$ and $R\beta = 1$, the direct utility gain for the average college student exceeds \$0.50 per \$1 of grant aid, and effects are at least 30 percent larger for students with family income resembling that of our Texas sample.[47] Thus, direct effects of increased grant aid on welfare appear to be strongly positive. Since we also find positive indirect effects, we conclude that total welfare effects of increased grant aid for low-income students in Texas are positive.

7.3 Welfare evaluation in other settings

The welfare effects of changing college prices depend on two components: direct effects for students who are unable to smooth consumption and indirect effects arising from the externalities of behavioral responses. To illustrate how our framework could be applied to other settings, we perform rough calculations of the welfare effects of grant aid programs studied by Marx and Turner (forthcoming) and Bettinger et al. (2016), in both cases relying on credible causal estimates generated by regression discontinuity designs. Details appear in Online Appendix E.

Marx and Turner (forthcoming) study the effect of additional grant aid provided to City University of New York (CUNY) students at the Pell Grant program's eligibility threshold and find no evidence that the additional Pell Grant aid affects college entry, choice, or attainment. However, additional Pell Grant aid generated significant reductions in borrowing. In expectation, borrowers will default on some portion of their federal student loan debt. As a result, the behavioral response to Pell Grant eligibility should increase welfare by reducing social expenditures via reductions in unpaid student loans. The total effect on welfare is positive even using the lowest estimate of direct benefits.

In our second example, Bettinger et al. (2016) examine the short- and long-run effects of the Cal Grant Program on college choice, degree receipt, and earnings. The program covers tuition and fees at four-year public institutions and heavily subsidizes tuition at four-year nonprofit schools. Cal Grant eligibility requirements generate two discontinuities based on income and high school GPA. We treat each threshold as a separate policy for the purpose of welfare analysis because estimated local average treatment effects and characteristics of grant aid recipients differ between thresholds.

The income threshold induced a change in Cal Grant eligibility for families with average income of \$60,500. At this threshold, eligibility did not affect college enrollment or years of college attendance but did increase the probability of attending a private four-year college. Impacts on earnings and AGI 10 to 14 years

[47] Jacob et al. (forthcoming) show that, in addition to academic spending, four-year institutions spend a considerable amount on what might be considered consumption amenities. To the extent that the consumption value of college is captured by the cost of on-campus room and board, it will be accounted for in our measure of consumption. However, institutional spending on student activities such as athletics will not be captured. As reported in Jacob et al. (forthcoming), the average public four-year institution charged \$5490 in tuition and provided \$3602 in consumption amenities in 2004, a ratio of 0.66. We impute the consumption value of college using this ratio and the expenditures on higher education reported in the CEX and report results in Online Appendix Table C.15. Estimate direct welfare effects remain similar to those reported in Table 8.

after college entry are small, negative, and statistically insignificant. Indirect effects are limited to changes in the social cost of educating students who were induced to switch between higher education sectors. Switches from public into nonprofit schools generate two fiscal externalities: changes in the public subsidy to the institution attended and increases in the amount of Cal Grant payments relative to what would have been paid if students did not switch, as the grant is larger for students in private four-year institutions. Both externalities are negative, and with small direct effects for these students from relatively high-income families, we conclude that the welfare effects of increasing grant aid at this margin are negative, on the order of -\$0.14 to -\$0.31 per \$1 increase in grant generosity.

At the GPA eligibility threshold, Bettinger et al. (2016) find no effect on enrollment or sector of institution attended. Eligibility marginally increased in years of attendance, significantly increased earnings, and generated insignificant AGI increases. Indirect welfare effects come from changes in social costs due to additional years of college attendance and increases in tax revenue paid by eligible students who experience earnings gains. We calculate that the net of these fiscal externalities is positive and equal to roughly \$0.30 per \$1 of Cal Grant aid. This calculation relies on a statistically-insignificant effect on AGI, but the 95-percent confidence interval excludes the negative effects that would be required to offset the direct benefits for students, which are large at this threshold because average family incomes are low. The two Cal Grant thresholds show that the increasing the generosity of a given grant can have positive welfare effects for some potential recipients and negative effects for others.

8 Conclusion

Using student-level administrative data from Texas and a regression discontinuity design, we show that eligibility for additional grant aid at college entry - approximately \$700, on average - substantially increases poor students' postsecondary attainment and earnings. Eligibility likely generates earnings gains through a combination of an increased likelihood of degree completion and/or decreased time to degree, and by shifting students into STEM degree receipt. Impacts on graduation and earnings remain positive for the duration of our panel, seven years after college entry, and the estimated increase in federal tax payments is large enough that government expenditures on grant aid are fully recouped within ten years.

Economists have studied a variety of grant programs for higher education and estimated a range of attainment effects. We present a general model that generates sufficient statistics for evaluating the welfare effects of changes in the price of schooling. These statistics include a direct consumption smoothing effect, which can be estimated from marginal utilities without knowledge about potential underlying frictions, and an indirect effect of externalities generated by behavioral responses to aid. We apply our framework and estimated direct effects to multiple settings and find that while welfare effects cannot be inferred from examining attainment outcomes in isolation, grants that target low-income students are the most likely to increase welfare. The benefits to students at the automatic zero threshold for Pell Grant aid are substantial, and increasing support for these students pays for itself through financial gains for the public.

References

Andreoni, James, "Philanthropy," in S. Kolm and Jean Mercier Ythier, eds., *Handbook on the Economics of Giving, Reciprocity and Altruism*, Vol. 1, Elsevier, 2006, chapter 18, pp. 1201–1269.

Andrews, Rodney J., Jing Li, and Michael F. Lovenheim, "Quantile Treatment Effects of College Quality on Earnings," *Journal of Human Resources*, 2016, *51* (1), 200–238.

Angrist, Joshua D and Guido W Imbens, "Two-stage least squares estimation of average causal effects in models with variable treatment intensity," *Journal of the American statistical Association*, 1995, *90* (430), 431–442.

Barreca, Alan I, Jason M Lindo, and Glen R Waddell, "Heaping-Induced Bias in Regression-Discontinuity Designs," *Economic Inquiry*, 2016, *54* (1), 268–293.

Barrow, Lisa and Ofer Malamud, "Is College a Worthwhile Investment?," *Annual Review of Economics*, 2015, *7*, 519–555.

Baum, Sandy, Jennifer Ma, Matea Pender, and Meredith Welch, "Trends in Student Aid," 2016. The College Board.

Belzil, Christian, Arnaud Maurel, and Modibo Sidibé, "Estimating the Value of Higher Education Financial Aid: Evidence from a Field Experiment," 2017. NBER Working Paper No. 23641.

Bettinger, Eric and Betsy Williams, "Federal and State Financial Aid during the Great Recession," in Jeffrey R. Brown and Caroline M. Hoxby, eds., *How the Financial Crisis and Great Recession Affected Higher Education*, Chicago, IL: University of Chicago Press, 2015.

__, Oded Gurantz, Laura Kawano, and Bruce Sacerdote, "The Long Run Impacts of Merit Aid: Evidence from California's Cal Grant," 2016. NBER Working Paper No. 22347.

Black, Dan A. and Jeffrey A. Smith, "Estimating the Returns to College Quality with Multiple Proxies for Quality," *Journal of Labor Economics*, 2006, *24* (3), 701–728.

Boatman, Angela, Brent J. Evans, and Adela Soliz, "Understanding Loan Aversion in Education: Evidence from High School Seniors, Community College Students, and Adults," *AERA Open*, 2017, *3* (1), 1–16.

Bound, John, Michael F. Lovenheim, and Sarah Turner, "Increasing Time to Baccalaureate Degree in the United States," *Education Finance and Policy*, 2012, *7* (4), 375–424.

Cadena, Brian C. and Benjamin J. Keys, "Can Self-Control Explain Avoiding Free Money? Evidence from Interest-Free Student Loans," *Review of Economics and Statistics*, 2013, *95* (4), 1117–1129.

Cameron, Stephen V. and Christopher Taber, "Estimaton of Educational Borrowing Constraints Using Returns to Schooling," *Journal of Political Economy*, 2004, *112* (1), 132–182.

Card, David, Raj Chetty, and Andrea Weber, "Cash-on-Hand and Competing Models of Intertemporal Behavior: New Evidence from the Labor Market," *Quarterly Journal of Economics*, 2007, *122* (4), 1511–1560.

Carroll, Christopher, "Buffer-Stock Saving and the Life Cycle/Permanent Income Hypothesis," *Quarterly Journal of Economics*, 1997, *112* (1), 1–56.

Carruthers, Celeste K. and Jilleah G. Welch, "Not Whether, but Where? Pell Grants and College Choices," 2015. Working paper.

Castleman, Benjamin L. and Bridget Terry Long, "Looking Beyond Enrollment: The Causal Effect of Need-based Grants on College Access, Persistence, and Graduation," *Journal of Labor Economics*, 2016, *34* (4), 1023–1073.

Chetty, Raj, "A General Formula for the Optimal Level of Social Insurance," *Journal of Public Economics*, 2006, *90*, 1879–1901.

— , "A New Method of Estimating Risk Aversion," *American Economic Review*, 2006, *96* (5), 1821–1834.

— , "Sufficient Statistics for Welfare Analysis: A Bridge Between Structural and Reduced-Form Methods," *Annual Review of Economics*, 2009, *1*, 451–488.

Cohen, Jeffrey, Lindsay Fox, Jeff Kutash, and Mukta Pandit, "Dollars for Degrees: Financial Aid and its Impact on Post-secondary Degree Completion in Texas," 2010. FSG Social Impact Advisors report.

Cohodes, Sarah R. and Joshua S. Goodman, "Merit Aid, College Quality, and College Completion: Massachusetts' Adams Scholarship as an In-Kind Subsidy," *American Economic Journal: Applied Economics*, 2014, *6* (4), 251–285.

Deming, David and Susan Dynarski, "Into College, Out of Poverty? Policies to increase Postsecondary Attainment of the Poor," in Phillip Levine and David Zimmerman, eds., *Targeting Investments in Children: Fighting Poverty When Resources are Limited*, The University of Chicago Press, 2010, pp. 283–302.

Denning, Jeffrey T., "Born Under a Lucky Star: Financial Aid, College Completion, Labor Supply, and Credit Constraints," 2017. IZA Discussion Paper No. 10913.

— **and Patrick Turley**, "Was that Smart? Institutional Financial Incentives and Field of Study," *Journal of Human Resources*, 2017, *52* (1), 152–186.

Diamond, Peter, "Optimal Tax Treatment of Private Contributions for Public Goods With and Without Warm Glow Preferences," *Journal of Public Economics*, 2006, *90*, 897–919.

Evans, Brent J., "SMART Money: Do Financial Incentives Encourage College Students to Study Science?," *Education Finance and Policy*, 2017, *12*, 342–368.

Feenberg, Daniel Richard and Elizabeth Coutts, "An Introduction to the TAXSIM Model," *Journal of Policy Analysis and Management*, 1993, *12* (1), 189–194.

Findeisen, Sebastian and Dominik Sachs, "Optimal Financial Aid Policies for Students," 2016. Society for Economic Dynamics 2016 Meeting Papers 1421.

Fitzpatrick, Maria D. and Damon Jones, "Post-Baccalaureate Migration and Merit-Based Scholarships," *Economics of Education Review*, 2016, *54*, 155–172.

Flood, Sarah, Miriam King, Steven Ruggles, and J. Robert Warren, "Integrated Public Use Microdata Series, Current Population Survey: Version 4.0," 2015. Minneapolis: University of Minnesota.

Flores-Lagunes, Alfonso and Audrey Light, "Interpreting Degree Effects in the Returns to Education," *Journal of Human Resources*, 2010, *45* (2), 439–467.

Goldrick-Rab, Sara, Douglas N. Harris, Robert Kelchen, and James Benson, "Reducing Income Inequality in Educational Attainment: Experimental Evidence on the Impact of Financial Aid on College Completion," *American Journal of Sociology*, 2016, *121* (6), 1762–1817.

Hahn, Jinyong, Petra Todd, and Wilbert Van der Klauuw, "Identification and Estimation of Treatment Effects with a Regression-Discontinuity Design," *Econometrica*, 2001, *69* (1), 201–209.

Hendren, Nathaniel, "The Policy Elasticity," *Tax Policy and the Economy*, 2016, *30* (1), 51–89.

Hoekstra, Mark, "The Effect of Attending the Flagship State University on Earnings: A Discontinuity-Based Approach," *Review of Economics and Statistics*, 2009, *91* (4), 717–724.

Hoxby, Caroline M., "The Productivity of U.S. Postsecondary Institutions," in Caroline M. Hoxby and Kevin Stange, eds., *Productivity in Higher Education*, Chicago, IL: University of Chicago Press, forthcoming.

Imbens, Guido and Karthik Kalyanaraman, "Optimal Bandwidth Choice for the Regression Discontinuity Estimator," *Review of Economic Studies*, 2012, *79* (3), 933–959.

Jacob, Brian, Brian McCall, and Kevin Stange, "College as Country Club: Do Colleges Cater to Students' Preferences for Consumption?," *Journal of Labor Economics*, forthcoming.

Johnson, Matthew, "Borrowing Constraints, College Enrollment, and Delayed Entry," *Journal of Labor Economics*, 2013, *31* (4), 669–725.

Kane, Thomas J., "Rising Public College Tuition and College Entry: How Well Do Public Subsidies Promote Access to College?," 1995. NBER Working Paper No. 5164.

Keane, Michael P. and Kenneth I. Wolpin, "The Effect of Parental Transfers and Borrowing Constraints on Educational Attainment," *International Economic Review*, 2001, *42* (4), 1051–1103.

Lawson, Nicholas, "Liquidity Constraints, Fiscal Externalities, and Optimal Tuition Subsidies," *American Economic Journal: Economic Policy*, 2017, *9* (4), 313–343.

Lee, David S., "Training, Wages, and Sample Selection: Estimating Sharp Bounds on Treatment Effects," *Review of Economic Studies*, 2009, *76* (3), 1071–1102.

— **and Thomas Lemieux**, "Regression Discontinuity Designs in Economics," *Journal of Economic Literature*, 2010, *48* (2), 281–355.

Lochner, Lance, "Nonproduction Benefits of Education: Crime, Health, and Good Citizenship," in Eric A. Hanushek, Stephen Machin, and Ludger Woessmann, eds., *Handbook of The Economics of Education*, Vol. 4, Elsevier, 2011, chapter 2, pp. 183–282.

— **and Alexander Monge-Naranjo**, "Credit Constraints in Education," *Annual Review of Economics*, 2012, *4*, 225–256.

Lochner, Lance J. and Alexander Monge-Naranjo, "The Nature of Credit Constraints and Human Capital," *American Economic Review*, 2011, *101* (6), 2487–2529.

Malamud, Ofer and Abigail Wozniak, "The Impact of College Education on Geographic Mobility," *Journal of Human Resources*, 2012, *47* (4), 913–950.

Marx, Benjamin M. and Lesley J. Turner, "Student Loan Nudges: Experimental Evidence on Borrowing and Educational Attainment," *NBER Working Paper No. 24060*, 2017.

— **and** — , "Borrowing Trouble? Student Loans, the Cost of Borrowing, and Implications for the Effectiveness of Need-Based Grant Aid," *American Economic Journal: Applied Economics*, forthcoming.

McCrary, Justin, "Manipulation of the Running Variable in the Regression Discontinuity Design: A Density Test," *Journal of Econometrics*, 2008, *142* (2), 698–714.

Meyer, Bruce D. and James X. Sullivan, "The Material Well-Being of the Poor and the Middle Class Since 1980," 2011. AEI Working Paper 2011-04.

National Center for Education Statistics, "Digest of Education Statistics," 2016. U.S. Department of Education.

Rothstein, Jesse and Cecilia Elena Rouse, "Constrained After College: Student Loans and Early Career Occupational Choices," *Journal of Public Economics*, 2011, *95*, 149–163.

Scott-Clayton, Judith, "On Money and Motivation: A Quasi-Experimental Analysis of Financial Incentives for College Achievement," *Journal of Human Resources*, 2011, *46* (3), 614–646.

— **and Basit Zafar**, "Financial Aid, Debt Management, and Socioeconomic Outcomes: Post-College Effects of Merit-Based Aid," 2016. NBER Working Paper No. 22574.

Seftor, Neil S. and Sarah E. Turner, "Back to School: Federal Student Aid Policy and Adult College Enrollment," *Journal of Labor Economics*, 2002, *109* (5), 336–352.

Stantcheva, Stefanie, "Optimal Taxation and Human Capital Policies over the Life Cycle," *Journal of Political Economy*, forthcoming.

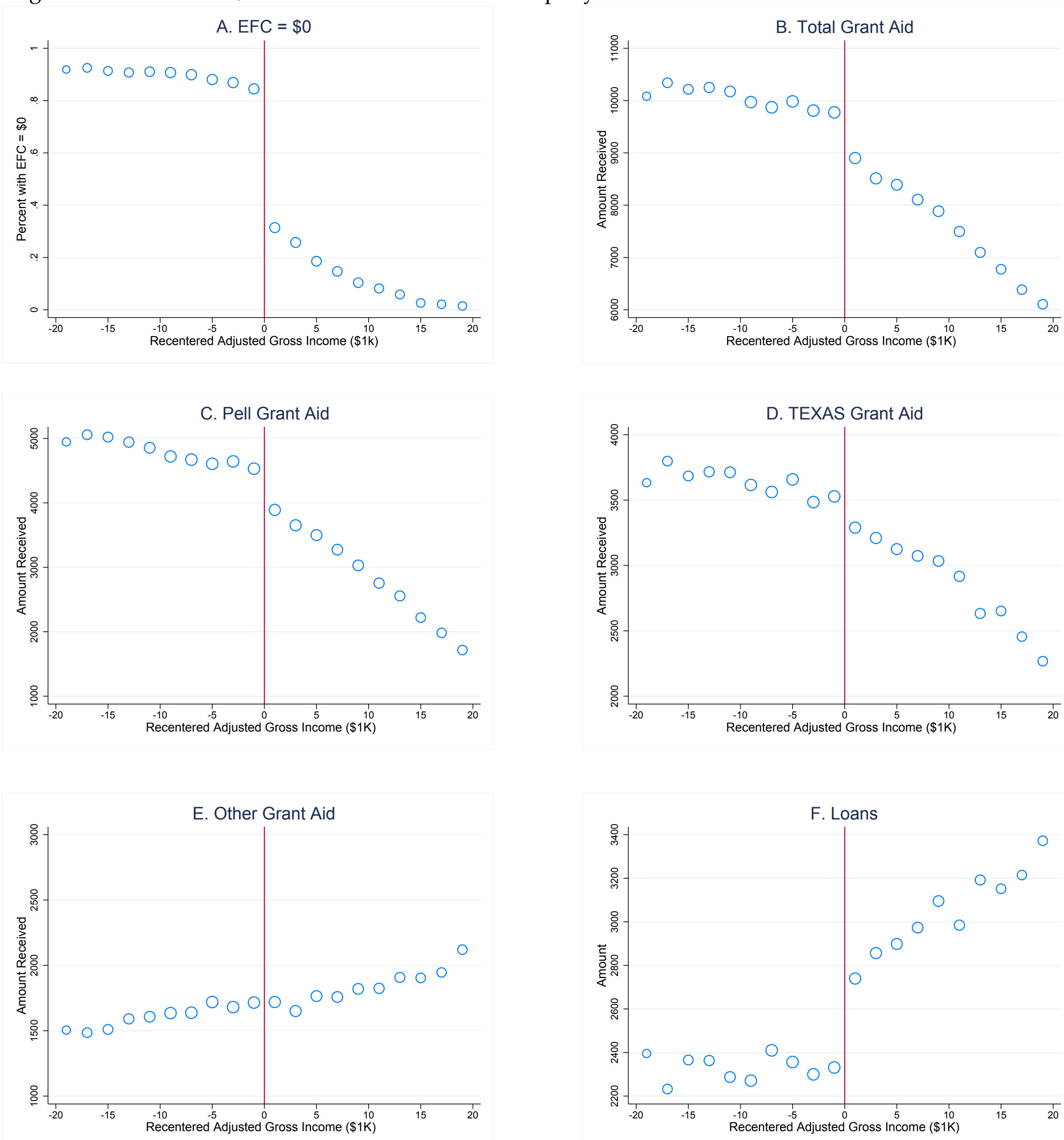
Stinebrickner, Ralph and Todd Stinebrickner, "The Effect of Credit Constraints on the College Drop-Out Decision: A Direct Approach Using a New Panel Study," *American Economic Review*, 2008, *98* (5), 2163–2184.

Turner, Lesley J., "The Economic Incidence of Federal Student Grant Aid," 2017. Working paper.

Zimmerman, Seth D., "The Returns to College Admission for Academically Marginal Students," *Journal of Labor Economics*, 2014, *32* (4), 711–754.

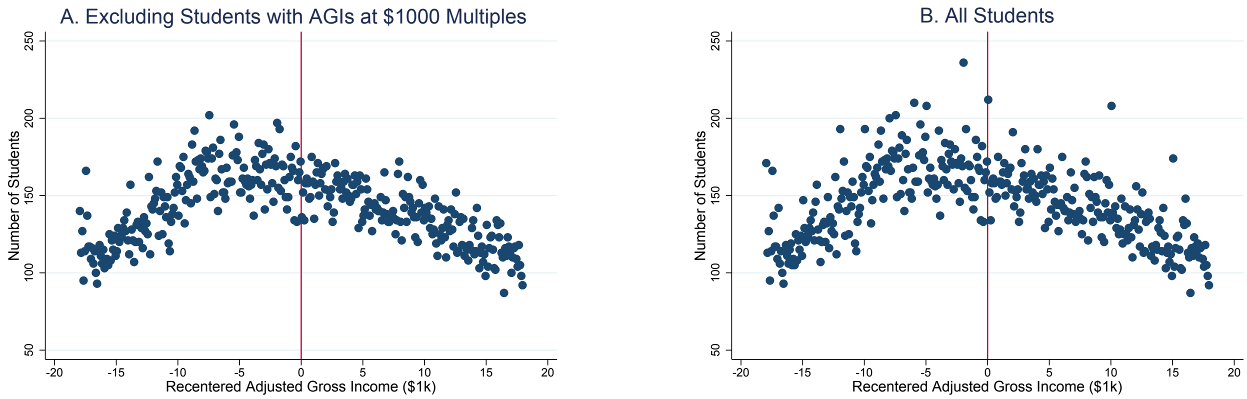
Figures and Tables

Figure 1: Percent with $0 EFC and Financial Aid Receipt by Distance to the Automatic Zero EFC Threshold



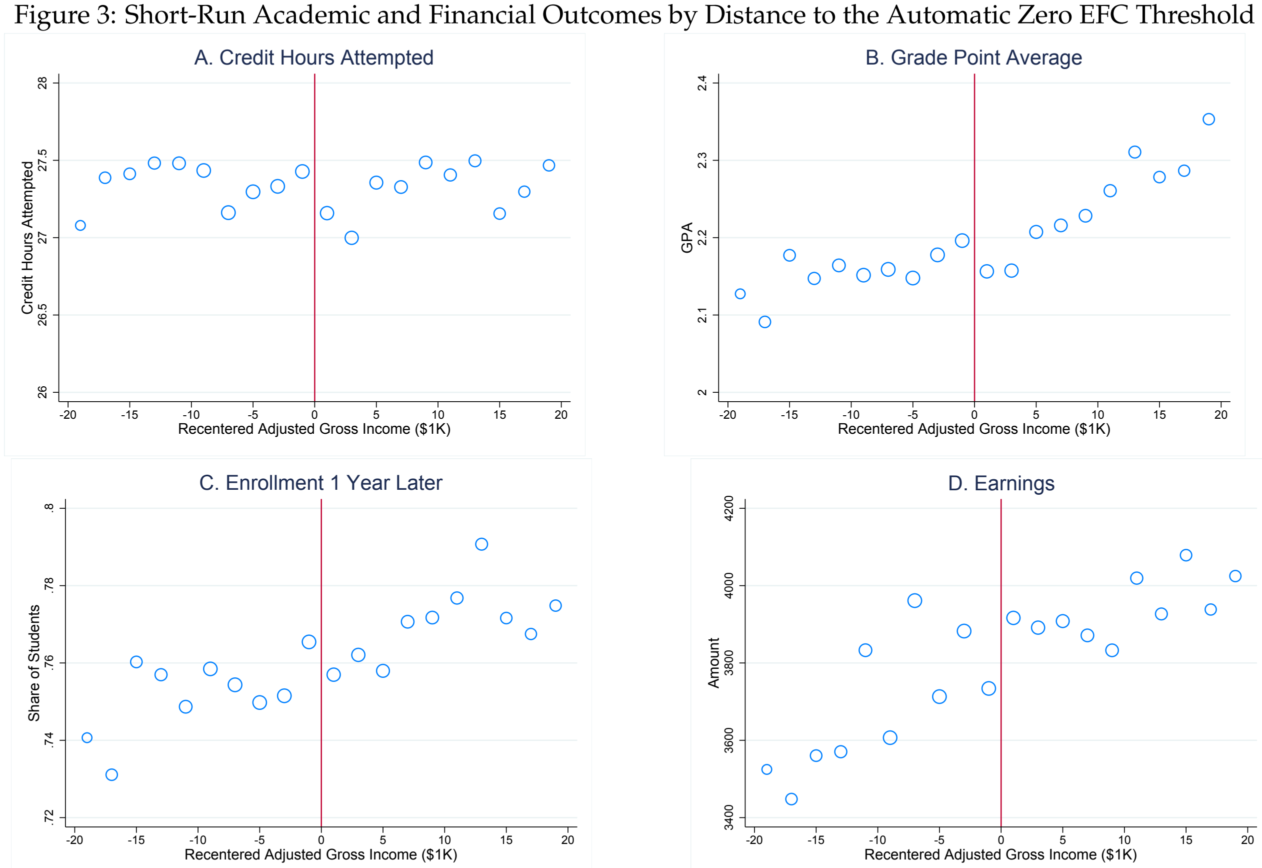
Notes: First-time dependent undergraduate students who enrolled in a four-year Texas public institution in 2008 through 2011 and whose family AGI fell within $20,000 of the income eligibility threshold for an automatic zero EFC. Students with AGIs at multiples of $1000 are excluded. $2000 AGI bins. Larger circles represent a larger underlying sample size. Each marker represents the average percentage of students receiving an $0 EFC in the bin (Panel A) or average amount of total grant aid (Panel B), Pell Grant aid (Panel C), TEXAS Grant aid (Panel D), other grant aid (Panel E), or loans (Panel F) received by students within the bin. Total grant aid includes Pell, Texas, other state, other federal, and institutional grant aid. $2000 AGI bins. All dollar amounts adjusted to represent constant 2013$.

Figure 2: Number of Students by Distance to the Automatic Zero EFC Threshold



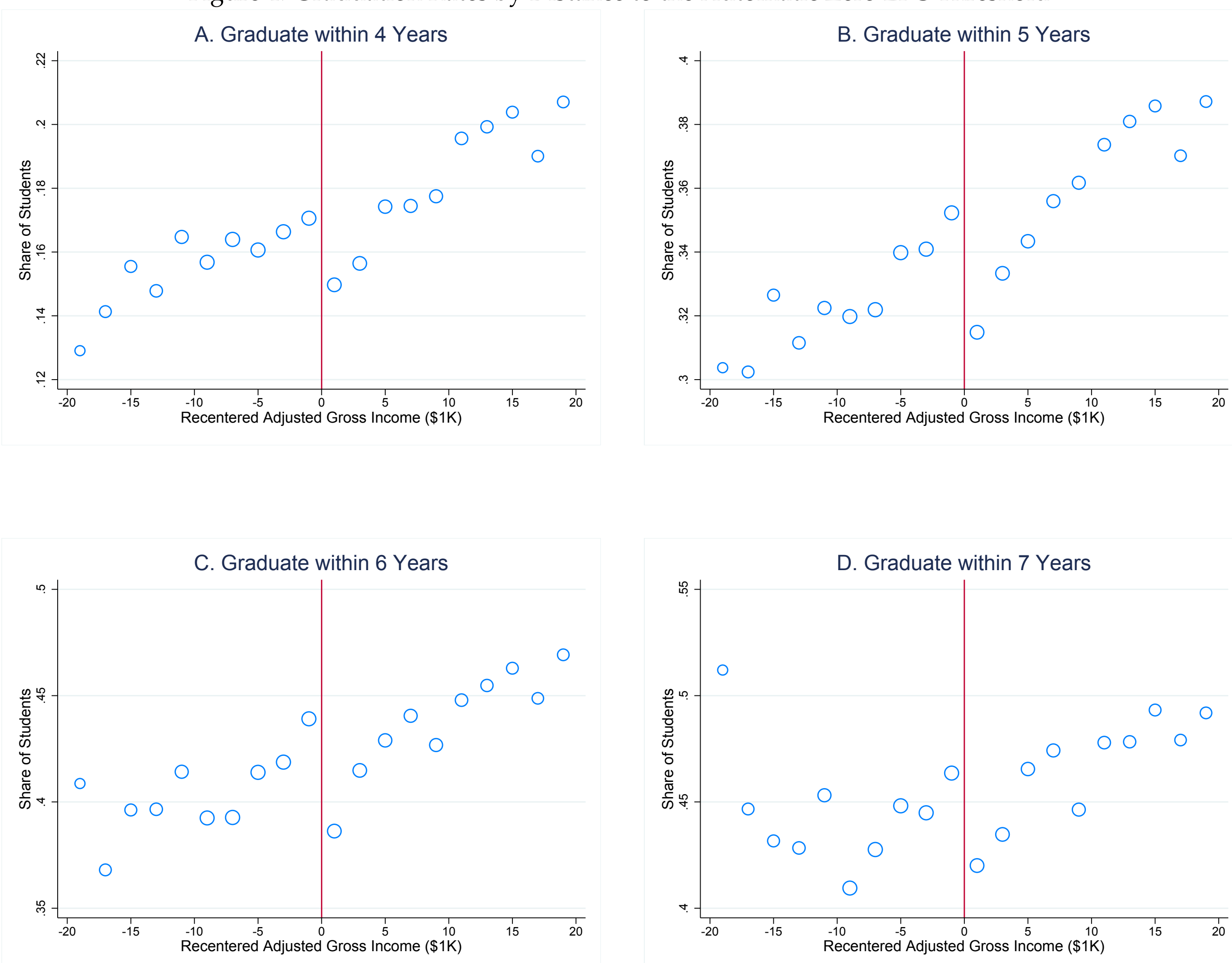
Notes: First-time dependent undergraduate students who enrolled in a four-year Texas public institution in 2008 through 2011 and whose family AGI fell within $18,000 of the eligibility threshold for an automatic zero EFC. Students with AGIs at $1000 intervals are excluded in Panel A. Each marker represents the number of students within a given $100 bin.

Figure 3: Short-Run Academic and Financial Outcomes by Distance to the Automatic Zero EFC Threshold



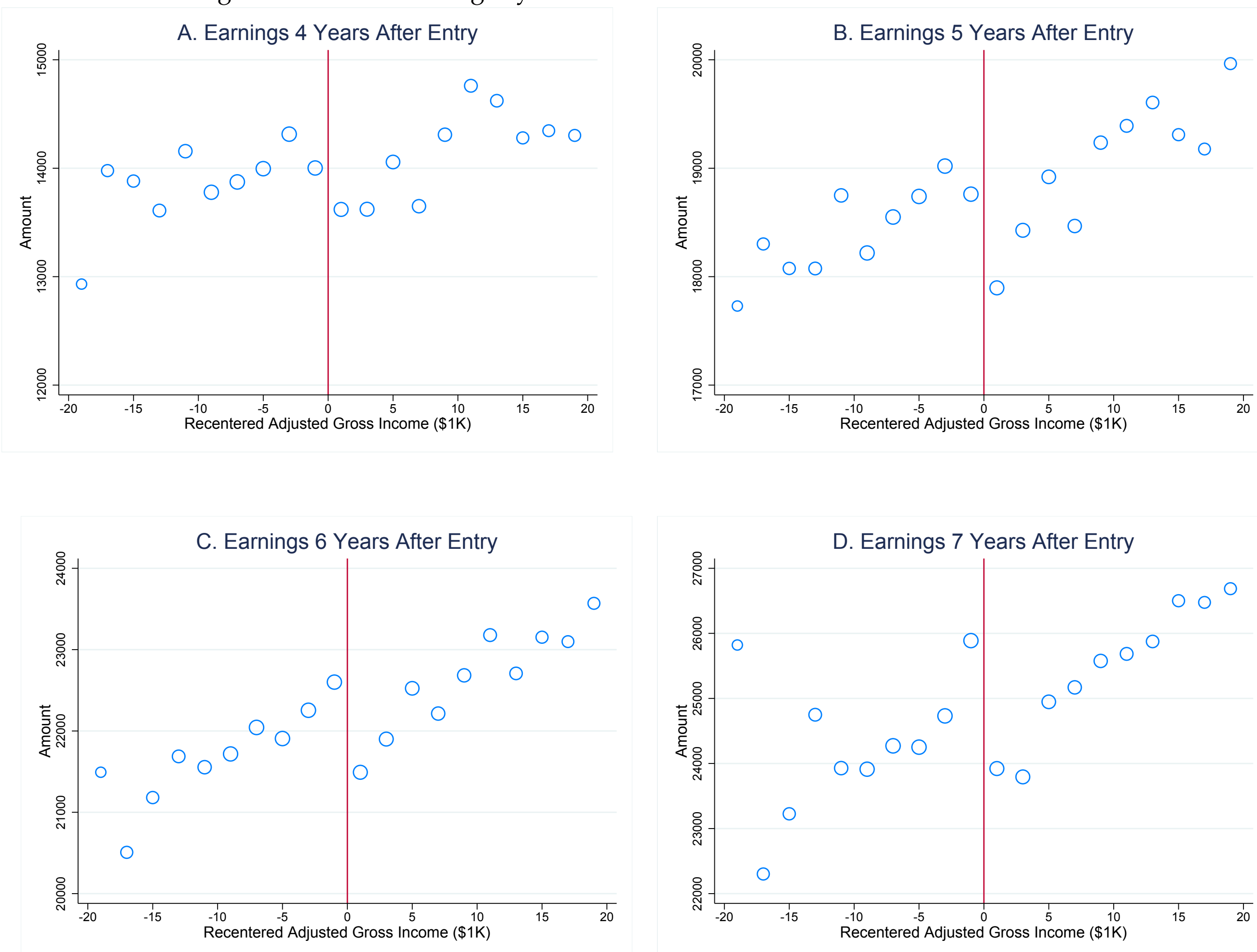
Notes: See Figure 1 notes for sample description. Each marker represents the average number of credit hours attempted (Panel A), average GPA (Panel B), share of students who reenrolled the next year(Panel C), or average earnings (Panel D) in the year of college entry within a given $2000 AGI bin. Earnings limited to students in UI-covered jobs in Texas. Larger circles represent a larger underlying sample size. All dollar amounts adjusted to represent constant 2013$.

Figure 4: Graduation Rates by Distance to the Automatic Zero EFC Threshold



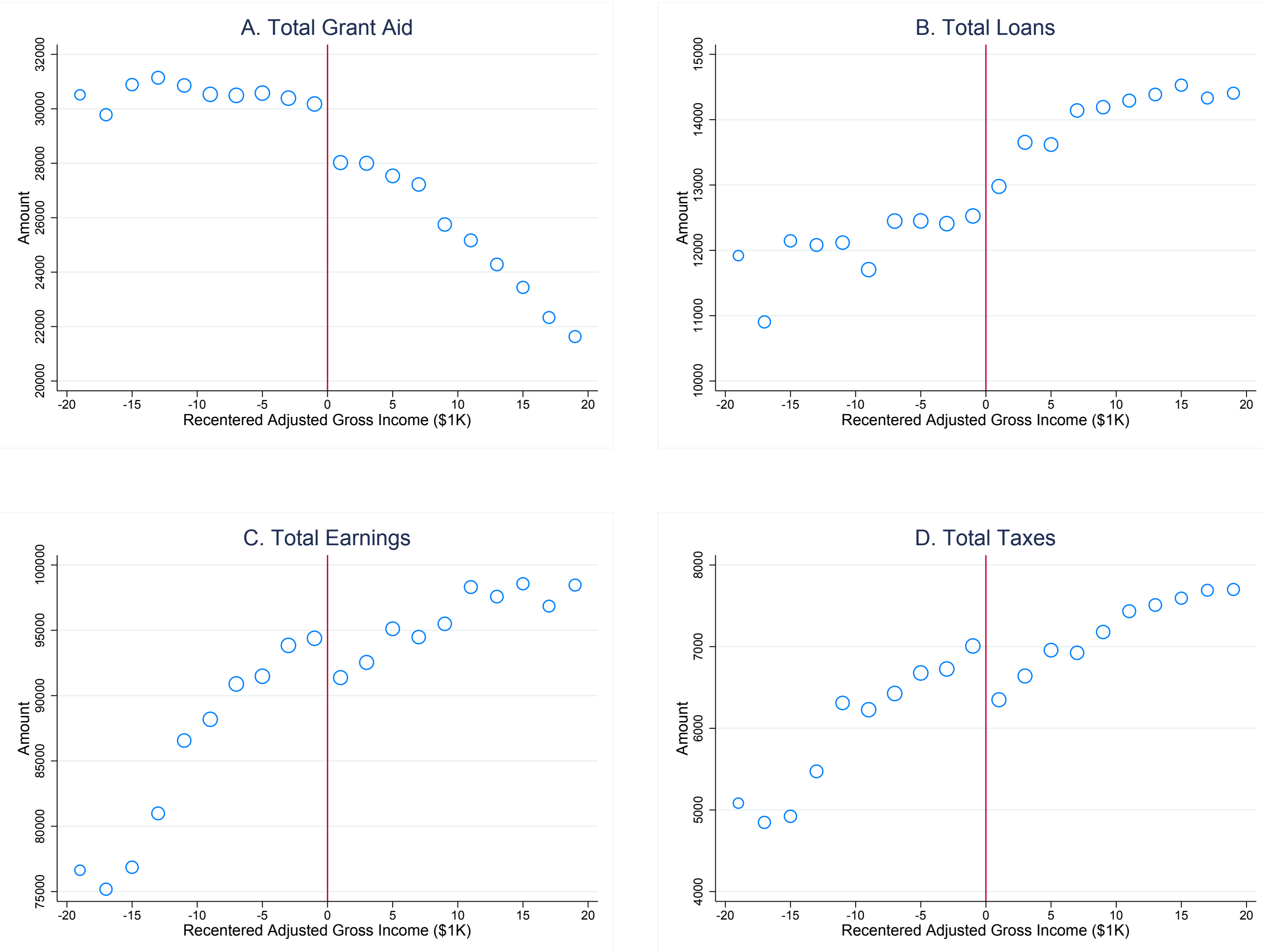
Notes: See Figure 1 notes for sample description. Each marker represents the share of students receiving a bachelor's degree within the specified number of years since entry within a given $2000 AGI bin. Standardized AGI represents the distance from the automatic zero EFC cut-off in the year of college entry. Larger circles represent a larger underlying sample size.

Figure 5: Annual Earnings by Distance to the Automatic Zero EFC Threshold



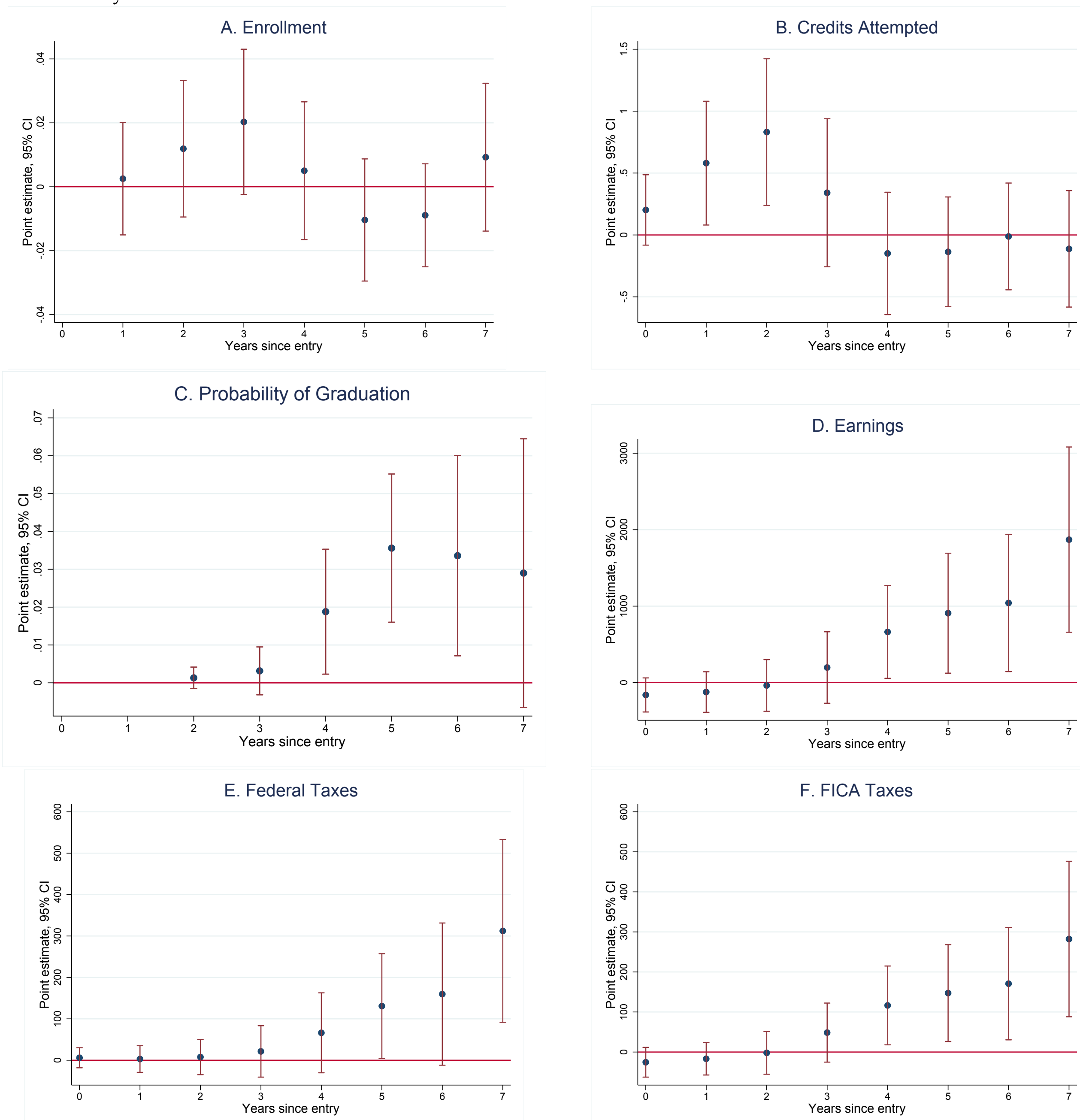
Notes: See Figure 1 notes for sample description. Each marker represents average earnings received by students in the specified number of years since entry within a given $2000 AGI bin. Standardized AGI represents the distance from the automatic zero EFC cut-off in the year of college entry. Earnings are limited to those received in UI-covered jobs in Texas. Larger circles represent a larger underlying sample size. All dollar amounts adjusted to represent constant 2013$.

Figure 6: Total Grants, Loans, and Earnings by Distance to the Automatic Zero EFC Threshold



Notes: See Figure 1 notes for sample description. Each marker represents average cumulative grant aid (Panel A), federal loans (Panel B), earnings (Panel C), or estimated federal income and FICA taxes (Panel D) received by students over the duration of years in which they are observed within a given $1000 AGI bin. Standardized AGI represents the distance from the automatic zero EFC cut-off in the year of college entry. Earnings are limited to those received in UI-covered jobs in Texas. Federal income and payroll taxes imputed using NBER TAXSIM (see Section 3 for details). Larger circles represent a larger underlying sample size. All dollar amounts adjusted to represent constant 2013$.

Figure 7: Effects of Automatic Zero EFC Eligibility on Academic and Labor Market Outcomes by Years Since Entry



Notes: First-time dependent undergraduate students who enrolled in a four-year Texas public institution in 2008 through 2011 and whose family AGI fell within $12,000 of the eligibility threshold for an automatic zero EFC. Students with AGIs at $1000 intervals are excluded. Point estimates and 95% CI from regressions of the probability of reenrollment (Panel A), credits attempted (Panel B), probability of bachelor's degree receipt (Panel C), annual earnings (Panel D), estimated federal income taxes (Panel E), or estimated federal payroll taxes (Panel F) within the specified number of years since entry on eligibility for the automatic zero EFC, a linear term in distance from the threshold (allowed to vary on either side), and indicators for parent education, race, gender, age, Texas residency, and cohort. Confidence intervals constructed using robust standard errors clustered at initial institution by entry cohort level. Earnings limited to students in UI-covered jobs in Texas. Federal income and payroll taxes imputed using NBER TAXSIM. All dollar amounts adjusted to represent constant 2013$.

Table 1: Sample Demographics and Contemporaneous Finances

	(1) Full sample	(2) Auto-zero eligible	(3) Auto-zero ineligible
A. Student demographics			
Male	0.46	0.45	0.46
Age	18.6	18.6	18.6
Texas Resident	0.97	0.97	0.96
Race			
Asian	0.05	0.05	0.05
Black	0.24	0.23	0.26
Hispanic	0.22	0.20	0.25
White	0.46	0.50	0.42
Parental education			
Father: <HS	0.13	0.15	0.12
Father: HS degree	0.46	0.45	0.47
Father: college degree	0.23	0.21	0.25
Mother: <HS	0.12	0.14	0.10
Mother: HS degree	0.49	0.49	0.48
Mother: college degree	0.29	0.26	0.32
B. Financial aid			
EFC = 0	0.55	0.88	0.18
Pell Grant aid	$4,053	$4,667	$3,372
Texas Grant aid	$3,365	$3,591	$3,115
Total Grants	$9,676	$10,413	$8,860
Loans	$2,740	$2,421	$3,094
Earnings	$3,844	$3,788	$3,905
Work Study	$141	$137	$145
Observations	36,697	19,275	17,422

Notes: First-time dependent undergraduate students who enrolled in a four-year Texas public institution in 2008 through 2011 and whose family AGI fell within $12,000 of the income eligibility threshold for an automatic zero EFC (see Section 2.1). Students with AGIs at $1000 intervals are excluded. Students with family AGI below the year specific threshold are eligible for an automatic zero EFC. Race and parent education categories will not sum to 100 percent due to missing values. All dollar amounts adjusted for inflation (2013$).

Table 2: Correlations between Automatic Zero Eligibility and Selected Student Characteristics

	(1) Linear prediction	(2) Father college deg.	(3) Mother college deg.	(4) White	(5) Black	(6) Hispanic	(7) Asian	(8) Age	(9) Texas resident
Automatic zero eligible	0.003	-0.003	-0.002	0.013	-0.032*	0.015	0.002	-0.018	0.012**
	(0.002)	(0.009)	(0.009)	(0.014)	(0.017)	(0.009)	(0.005)	(0.013)	(0.005)
Mean | ineligible	0.16	0.22	0.28	0.46	0.27	0.20	0.05	18.6	0.96
Observations	36,697	36,697	36,697	36,697	36,697	36,697	36,697	36,697	36,697

*Note*s: First-time dependent undergraduate students who enrolled in a four-year Texas public institution in 2008 through 2011 and whose family AGI fell within $9000 of the eligibility threshold for an automatic zero EFC. Students with AGIs at $1000 intervals are excluded. Point estimates from OLS regressions of the dependent variable specified in each column on eligibility for the automatic zero EFC. All models also include controls for a linear term in distance from the AGI threshold (allowed to vary on either side of the threshold). Robust standard errors, clustered by initial institution by entry cohort, in parentheses; *** p<0.01, ** p<0.05, * p<0.1. "Mean | ineligible" represents the limit of the expected value of the dependent variable as the AGI threshold is approached from above the threshold.

Table 3: The Effect of Automatic Zero Eligibility on First Year Financial Outcomes

	(1) EFC=0	(2) Total grant aid	(3) Pell Grant aid	(4) TEXAS Grant aid	(5) Other grant aid	(6) Earnings	(7) Loans
Automatic zero eligible	0.522***	711***	510***	122*	78	-161	-333***
	(0.014)	(94)	(37)	(63)	(66)	(114)	(65)
Mean | ineligible	0.32	$9,008	$4,020	$3,319	$1,669	$3,882	$2,887
Observations	36,697	36,697	36,697	36,697	36,697	36,697	36,697

*Note*s: See Table 1 notes for sample description. Point estimates from OLS regressions of the dependent variable specified in each column on eligibility for the automatic zero EFC. All models also include controls for a linear term in distance from the AGI threshold (allowed to vary on either side of the threshold), parent education, race, gender, age, Texas residency, and entry cohort. Robust standard errors, clustered by initial institution by entry cohort, in parentheses; *** p<0.01, ** p<0.05, * p<0.1. "Mean | ineligible" represents the limit of the expected value of the dependent variable as the AGI threshold is approached from above. All dollar amounts adjusted for inflation (2013$).

Table 4: The Effect of Automatic Zero Eligibility on Contemporaneous Academic Outcomes

	(1) Credit hours attempted	(2) Persistence	(3) GPA
Automatic zero eligible	0.202	0.003	0.032
	(0.145)	(0.009)	(0.027)
Mean | ineligible	27.1	0.76	2.14
Observations	36,697	36,697	36,697

Notes: See Table 1 notes for sample description. See Table 3 notes for specification. Robust standard errors, clustered by initial institution by entry cohort, in parentheses; *** p<0.01, ** p<0.05, * p<0.1. "Mean | ineligible" represents the limit of the expected value of the dependent variable as the AGI threshold is approached from above the threshold.

Table 5: The Effect of Automatic Zero Eligibility on Degree Receipt: STEM and Other Degree Completion

	Graduate in 4 years			Graduate in 6 years		
	(1) All majors	(2) STEM major	(3) Non-STEM major	(4) All majors	(5) STEM major	(6) Non-STEM major
Automatic zero eligible	0.019**	0.010***	0.009	0.034**	0.011*	0.023**
	(0.008)	(0.004)	(0.007)	(0.014)	(0.006)	(0.011)
Mean | ineligible	0.15	0.03	0.12	0.40	0.07	0.32
Observations	36,697	36,697	36,697	26,389	26,389	26,389

Notes: See Table 1 notes for sample description. See Table 3 notes for specification. Robust standard errors, clustered by initial institution by entry cohort, in parentheses; *** p<0.01, ** p<0.05, * p<0.1. "Mean | ineligible" represents the limit of the expected value of the dependent variable as the AGI threshold is approached from above the threshold. STEM majors are those with CIP codes included in the National Science Foundation's list of STEM disciplines (available at: https://www.lsamp.org/help/help_stem_cip_2010.cfm). Non-STEM majors are all other majors. Categories are not mutually exclusive (double majors may receive both STEM and non-STEM degrees).

Table 6: The Effect of Automatic Zero Eligibility on Borrowing

	(1) Max loan	(2) Any loan	(3) Total grants	(4) Total loans
Automatic zero eligible	-0.033**	-0.040**	544***	-620***
	(0.015)	(0.015)	(144)	(140)
Mean | ineligible	0.119	0.727	$8,880	$4,308
Crowd-out | would-be-borrower				-1.57
Observations	11,375	11,375	11,375	11,375

Notes: See Table 1 notes for sample description. Students are considered to have maximized their federal loan if their total loan aid at entry equals either the maximum Direct Loan available to dependent first-year undergraduate students ($3500 in 2008 and $5500 in 2009-2011) or the maximum Direct Loan available to dependent first-year undergraduate students' whose parents have been denied a PLUS loan ($7500 in 2008 and $9500 in 2009-2011). Students with unmet need less than $13,500 are also excluded. Point estimates from OLS regressions of the dependent variable specified in each column on eligibility for the automatic zero EFC. All models also include controls for a linear term in distance from the AGI threshold (allowed to vary on either side of the threshold) and entry cohort. Robust standard errors, clustered by initial institution by entry cohort, in parentheses; *** p<0.01, ** p<0.05, * p<0.1. "Mean | ineligible" represents the limit of the expected value of the dependent variable as the AGI threshold is approached from above. Crowd-out | would-be-borrower equals the change in amount borrowed per dollar increase in grant aid divided by the share of ineligible students who borrow (see Marx and Turner (forthcoming) for details). All dollar amounts adjusted for inflation (2013$).

Table 7: Impacts on Cumulative Financial Aid, Earnings, and Estimated Federal Taxes

	(1) Cumulative grant aid	(2) Cumulative loans	Sum of estimated effects on: (3) Earnings	(4) Fed. income taxes	(5) FICA taxes
Automatic zero eligible	1310***	-39	4327**	707***	721***
	(458)	(298)	(1736)	(273)	(276)
Mean | ineligible	$28,759	$12,712	$103,063	$7,538	$16,369
Observations	36,697	36,697			

Notes: See Table 1 notes for sample description. See Table 3 notes for specification. The sum of estimated effects on cumulative earnings is equal to the sum of point estimates displayed in Figure 5. Robust standard errors, clustered by initial institution by entry cohort, in parentheses; *** p<0.01, ** p<0.05, * p<0.1. The sum of estimated effects on imputed federal taxes is equal to the sum of point estimates from regressions of estimated federal income or payroll taxes on an indicator for eligibility for the automatic zero EFC in models that also include controls for a linear term in distance from the AGI threshold (allowed to vary on either side of the threshold), parent education, race, gender, age, Texas residency, and entry cohort. All dollar amounts adjusted for inflation (2013$).

Table 8: Direct Utility Gain from $1 Increase in Grant Aid

	Risk aversion parameter (γ) = 1	2	3
A. All college students			
Net discount rate ($R\beta$) =			
1.03	[0.03, 0.05]	[0.27, 0.35]	[0.57, 0.77]
1	[0.23, 0.25]	[0.52, 0.61]	[0.88, 1.11]
0.97	[0.48, 0.50]	[0.82, 0.94]	[1.25, 1.53]
B. Low AGI college students			
Net discount rate ($R\beta$) =			
1.03	[0.07, 0.29]	[0.40, 1.05]	[0.84, 2.31]
1	[0.28, 0.54]	[0.67, 1.45]	[1.19, 2.95]
0.97	[0.54, 0.85]	[1.00, 1.94]	[1.63, 3.74]
C. High AGI college students			
Net discount rate ($R\beta$) =			
1.03	[-0.10, -0.05]	[-0.03, 0.11]	[0.07, 0.32]
1	[0.07, 0.13]	[0.16, 0.33]	[0.27, 0.58]
0.97	[0.28, 0.36]	[0.39, 0.59]	[0.53, 0.89]

Notes: Lower and upper bounds for the normalized present value of the direct welfare effect in equation 5 as a function of the risk aversion parameter (γ), college student sample, and $R\beta$. See Section 7 for details. All dollar amounts adjusted for inflation (2013$).