

The Long-Run Impacts of Financial Aid: Evidence from California's Cal Grant[†]

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We examine the long-term impacts of California's state-based financial aid by tracking educational and labor force outcomes for up to 14 years after high school graduation. We identify program impacts by exploiting variation in eligibility rules using GPA and family income cutoffs that are ex ante unknown to applicants. Aid eligibility increases undergraduate and graduate degree completion, and for some subgroups, raises longer-run annual earnings and the likelihood that young adults reside in California. These findings suggest that the net cost of financial aid programs may frequently be overstated, though our results are too imprecise to provide exact cost-benefit estimates. (JEL H75, I21, I22, I23, I26, I28)

Educational investment is widely considered a key input into economic growth (Hanushek and Woessmann 2015). Although primary and secondary education has been compulsory and free in the United States for decades, postsecondary education remains both optional and increasingly expensive, even as a college degree has essentially become a requirement for stable employment. The cost of college provides a rationale for government intervention into postsecondary financing, particularly through targeted loans and grants needed to reduce financial constraints for low income students (Becker 1975). Whereas research has begun to examine

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the long-term impacts of government investment in a variety of educational policy areas, there is less such research in the postsecondary sector.¹

State programs that allocate aid based on either need or merit considerations have become more prominent over the past two decades, with funding increasing by 83 percent from 2002 to 2012 (National Association of State Student Grant and Aid Programs 2012). Merit-aid programs in particular have expanded from Arkansas and Georgia in the early 1990s to over 20 state programs (Domina 2014, Doyle 2006). However, there is relatively little research to date that would allow financial aid programs to measure their long-run return on investment. Causal impacts of financial aid have been predominately restricted to short-term college attendance and bachelor's degree completion outcomes. Recent work in other areas, such as early childhood education, suggest that a program's long-term impacts may swamp short-term gains (Chetty et al. 2011; Dynarski, Hyman, and Schanzenbach 2013) and that educational programs may actually pay for themselves through increased future tax revenues (Bettinger et al. 2014). Evaluating the ultimate returns to financial aid requires policymakers to observe a diverse set of outcomes, including labor force decisions, mobility, health, family formation, and other economically critical decisions. This requires the ability to follow students over a much longer time frame than has previously been available.

We examine the impacts of California's Cal Grant program, one of the largest and most generous state-based financial aid programs, as measured by number of students and overall expenditure.² The Cal Grant system contains a number of features that make it ideal for examining financial aid's long-term causal impact. First, eligibility for the grant is based on a series of strict cutoffs in family income and high school GPA. Crucially, in the years of our analysis, the locations of these cutoffs were not known to applicants ahead of time. These eligibility thresholds allow us to use a regression discontinuity design to identify two subpopulations of students: students who are income eligible but whose GPAs are near the minimum GPA cutoff, and students who are GPA eligible but whose family incomes are near the income threshold. These discontinuities represent separate populations—the first group are generally low income, low GPA high school graduates, whereas the second group contains a range of GPAs for middle income families—and the heterogeneity in estimated impacts between discontinuities is informative.

Second, individual-level data on Cal Grant applicants exist beginning with the 1998 high school graduating cohort. Given that the two best administrative data sources for college-going students—National Student Clearinghouse (NSC) and 1098-T tax forms—only become available or reliable around this time period, these data are likely to serve as the best source for estimating aid's impacts on college-going and degree completion available in the United States. Population-based administrative tax return data also become available in 1999, allowing us to observe the take-up of federal education tax credits in the years immediately following

¹For example, economists have examined the long-term impacts of inputs into early childhood (Chetty et al. 2011), primary school (Dynarski, Hyman, and Schanzenbach 2013), or secondary school (Jackson, Johnson, and Persico 2016) settings.

²For example, the Cal Grant awarded over \$1.6 billion in grants for the 2013–2014 academic year.

college application, and to track labor market, income, geographic mobility, and family formation outcomes for these students for over 14 years after applicants graduate from high school.³

Our work builds upon previous research by Kane (2003), which reports that the Cal Grant increased the likelihood that some students immediately enrolled in college. We improve on this earlier analysis by using a larger sample, a longer follow-up period, and a broader set of outcomes than previously available. In addition to the NSC and tax-based outcomes described above, we use federal student loan data to estimate the causal impact of the Cal Grant on other forms of federal financial aid.

In contrast to Kane (2003), we find that Cal Grant receipt has no meaningful effect on overall college attendance, in part due to high college-going rates among the population of applicants. However, at the GPA discontinuity, we find noticeable effects on four-year college attendance three and four years after high school graduation, likely because the Cal Grant increases persistence in college. We find that the Cal Grant significantly increases the probability of earning a bachelor's or graduate degree among this relatively lower-achieving population by 4.6 and 3.1 percentage points, respectively, which correspond to increases of roughly 10 and 27 percent over baseline. Point estimates suggest that the Cal Grant raises earnings by 1.3 percentile points in the income distribution. This corresponds to an increase in earnings of 3–4 percent, on average, between ages 28 and 32 for those at the GPA discontinuity; however, the year-by-year estimates are quite imprecise. We do not detect any evidence of shifting of institution type following high school graduation, though alternate metrics of college quality suggest some upgrading as a result of the Cal Grant.

At the income discontinuity, we find shifts in the type of college a student attends: attendance at four-year private institutions increases by 5.6 percentage points, or roughly 30 percent, with an offsetting reduction in attendance rates at public two-year and four-year colleges and universities. These shifts are not accompanied with increases in school quality, as measured by graduation rates, median freshmen SAT scores, or "College Scorecard" average earnings, but are instead associated with increased tuition costs and reduced per-student expenditures. For these students, point estimates suggest that the Cal Grant raises bachelor's degree completion and graduate degree completion by 3.0 percentage points, but these results are not statistically significant at conventional levels. Given the lack of institutional quality improvement, it is perhaps unsurprising that students at the income threshold display no impact of the Cal Grant on labor income, as measured by the sum of W-2 earnings and nonemployee compensation, or adjusted gross income, between ages 28 and 32. We find suggestive evidence, however, that the program is successful at retaining individuals in-state in the longer run: students at the income discontinuity are 2.4 percentage points more likely to reside in California between these ages, on

³ All of our estimates are within the set of Cal Grant applicants. Using California Student Aid Commission (CSAC) and US Department of Education (DOE) data, we estimate that in 1999, 300,000 California students graduated from high school. Of those, 206,000 started the Free Application for Federal Student Aid (FAFSA) and 110,000 applied for the Cal Grant. Cal Grant applicants cover more than one-third of all graduating seniors and at least half of all college-bound seniors. Many graduating seniors who do not apply for the Cal Grant may be high income or very low GPA, suggesting we capture a large portion of the eligible applicant group.

average, as a result of Cal Grant eligibility, although these results are sensitive to functional form assumptions.

We are among the first to examine the impacts of merit aid on graduation and earnings. Two important exceptions are Scott-Clayton and Zafar (2016), who study long-term impacts from West Virginia's PROMISE scholarship, and Denning, Marx, and Turner (forthcoming), who study the long-term impacts of the Pell Grant on low-income students in Texas. Estimates from West Virginia's PROMISE, which uses merit-based eligibility criteria, are remarkably similar to those we find at the GPA threshold. Our results on graduate degree completion are similar in magnitude; however, impacts on bachelor's degree completion for PROMISE scholarship recipients disappear over time. The earnings impacts are also of a similar magnitude with Scott-Clayton and Zafar (2016) finding a 7 percent increase in earnings conditional on employment. The estimates in Denning, Marx, and Turner (forthcoming) are also comparable to ours at the GPA threshold. The bachelor's degree attainment effects are roughly proportional to the increase in aid at the threshold that they study, and the cumulative earnings for eligible students increases by 4 percent over a seven-year period. In all of these cases, the earnings point estimates imply a very large and likely unrealistic return to bachelor's degree attainment if effects work solely through degree completion. Our wide confidence intervals, however, include plausible returns. We suspect both that our point estimates overstate the true effect, and that earnings are impacted through channels other than degree receipt. The similarity in our estimates is notable because the populations studied face different tuition implications and state school systems.

Our analyses inform the design of federal student aid policies, such as the American Opportunity Tax Credit, the suspended HOPE Scholarship, and the Lifetime Learning Credit. Like the Cal Grant we study, these federal tax incentives for higher education are targeted to middle- and higher-income students and provide financial support of a similar magnitude.⁴ Hoxby and Bulman (2016) finds no impacts on college persistence from the federal tax deduction for tuition and fees, and suggests that timing and a lack of saliency could explain this result. We find meaningful effects from a similarly sized grant that uses a different mechanism to distribute aid. Importantly, the students who exhibit the largest longer-run earnings gains are those at the margin of GPA eligibility, a dimension that is not considered for these federal aid programs. In addition, our analyses inform the extent to which state-based aid programs impact the utilization of these and other sources of federal student aid, and their implications for long-run residency and earnings.

I. Prior Literature

The Human Capital model (e.g., Becker 1975) suggests that individuals attend college when the expected benefits exceed the costs. Broadly, the goal of financial aid is to decrease the cost of college, especially among those who are liquidity-constrained. Aid can alter students' cost-benefit calculus and induce

⁴Unlike the Cal Grant and other state merit aid, the federal subsidies do not target students based on high school or college achievement levels.

additional students to enroll and persist. Indeed, the literature has documented positive effects of financial aid on attendance, persistence, and completion (Bettinger 2004, Bettinger et al. 2012, Dynarski 2003, Goldrick-Rab et al. 2016, Hoxby and Turner 2013, Kane 2007, Scott-Clayton 2011, and Seftor and Turner 2002).

State-based merit-aid programs have multiple goals. First, by setting minimum academic thresholds for eligibility, they can incentivize additional academic effort at the high school level, a key predictor of college completion. A number of authors find that well-designed incentives can increase human capital accumulation in high school, potentially reduce state expenditures (e.g., by reducing time to degree), and accelerate students' entry into the labor market by one or more years (Domina 2014, Henry and Rubenstein 2002, Pallais 2009, and Scott-Clayton 2011).

Second, financial aid may directly affect college attendance and completion rates by reducing liquidity constraints, enabling students to travel farther to better institutions, or decreasing the need to work during college. There is significant evidence that state aid programs, whether through merit-based, need-based, or hybrid programs, can increase college attendance rates and completion rates, though results vary by state (Castleman and Long 2016; Cornwell, Mustard, and Sridhar 2006; Dynarski 2000, 2004, 2008; Kane 2003; Scott-Clayton 2011; Singell and Stone 2002; and Van Der Klaauw 2002).⁵ Merit aid may also increase human capital accumulation if it produces additional effort or alters students' use of time by, for example, reducing the hours needed to work (DesJardins et al. 2010).

Finally, state-based programs aim to decrease "brain drain" by increasing the likelihood that top-performing students stay locally for college and enhance the stock of college-educated adults within the state. Unlike other forms of aid (e.g., Pell Grants), state-based merit aid prioritizes specific institutions to keep the strongest students within state, which is particularly important as the market for high-performing students becomes increasingly national (Hoxby 2009). In doing so, states hope to experience stronger economic growth, increase their tax base (Groen 2004), and generate other benefits to individuals within their state (Oreopoulos and Petronijevic 2013). Evidence on whether aid induces students to attend college in-state is mixed, with research suggesting aid reduced out-migration in Georgia, with no equivalent effect in Tennessee (Cornwell, Mustard, and Sridhar 2006; Pallais 2009). The few available studies that examine long-term workforce outcomes rely on large panel data estimates and find that merit aid increased the likelihood that students resided within state through their early thirties, though estimated effects are generally small (Fitzpatrick and Jones 2012, Sjoquist and Winters 2013, 2014, and Zhang and Ness 2010). However, the only study that relied on student-level microdata found no effect on long-term retention within Georgia (Sjoquist and Winters 2013).

The effects of state aid programs likely depend on program details, such as minimum academic thresholds, income limits, the size of the award, or the renewal requirements while in college (Domina 2014, Long 2004, Sjoquist and Winters 2014). As one example, during the time period studied in this paper, the Cal Grant provided larger tuition subsidies for private institutions than for public

⁵Only a few of the papers on financial aid use a regression discontinuity design, with other work relying on difference-in-difference estimation using large-scale nationally representative datasets (e.g., Dynarski 2008).

institutions; most states provide either equal or smaller tuition payments to private institutions (Domina 2014). The heterogeneity in program design across states may partly explain the divergence in results found across the literature.

Along with Scott-Clayton and Zafar (2016) and Denning, Marx, and Turner (forthcoming), our study is among the first to construct causal regression discontinuity estimates of the impacts of financial aid receipt on long-term mobility and employment outcomes. An additional strength of our study is the time frame currently available, which includes over a dozen years of follow-up data to estimate academic and workforce outcomes. This longer time frame is crucial for studying workforce outcomes, as individual earning profiles flatten significantly for individuals in their early thirties (Chetty et al. 2014, Haider and Solon 2006), the age at which we can now observe these students. An additional benefit of using individual-level data is that we estimate returns to aid, as measured by both college completion and administrative earnings records. We compare these returns to the monetary amount spent on each student. Our results shed light on whether financial aid expenditures, which have totaled billions of dollars over the last few decades, are producing their intended effects.

II. Institutional Details, Research Design, and Sample Construction

A. Overview of the Cal Grant Program

The Cal Grant program is a need- and merit-based financial aid program administered by the California Student Aid Commission (CSAC). CSAC offers several awards that vary in their target populations and benefits. We focus on what is referred to as “Cal Grant A” for the high school graduating cohorts who enter college from 1998–1999 through 2000–2001. This award is a “first-dollar” scholarship—meaning that the award is allocated prior to other forms of financial aid, such as the Pell Grant—and provides four years of full-time tuition assistance. Tuition at California State University (CSU) or the University of California (UC) was approximately \$1,500 and \$3,500, respectively, in the late 1990s. In addition, students could use Cal Grant A to attend any in-state private institution, with the award subsidizing between \$9,000 and \$9,700 depending on the year.⁶ Students could not use Cal Grant A to attend a community college, but the award could be put on hold for up to two years for students who wished to delay enrollment in a four-year institution.⁷

Baseline eligibility for the Cal Grant requires an applicant to be a California resident (either a US citizen, permanent resident, or eligible noncitizen), have no defaults on federal loans, and have not previously earned a bachelor’s degree. Students must have submitted the FAFSA and a GPA verification form, which was to be completed by the high school attended, by March 2.⁸ The GPA verification form is completed by the high school and sent directly to CSAC.

⁶Subsidy amounts were \$9,036, \$9,420, and \$9,708 for the 1998, 1999, and 2000 cohorts, respectively.

⁷California community college tuition was \$11 per unit in 1999–2000, which was the lowest rate in the nation.

⁸In practice, CSAC included all applications received by March 12, to allow for potential complications in the mail.

In addition, applicants are disqualified if their assets (excluding housing value and retirement funds) exceed specific limits, though this impacts few students.⁹ Our sample uses only the set of people who applied for a Cal Grant. We estimate that this sample represents approximately 50 percent of college-bound seniors in California, and hence a large group of considerable interest (see footnote 3).

The primary form of eligibility for recent high school graduates depends on a student meeting a minimum GPA requirement and being below specific income thresholds. Importantly, these rules fluctuated because of changes in annual funding during our analysis period, resulting in several plausibly exogenous discontinuities in eligibility. First, the income limits varied from year to year using cost of living increases based on the California constitution. These limits would have been virtually impossible for families to calculate and anticipate. In 1998, the income limits ranged from \$53,100 for families of 3 or fewer to \$67,000 for families of 6 or larger, and in 2000, ranged from \$59,000 to \$74,100 for the same categories. Second, income-eligible applicants were ranked by GPA in descending order and were offered awards until funding was exhausted. This produced a GPA-eligibility cutoff that was unknown to applicants a priori. The resulting GPA cutoffs were 3.15, 3.09, and 2.95 for 1998, 1999, and 2000, respectively. Figure 1 shows how the GPA cutoff varied by year until 2001–2002 when it was fixed (and publicly known) at 3.0.

We remove a number of extremely low-income students from our sample who were eligible for an alternative financial aid package. Specifically, there is a second income limit—about half the size of the maximum income allowed—below which “low-income” students are entered into a separate competition that produces a point total that makes them eligible for an alternate grant award, Cal Grant B.¹⁰ For these point-eligible students, crossing the GPA threshold has no meaningful impact on award eligibility.

Simply meeting the income or GPA requirements is a necessary but not sufficient condition for being eligible for the Cal Grant. In addition, a student or their family must also have sufficient “unmet financial need,” which is calculated based on a student’s potential expenses and expected family contributions.¹¹ We ignore this

⁹During our sample period, dependent students and independent students with dependents were disqualified if they had assets (excluding housing value) exceeding \$42,000–\$49,600 (depending on the year). Independent students without dependents (other than a spouse) were required to have assets below \$20,000–\$25,110 (depending on the year).

¹⁰Cal Grant B offers similar tuition payments to Cal Grant A though for three years rather than four, though additionally offers a subsistence (cash) payment of approximately \$1,500 per year. At the time of our study, points were earned through higher GPA and measures of disadvantage, including lower family income, and the point threshold varied from year to year. For the sample of students who meet the points requirement, we find that crossing the GPA threshold has a precisely estimated null effect on award utilization, thus meriting their exclusion. Although this point system offers the promise of an additional RD analysis, we do not study it here due to the relatively small sample size, as well as other technical details specific to how Cal Grant B was handled in those years. Eligibility for Cal Grant B changed in 2001; at that point, many more students become eligible for Cal Grant B and could choose between the two awards, but that is not the case in our sample period.

¹¹CSAC’s “unmet need” requirement is different than what is generally reported from the FAFSA. To calculate whether a student has unmet need, a Cost of Attendance (COA) is assigned to each of the up to six schools a student has listed on their FAFSA. CSAC then subtracts a student’s Expected Family Contribution (EFC) from each school’s COA to create the unmet need value. For a student to be Cal Grant A eligible, a student must have unmet need equal to the maximum Cal Grant award amount available for that institution plus \$1,500, rather than simply having a positive COA less EFC value.

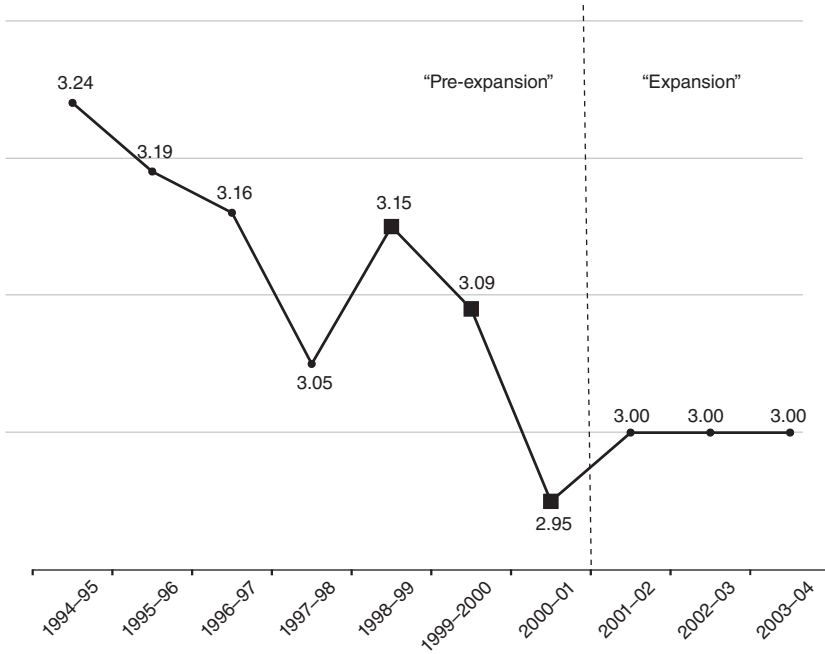


FIGURE 1. GPA CUTOFFS OVER TIME

Notes: This figure depicts the year-specific GPA thresholds for eligibility for Cal Grant A. Squares indicate the years that are included in our analysis.

distinction and present reduced form results that include these students because of the difficulty in calculating CSAC's unmet need, and the potential endogeneity of student expenses that are directly related to the types of institutions they wish to attend.

California expanded the Cal Grant program significantly in 2001–2002, changing how awards were allocated (though the monetary value of the awards remained constant). Beginning in this year, the GPA threshold for Cal Grant A was set at 3.0 in perpetuity, and so could be known by applicants a priori. In addition, family income thresholds were more widely publicized at this time. We find evidence that applicants were likely aware of the eligibility thresholds beginning in these years.¹² Thus, we restrict our analysis to applicants prior to the 2001–2002 academic year.

B. Research Design

Because the Cal Grant is allocated by a combination of academic achievement and financial need, simple comparisons of outcomes between financial aid recipients and non-recipients will likely produce biased estimates of the impact of financial

¹²Correspondence with CSAC personnel indicates that 2002 was the first year that CSAC's "Fund Your Future Workbook" published the exact income limits. We find clear evidence of violations in the density of applicants around the income cutoff in later years, though the violation appears to be that ineligible families simply did not apply, rather than altered their income.

aid; family background and academic preparation are correlated with the likelihood of receiving aid, the amount of aid received, and the likelihood of attending and graduating from college. To estimate the causal impact of the Cal Grant on student outcomes, we exploit the GPA and income eligibility cutoffs using a regression discontinuity (RD) design, where we compare students who just qualified for a grant to similar students who were just ineligible by utilizing equation (1):

$$(1) \quad Y_{it} = \beta_0 + \beta_1 \text{Distance}_i + \beta_2 \text{CG}_i + \beta_3 \text{CG}_i \times \text{Distance}_i + \Gamma \mathbf{X}_i + \varepsilon_{it}.$$

In this regression, Y_{it} is an outcome of interest (such as college enrollment or earnings) for student i in year t , CG_i is a variable that equals one if a student is Cal Grant eligible, and Distance_i is a continuous running variable that determines assignment to treatment, centered at the application year-specific eligibility cutoff. We show a linear specification here, but Distance_i can take a flexible functional form that includes higher order polynomials. The vector \mathbf{X}_i contains the following baseline observable characteristics: student age, citizenship status, parent's marital status, and parental education. All regressions also include family size-by-year fixed effects (where family size varies from two to "six or more") to account for the varying income-eligibility cutoffs. The parameter of interest, β_2 , represents the intent-to-treat parameter or the causal effect of the offer of the Cal Grant award on our outcomes of interest. In practice, the inclusion of observable characteristics \mathbf{X}_i is optional; their inclusion does not result in significant changes to our estimates of β_2 , but improves precision for some of our outcomes, particularly for earnings. Standard errors are clustered by standardized GPA when exploiting the GPA cutoff because the assignment to treatment variable is discrete (Lee and Card 2008). We report heteroscedasticity-robust standard errors for regressions using the income cutoff.

We run these regressions for the GPA cutoff and the income cutoff separately. The GPA threshold compares income-eligible students just above and below the GPA eligibility criteria who were not eligible for Cal Grant B. The income threshold compares GPA-eligible students just above and below the maximum income eligibility limits. In both of these cases, students who meet the respective GPA or income requirement are eligible for Cal Grant A, provided that they satisfy the "unmet need" requirement. Students who do not meet the cutoff are not immediately eligible for any Cal Grant award. We describe the differences in the samples around these two distinct cutoffs in the next section.

We also run the following instrumental variables (IV) regressions:

$$(2) \quad \text{Award}_{it} = \alpha_0 + \alpha_1 \text{Distance}_{it} + \alpha_2 \text{CG}_{it} + \alpha_3 \text{CG}_{it} \times \text{Distance}_{it} \\ + \Omega \mathbf{X}_{it} + \varepsilon_{it},$$

$$Y_{it} = \beta_0 + \beta_1 \text{Distance}_{it} + \beta_2 \widehat{\text{Award}}_{it} + \beta_3 \text{CG}_{it} \times \text{Distance}_{it} \\ + \Gamma \mathbf{X}_{it} + \varepsilon_{it}.$$

The first-stage regression predicts the likelihood that students utilize the Cal Grant ($Award_{it}$) at the margin. We use these predicted values to estimate a Local Average Treatment Effect (LATE) for those induced to use the Cal Grant. This parameter estimates the effect for those who take up the treatment, as compared to those who were unlikely to use the treatment irrespective of their assignment.

There are several reasons why an applicant who satisfied the GPA and income eligibility requirements may not be awarded a grant. Some students may choose to not attend college or attend an out-of-state institution. Other students may be denied an award based on the unmet need requirement, which we are unable to precisely estimate. In addition, Cal Grant A cannot be used at a community college, which is a commonly attended institution for many students at the margins of GPA eligibility. Students who are initially ineligible for the Cal Grant may later receive an award, generally via one of two ways. First, students initially apply for the award in 12th grade with their cumulative 10th and 11th grade GPA. If their 12th grade GPA pushes them above the required margin, they can apply in the subsequent year with their new cumulative GPA. Second, CSAC began to offer an alternative “competitive” award for older, nontraditional students who are two or more years out of high school, and some initially ineligible students may later qualify for this financially equivalent award.

C. Data and Sample Construction

Our sample consists of retrospective data on all students in California who were minimally eligible for the Cal Grant program, and submitted both a FAFSA and GPA verification form to CSAC during their final year of high school, which occurred between 1998 and 2000. Data on these hundreds of thousands of high school graduates who applied for the Cal Grant are provided by CSAC.

We gather outcome data from several sources. One source of data on college enrollment and degree completion comes from the National Student Clearinghouse (NSC). The NSC data cover about 94 percent of all college enrollments and have significant degree completion records. NSC data provide information on all institutions that a student attended, dates attended, whether the student transferred, whether degrees were conferred, the types of institutions attended, the intensity of enrollment, and the length of time required for degree completion.¹³ One potential concern is that low NSC coverage rates in the late 1990s may bias results.¹⁴ We address this issue by identifying schools with low NSC coverage rates and

¹³NSC data are increasingly used for tracking postsecondary outcomes, but is subject to bias due to missing data and errors in matching that rely on students’ names and birthdates (Dynarski, Hemelt, and Hyman 2015). In general, we find that NSC data provide similar estimates of college-going as 1098-T tax forms.

¹⁴California’s NSC institutional coverage rate was roughly 59 percent in 1998 (Dynarski, Hemelt, and Hyman 2015), though four-year public and private coverage rates were 93 and 71 percent in 1998, respectively. The state’s overall coverage rate was 93 percent by 2001, when students would first be earning their degrees. If we believe the aid is likely to push students toward more expensive institutions, then we may underestimate shifts away from community colleges, which offered no bachelor’s degrees at the time, and underestimate shifts toward private institutions; depending on the quality difference between the four-year public and specific private institutions, we could be slightly overestimating or underestimating true bachelor’s degree completion effects.

focusing our NSC analysis on students who only list well-covered schools on the FAFSA.¹⁵

As an alternative source of information on college attendance, we collect information returns that colleges submit to the Internal Revenue Service (IRS) to report “qualified educational expenses” in a given year, Form 1098-T. These are drawn from the population-based, administrative tax records for each student, available beginning in 1999. We use the 1098-T data as our primary source for measuring college attendance, as these data do not suffer from the missing data problems of the NSC. We match both NSC and 1098-T data to the Integrated Postsecondary Education Data System (IPEDS) to identify the type of institution that a student attends. We also collect several institutional characteristics: school size, tuition, 150 percent time to graduation, SAT score at the twenty-fifth and seventy-fifth percentiles, and expenditures per full-time equivalent (total, and across spending categories).

For each Cal Grant applicant, we construct information on federal student loans and Pell Grant awards that they have received. These data come from the National Student Loan Data System (NSLDS), a comprehensive national database of information on federal financial aid. We also collect information on the federal education tax credits that are claimed by the tax filing unit associated with the Cal Grant applicant beginning in 1999. To construct these data, we identify the tax return that includes a Cal Grant applicant in each tax year, which may be the applicant’s own return, and collect information on the American Opportunity Tax Credit and Lifetime Learning Credit.¹⁶

Our primary measure of college attendance identifies whether a student is enrolled at a postsecondary institution at any point within four years of Cal Grant application, rather than identifying the more typical “immediate postsecondary attendance.” We choose this method for two primary reasons: somewhat lower coverage rates for NSC data in their earliest years of implementation in the late 1990s, that improve rapidly over time; and the reliance of 1098-T data on calendar year enrollment, rather than academic year, which could lead to attendance overestimates (for high school students taking college courses who do not continue their education) or underestimates (if students attend college in the fall after high school graduation but quickly drop out). Alternate methods produce similar results. Analyses focused on school quality identify the first college attended. We similarly construct enrollment measures for specific postsecondary sectors within California schools.

¹⁵There are two main methods for identifying low coverage rates. The first utilizes information provided by the NSC, which identifies nonparticipating schools and schools with high Family Educational Rights and Privacy Act (FERPA) block rates. A second—specific to our dataset—is to examine all students who received a Cal Grant payment, which we know to be highly accurate, calculate the percentage of students of payees who do not show up as attending that school in the NSC data, and assign this value as an “incompleteness” rate to the school. Both methods produce similar results. When comparing results on our NSC subset and the full population, we find that degree completion impacts are underestimated at the GPA threshold, consistent with the idea that non-reporting underestimates positive impacts. We find degree impacts are likely overestimated at the income threshold, as students switch into private colleges that may be less likely to report data.

¹⁶Data on the link between tax filers and dependents are incomplete prior to 2001. We fill in missing observations by assuming that the modal tax filer that claims the applicant on a tax return between 1999 and 2006 is the tax filer that claims the applicant in the years of missing data problems. We also collect information on the tuition and fee deduction, but it is rarely utilized by this population and so do not study it here.

Labor market, income, demographics, and mobility data are drawn from administrative, population-level US federal tax filings. For each Cal Grant applicant, we construct a panel of tax returns spanning tax years 1999 through 2013, supplemented with several information returns filed with the IRS by third parties. Tax return data provide information on household-level wage income and adjusted gross income (AGI). We additionally collect the limited demographic information available on a tax return: marital status, number of children, and state of residence. We compute a household size-adjusted measure of AGI, which divides AGI by the square root of the number of individuals in the tax unit (i.e., the primary filer plus secondary and dependents), to account for income level differences due to differences in family structures.

Because tax returns provide data conditional on filing a tax return, and because earnings are reported at the household level when married filing jointly, we also consider individual-level earnings data. These data come from Form W-2, the information return on wage and salary income, and Form 1099-MISC, the information return on nonemployee compensation, both filed by businesses to the IRS. We compute labor income as the sum of earnings on these two tax forms. We account for outliers in these unedited data by winsorizing all monetary variables derived from tax return data at the ninety-ninth percentile. We translate labor income into percentiles in the income distribution using data from table 6 of Chetty et al. (2017), which provides the correspondence between child income and income percentiles by child birth cohort.

In our baseline analysis, we use a 0.3 point bandwidth around the GPA eligibility cutoff, and a \$10,000 bandwidth around the income eligibility cutoff, as suggested by cross-validation and Imbens and Kalyanaraman (2012) optimal bandwidth techniques.¹⁷ Table 1 shows summary statistics for the sample of 31,500 applicants who are within 0.3 points of the GPA discontinuity and 18,097 applicants who are within \$10,000 of the income discontinuity. Our two analytic samples are quite different from applicants in general and from each other because we focus on students near the eligibility thresholds. At the GPA discontinuity, 56 percent of applicants are female, and 86 percent are US citizens. Forty-four percent initially attended a California public four-year institution, with an additional 9 percent initially attending some form of private college. Mean family income was \$35,100 at the time of application. At some point within 10 to 14 years after applying, 89 percent of the sample is living in California, 52 percent are married, and 46 percent have children. Students at the income discontinuity have higher family incomes and high school GPAs, are more likely to attend private colleges or four-year public institutions, and have higher average incomes between 10 and 14 years after initial application. They were also more likely to be married but less likely to have children. These differences potentially shed light on why results might vary across the two analytic samples.

¹⁷In general, the optimal bandwidth varies by both validation technique and outcome chosen (e.g., first-stage award utilization, degree completion). We present results using a range of bandwidths, including those using the Calonico, Cattaneo, and Titiunik (2014) optimal bandwidth selection, in online Appendix tables. We note cases where bandwidth choice affects the results.

TABLE 1—SUMMARY STATISTICS

	GPA threshold		Income threshold	
	Mean	SD	Mean	SD
Demographics				
Age	18.54	2.33	18.24	1.18
Female	0.56	0.50	0.58	0.49
Citizen	0.86	0.34	0.94	0.24
Parents married	0.44	0.50	0.53	0.50
Dependent	0.97	0.16	1.00	0.07
GPA	3.08	0.18	3.55	0.28
Family income	35,103	14,490	60,492	7,971
Year 1 after application				
Attends school	0.83	0.37	0.93	0.25
School in California	0.77	0.42	0.86	0.35
Two-year public	0.25	0.43	0.16	0.37
Four-year public	0.44	0.50	0.54	0.50
Private school	0.09	0.29	0.15	0.36
Years 1–4 after application				
Attends school	0.95	0.22	0.99	0.12
School in California	0.91	0.29	0.93	0.25
Two-year public	0.42	0.49	0.27	0.44
Four-year public	0.58	0.49	0.67	0.47
Private school	0.09	0.29	0.15	0.36
Years 10–14 after application				
Files a tax return	0.93	0.22	0.95	0.18
Labor income	39,014	28,301	46,978	31,481
Household size-adjusted AGI	45,665	53,051	57,346	63,327
Married	0.52	0.50	0.57	0.50
Has kids	0.46	0.50	0.41	0.49
Lives in California	0.89	0.31	0.86	0.35

Notes: The table provides means and standard deviations for all students at the GPA and income thresholds. The GPA subsample includes income-eligible students within 0.3 of the GPA threshold, and the income subsample include GPA-eligible students within \$10,000 of the income threshold. The demographic and college attendance variables are available for all applicants: 31,500 observations at the GPA threshold and 18,097 observations at the income threshold. The indicator for filing a tax return and wage income (based off of W-2s) are also available for all applicants. The tax-return-based variables are only available for tax filers: 29,250 students at the GPA threshold and 17,252 students at the income threshold have available data at some point between 10 and 14 years after applying for the Cal Grant.

D. Validation of the RD Design

Before turning to our main results, we provide evidence that the discontinuities in award eligibility can serve to produce unbiased estimates of the effects of state-based aid. The three key assumptions for the validity of an RD design are that: the predicted discontinuity creates a large change in assignment to treatment as a function of the running variable; any observed differences in the neighborhood of the discontinuity occur only as a result of differences in the running variables; and there is no evidence of manipulation in assignment to treatment near the discontinuity. We address each of these assumptions in turn.

Figure 2 shows that Cal Grant A utilization rates vary discretely at each eligibility cutoff. We pool our data across all years and center the running variable at zero for each year-specific threshold. Panel A of Figure 2 shows that the GPA threshold predicts close to a 40 percentage point increase in ever receiving a Cal

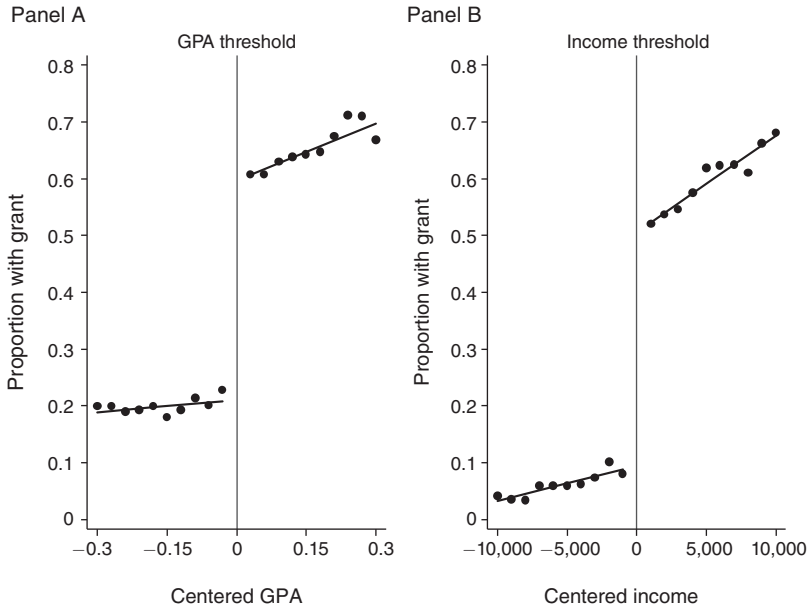


FIGURE 2. CAL GRANT UTILIZATION

Notes: This figure depicts the proportion of students who ever received a Cal Grant payment by bins of GPA and income around the Cal Grant eligibility threshold. Panel A bins students by 0.03 GPA points relative to the year-specific eligibility threshold, pooled across years. Panel B bins students by \$1,000 relative to the year-specific eligibility threshold, pooled across years. Income is reversed so that values above the cutoff represent lower family incomes; in both panels, crossing the threshold from left to right indicates becoming eligible for the Cal Grant.

Grant payment. At the bottom of Table 2, we provide corresponding point estimates for Cal Grant receipt, which shows that total CSAC payments increases by roughly \$4,300 for the average student at the GPA cutoff.¹⁸ There is a similar shift in Cal Grant utilization at the income threshold shown in panel B of Figure 2. In this and all future income-based figures, we multiply the running variable by -1 so that positive (negative) values correspond to Cal Grant eligibility (ineligibility). The bottom panel of Table 2 shows that total payments received are significantly larger at the income threshold, at roughly \$8,100 per award offer. This larger amount derives in large part from students at this threshold attending more expensive UC and private schools. IV estimates suggest that the shifts in average total payments for students who utilized the Cal Grant payments award were close to \$11,200 and \$19,300 at the GPA and income thresholds, respectively.

¹⁸There are some students below the GPA cutoff who received Cal Grant awards. This is primarily due to three reasons: students who applied in their senior year could resubmit the following year by incorporating their 12th grade GPA; CSAC's competitive award that became available in 2002 and was applicable for students more than 1 year removed from high school; we were able to eliminate some but not all point-eligible students at the "low income" GPA threshold. In all cases, we keep only the earliest Cal Grant application for each student, so thresholds are exogenous.

TABLE 2—EDUCATIONAL OUTCOMES, NSC, AND 1098-T DATA

	GPA threshold			Income threshold		
	Control mean (1)	Reduced form (2)	IV (3)	Control mean (4)	Reduced form (5)	IV (6)
<i>Degree attainment (NSC)</i>						
Bachelor's degree	0.46	0.046 (0.015)	0.146 (0.047)	0.67	0.030 (0.019)	0.076 (0.049)
Graduate degree	0.12	0.031 (0.011)	0.097 (0.034)	0.21	0.029 (0.017)	0.075 (0.044)
<i>College attendance (1098-T)</i>						
Attends college	0.94	0.005 (0.005)	0.013 (0.012)	0.99	-0.001 (0.003)	-0.002 (0.008)
California community college	0.44	-0.011 (0.011)	-0.029 (0.027)	0.28	-0.029 (0.013)	-0.069 (0.031)
California four-year public	0.57	0.001 (0.012)	0.003 (0.031)	0.71	-0.046 (0.014)	-0.110 (0.033)
California private	0.13	0.002 (0.007)	0.006 (0.018)	0.18	0.056 (0.012)	0.133 (0.028)
<i>First stage</i>						
Ever received a Cal Grant payment, 1098-T	0.20	0.386 (0.010)	—	0.08	0.420 (0.012)	—
Total Cal Grant aid received, 1098-T	\$1,759	4,311.169 (172.946)	—	\$1,037	8,115.300 (286.167)	—
Ever received a Cal Grant payment, NSC	0.20	0.315 (0.013)	—	0.07	0.391 (0.016)	—
Total Cal Grant aid received, NSC	\$1,520	3,009.515 (210.425)	—	\$924	7,674.893 (408.978)	—

Notes: This table presents estimates of the effect of Cal Grant eligibility on college attendance outcomes. The IV regressions instrument for receiving a Cal Grant with eligibility status; results from the first stage are presented in the bottom panel of the table. Regressions at the GPA threshold include income-eligible applicants within 0.3 points of the GPA threshold, and regressions at the income threshold include GPA-eligible applicants within \$10,000 of the income threshold. In specifications using NSC data, there are 31,836 and 18,588 observations at the GPA and income thresholds, respectively. In specifications using the 1098-T data, there are 31,500 and 18,097 observations at the income threshold, respectively. All regressions include student age, parental education, parental marital status, citizen status, and year-by-family size fixed effects. A linear function of the running variable and a uniform kernel are used. Standard errors are clustered by GPA for GPA threshold regressions and are heteroscedasticity robust in income threshold regressions. Reduced form control value means are average values for students within 0.05 points below the GPA threshold or within \$1,000 above the income threshold.

Next, we examine whether factors that are correlated with student outcomes change discontinuously at the thresholds that determine assignment to treatment. For each observable characteristic, X_{it} , we run the following regression:

$$(3) \quad X_i = \beta_0 + \beta_1 \text{Distance}_i + \beta_2 \text{CG}_i + \beta_3 \text{CG}_i \times \text{Distance}_i + \varepsilon_i.$$

In online Appendix Table A2, we present estimates for β_2 , which capture the differences in covariates between those just above and just below the eligibility threshold. These results provide evidence of continuity across the thresholds. Importantly, we find that GPA is smooth at the income discontinuity, and vice versa, suggesting there is no systematic sorting of eligible students. We find no imbalance in the likelihood of being female, a US citizen, or having married parents at the threshold.

Online Appendix Figures A1 and A2 provides corresponding graphical evidence at the GPA and income thresholds, respectively.

Finally, if students were able to manipulate assignment to treatment, then observable or unobservable characteristics of applicants may differ around the cutoff. In principle, there is limited scope for manipulation because it would have been difficult, if not impossible, to know the eligibility cutoffs a priori. Nevertheless, we provide evidence that there is no manipulation in the years of our analysis. Directly examining manipulation for the GPA threshold is difficult for two reasons. First, the McCrary test, which relies on nonparametric estimation, is problematic for discrete distributions (McCrary 2008). Second, Cal Grant applicants who are high school seniors utilize their unadjusted 10th and 11th grade GPA, leading to a “lumpy” distribution. Online Appendix Figure A3 shows the exact distributions for GPA in each year. Although the number of applicants bunches at specific GPA points, especially at 3.0, this lumping is equivalent across the three years, with little observational evidence that students are sorting differentially with respect to the cutoff. An overlay of the three years shows that distributions are similar, even though the GPA thresholds changed markedly between years. At the income cutoff, we check against the possibility of manipulation using the traditional McCrary density test (McCrary 2008). Online Appendix Figure A4 shows that the distributions are smooth with no evidence of manipulation around income thresholds in the pre-expansion years.¹⁹

III. Results

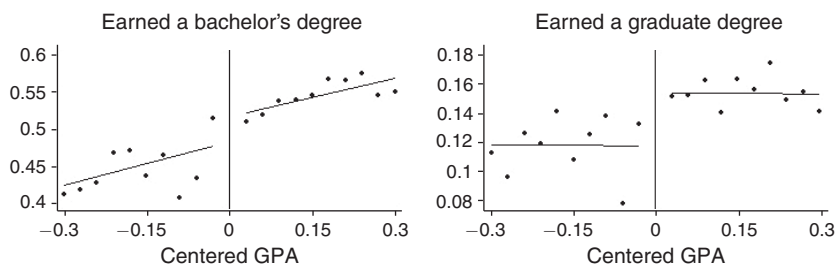
In this section, we present results in three broad outcome categories: college attendance and degree attainment; longer-run earnings, family formation, and mobility outcomes; and school quality and college financing. We examine effects at the GPA and income discontinuities separately. Importantly, the effects of Cal Grant eligibility (equation (1)) and of Cal Grant utilization (equation (2)) are identified using somewhat different groups of students depending on which discontinuity is being utilized. The students at the margin of the GPA threshold are, on average, of lower income and entering college with weaker academic preparation than students at the income threshold. Scatter plots showing how dependent variables vary across the year-specific eligibility thresholds appear in online Appendix 3, unless presented here.

A. College Attendance and Completion

Table 2 presents results from estimating equations (1) and (2) on our educational attendance outcomes using linear slopes with rectangular kernels. Because college attendance outcomes using 1098-T data produce roughly similar results but on a more representative sample, we place NSC-based attendance results in

¹⁹The McCrary test at the income cutoff provides an estimate of 0.0033 with a standard error of 0.0365 (t -stat = 0.09) for the NSC sample and 0.0194 with a standard error of 0.0382 (t -stat = 0.51) for the 1098-T sample.

Panel A. GPA threshold (binwidth = 0.03 GPA)



Panel B. Income threshold (binwidth = \$1,000)

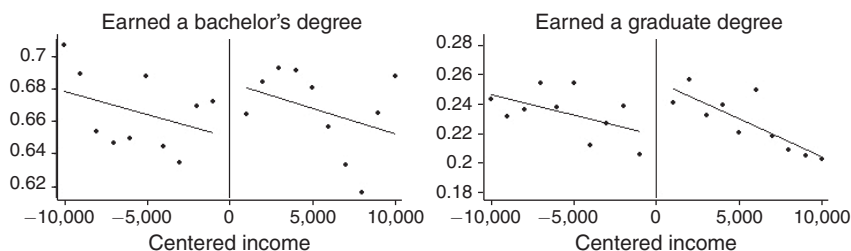


FIGURE 3. DEGREE ATTAINMENT RESULTS

Notes: This figure depicts the proportion of students who earned a bachelor's degree or a graduate degree, based on National Student Clearinghouse data for students whose FAFSA indicates strong NSC coverage. Panel A bins students by 0.03 GPA points relative to the year-specific eligibility threshold, pooled across years. Panel B bins students by \$1,000 relative to the year-specific eligibility threshold, pooled across years. Income is reversed so that values above the cutoff represent lower family incomes; in both panels, crossing the threshold from left to right indicates becoming eligible for the Cal Grant.

online Appendix Table A3.²⁰ In online Appendix Tables A4, A5, and A6, we show that our results are robust to several factors: alternate bandwidths and the Calonico, Cattaneo, and Titiunik (2014) optimal bandwidths; covariates and functional forms; and triangular kernels, respectively. In the top panel of online Appendix Table A11, we present results for low-income and middle-income students at the GPA threshold separately, and high-GPA and low-GPA students at the income threshold separately. In online Appendix Table A12, we present results split by student gender and application year.

The first two rows of Table 2 show results on degree attainment, with corresponding graphical results presented in Figure 3. At the GPA threshold, reduced form estimates show that the likelihood that students achieved a bachelor's degree increased by 4.6 percentage points. The IV results suggest a 14.6 percentage point increase in bachelor's degree completion. Undergraduate aid has additional long-term educational impacts, raising graduate degree completion at the GPA

²⁰ Although attendance results using NSC data are relatively similar in magnitude as results based on 1098-T data, in some cases they lack statistical significance due to the smaller sample sizes that result from focusing on the subsample of students interested in colleges well covered by NSC data.

discontinuity by 3.1 and 9.7 percentage points in the reduced form and IV estimations, respectively. Although we do not show results here, the increased graduate degree completion at the GPA margin also occurs almost exclusively within California colleges rather than out of state.

At the income threshold, we find roughly similar results on bachelor's degree completion, with reduced form and IV estimates showing a 3.0 and 7.6 percentage point increase, respectively. Similar to the GPA threshold, we find positive OLS and IV effects on graduate degree completion of 2.9 and 7.5 percentage points, respectively. None of these effects are statistically significant at the 5 percent level, as regressions are restricted to students who only favor NSC-covered institutions, significantly reducing our power.

Next, we examine the effect of financial aid on traditional measures of college attendance. Cal Grant eligibility had no meaningful impact on overall attendance at a postsecondary institution at either the GPA or the income thresholds (Table 2, row 3). The immediate college-going rates for students at the GPA and income thresholds are 83 and 93 percent, respectively (Table 1), with eventual college-going rates of well over 90 percent for both groups. These attendance results differ from those reported in Kane (2003), and we discuss the likely sources of these differences in online Appendix 1.

At the GPA discontinuity, we find few shifts in the institution sector that students attend (Table 2, columns 2–3, rows 4–6). These students are predominately choosing among very few options, generally consisting of a community college or a lower tier four-year institution (e.g., California State University). Although there is no initial impact on attendance or institution sector, a number of results suggest that the degree completion increase we document above is in part due to increased persistence among those who would have attended college in the absence of aid. Specifically, Cal Grant does not impact the likelihood of initially enrolling in a four-year institution, but it does impact the likelihood of being in a four-year institution three and four years after high school graduation (online Appendix Figure A6).

In contrast, we find large shifts in the sector of college attendance at the income threshold (Table 2, columns 5–6, rows 3–6, and Figure 4). Cal Grant eligibility leads to a statistically significant 5.6 percentage point increase in private school attendance, coupled with significant decreases in four-year public and community college attendance. Because students can enroll in more than one sector within four years of application, these RD estimates need not sum to one across sectors. Online Appendix Figure A7 shows that the effects for four-year public and private college attendance are fairly stable within the one to four years after Cal Grant application. In results not shown, we find that the negative effect on attending a community college is concentrated in the first two years after application.²¹

²¹ Results in online Appendix Table A12 show educational impacts disaggregated by student sex and application year. Although there is suggestive evidence for larger impacts for some outcomes and groups (e.g., females at the GPA threshold may be more likely to earn bachelor's degrees than males), most of the results are fairly similar across groups.

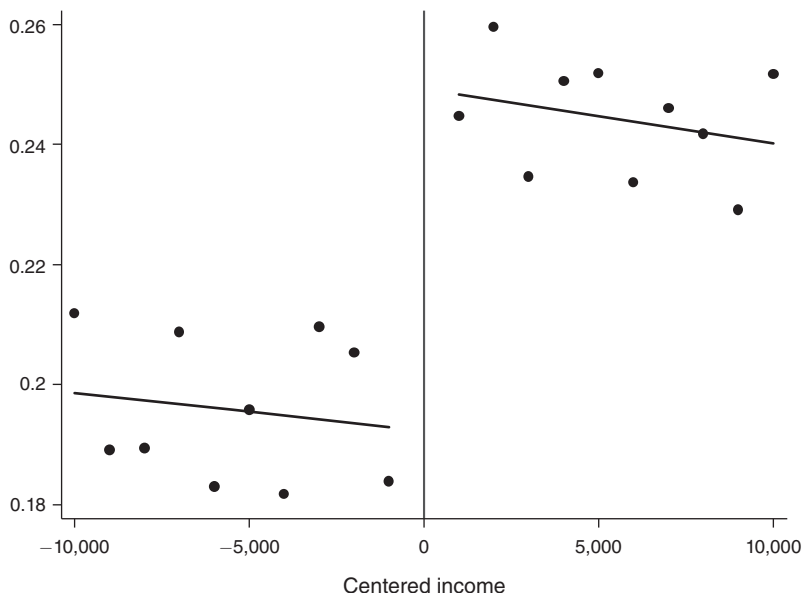


FIGURE 4. ATTENDS PRIVATE INSTITUTION, INCOME THRESHOLD

Notes: This figure plots the proportion of students around the income threshold who attended a private institution at any point between one and four years since their Cal Grant application, pooled over cohorts. Students are binned by \$1,000 relative to the year-specific eligibility threshold. College attendance is based off of Form 1098-T, and institution types are derived using IPEDs data. Income is reversed so that values above the cutoff represent lower family incomes; crossing the threshold from left to right indicates becoming eligible for the Cal Grant.

TABLE 3—LONGER RUN INCOME AND DEMOGRAPHIC OUTCOMES, 10–14 YEARS AFTER APPLICATION

	GPA threshold			Income threshold		
	Observations	Control mean	Reduced form	Observations	Control mean	Reduced form
Filed a tax return	157,500	0.93	0.004 (0.005)	90,485	0.96	−0.006 (0.005)
log labor income	138,932	11.07	0.054 (0.019)	81,651	10.49	−0.020 (0.024)
Income percentile	157,442	41.89	1.334 (0.491)	90,375	50.83	−0.483 (0.672)
log household size-adjusted AGI	144,369	10.50	0.032 (0.016)	84,999	10.77	−0.005 (0.022)
Lives in California	146,027	0.90	−0.004 (0.007)	85,991	0.86	0.024 (0.011)
Married	146,424	0.53	−0.014 (0.010)	86,162	0.57	0.005 (0.013)
Has kids	146,027	0.48	−0.023 (0.010)	85,991	0.41	0.003 (0.012)

Notes: This table presents estimates of the effect of Cal Grant eligibility on long-run outcomes based on administrative tax data. Regressions at the GPA threshold include income-eligible applicants within 0.3 points of the GPA threshold, and regressions at the income threshold include GPA-eligible applicants within \$10,000 of the income threshold. All regressions include student age, parental education, parental marital status, citizen status, and year-by-family size fixed effects. A linear function of the running variable and a uniform kernel are used. Standard errors are clustered by GPA for GPA threshold regressions and are heteroscedasticity-robust in income threshold regressions. Reduced form control value means are average values for students within 0.05 points below the GPA threshold or within \$1,000 above the income threshold.

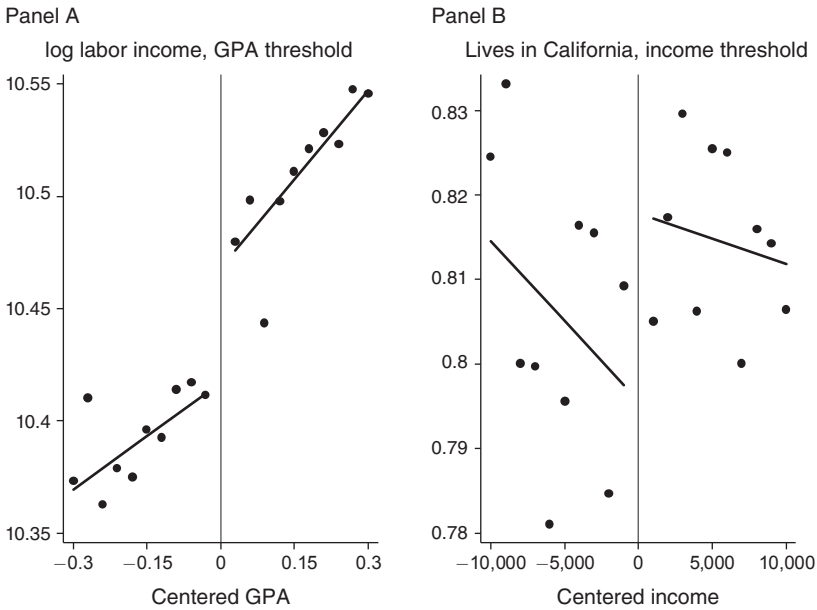


FIGURE 5. SELECT LONGER RUN OUTCOMES, 10–14 YEARS AFTER APPLICATION

Notes: This figure plots the average log labor income (defined as the sum of W-2 wage and salary income and 1099-MISC nonemployee compensation) for students around the GPA threshold, and the proportion of students living in California around the GPA threshold, stacked over 10–14 years after Cal Grant application and pooled over cohorts. Panel A bins students by 0.03 GPA points relative to the year-specific eligibility threshold; Panel B bins students by \$1,000 relative to the year-specific eligibility threshold. Income is reversed so that values above the cutoff represent lower family incomes; in both panels, crossing the threshold from left to right indicates becoming eligible for the Cal Grant.

B. Long-Run Earnings, Family Formation, and Mobility

We now turn to Cal Grant's impacts on earnings, family formation, tax filing status, and likelihood of living in California. Main results are in Table 3, with robustness tests in online Appendix Table A7, A8, and A9, which match online Appendix Tables A4, A5, and A6, respectively. In the bottom panel of online Appendix Table A11, we present results for low-income and middle-income students at the GPA threshold separately, and high-GPA and low-GPA students at the income threshold separately. In online Appendix Table A13, we present results split by student gender and application year. For all outcomes, we run stacked regressions for 10 through 14 years after the student applied for a Cal Grant, when most applicants would be between 28 and 32 years old. Standard errors in these specifications are clustered at the individual level. We first test whether Cal Grant eligibility impacts the likelihood that a student files a tax return (Table 3, row 1). We find economically small and statistically insignificant differences in filing propensities at both discontinuities. In online Appendix Figure A8, we show year-by-year point estimates of the likelihood of filing a return. In all cases, the effects on filing a return are statistically insignificant, with the largest values approximately 1 percentage point. At the GPA discontinuity, our estimated effect of Cal Grant eligibility on tax filing is 0.4 percent (relative to a mean of 93 percent), and we can reject impacts larger than 1.3 percentage points.

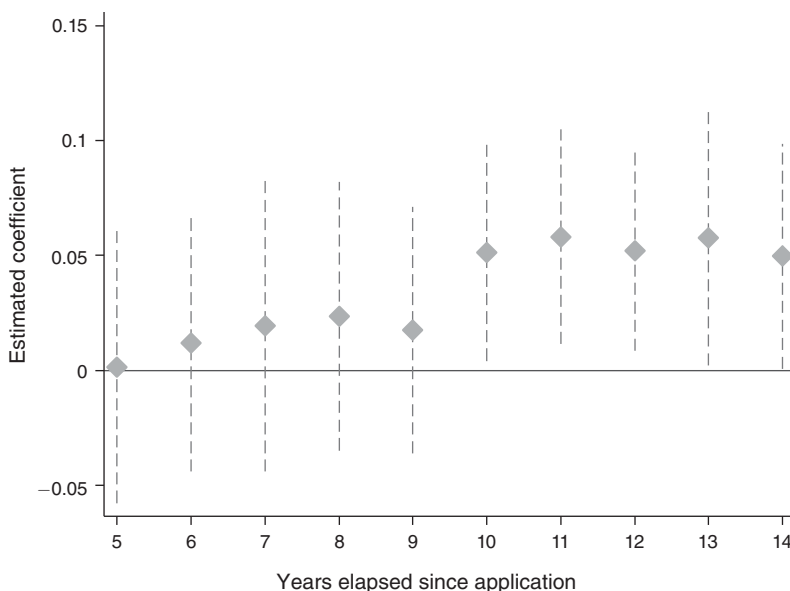


FIGURE 6. LOG LABOR INCOME, GPA THRESHOLD

Notes: This figure plots the regression coefficients and 95 percent confidence intervals on a series of interactions between Cal Grant eligibility and years elapsed since application to the Cal Grant. The dependent variable is the log of labor income (defined as the sum of W-2 wage and salary income and 1099-MISC nonemployee compensation). The regressions include students within 0.3 GPA points of the GPA threshold. The regressions include the student's age, a dummy for parental college attainment, a dummy for US citizenship, a dummy for parents being married, family size by year fixed effects, and zip code fixed effects. Standard errors are clustered by the running variable.

We find evidence that individual earnings increase at the GPA threshold, though estimates are noisy. Cal Grant eligible students rise by 1.33 percentile points in the income distribution of earners in their cohort around age 30, although this result is not particularly robust to alternative bandwidths. When we use log earnings as the dependent variable, we estimate that Cal Grant eligibility increases earnings by roughly 5.4 percent. This result is presented graphically in panel A of Figure 5. This 1.33 percentile point earnings increase translates into about a \$1,800 increase in earnings, or about 4.9 percent of earnings at the mean of \$37,000. Results using earnings rank (percentile) differ somewhat from results using log earnings because the latter excludes individuals with zero earnings and imposes a different functional form. There is also a positive impact on the log of household-size-adjusted AGI, with an increase of 3.2 percent; however, this effect is not statistically significant. We examine the wage income effects at the GPA threshold for each year post-application, and there is additional evidence suggesting impacts on earnings (Figure 6). The point estimates for years 8–14 are all positive and trending upward, with statistically significant effects in years 10 through 14. We find no impacts on labor income and AGI at the income threshold (Table 3, column 6, rows 2–4, and online Appendix Figure A9). In particular, the estimated effects on the log of household-size adjusted AGI and the log of labor income averaged over years 10 through 14 are both near 0.

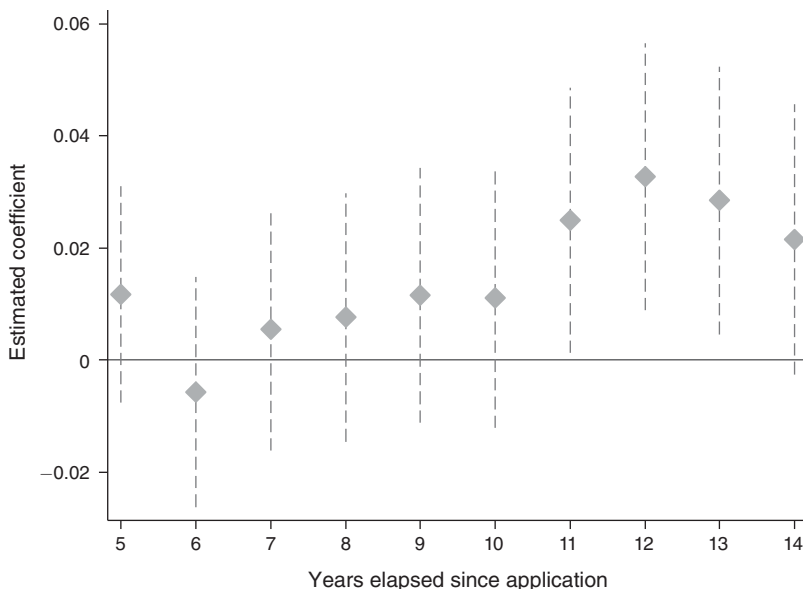


FIGURE 7. RESIDENCY RESULTS, INCOME THRESHOLD

Notes: This figure plots the regression coefficients and 95 percent confidence intervals on a series of interactions between Cal Grant eligibility and years elapsed since application to the Cal Grant. The dependent variable is an indicator variable for being a California resident, based on the filing address on a tax return. The regressions include students within \$10,000 of the income threshold. The regressions include the student's age, a dummy for parental college attainment, a dummy for US citizenship, a dummy for parents being married, family size by year fixed effects, and zip code fixed effects. Standard errors are heteroscedasticity robust.

The point estimates are negative, and at some alternative bandwidths, the negative effects are statistically significant (online Appendix Table A7). Although we emphasize that our estimates are imprecise, the lack of positive earnings effects is consistent with the fact that students at the income discontinuity are being induced into attending moderately selective or nonselective four-year private colleges, largely at the expense of four-year public institutions. We return to this issue of impacts on school quality in the next section.

In the fifth row of Table 3, we examine impacts of Cal Grant eligibility on geographic mobility. At the income discontinuity, we see a 2.4 percentage point increase in the likelihood of remaining in California, with the graphical result presented in panel B of Figure 5, and year-by-year results plotted in Figure 7. Results on residency are particularly relevant, as these students are the highest earners in our sample, and so might provide California the greatest marginal return in increased tax revenues. This result is sensitive to functional form assumptions and bandwidth selection; in online Appendix Table A7, this effect is only statistically significant at the 5 percent level in 4 of 7 specifications, with most specifications varying between 1.0 and 2.4 percentage points. At the GPA discontinuity, we do not find any impact of the Cal Grant on remaining within California 10 to 14 years after award receipt; year-by-year results are plotted in online Appendix Figure A10. Although this may be evidence against merit aid impacting out-migration, it also suggests that the

additional graduates produced by the award are as likely to remain within the state as other graduates.

The last two rows of Table 3 examine family formation by estimating whether aid eligibility impacts whether students are married or have kids in their late twenties to early thirties. Our point estimates generally indicate that there was no significant impact of the Cal Grant on any of these household formation outcomes. The exception is a significant 2.3 percentage point reduction in having children at the GPA threshold. These results do not appear to be robust, however, ranging from less than -0.0001 to -2.3 percentage points across bandwidths and model specification (online Appendix Table A7), and are rarely statistically significant.²²

C. School Quality and Financing of College Costs

The previous subsections suggest distinct impacts of the Cal Grant on the two subpopulations that we examine: students at the lower bound of GPA eligibility see improvements in college degree attainment and longer-run earnings, while eligible applicants who are the most financially well-off shift from attending public institutions to private institutions, with a corresponding increase in college degree attainment but no impact on longer-run earnings. To further understand these different experiences, we first examine how Cal Grant eligibility impacts traditional measures of college environment and quality in the top panel of Table 4. All college characteristics measures are derived from IPEDS data on institutional characteristics based on the first postsecondary institution attended.²³

Although we find no large shifts in the sector of postsecondary attendance at the GPA threshold, there is suggestive evidence of some movement toward higher quality colleges, with small increases in graduation rates, median freshmen SAT scores, and research expenditures per full-time equivalent. Changes in the measures of postsecondary environment are much larger at the income threshold, as the shift toward private colleges results in students attending smaller but more expensive colleges. Interestingly, these changes do not correspond to increases in measures of school quality, such as time to graduation or median freshmen SAT score, and are associated with *lower* per-student expenditures, particularly in the areas of academic support and research.²⁴ These findings suggest that students at the income threshold use the Cal Grant to attend more expensive private institutions that offer no discernable upgrades in school quality.

²²Results in online Appendix Table A13 show impacts on long-run income and demographic outcomes disaggregated by student sex and application year. There is suggestive evidence for larger impacts for some outcomes and groups (e.g., there is a larger estimated earnings gain for males at the GPA threshold). At the income threshold, we find a positive effect of Cal Grant eligibility on being married and having children for men, and a negative effect for women.

²³We apply estimates from 2001 IPEDS data to each year in our sample as most values are consistent across years and there is a higher proportion of missing data in earlier IPEDS years. The graduation rate is the percent of students completing a degree within 150 percent of normal time, irrespective of degree type (i.e., associate's degree completion at two-year schools and bachelor's degree completion at four-year schools).

²⁴We also examined whether Cal Grant recipients attend colleges with different mean earnings as measured by the "College Scorecard." We do not find any evidence of differences in scorecard earnings for Cal Grant recipients. For instance, at the GPA discontinuity, Cal Grant recipients attend colleges with College Scorecard earnings that are \$354 per year higher (measured 10 years after college entry). At the income discontinuity, the effect is \$54 per year.

TABLE 4—COLLEGE FINANCING AND COLLEGE CHARACTERISTICS

	GPA threshold			Income threshold		
	Control mean	Reduced form	IV	Control mean	Reduced form	IV
<i>College characteristics</i>						
School size	19,916	232.800 (221.467)	565.622 (528.308)	20,727	-772.875 (319.756)	-1,822.013 (753.593)
Tuition	3,787	136.277 (121.892)	331.104 (292.823)	6,067	903.470 (238.533)	2,129.993 (553.801)
150% time to graduation	0.43	0.006 (0.003)	0.015 (0.007)	0.55	0.005 (0.006)	0.011 (0.014)
“Median” SAT score	1,046	4.741 (2.887)	9.016 (5.445)	1,122	-0.015 (4.067)	-0.034 (9.024)
Instruction expenditure per FTE	6,115	113.158 (94.358)	266.986 (218.982)	8,914	-103.078 (187.745)	-239.890 (437.574)
Academic support per FTE	3,633	-2.886 (65.909)	-7.011 (158.777)	4,629	-353.738 (110.637)	-833.920 (263.080)
Research expenditures per FTE	1,729	143.911 (80.266)	339.545 (185.389)	4,344	-396.480 (213.108)	-922.718 (498.708)
Expenditures per FTE	16,972	163.892 (316.172)	386.688 (737.474)	25,718	-820.677 (823.184)	-1,909.939 (1,919.428)
<i>College financing</i>						
Claims student loan	0.70	-0.006 (0.009)	-0.016 (0.023)	0.74	-0.002 (0.013)	-0.004 (0.030)
Student loan amount	22,115	387.268 (946.225)	1,003.287 (2,424.087)	27,809	1,677.269 (1,315.716)	3,991.815 (3,118.000)
Has Pell Grant	0.14	-0.011 (0.009)	-0.029 (0.022)	0.03	-0.011 (0.006)	-0.027 (0.014)
Pell Grant amount	959.9	-25.424 (18.367)	-65.865 (47.423)	58.6	-13.826 (9.358)	-32.904 (22.343)
Claims federal education tax credits	0.30	-0.001 (0.010)	-0.004 (0.026)	0.51	-0.004 (0.014)	-0.008 (0.033)
Federal education tax credit amount	483	4.562 (33.908)	11.817 (87.099)	617	73.436 (58.345)	174.774 (138.638)

Notes: This table presents estimates of the effect of Cal Grant eligibility on the college quality and financing measures. The IV regressions instrument for receiving a Cal Grant with eligibility status. The dependent variables in the top panel are derived from IPEDs data that are matched to the first Form 1098-T matched to each applicant after their Cal Grant application (within 4 years). The dependent variables in the bottom panel come from National Student Loan Data System or from the tax return of either the applicant or her parent. Regressions at the GPA threshold include income-eligible applicants within 0.3 points of the GPA threshold, and regressions at the income threshold include GPA-eligible applicants within \$10,000 of the income threshold. There are 31,500 and 18,097 observations at the income threshold, respectively. All regressions include student age, parental education, parental marital status, citizen status, and year-by-family size fixed effects. A linear function of the running variable and a uniform kernel are used. Standard errors are clustered by GPA for GPA threshold regressions and are heteroscedasticity robust in income threshold regressions. Reduced form control value means are average values for students within 0.05 points below the GPA threshold or within \$1,000 above the income threshold.

To further explore the relationship between sectoral shifts and student outcomes, we test whether degree completion rates vary by self-reported college type preferences. We disaggregate applicants into three groups based on the types of schools that they listed on their FAFSA form: students who listed only four-year public schools; students who listed any private schools; and the remainder of

students, including the majority of students who listed a community college.²⁵ Online Appendix Table A10 presents results on the type of institution ultimately attended and degree attainment for each group separately. Although the estimates are often imprecise, the results suggest that bachelor's degree completion (column 4) is not restricted to any specific group. In particular, students who list private colleges on the FAFSA exhibit the biggest shifts in the sector of postsecondary enrollment, yet their degree completion rates are not consistently larger than any other group. For example, students at the GPA margin who list a private college on the FAFSA are 2.3 percentage points more likely to attend a private college but only 0.3 percentage points more likely to earn a bachelor's degree, whereas students who only list four-year public colleges are close to 8 percentage points more likely to earn a bachelor's degree. For students at the income threshold, showing some preference for a private institution is predictive of attending one, but this does not translate into significantly higher degree completion effects. The point estimates suggest that degree completion effects are largest for this group, so while there may be positive impacts of private college attendance, they do not singularly drive the observed increases in degree completion. Online Appendix Table A11 shows similar results that disaggregate the sample into high and low GPA at the income threshold, finding that bachelor's degree completion shifts more for low-GPA students, even though both groups exhibit large shifts toward private schools.

An implication of the sectoral shifts that result from the Cal Grant is that financial aid might not lower the total cost of college that students face. Both the shift into more expensive institutions at the income threshold and increased postsecondary persistence at the GPA threshold may result in higher expenditures. The bottom panel of Table 4 examines whether there are other sources of financial relief that are utilized, namely federal loans and grants received, and federal education tax credits taken. Although results are noisy, there are no decreases in total student loans, with statistically insignificant but positive increases in observed loan amounts, particularly among students at the income threshold. Our lack of an impact on loans contrasts with Chapman (2015), who looks across merit aid programs in 13 states and finds that merit aid reduces total loans by about \$6,000 total. However, our findings are consistent with Scott-Clayton and Zafar (2016) and Denning, Marx, and Turner (forthcoming), who find no increase in cumulative total loans. At the GPA cutoff, we find no changes in Pell Grant dollars received; the "first-dollar" structure of the Cal Grant program should theoretically leave Pell Grants unaffected, and there is no reason for students to forgo this aid that can help pay for room and board or other expenses. At the income cutoff, Cal Grant eligibility lowers Pell use by 1.1 percent, which is statistically significant at the 10 percent level.²⁶ Lastly, we find that there is no impact on the take-up of federal education tax credits or the amount of credits that are claimed in years one through four after application.

²⁵We lose about 1.5 percent of the sample due to missing FAFSA preferences or unidentified schools.

²⁶The coefficient at the GPA cutoff is similar in magnitude. But at the GPA cutoff, the estimate is both noisy and much smaller when scaled by average Pell Grant use at the GPA cutoff.

Overall, the large shifts in the sector of postsecondary enrollment at the income threshold are not necessarily indicative of improvements in postsecondary environment. The types of private institutions attended at this threshold are generally mid-tier, denominational schools, rather than California's top-ranked private institutions. As a result, these shifts have unclear impacts on labor force outcomes. For high-performing students, these shifts could increase degree completion while simultaneously decreasing human capital investment.²⁷ In contrast, bachelor's degree completion for students at the GPA threshold appears to represent an unambiguously positive impact, rather than a tradeoff in the quality of bachelor's degree earned.

D. Cost-Benefit Discussion

In order to provide a cost-benefit analysis for the Cal Grant program, we must first estimate the total cost of the program for the marginal student. Using data on total payments for each individual, the RD specification indicates that the marginal student received total payments across all years of \$4,311 at the GPA discontinuity and \$8,115 at the income discontinuity. This is substantially lower than the potential cost of roughly \$36,000 per student, which would be the case if all individuals received the full four years of private subsidy. The net costs are lower because not everyone above the threshold qualifies for the award, many do not attend more expensive private schools or choose to not use it, and some students leave college without using all four years of payments.

The implied wage gains at the GPA discontinuity suggest large benefits for this subset of Cal Grant recipients. We find wage increases of 5.4 percent for the entire group of Cal Grant winners. If labor earnings average \$39,000 (Table 1), then the Cal Grant potentially creates a 40-year annuity of almost \$2,106 (i.e., $\$39,000 \times 0.054$). At a 3 percent real discount rate, this annuity is worth roughly \$48,700 per recipient. This annuity is far in excess of the \$4,000 per recipient average cost. In fact, if we multiply the likelihood of remaining in California by the marginal tax rate, the program would potentially pay for itself. This calculation, of course, assumes no general equilibrium effects on wages. If we instead use our estimate of a 1.33 percentile point increase in the wage distribution, this corresponds to an annuity of roughly \$1,800, which is worth roughly \$42,800.

The estimated wage gains are high and imprecise. We can also consider cost-benefit calculations using our estimated impacts on degree receipt. Our reduced form point estimate is that Cal Grant eligibility raises bachelor's degree receipt by 3 to 4 percentage points. The reduced form impacts vary across the two thresholds and suggest that the cost per degree differs: \$93,717 ($\$4,311/0.046$) at the GPA threshold, and \$270,500 ($\$8,115/0.030$) at the income threshold. These results suggest cost-inefficiencies for offering financial aid to theoretically less-prepared, lower GPA students, as they attend less expensive postsecondary

²⁷ Regressions that use as outcome variables the interaction between earning a bachelor's degree (0/1 dummy) and a continuous measure of degree quality such as graduation rate or expenditures are statistically insignificant, though point in the expected direction.

institutions and, at baseline, are less likely to remain enrolled for at least four years. Obviously, these estimates ignore any benefits that derive from earning a graduate degree or other unobserved effects.

Because we find a larger response for lower-income, modest-GPA students, our results suggest that targeting aid to lower-income students (perhaps with some academic requirement) can be more efficient than broader aid programs. Further, online Appendix Table A11 shows that these positive earnings effects are concentrated among the students coming from middle-income households, again highlighting that targeting is an important consideration for the design of financial aid programs. In particular, we suspect that some of the students near the income threshold (i.e., higher income but still aid-eligible students) are induced to attend more expensive private schools that may not increase students' graduation probabilities or long-run earnings.

Using cost per degree as a primary metric, Cal Grant appears to be roughly as expensive as many other interventions studied in the literature. Financial aid studies using four-year college students and military veterans are roughly in line with our estimates (Barr 2016, Goldrick-Rab et al. 2016). Some studies focusing on need-based aid find estimates with a lower cost per additional bachelor's degree (Castleman and Long 2016; Mayer, Patel, and Gutierrez 2016). Earlier interventions such as Head Start and the Abecedarian experiments suggest costs of \$133,000 and \$400,000 simply to induce an additional child to attend college (Dynarski, Hyman, and Schanzenbach 2013), and degree completion impacts are not guaranteed to be any larger. All of these programs have significant implementation and support costs, whereas interventions targeting small administrative barriers to college enrollment appear to be significantly cheaper (Bettinger et al. 2012; Castleman, Page, and Schooley 2014; Hoxby and Turner 2013). Unfortunately, few of these studies have yet been able to assess long-term impacts on completion.

Using conventional returns to degree estimates, we note that moving an adult from some college to a bachelor's degree might raise earnings by an annuity of \$20,000 for 40 years for a net present value of around \$462,000 at a 3 percent interest rate. This compares favorably to the cost per additional degree of \$81,000 to \$192,000. This back-of-the-envelope calculation suggests that Cal Grant's increased graduation rates could easily "pay" for the program if we think of program costs as being more than offset by the increased earnings. This analysis is obviously highly simplistic because Cal Grant is really a transfer, just as the increased earnings could be a transfer from one worker to another as opposed to a societal gain. Importantly, the Cal Grant is largely a transfer from taxpayers to students and their families. In other words, the Cal Grant is not a pure deadweight loss but rather a transfer that may or may not have a deadweight loss. Even if the earnings gains for the average student are smaller than the costs of administering the program, the program could still be welfare enhancing.

IV. Conclusion

State sponsored merit- and need-based aid constitutes one of the most important and fastest growing sources of student assistance for postsecondary education.

Cal Grant eligibility raises bachelor's degree attainment by 3 to 4.6 percentage points, with estimates based on award utilization over twice as large. Importantly, we do not find strong evidence that the increase in degree completion is strongly related to the sector of postsecondary enrollment. Our results suggest that large increases in degree completion occur primarily from increased persistence among students who begin in four-year institutions, rather than through changes in the postsecondary sector. In fact, aid eligibility produces significant shifts from public to private universities for some students, but does not appear to drive any significant degree increases for this group above and beyond what is observed for other students.

We document that financial aid has additional effects on students' outcomes long after they have completed their undergraduate education. First, students at the GPA-eligibility margin are induced to complete graduate school by an additional 3 percentage points, an increase of roughly 26 percent. These findings show that financial aid can have a causal impact on additional human capital investment, particularly for lower-skilled students, perhaps through reducing debt that might prevent a student from temporarily exiting the workforce to pursue their graduate education. Another key insight is the long time frame required to estimate these results, lending support to the importance of a life-cycle approach to estimating the returns to aid. Year-by-year analysis suggests that the graduate degree effect only becomes statistically significant seven years after entering college.

Our results also suggest positive impacts on in-state residency and earnings. These findings confirm the notion that state-based financial aid programs can improve state finances, and the costs of these programs may be overestimated without taking into account mobility and labor force outcomes. However, we cannot say with precision how the changes in institution type and bachelor's degree attainment translate into effects on lifetime earnings, as our standard errors incorporate much smaller earnings gains. Given our 3 to 4.6 percentage point increase in bachelor's degree attainment, a 5.4 percentage point increase in earnings would imply a very large return to bachelor's degree receipt if that were the main channel for the earnings effect. However, it is quite possible that Cal Grant eligibility impacts earnings through many channels, as it induces students to attend graduate school and shifts which institutions are attended. We also cannot observe other potential changes, such as increased time studying or other extracurricular activities (e.g., internships) that might become available as students are offered aid. More data and longer follow-up periods will be needed to truly understand earnings impacts.

One concern with our analysis is that all of these effects may be particular to the institutional context of California. California is a geographically large and diverse economy making out-migration already less likely than migration from smaller states. More importantly, Cal Grant is offered on top of a highly subsidized and broad-reaching public university and community college system. Smaller states with lower college-going rates might see larger impacts on college-going and out-migration (e.g., the large impacts on college attendance from Georgia's HOPE program, Dynarski 2000). Equally important is that our inferences are restricted to a particular set of applicants: a set of students who have taken the time to file a FAFSA form and a Cal Grant application, and virtually all participate in college

at some point following high school. Our estimates are also restricted to students at the eligibility cutoffs, whereas the largest effects on attendance and persistence might be concentrated on very low-income students, who are least likely to attend college. However, the Cal Grant award appears well designed, as we show that the “first-dollar” structure does not crowd out other forms of financial aid such as Pell or federal tax credits. Alternative aid programs without these safeguards could produce smaller reduced form estimates than those observed in our study. Overall, our results suggest that financial aid does indeed produce long-term impacts on both educational and labor force outcomes, and that states should consider these potential benefits when designing or weighing the explicit costs of these programs.

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