

# THE LONG RUN IMPACTS OF MERIT AID: CALCULATIONS FROM CALIFORNIA'S CAL GRANT

PRELIMINARY. DO NOT CITE.

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## Abstract

We examine the impacts of being awarded a Cal Grant, which is among the most generous of U.S. state based merit aid programs for high school graduates. We instrument for Cal Grant receipt using high school GPA and family income cutoffs for eligibility which are time varying and *ex ante* unknown to applicants. Cal Grant receipt has only modest impacts on the choice of institution and degree completion, with reduced form estimates of four-year attendance, private school attendance, and Bachelor's degree completion ranging from zero to four percentage points depending on the population studied. Receiving a Cal Grant significantly reduces total student loans while in school. Although merit-aid programs aim to decrease outmigration of college-educated individuals, Cal Grant receipt does not have statistically significant impacts on the likelihood of living in California at age 30. Measured earnings impacts are imprecise with IV estimates of award utilization ranging from -10 to 15 percentage point increases in earnings.

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## **I. Introduction**

Over the last twenty years, the United States has gone from being the world leader in the percentage of high school students that go on to graduate with a B.A. or other four-year college degree to ranking 19<sup>th</sup> in the world.<sup>1</sup> Increasing college enrollment and completion is a major goal of the Obama Administration, state governments, and schools and policy makers at all levels.

Need- and merit-based aid is perhaps the most visible policy lever that states use to offset tuition and other costs. State aid programs have become more prominent over the past two decades, with funding increasing by 83% from 2002 to 2012 (NASSGAP, 2012). Merit-aid programs in particular have expanded from Arkansas and Georgia in the early 1990s to over twenty state programs (Domina, 2014; Doyle, 2006). Such financial aid programs have a variety of goals including decreasing the net cost of attendance, reducing “brain drain” out of state, and making salient the fact that college attendance can be low cost or tuition free for large groups of targeted students (see, for example, Dynarski 2000; Scott-Clayton 2011; and Goodman and Cohodes 2014).

The estimated effects of merit aid vary significantly across states, and by the institutional context and program details. Dynarski (2000) finds that Georgia’s HOPE scholarship raised college attendance by a full 7 to 8 percentage points, whereas Cohodes and Goodman (2014) find that Massachusetts’ Adams Scholarship actually lowered overall college graduation by attracting students to in-state public institutions with lower graduation rates.

There is relatively little research to date that would allow financial aid granting institutions to measure their return on investment. Given that billions of dollars are spent each year between federal programs that target needy students (e.g., Pell Grant, work study), state programs that predominately focus on academically meritorious high school graduates, and the untold number of philanthropic and corporate scholarships, more careful analysis is needed to judge whether these programs are well-designed, especially as this diverse patchwork of programs may be working at cross-purposes (Turner, 2014). Instead, causal impacts of financial aid have been predominately restricted to short-term college attendance and bachelor degree completion outcomes, even though recent work in other areas, such as early childhood education and class size suggest that a program’s long-term impacts may swamp short-term gains (Chetty et al., 2011; Dynarski, Hyman, & Schanzenbach, 2013). The ultimate returns of financial aid require policymakers to observe a more diverse set of outcomes, which would include how aid impacts labor force decisions, mobility, health, family formation, and other economically critical decisions. Clearly, this requires the ability to follow students over a much longer time-frame than has previously been available.

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<sup>1</sup> *OECD Education at a Glance* 2014.

We examine impacts from California's Cal Grant program, one of the largest and most generous state merit aid programs as measured by number of students and overall expenditure.<sup>2</sup> The Cal Grant system contains a number of important features that position it as the preeminent source of information on financial aid's long-term impact. Administrative, individual-level data on Cal Grant applicants exist beginning with the high school graduating cohort of 1998, which allows us to track students for over fifteen years after they enter college. Given that the two best administrative data sources for college-going – National Student Clearinghouse and 1098-T tax forms – only become available and reliable around this time period, these data are likely to serve as the best source of aid's long-term impacts on degree completion available in the United States. In contrast to many other state aid programs, Cal Grant can be applied to tuition at any in-state public or private institution. Tuition at public institutions is completely covered, and private school tuition is subsidized between nine to ten thousand dollars per year.

The Cal Grant also presents an ideal opportunity for analysis because eligibility is based upon a series of strict cutoffs in family income and high school GPA. Crucially, in the years of our analysis, the location of these cutoffs was not known to applicants ahead of time. We use these discontinuities to define two subpopulations of interest: (1) students whose family incomes lie below the income cutoff, but whose student GPAs are near the minimum GPA cutoff; and (2) students who meet the minimum GPA requirement, but whose family incomes are near the income threshold. We estimate the impact of the Cal Grant on a variety of outcome variables using a regression discontinuity design. We improve on Kane's (2003) earlier analysis of the Cal Grant by using a larger sample, a longer follow-up period, and a broader set of outcomes than previously available. Specifically, we combine Cal Grant application and receipt data with data from the National Student Clearinghouse, administrative tax returns, and federal student loan data to estimate impacts of the Cal Grant on college enrollment and completion, student loans, earnings and employment status, geographic mobility, and family formation.

We find that Cal Grant receipt has no effect on overall college attendance, in part due to college-going rates among this population being quite high. At the income discontinuity, we find shifts in the type of college a student attends: attendance at a four-year private institution increases by 4 percentage points, with an offsetting reduction in attendance rates at a public California university. Accompanying these shifts towards private institutions, we find that conditional on going to a four-year private institution, the Cal Grant significantly reduces total federal student loans. The Cal Grant also raises graduation rates by 3 percentage points at the income discontinuity. We do not detect any evidence of shifting of institution type near the GPA threshold, although we do find that the Cal Grant significantly increases the probability of earning a graduate degree among this relatively lower-achieving population by over 2 percentage points (roughly 15%). Point estimates on earnings suggest that there is no impact of the Cal Grant on earnings at ages 25-30, however, the estimates are quite imprecise.

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<sup>2</sup> For example, the Cal Grant awarded over \$1.6 billion in grants for the 2013-14 academic year.

There are many policies aimed at easing the financial constraints of higher education at the federal level, such as the Pell Grant, federal student loans, and education tax credits. Understanding the impacts of programs like the Cal Grant can inform the design of other federal student aid programs. In addition, our analyses inform the extent to which state-based aid programs impact the utilization of these other sources of aid.

## II. 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, which can alter students' cost-benefit calculus and induce additional students to enroll and persist. Indeed, the literature has documented positive effects of financial aid on attendance, persistence, and completion (Bettinger, 2004; Bettinger, Long, Oreopoulos, & Sanbonmatsu, 2012; Dynarski, 2003; Goldrick-Rab, Harris, Kelchen, & Benson, 2012; Hoxby & Turner, 2013; Kane, 2007; Scott-Clayton, 2011; Seftor & 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 reducing state expenditures, such as lowering time to degree, and increasing students' economic contribution by one or more years (Domina, 2014; Henry & Rubenstein, 2002; Pallais, 2009).

Second, merit aid may directly affect college attendance and completion rates through: a) reducing liquidity constraints that prevent students from attending, b) enabling students to travel farther to better institutions, c) decreasing the need to work during college, thus allowing students to concentrate more on their studies. 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 & Long, 2013; Cornwell, Mustard, & Sridhar, 2006; Dynarski, 2000, 2004, 2008; Kane, 2003; Scott-Clayton, 2011; Singell & Stone, 2002; Van Der Klaauw, 2002)).<sup>3</sup>

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, McCall, Ott, & Kim, 2010). The effects of state aid programs are likely to depend on program details such as minimum academic thresholds, income limits that target aid toward specific populations, the size of the award, the renewal requirements while in college, and other dimensions that might

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<sup>3</sup> Only a few of 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)).

influence utilization (Domina, 2014; Long, 2004; Sjoquist & Winters, 2014). As one example, Cal Grant provides larger tuition subsidies for private institutions than it does for public institutions, whereas most states provide either equal or smaller tuition payments (Domina, 2014).

Finally, a third goal of state-based programs is to decrease “brain drain” by increasing the likelihood that top-performing students stay locally for college and increase 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 & 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 et al., 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 30s, though estimated effects are generally small (Fitzpatrick & Jones, 2012; Sjoquist & Winters, 2013, 2014; Zhang & Ness, 2010). However the only study that relied on student-level microdata found no effect on long-term retention within Georgia (Sjoquist & Winters, 2013).

Our study is the first to construct a causal regression discontinuity estimate of merit-aid receipt on long-term mobility and employment outcomes. An additional strength is the timeframe currently available, which includes over a dozen years of follow up data to estimate academic and workforce outcomes. This longer timeframe is crucial for studying workforce outcomes, as individual earning profiles flatten significantly for individuals in their early 30s (Chetty, Hendren, Kline, & Saez, 2014; Haider & 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 tax records, to precisely determine the impacts of merit-aid on taxes paid and whether these benefits accrue back to the state that offered the aid. We compare these returns to the precise monetary amount spent on each student. Our results shed light on whether merit-based aid expenditures, which have totaled billions of dollars over the last few decades, are producing their intended effects.

### **III. Institutional Details, Research Design and Sample Construction**

#### *A. Overview of the Cal Grant Program*

The Cal Grant Entitlement 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 of 1998-99 through 2000-01. This award provided 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 \$10,000 depending on the year. 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 four-year enrollment.<sup>4</sup>

Baseline eligibility for the Cal Grant requires applicants to be a California resident (either a U.S. citizen, permanent resident, or eligible non-citizen), have no defaults on federal loans, and have not previously earned a Bachelor degree. Students must have submitted the FAFSA and a GPA verification form, which was to be completed by the school attended, by March 2<sup>nd</sup>.<sup>5</sup> The GPA verification form is completed by the high school and sent directly to CSAC. In addition, applicants are disqualified if their assets (excluding housing value) exceed some limit.<sup>6</sup>

The primary form of eligibility for a Cal Grant depends on a student meeting a minimum GPA requirement and being below specific income thresholds.<sup>7</sup> Importantly, these eligibility rules fluctuated because of changes in annual funding, resulting in several plausibly exogenous discontinuities in eligibility that we exploit. First, income-eligible applicants were ranked by GPA in descending order and were offered awards until funding was exhausted. This produced a GPA cutoff for eligibility 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, and are depicted in Figure 1. The second eligibility threshold is for students above the GPA cutoff, but who fell on either side of the designated income limits. These income limits varied from year to year using cost of living increases based on the California Constitution, and would have been almost impossible for families to calculate. Figure 2 shows that upper income limits in 1999 ranged from \$54,500 for family of three or fewer to \$68,700 for families of six or larger, and in 2000 ranged from \$59,000 to \$74,100 for the same categories. Simply meeting the income or GPA requirements is a sufficient but not necessary condition for receiving the Cal Grant. In addition, a student or their

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<sup>4</sup> California community college tuition was \$11 per unit in 1999-2000, which was the lowest rate in the nation.

<sup>5</sup> In practice, CSAC included all applications received by March 12<sup>th</sup>, to allow for potential complications in the mail.

<sup>6</sup> During our sample period dependent students and independent students with dependents were disqualified if they had assets (excluding housing value) between \$42,000 and \$52,774 (depending on the year). Independent students without dependents (other than a spouse) were required to have assets below \$20,000 and \$25,110 (depending on the year).

<sup>7</sup> CSAC also offers “Competitive” awards for non-traditional students that are based on a composite point index of various academic and background characteristics, but we do not study this award here.

family must also have sufficient “unmet need,” which is calculated based on a student’s potential expenses and expected family contributions.<sup>8</sup>

California expanded the Cal Grant program significantly in 2001-02, 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 publicized beginning in the 2002-03 academic year.<sup>9</sup> We find evidence that applicants were likely aware of the eligibility thresholds beginning in these years.<sup>10</sup> Thus, we restrict our analysis to applicants prior to the 2001-02 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 aid, as family background and academic preparation are correlated with the likelihood of receiving aid, the amount of aid students receive, 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 the Equation 1:

$$(1) \quad Y_{it} = \beta_0 + \beta_1 * Distance_{it} + \beta_2 * CG_{it} + \beta_3 * CG_{it} * Distance_{it} + X_{it} + \varepsilon_{it}.$$

In this regression,  $Y_{it}$  is an outcome of interest (such as college enrollment or earnings) for student  $i$  in year  $t$ ,  $CG_{it}$  is a variable that equals one if a student is Cal Grant eligible in year  $t$ , and  $Distance_{it}$  is a continuous running variable that determines assignment to treatment in year  $t$ , centered at the year-specific eligibility cutoff. We run these regressions separately for the GPA cutoff and for the income cutoff. We show a linear specification here, but  $Distance_{it}$  can take a flexible functional form that includes higher-order polynomials. The vector  $X_i$  may contain baseline observable characteristics including cohort, family composition, gender, family assets,

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<sup>8</sup> To calculate whether a student has unmet need requires three steps. First, a student has listed up to six schools on their FAFSA, and each is assigned a Cost of Attendance. Second, CSAC subtracts a student’s Expected Family Contribution from each school’s Cost of Attendance to create the unmet need value. For a student to be Cal Grant eligible, a student must have unmet need equal to the maximum Cal Grant award amount plus \$1500 (for A) or \$700 (for B).

<sup>9</sup> Correspondence with CSAC personnel indicates that 2002 was the first year that the “Fund Your Future Workbook” published the exact income limits.

<sup>10</sup> 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. We do not find strong evidence of violations around the GPA cutoff, but choose not to use these cutoffs at this time.

and mother and father education.<sup>11</sup> In practice the inclusion of observable characteristics  $X_i$  is optional; their inclusion should not result in significant changes to our estimation of  $\beta_2$ , though it can improve precision. 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.

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. Thus, the parameter of interest,  $\beta_2$ , represents the intent-to-treat parameter, or the causal effect of the offer of the Cal Grant award on our outcomes of interest.

We also run the following two-stage least squares regression:

$$(2) \quad \begin{aligned} Award_{it} &= \alpha_0 + \alpha_1 * Distance_{it} + \alpha_2 * CG_{it} + \alpha_3 * CG_{it} * Distance_{it} + X_{it} + \varepsilon_{it} \\ Y_{it} &= \beta_0 + \beta_1 * Distance_{it} + \beta_2 * \widehat{Award}_{it} + \beta_3 * \widehat{Award}_{it} * Distance_{it} + X_{it} + \varepsilon_{it} \end{aligned}$$

The first-stage regression predicts the likelihood that students utilize the Cal Grant at the margin. We then 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.

### *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. Data on college enrollment and degree completion come 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

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<sup>11</sup> All income-based cutoffs will include a “family size by year” fixed effect (where family size varies from two to six) to account for the varying income cutoffs.



transferred, whether degrees were conferred, the types of institutions attended, the intensity of enrollment, and the length of time required for degree completion.<sup>12</sup>

As a supplemental source of information on college attendance, we collect information returns that colleges provide for students who paid “qualified educational expenses” in a given year, Form 1098-T. These are drawn from the U.S. Treasury’s administrative records for each student between 1999 and 2013. We match colleges on these information returns to institutions in the Integrated Postsecondary Education Data System (IPEDS) to identify the type of institution that a student attends. For each Cal Grant applicant, we also construct information on federal student aid 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.

Labor market, mobility, and family formation are drawn from administrative, population-level U.S. 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 workforce outcomes, including wage and non-wage earnings, and employment status. We additionally collect the limited demographic information available on a tax return: marital status, number of children, and state of residence. Because tax returns provide earnings data conditional on filing a tax return, and because earnings are reported at the household level when married filing jointly, we additionally construct individual-level earnings data. These data come from Form W-2, the information return on earnings filed by employers, and Form 1099-MISC, the information return on non-employee compensation.

Table 1 shows summary statistics for the full sample of 819,000 applicants. Roughly 58 percent of applicants – meaning all students who submitted both a completed FAFSA and a GPA verification form – are female, and 85 percent are U.S. citizens. Fifty-six percent attended a California public four-year institution, with an additional 15 percent initially attending some form of private college. Mean family income was \$32,000 at the time of application. Ten years after applying, 85 percent of the sample is employed and 86 percent of the sample is living in California. Forty percent are married and 38 percent have children. About 66 percent of the applicants ever took out a Federal student loan. Among attendees of four-year institutions, the average indebtedness is \$16,000.

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<sup>12</sup> NSC data is 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, & Hyman, 2013). Although we do not present both results here, we find that 1098-T tax forms provide similar estimates of college-going as NSC data. We also use data on actual Cal Grant payments made to receiving institutions, which are highly accurate considering that they are tracked through a student’s social security number and constitute real costs to CSAC. Although we only observe Cal Grant payments on one side of the discontinuity, these data suggest that we may be slightly underestimating some of the effects on degree completion as institutions with weaker NSC data tend to be of higher quality.

Our analytic samples are quite different because we focus on students near the eligibility thresholds. We use a 0.3 point bandwidth around the GPA eligibility cutoff, and a \$10,000 bandwidth around the income eligibility cutoff. In general, students at the Cal Grant A discontinuities have higher incomes and high school GPAs, are more likely to attend private colleges or four-year institutions, and were more likely to be employed. They were also less likely to be married or have children.

#### *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: (1) that the predicted discontinuity results in a large change in assignment to treatment as a function of the running variable; (2) that there is no evidence of manipulation in assignment to treatment near the discontinuity; and (3) all other covariates are smooth in the neighborhood of the discontinuity. We address each of these assumptions in turn.

First, Figure 4 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. The left panel of Figure 4 shows that for students at the GPA threshold, use of a Cal Grant (i.e., receiving a Cal Grant A payment) was determined by the GPA sorting variable.<sup>13</sup> We find similar results at the income threshold in the right panel of Figure 4. 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). This figure illustrates that first-stage effects on Cal Grant utilization were only on the order of 40 percent; as stated above, many students would not utilize Cal Grant A if they did not qualify via the unmet need requirement, chose to attend community college, or attended college out of state.

Second, 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 non-parametric estimation, is problematic for discrete distributions (Lee & Card, 2008). Second, Cal Grant applicants who are high school seniors utilize their unadjusted 10<sup>th</sup> and 11<sup>th</sup> grade GPA, leading to a “lumpy” distribution.

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<sup>13</sup> There is some small evidence that students below the GPA cutoff received Cal Grant awards. This is primarily due to two reasons: students who applied in their senior year could resubmit the following year by incorporating their 12<sup>th</sup> grade GPA; CSAC’s Competitive award that was applicable for students more than one year removed from high school. (Although the Competitive award officially began in 2001, a slightly different version existed in prior years). In all cases we keep only the earliest Cal Grant application for each student.

Figure 5 shows the exact distributions for GPA in 1999 and 2000. Although the number of applicants bunches at specific GPA points, especially at 3.0, this lumping is equivalent across the two distributions, with little observational evidence that students are sorting differentially with respect to the cutoff. An overlay of the two charts shows that distributions from 1999 and 2000 are similar, even though the GPA thresholds changed markedly between years.<sup>14</sup> Results are similar when 1998 is included. To check against the possibility of manipulation around the income cutoff, we examine the density of observations around the threshold using the McCrary test (McCrary, 2008). Figure 6 Panel A shows that the distributions are smooth with no evidence of manipulation around income thresholds in the pre-expansion years.

Lastly, 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:

$$(3) X_{it} = \beta_0 + \beta_1 * Distance_{it} + \beta_2 * CG_{it} + \beta_3 * CG_{it} * Distance_{it} + \varepsilon_{it}$$

We present estimates for  $\beta_2$ , which captures the difference between those just above and just below the eligibility threshold, for each characteristic in Appendix Table 1. Panels A and B provide evidence of continuity across the thresholds, with two isolated results statistically significant at the 10 percent level. Importantly, we find that GPA is smooth at the income discontinuity, and vice versa, suggesting there is no systematic sorting of eligible students.

#### IV. Results

In this section, we present results in four broad outcome categories: (1) college attendance; (2) educational attainment; (3) federal student loans; and (4) longer-run income, mobility and household formation. We examine effects at the GPA and income effects separately. Importantly, the effects of Cal Grant eligibility (equation 1), and of Cal Grant utilization (equation 2) are identified off of somewhat different groups of students depending on which discontinuity is being utilized. Namely, at the GPA cutoff, these students are those at the margin

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<sup>14</sup> As additional evidence, we ran a set of regressions that use the ratio of 2000 to 1999 applicants at each GPA point and tests for evidence of a discontinuity at the 2000 threshold; this is basically a parametric approach to discovering larger than expected jumps after accounting for year-over-year changes. (I use various GPA bandwidths and functional forms to produce this range of estimates. I also run these regressions with analytic weights for each GPA point that derive from the number of applications in 1999.) We find no evidence of a discontinuity at the threshold in number of applicants, with cutoff estimates ranging from about a four percentage point to negative 7 percentage point change in applications at the threshold, with all estimates being statistically insignificant and equally likely to be positive as negative (a negative percentage indicates that there are fewer than expected applicants just above the threshold, the opposite of what would be expected in the case of manipulation into treatment).

of the GPA requirement who are, on average, entering college with weaker academic preparation. We end the section with a discussion of the cost-benefit implications of our results.

### *A. College Attendance*

Table 2 presents results from estimating equation (1) on our educational attendance outcomes.<sup>15</sup> We report reduced form impacts using linear slopes with rectangular kernels. In this and all future tables, the top panel presents results utilizing the GPA discontinuity, which includes students whose family incomes made the student eligible for Cal Grant A and are within 0.3 GPA points of the bandwidth, and the bottom panel presents results utilizing the income discontinuity, which includes all students meeting the GPA requirement but lying within \$10,000 of the income cutoff. Results using quadratic slopes over longer bandwidths or triangular kernels produce similar results. As college attendance outcomes using NSC data produce similar results as the 1098-T data, we do not present these regressions results for brevity.

Table 2 indicates that Cal Grant eligibility had no meaningful impact on whether a student ever attended a post-secondary institution (Column 1) or a four-year institution (Column 2), though overall college-going rate of these populations are well above 90 percent. For students around the GPA discontinuity (top panel), we also find that Cal Grant eligibility had no meaningful impact on the college sector attended (Columns 3 and 4) or average tuition at the school attended (Column 5).

More importantly, among students who have overcome the margin of GPA requirement, and are thus more prepared for college than those around the GPA cutoff, but are near the income threshold, we find that the Cal Grant subsidy impacts college school choice. The bottom panel of Table 2 shows that at the income discontinuity, Cal Grant eligibility led to a statistically significant four percentage point increase in private school attendance (Column 3), which is offset by a similar decline in four-year public school attendance (Column 4). This implies that a subsidy of approximately \$6,000 to \$9,000 per year – the difference between the private subsidy and UC or community college tuition - increases private school enrollment by almost 20 percent among this population. As a result of this shift, we estimate that the average published tuition for the school attended rises by \$1,200 (Column 5).

### *B. Educational Attainment*

In Table 3, we present results on college persistence and completion that we measure through two separate data sources. First, we count the total number of years students submitted a 1098-T form (Column 1). Second, NSC provides more accurate degree completion data, though at this time we only have results for two cohorts who applied in 1999 and 2000.

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<sup>15</sup> We do not show IV estimates for college-going results as award utilization implies attendance in California, though this does suggest that Bachelor degree completion for award users increases by approximately seven percentage points.

The top panel of Table 3 shows that at the GPA discontinuity, Cal Grant eligibility may have positive impacts on degree completion. Although imprecisely estimated, the award suggests an increase in Bachelor degree completion by almost three percentage points, which is similar in magnitude to the effect found at the income cutoff. In addition, graduate degree completion increases by a statistically significant two percentage points, or roughly a 15 percent increase in the likelihood of earning a degree beyond a Bachelors; this is due to the relatively low graduate degree completion among this sample of lower GPA students. Consistent with this result, Cal Grant eligibility increases the number of years with a 1098-T by approximately 2.8 percent. The bottom panel of Table 3 shows that at the income discontinuity, Cal Grant eligibility increases Bachelor degree completion by 3.5 percentage points, perhaps at the cost of decreasing Associate degree completion (Columns 7 and 8).<sup>16</sup>

### C. Federal Student Loans

Table 4 examines total student loans borrowed under Federal programs. We provide both estimates of the impact of Cal Grant eligibility from estimating equation (1), and estimates of the LATE from estimating equation (2). To estimate the LATE, we instrument a dummy variable that equals one if a student has ever received a Cal Grant payment with Cal Grant eligibility. Similar results obtain when we use total Cal Grant payment amounts in place of the indicator for ever receiving a Cal Grant payment.

First, we examine the reduced form regressions. Across both eligibility discontinuities, we find there are no impacts on whether a student ever took out a Federal loan (Column 1). For students at the GPA cutoff, although noisily estimated, our confidence interval precludes loan decreases for four-year attendees of over \$900, or about half of one year's tuition to a less expensive CSU. For students at the income cutoff, however, we do find that among students who attend four year institutions (admittedly an endogenous choice), winning a Cal Grant A is associated with reduced total loans of \$900 per year, though the estimate is imprecise. Restricting to students attending private four year institutions, Cal Grant A reduces borrowings by a statistically significant \$3,300.

Next, we turn to our IV results, which estimate the effects of *using/receiving* a Cal Grant award. As described above, there are a number of ways that students might not take up the award, generally related to not meeting alternative eligibility requirements or having college-going plans that do not involve California's four-year sector. As a result, the take up of the award – indicating ever receiving a Cal Grant payment – is about 40 percent, though this varies slightly

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<sup>16</sup> Although we do not present results here, we classify degree outcomes as STEM or non-STEM using available CIP codes plus hand-coding based on what are clearly STEM degree titles when colleges do not provide CIP code data. We find no negative effects of Cal Grant eligibility on STEM degree, which is in contrast to other recent work (Sjoquist & Winters, 2015). Although we would expect state-level estimates to vary, we note that California does not have stringent renewal requirements, such as meeting a minimum GPA threshold, which are hypothesized to negatively impact STEM attainment.

across discontinuities; first-stage effects of being above the threshold on using a Cal Grant are 35 and 43 percent at the GPA and income discontinuities, respectively. As a result, the IV estimates are larger than the reduced form effects by a factor of about two to three.

As with the reduced form results, the IV effects in Table 4 are modest in the point estimates and statistically insignificant at the GPA discontinuity. We find similar result using the income discontinuity. Among this population, students using a Cal Grant and attending a four-year institution, particularly private colleges, reduce their total Federal loan debt as much as \$5,000.

#### *D. Income, Family Formation and Mobility*

Table 5 presents results on earnings and employment at various intervals since a student's initial application. First, we examine total log wages in the first two or first four years of college, an indicator of whether a student altered their hours worked during college. In columns (1) and (2) of Table 5, we find no significant impacts. Given that we found no evidence of an impact of the Cal Grant on college enrollment, this result suggests that the Cal Grant additionally does not alter choices over employment while in school.

Next, one might expect that the shifts in institution attended and small increases in degree completion could lead to effects on employment and earnings; however, we do not find compelling evidence of such long-run effects. We do find suggestive evidence of differential impacts across the two eligibility discontinuities.

At the income discontinuity, the estimated impact on wages (averaged included years 6 to 10 since applying for the grant) is an insignificant negative four percentage points. We also calculate wage impacts for each year since application, and look for time trends in the annual estimates (Figure 7). The year-to-year estimates of Cal Grant A impacts are similarly noisy, and the effects four years after application appear to be generally centered around zero or slightly negative. In the IV specification, however, effects on likelihood of working and on wages are economically but not statistically significant. Estimated effects from using a Cal Grant on average wages during years 6-10 from high school graduation range are -10 percent and not close to statistical significance.

Interestingly, we do find positive point estimates suggesting long-run wage effects at the GPA discontinuity. Cal Grant eligible students have wages that are 6 percentage points higher averaged over years 6-10 since high school graduation. While the year to year estimates of Cal Grant A impacts are similarly noisy (Figure 8), the effects four years after application appear to be generally centered around one to four log points above zero, but do not show evidence of increasing over time. In the IV specifications, the impact of using a Cal Grant on being employed ten years after application is an insignificant -0.01 percentage points and the estimated effect on average wages 6-10 years out is a 16 percentage point increase though the effect is not statistically significant.

In Table 6 we examine additional impacts of Cal Grant eligibility on mobility and family formation. Importantly we do not find any impact from Cal Grant A on remaining within California either 5 or 10 years after award receipt. Although this may be evidence against merit aid impacting out-migration, it also suggests that the additional graduates produced by the award are likely to remain within the state. Although we do not present the results here, we also find that the increase in graduate degree completion found at the GPA discontinuity appears to take place almost entirely within California. In the IV specification at the income discontinuity, we see some hints of Cal Grant awardees being more likely to remain in California 5 years after receipt, however this effect disappears when we examine remaining in California 10 years after graduation. We do not find impacts of the award on marital status or whether an individual has a child.

### *E. Cost-Benefit Discussion*

Although economists recognize need to lower college costs for liquidity constrained students, there is debate of whether aid is best allocated through formulaic merit- or need-based programs. Proponents of merit-aid programs suggest that aid is more effective when targeted towards students who have the necessary preparation to complete college, but this also suggests that the majority of merit payments may be subsidies to families who would have been willing to pay for college even in the absence of the program. Poorly designed programs might also have a negative educational impact on individuals, leading students to strategically reduced course loads or shift out of demanding STEM fields, possibly increasing time to degree or lowering completion rates (Cohodes & Goodman, 2014; Cornwell, Lee, & Mustard, 2005; Scott-Clayton, 2011; Sjoquist & Winters, 2015). To the extent that aid subsidizes students to enroll in lower-quality institutions, states might not experience the gains in educated labor force or tax base as expected (Peltzman, 1973), though contextual factors such as the specific renewal requirements and the availability of competitive institutions play a role in the effectiveness of the program (e.g., Scott-Clayton (2011)). Finally, even if merit aid increases college enrollment and completion it may not necessarily lead to a stronger labor force, as recent work suggests that college may induce migration above and beyond where students initially attend (Malamud & Wozniak, 2012; Wozniak, 2010). Nonetheless, Dynarski (2008) provides one of the only cost-benefit analyses of state-based programs, and finds that they are socially efficient even if one assumes a low rate of return to schooling.

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 \$3,960 at the GPA discontinuity and \$8,250 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 choose to not use it, and some students leave college without using all four years of payments.

Our reduced form point estimate is that the Cal Grant A receipt raises bachelor degree receipt by 3 percentage points. Consider the strong assumption that the only impact of the program is to raise three of each one hundred students from “some college” to college completion, thus ignoring any graduate degree or other unobserved effects. The expenditure is equivalent to spending from \$132,500 (i.e.,  $\$3,960/0.03$ , in the case of the GPA discontinuity) to \$275,000 per additional B.A., (i.e.,  $\$8250/0.03$ , at the income discontinuity). Moving an adult from some college to a bachelor’s degree might raise earnings by an annuity of \$20,000 for forty years for a net present value of around \$360,000 at a 5% interest rate.

This back of the envelope 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 is obviously a highly simplistic analysis 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. Additionally, we do not have precise estimates of the actual earnings gains of the Cal Grant recipients.

A more realistic analysis would take into account the fact that Cal Grant may impact earnings through a whole variety of mechanisms including choice of institution, locational decisions, marital status, and student loan take up, among others. The challenge is that our earnings estimates are both large and noisy and encompass both positive and negative estimates. This makes it essentially impossible to ask whether the estimated earnings effects exceed the known costs.

Importantly the Cal Grant is largely a transfer from tax payers to students and their families. In other words the Cal Grant is not a pure deadweight loss but rather a transfer which may or may not have a deadweight loss. So 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.

## **V. Conclusion**

State-based merit- and need-based aid constitutes one of the most important and fastest growing sources of student assistance for postsecondary education. The income and GPA discontinuities for Cal Grant eligibility produce sharp changes in grant receipt but produce no changes in overall college attendance. We find evidence of some impacts on the types of colleges attended and degree completion, although these vary by the subpopulation examined. For students meeting the minimum GPA requirement but near the income threshold, we detect shifts into private institutions and away from public four-year colleges in California. Although these shifts are relatively small in magnitude, the results suggest that \$6,000 to \$8,000 increases private school attendance by approximately 15 percent (four percentage points). Furthermore, using a Cal Grant appears to reduce financial constraints on these students as the average recipient at a private



institution has reduced student loans (in total) of \$5,000. These students are also over three percentage points more likely to obtain a Bachelor's degree, as measured by NSC data.

In contrast, we find no evidence of shifting in the type of college attended amongst income-eligible students near the GPA eligibility cutoff. We instead find that the Cal Grant has indirect effects on these students' higher education, inducing students with typically low overall graduate degree completion rates to complete graduate school by an additional two percentage points, or an increase of roughly 15 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 suggest that the graduate degree effect is precisely zero for the first six years after completing high school before gradually increasing, becoming statistically significant only a dozen years after entering college (results available upon request).

However, we cannot say with precision that the modest changes in institution type and Bachelor's degree attainment translate into measurable effects on long-run outcomes. In some of our IV specifications, Cal Grant A raises estimated log wages by as much 0.16, with correspondingly large standard errors. Given the modest increases in degree completion (3 percentage points) and assuming that earnings only increased as a result of degree completion, the returns to earnings would have had to have abnormally large (over 2.60) for us to find a significant impact given our sample size.

Interestingly, we find that ten years after Cal Grant receipt, typical awardees appear no more likely to live in California. This particular state merit-aid program does not appear to reduce outmigration of talented workers from California, one of two important margins that merit aid must impact for states to consider the program cost effective.

All of these effects may be particular to the institutional context of California. California is such a geographically large and diverse economy that outmigration is already less likely than migration from smaller and less economically diverse states. More importantly, Cal Grant is offered on top of a highly subsidized and broad reaching public university and community college system. Equally important is that our inferences are restricted to a particular set of Cal Grant 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. That said, the marginal effects of offering merit aid appear small for most educational and mobility outcomes and to have no measured impact on long-run earnings.

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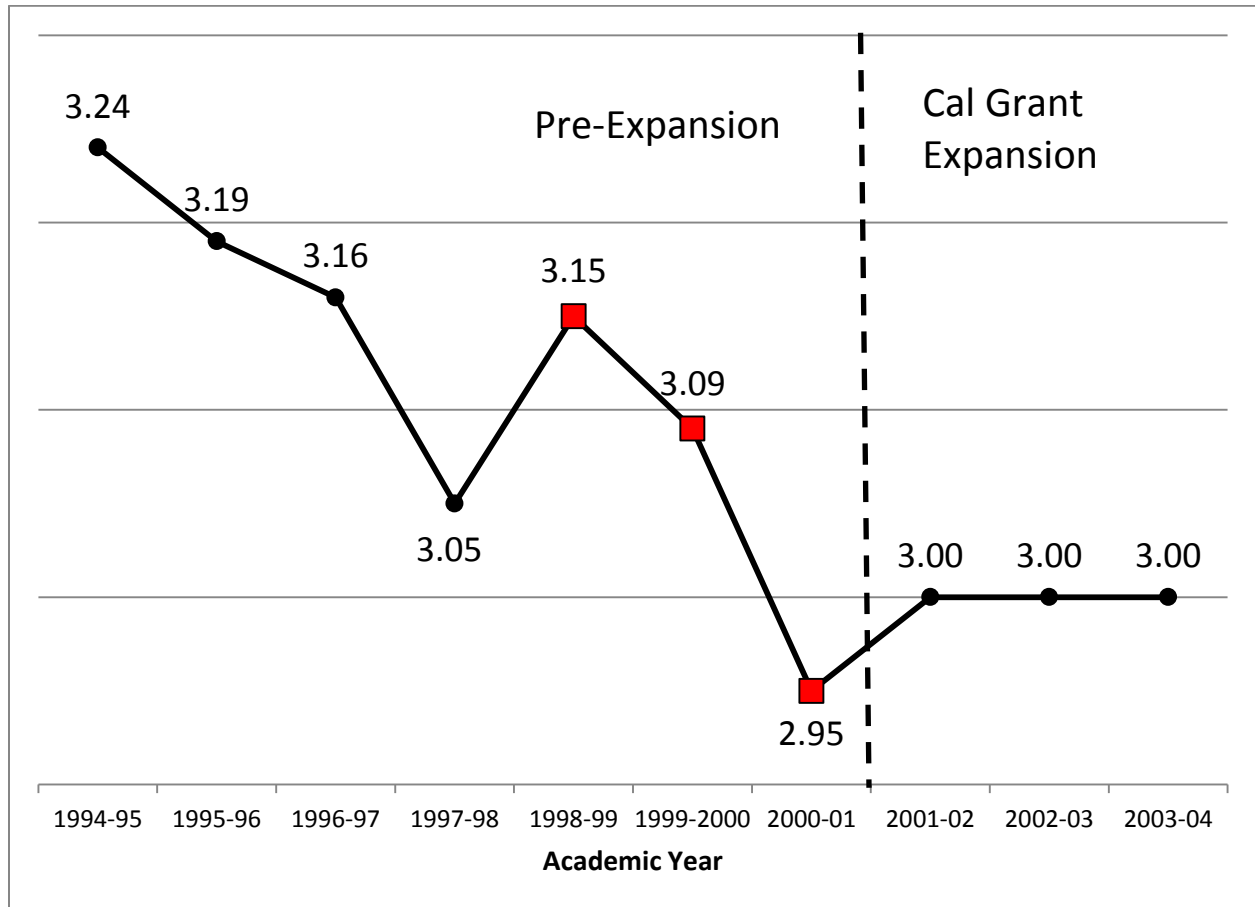
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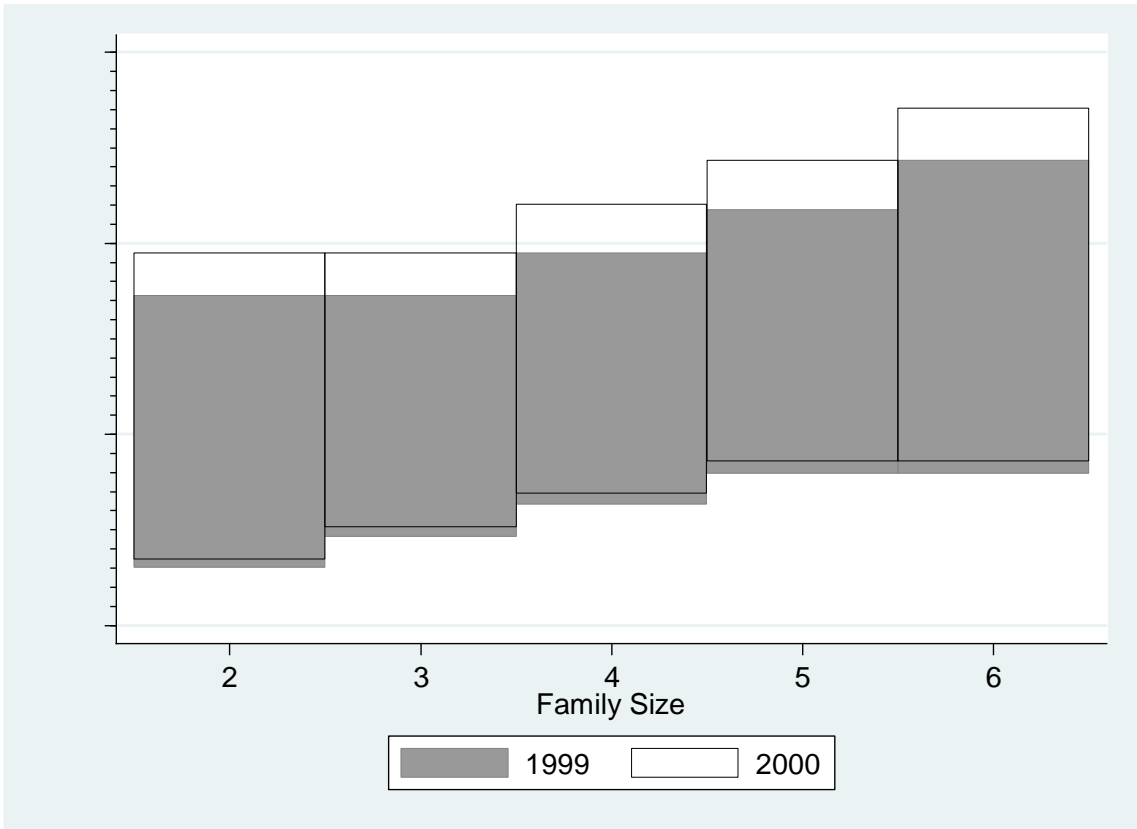
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**Figure 1: Minimum GPA Thresholds for Cal Grant A Eligibility**



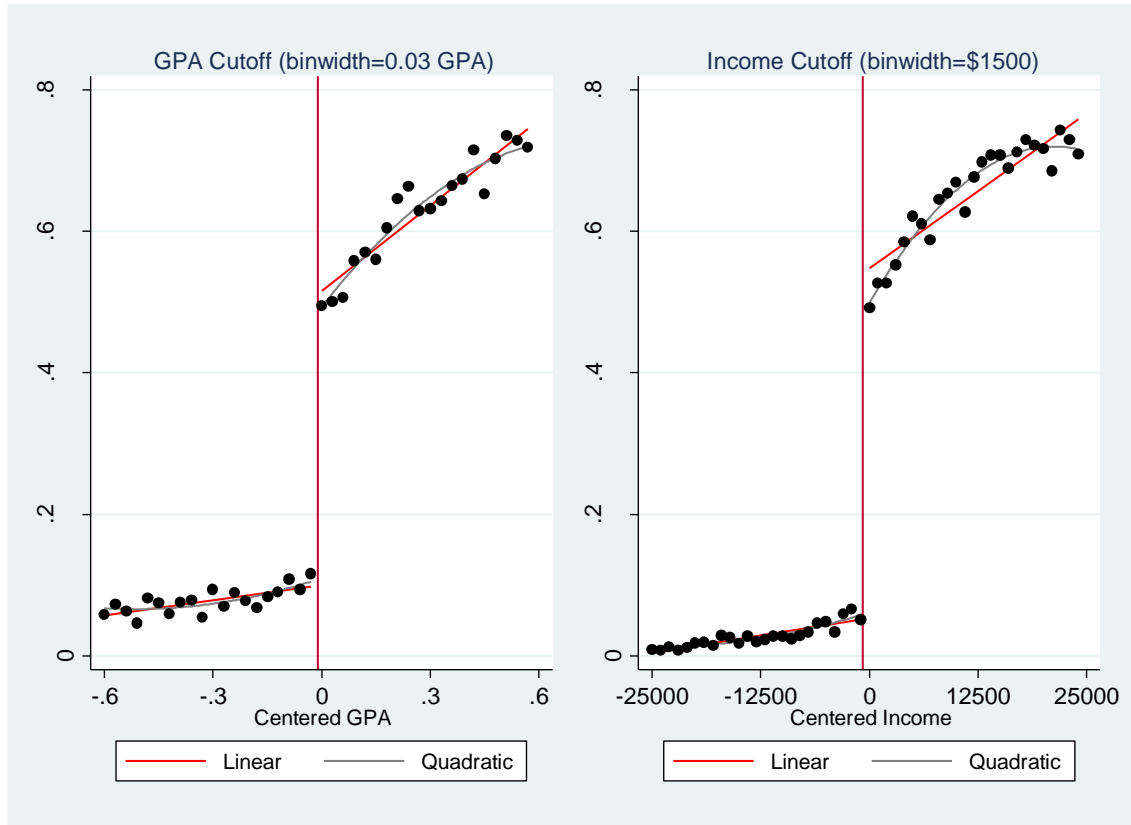
Notes: This figure shows the time-varying high school GPA cutoff for Cal Grant eligibility. This cutoff was unknown to applicants and determined after the receipt of applications. Red boxes indicate years utilized in this paper. Cal Grant underwent a significant expansion in 2001-02, leading to a number of changes in the program design. Principally, the minimum GPA cutoff for students not eligible for Cal Grant B was fixed at a GPA value of 3.0.

**Figure 2: Minimum and Maximum Income Thresholds for Students Only Eligible for Cal Grant A**



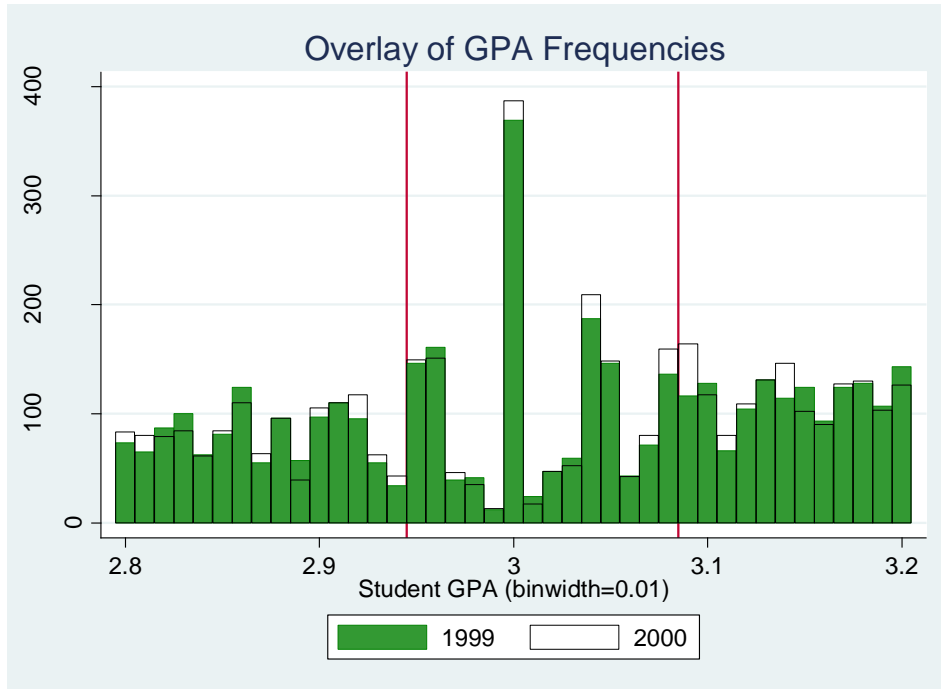
Notes: Boxes show minimum and maximum income limits in 1999-2000 and 2000-2001 for students who were eligible for Cal Grant A only. Students above the income limits were not eligible for any Cal Grant award. Students below the income limits were eligible for Cal Grant A or B depending on a variety of characteristics, thus producing a treatment contrast not explored in this paper.

**Figure 4: Cal Grant Utilization, 1999 and 2000**



Notes: This figure depicts the proportion of students who “Ever Received a Cal Grant A payment.” The left panel bins students by GPA relative to the year-specific eligibility threshold, pooled across years. The right panel bins students by \$1,500 relative to the year-specific eligibility threshold, pooled across years. Income is reversed so that values above the cutoff represent lower family incomes.

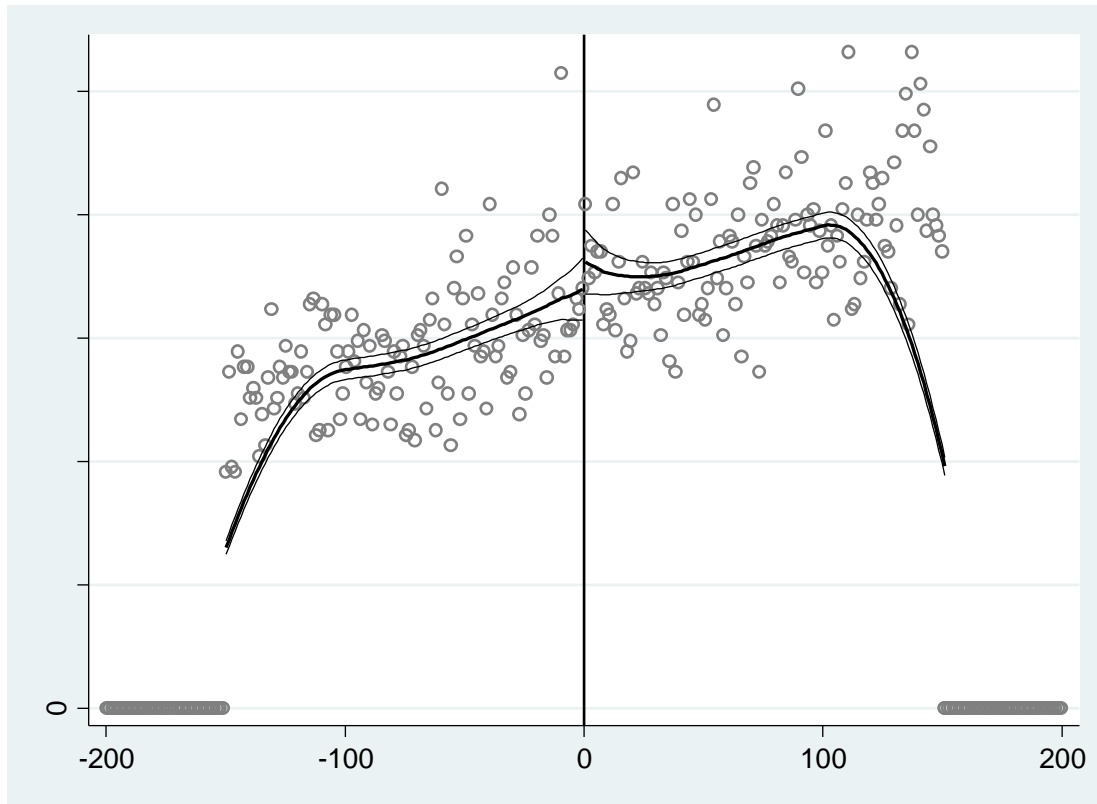
**Figure 5: Histograms of GPA Distribution, 1999 and 2000**



Notes: Cutoffs for 1999 and 2000 are marked in red.



**Figure 6: McCrary Test of Applicant Density at Income Threshold, 1998-2000**



**Figure 7: Effect of Cal Grant A Eligibility on Earnings, Income Threshold**



Notes: Each point represents a coefficient from the estimating equation (1) where the dependent variable is log earnings  $t$  years after application.

**Figure 8: Yearly Estimates of Cal Grant A Eligibility on Earnings, GPA Threshold**



Notes: Each point represents a coefficient from the estimating equation (1) where the dependent variable is log earnings  $t$  years after application.

Table 1: Summary Statistics

	All applicants			GPA discontinuity			Income discontinuity		
	Mean	Std. Dev.	N	Mean	Std. Dev.	N	Mean	Std. Dev.	N
Age	22.49	7.35	819,326	19.50	4.15	21,881	19.24	3.78	15,663
Female	0.58	0.49	819,326	0.56	0.50	21,881	0.61	0.49	15,663
U.S. Citizen	0.85	0.36	819,326	0.91	0.28	21,881	0.93	0.26	15,663
Parents married	0.10	0.30	819,326	0.04	0.20	21,881	0.03	0.18	15,663
Dependent	0.68	0.47	819,326	0.94	0.24	21,881	0.96	0.20	15,663
Student GPA	3.03	0.56	819,326	3.06	0.23	21,881	3.49	0.33	15,663
Family income	32,077	25,903	819,326	45,279	9,278	21,881	61,768	7,908	15,663
Attended college	0.93	0.25	819,326	0.96	0.18	21,881	0.98	0.14	15,663
Attended four-year college	0.78	0.41	747,928	0.88	0.32	20,716	0.95	0.22	15,230
Attended private college	0.15	0.36	745,977	0.18	0.39	20,607	0.21	0.40	15,090
Attended school in California	0.82	0.39	748,796	0.82	0.38	20,806	0.83	0.38	15,232
Attended four-year public	0.56	0.50	765,049	0.66	0.47	21,109	0.72	0.45	15,366
Ever took a federal loan	0.66	0.47	819,326	0.73	0.44	21,881	0.75	0.43	15,663
Avg. loans, 4-year attendees	16,693	16,478	585,367	16,537	15,258	18,256	16,645	16,478	14,470
Avg. log earnings, 6-10 years after app.	9.71	2.28	811,561	10.04	1.72	21,792	10.13	1.70	15,626
Outcomes 10 years after application:									
Employed	0.85	0.36	811,561	0.90	0.30	21,792	0.90	0.31	15,626
Living in California	0.86	0.35	713,962	0.87	0.34	19,351	0.82	0.38	14,741
Married	0.40	0.49	713,962	0.37	0.48	19,351	0.42	0.49	14,741
Has kids	0.38	0.49	811,561	0.30	0.46	21,792	0.23	0.42	15,626

Notes: This table presents means, standard deviations, and number of observations of our variables for all Cal Grant applicants, for those in the GPA discontinuity sample, and for those in the income discontinuity sample. For the GPA discontinuity, we use a 0.3 GPA point bandwidth around the GPA cutoff. For the income discontinuity, we use a \$10,000 bandwidth around the income discontinuity.

Table 2: Educational Attendance Results

	(1)	(2)	(3)	(4)	(5)
	College Attendance (1098-T)				
	Attend Postsecondary	Four-Year College	CA Four- Year Public	Private College	Tuition
Threshold 1: GPA	0.004	-0.005	-0.005	-0.005	-129.822
Discontinuity	(0.005)	(0.008)	(0.012)	(0.014)	(361.025)
Constant	0.965***	0.934***	0.711***	0.189***	13,425.353***
	(0.006)	(0.010)	(0.019)	(0.017)	(490.104)
N	21,881	20,716	21,109	20,607	19,686
Threshold 2: Income	0.000	0.005	-0.040***	0.040***	1,176.063***
Discontinuity	(0.004)	(0.007)	(0.014)	(0.013)	(433.880)
Constant	0.987***	0.977***	0.766***	0.251***	17,572.630***
	(0.006)	(0.008)	(0.021)	(0.021)	(698.784)
N	15,696	15,263	15,399	15,122	14,606

Notes: This table presents estimates of the coefficient on Cal Grant eligibility from Equation (1). The top panel presents results around the GPA discontinuity, where the estimation sample includes those within a 0.3 GPA point bandwidth around the eligibility cutoff. The bottom panel presents results around the income discontinuity, where the estimation sample includes those within a \$10,000 bandwidth around the eligibility cutoff. All regressions include year-by-family size fixed effects. Standard errors in the top panel are clustered by standardized GPA; standard errors in the bottom panel are heteroscedasticity-robust. \*\*\* p<0.001, \*\*, p<0.01, \* p<0.1.

Table 3: Educational Attainment Results

	(1)	(2)	(3)	(4)
	Number of	Degree Completion (NSC)		
	1098-Ts	Associate	Bachelor	Graduate
Threshold 1: GPA	0.160*	0.006	0.028	0.023**
Discontinuity	(0.094)	(0.010)	(0.019)	(0.012)
Constant	5.885***	0.133***	0.487***	0.122***
	(0.140)	(0.008)	(0.015)	(0.007)
N	21,881	11,759	11,759	11,759
Threshold 2: Income	-0.054	-0.018*	0.035**	0.002
Discontinuity	(0.088)	(0.010)	(0.017)	(0.016)
Constant	7.092***	0.103***	0.655***	0.265***
	(0.134)	(0.008)	(0.012)	(0.011)
N	15,696	12,444	12,444	12,444

Notes: This table presents estimates of the coefficient on Cal Grant eligibility from Equation (1). The top panel presents results around the GPA discontinuity, where the estimation sample includes those within a 0.3 GPA point bandwidth around the eligibility cutoff. The bottom panel presents results around the income discontinuity, where the estimation sample includes those within a \$10,000 bandwidth around the eligibility cutoff. All regressions include year-by-family size fixed effects. Standard errors in the top panel are clustered by standardized GPA; standard errors in the bottom panel are heteroscedasticity-robust. \*\*\* p<0.001, \*\*, p<0.01, \* p<0.1.

Table 4: Federal Student Loan Results

		(1)	(2)	(3)	(4)
		Loans			
	Estimates	Received Federal Loan	Only Four-Year Attendees	Only Private Attendees	Only CA Four-Year Public Attendees
Threshold 1: GPA Discontinuity	Reduced Form	-0.002 (0.016)	-100.586 (413.623)	-492.654 (1,046.826)	-305.395 (413.559)
	Constant	0.773*** (0.017)	17,148.769*** (515.958)	26,045.746*** (1,604.159)	15,216.971*** (681.416)
N		21,881	18,256	3,523	13,905
	IV (Received Payment)	-0.005 (0.034)	-243.171 (1,102.622)	-1,165.402 (2,621.935)	-669.778 (1,064.780)
		0.480 (0.312)	7,399.486 (10,780.738)	37,660.740** (15,891.665)	304.368 (14,193.005)
N		21,881	18,256	3,523	13,905
Threshold 2: Income Discontinuity	Reduced Form	-0.009 (0.014)	-869.056 (544.936)	-3,287.626** (1,278.420)	-745.795 (590.407)
	Constant	0.747*** (0.021)	14,721.620*** (778.035)	21,498.380*** (1,784.835)	12,770.755*** (807.418)
N		15,696	14,503	3,058	11,142
	IV (Received Payment)	-0.020 (0.032)	-1,928.663 (1,213.167)	-5,021.156*** (1,946.757)	-1,812.510 (1,440.406)
		0.988*** (0.023)	21,112.716*** (6,649.755)	21,549.041** (8,388.202)	18,804.448** (7,813.667)
N		15,696	14,503	3,058	11,142

Notes: This table presents estimates of the coefficient on Cal Grant eligibility from Equation (1) in the reduced form results, and the coefficient on Cal Grant payments from Equation (2) in the IV results. The top panel presents results around the GPA discontinuity, where the estimation sample includes those within a 0.3 GPA point bandwidth around the eligibility cutoff. The bottom panel presents results around the income discontinuity, where the estimation sample includes those within a \$10,000 bandwidth around the eligibility cutoff. All regressions include year-by-family size fixed effects. Standard errors in the top panel are clustered by standardized GPA; standard errors in the bottom panel are heteroscedasticity-robust. \*\*\* p<0.001, \*\*, p<0.01, \* p<0.1.

Table 5: Employment Results

		(1)	(2)	(3)	(4)
		Employment			
		Average Log Wages (Years 1 to 2)	Average Log Wages (Years 1 to 4)	Average Log Wages (Years 6-10)	Employed (Year 10)
Threshold 1: GPA Discontinuity	Reduced Form	0.001 (0.033)	0.031 (0.031)	0.056 (0.039)	-0.000 (0.009)
	Constant	8.528*** (0.049)	8.753*** (0.044)	10.159*** (0.051)	0.926*** (0.011)
N		18,466	19,756	21,790	21,792
	IV (Received Payment)	0.003 (0.086)	0.085 (0.082)	0.161 (0.133)	-0.001 (0.023)
	Constant	7.639*** (0.770)	8.219*** (0.741)	10.606*** (1.214)	1.000*** (0.213)
N		18,466	19,756	21,790	21,792
Threshold 2: Income Discontinuity	Reduced Form	-0.012 (0.037)	0.018 (0.034)	-0.041 (0.050)	-0.006 (0.009)
	Constant	8.268*** (0.053)	8.498*** (0.050)	10.144*** (0.082)	0.894*** (0.015)
N		13,808	14,641	15,659	15,659
	IV (Received Payment)	-0.029 (0.086)	0.043 (0.078)	-0.095 (0.116)	-0.013 (0.022)
	Constant	9.543*** (0.058)	9.527*** (0.053)	9.313*** (1.259)	1.024*** (0.011)
N		13,808	14,641	15,659	15,659

Notes: This table presents estimates of the coefficient on Cal Grant eligibility from Equation (1) in the reduced form results, and the coefficient on Cal Grant payments from Equation (2) in the IV results. The top panel presents results around the GPA discontinuity, where the estimation sample includes those within a 0.3 GPA point bandwidth around the eligibility cutoff. The bottom panel presents results around the income discontinuity, where the estimation sample includes those within a \$10,000 bandwidth around the eligibility cutoff. All regressions include year-by-family size fixed effects. Standard errors in the top panel are clustered by standardized GPA; standard errors in the bottom panel are heteroscedasticity-robust. \*\*\* p<0.001, \*\* p<0.01, \* p<0.1.



Table 6: Family Formation and Mobility Results

	Estimates	Living in California		Married		Has Children	
		5 Years	10 Years	5 Years	10 Years	5 Years	10 Years
Threshold 1: GPA	Reduced	0.002	-0.000	-0.004	-0.002	-0.011	-0.012
Discontinuity	Form	(0.007)	(0.012)	(0.014)	(0.015)	(0.014)	(0.016)
Constant		0.885***	0.785***	0.088***	0.277***	0.120***	0.261***
		(0.012)	(0.018)	(0.014)	(0.022)	(0.015)	(0.018)
N		18,856	19,351	18,856	19,351	21,792	21,792
	IV (Received	0.004	-0.000	-0.012	-0.006	-0.031	-0.033
	Payment)	(0.024)	(0.028)	(0.032)	(0.039)	(0.027)	(0.035)
		0.504**	0.506**	0.510*	0.498	0.518**	0.521
		(0.199)	(0.241)	(0.266)	(0.341)	(0.246)	(0.321)
		18,856	19,351	18,856	19,351	21,792	21,792
Threshold 2: High-Income	Reduced	0.012	-0.003	0.002	-0.013	-0.007	-0.009
Discontinuity	Form	(0.010)	(0.012)	(0.013)	(0.016)	(0.009)	(0.013)
Constant		0.865***	0.775***	0.063***	0.344***	0.043***	0.142***
N		(0.018)	(0.021)	(0.015)	(0.024)	(0.011)	(0.018)
		14,062	14,770	14,062	14,770	15,659	15,659
	IV (Received	0.029	-0.007	0.006	-0.029	-0.015	-0.021
	Payment)	(0.025)	(0.029)	(0.030)	(0.037)	(0.020)	(0.031)
		-0.006	0.014	0.965***	0.996***	0.491	0.503
		(0.017)	(0.019)	(0.020)	(0.025)	(0.348)	(0.342)
		14,062	14,770	14,062	14,770	15,659	15,659

Notes: This table presents estimates of the coefficient on Cal Grant eligibility from Equation (1) in the reduced form results, and the coefficient on Cal Grant payments from Equation (2) in the IV results. The top panel presents results around the GPA discontinuity, where the estimation sample includes those within a 0.3 GPA point bandwidth around the eligibility cutoff. The bottom panel presents results around the income discontinuity, where the estimation sample includes those within a \$10,000 bandwidth around the eligibility cutoff. All regressions include year-by-family size fixed effects. Standard errors in the top panel are clustered by standardized GPA; standard errors in the bottom panel are heteroscedasticity-robust. \*\*\* p<0.001, \*\*, p<0.01, \* p<0.1.

Appendix Table 1: Balance of Covariates

	Age	Female	U.S. citizen	Parents married	Dependent	GPA	Family Income
<i>Panel A: GPA Discontinuity</i>							
Cal Grant	-0.25 (0.22)	-0.02* (0.01)	0.01 (0.01)	-0.004 (0.01)	0.01 (0.01)	---	-81.07 (166.03)
Constant	19.87*** (0.29)	0.54*** (0.02)	0.97*** (0.01)	0.01* (0.01)	0.94*** (0.01)		38,729.28*** (288.36)
Observations	21,881	21,881	21,881	21,881	21,881		21,881
R-squared	0.01	0.01	0.01	0.01	0.01		0.25
<i>Panel B: Income Discontinuity</i>							
Cal Grant	-0.18 (0.12)	0.01 (0.02)	0.01 (0.01)	-0.01 (0.01)	0.01* (0.01)	0.003 (0.01)	---
Constant	19.14*** (0.16)	0.61*** (0.02)	0.96*** (0.01)	0.01 (0.01)	0.97*** (0.01)	3.55*** (0.01)	
Observations	15,696	15,696	15,696	15,696	15,696	15,696	
R-squared	0.01	0.00	0.00	0.01	0.01	0.03	

Notes: This table presents estimates of the coefficient on Cal Grant eligibility from Equation (3). Panel A presents results around the GPA discontinuity, where the estimation sample includes those within a 0.3 GPA point bandwidth around the eligibility cutoff. Panel B presents results around the income discontinuity, where the estimation sample includes those within a \$10,000 bandwidth around the eligibility cutoff. All regressions include year-by-family size fixed effects. Standard errors in Panel A are clustered by standardized GPA; standard errors in Panel B are heteroscedasticity-robust. \*\*\* p<0.001, \*\*, p<0.01, \* p<0.1.