Do Tuition Subsidies Raise Political Participation?*

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Abstract

Civic externalities motivate public education expenditures, but estimates of the civic returns to large-scale education subsidies are scarce. We use 16 million financial aid applications and a regression discontinuity (RD) design to estimate how the United States' largest tuition-free college program impacts political participation. We find that each of the 2.6 million awards increased a student's voter turnout rate by 4 to 12 percentage points in 2020, raising total voter turnout by 1 percentage point and Biden's margin of victory by 0.5 percentage points in the awarding state. We calculate that 1 out of every 66 voters cast a ballot because of the tuition subsidy and find evidence consistent with peer socialization, among other mechanisms. The results are externally validated with another RD design using 2.5 million students local to a notch in the generosity of another financial aid program. Our findings demonstrate that the civic externalities of education spending can be large enough to sway elections.

Keywords: Financial Aid, Externalities, Voting Behavior

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1 Introduction

Identifying the social returns to education expenditures requires estimates of private labor market returns as well as externalities accruing to other parties. Extant research explores the effects of education spending on health, crime, and innovation but overlooks civic externalities despite their historical role in economics.[1] Economists like Adam Smith and Milton Friedman justified public spending on primary and secondary schools by asserting that they produce informed and politically active citizens (Friedman, 2020; Frame and Schwarze, 2022). Policymakers enacted costly compulsory and universal schooling laws on the same basis, explicitly citing returns to civic participation rather than earnings (Mann, 1957). Omitting civic externalities from the social returns to education spending therefore risks ignoring a key external benefit and narrowing the scope of public finance.

A well-documented positive association between educational attainment and political participation exists in most contexts, with educated people more likely to vote, follow the news, and hold public office (Gethin et al., 2021a; Chetty et al., 2023). Existing work, however, is mixed on the civic returns to education expenditures, especially later in life.[2] In particular, we know little about whether large-scale public education expenditures like college tuition subsidies impact civic engagement, despite 235 billion dollars of financial aid dispersed annually in the US alone (College Board, 2023). Estimates of the civic externalities of higher education spending are limited by the dearth of instruments that allow for a credible identification strategy.[3]

[1] For example, recent empirical work shows that education expenditures can have large impacts on criminal activity (Bell et al., 2022; Anders et al., 2023; Gray-Lobe et al., 2023; Baron et al., 2024), estimates the impact of costly education policies on mortality and long-run health (Clark and Royer, 2013; Meghir et al., 2018; Lundborg et al., 2022), and finds that education spending can foster innovation (Toivanen and Vaananen, 2016; Andrews, 2023; Babina et al., 2023).

[2] Papers on primary schools generally find positive effects (Dee, 2004; Sondheimer and Green, 2010; Wantchekon et al., 2015) but evidence from secondary schools is inconsistent (Milligan et al., 2004; Tenn, 2007; Marshall, 2016, 2019; Cohodes and Feigenbaum, 2021; Willeck and Mendelberg, 2022; Bommel and Heineck, 2023).

[3] Most papers match students on observables to estimate the civic externalities of higher educational attainment rather than higher education expenditures, finding conflicting results (Kam and Palmer, 2008; Henderson and Chatfield, 2011; Mayer, 2011; Willeck and Mendelberg, 2022; Scott, 2022; Bell et al., 2024). Three studies use instruments for college enrollment – specifically distance to college and conscription – and

Recent work by Firoozi (2023) identifies the effects of admission to selective colleges on student partisanship, finding that elite institutions do more to influence students' party preferences than their less selective counterparts. While this paper provides insight into the role of elite institutions in shaping student political behavior, it leaves open the question of how broad-scale *public expenditures* on higher education shape civic engagement, particularly among the less wealthy students and less selective institutions that receive the bulk of college tuition subsidies. A related literature that estimates the returns to tuition subsidies often focuses squarely on graduation rates and earnings, overlooking plausibly large civic returns that accrue to conditional cash transfers in many contexts (Manacorda et al., 2011). Identifying the civic externalities of higher education spending remains critical to understanding its social returns, especially given its often modest marginal treatment effects on labor market outcomes.

In this paper, we use data on 16.4 million Free Applications for Federal Student Aid (FAFSAs) and a regression discontinuity (RD) design to identify the micro-level and macro-level civic returns to tuition subsidies. We use strict eligibility rules for the United States' largest tuition-free 4-year college program, the Cal Grant, to estimate impacts on several measures of voter registration and turnout (Kane, 2003; Bettinger et al., 2019; Dickler, 2022). The setting is ideal because (1) extant evidence on the program's labor market returns are critical for understanding its civic externalities, (2) California is the largest democratic setting where most voters (roughly 3/4ths) self-report party preferences in administrative data, and (3) California has the highest college student retention rate in the United States (Hendren and Sprung-Keyser, 2020; Van Dam, 2022; Firoozi, 2023).

We find that each Cal Grant awarded by the California Student Aid Commission (CSAC) raises a student's probability of casting a ballot in 2020 by roughly 10 percentage points relative to a baseline turnout of 56 percent. This implies that the 2.6 million grants awarded over the 2010s led to an additional 259,000 votes (i.e. 1 percentage point of the citizen voting

again arrive at different conclusions (Dee, 2004; Berinsky and Lenz, 2011; Doyle and Skinner, 2017).

eligible population) being cast in California during the 2020 general election alone. Effects are similar across demographics and place of origin but are larger among students with the highest GPAs. Our results are robust to multiple definitions of voter turnout as well as a number of RD implementation choices.

Using location and partisanship records to track Cal Grant recipients and relying on conservative assumptions, we estimate that 1 out of every 66 voters in California cast a ballot in 2020 due to the Cal Grant. The program raises geographic polarization because its largest effects are in politically competitive locations that are home to public research universities. We find that nearly the entire increase in participation occurs among registered independents and Democrats due to the leftward lean of college-educated youth, implying that Cal Grants issued since 2010 raised Democratic margins of victory in California by 0.5 percentage points in 2020.

Evidence from intermediate outcomes and time variation highlight the importance of peer socialization and in-person college attendance among other mechanisms. Cal Grants increase peer socialization by raising the rate at which students enroll in 4-year colleges and live on campus. Effects appear within two years of award receipt, are absent during the year of COVID-19 remote instruction, and raise students' total voter turnout across post-treatment elections between 2010 and 2020. We find suggestive evidence that is relatively more mixed for alternative mechanisms like increased political trust, voter reciprocity, and income effects.

We externally validate our findings with another RD design and 2.5 million students subject to a generosity notch in the Pell Grant, a nationwide financial aid program (Denning et al., 2019). Our findings generalize despite the program targeting different types of students and institutions at a different treatment margin. We calculate that the effects of the Pell Grant were likely large enough to change the outcome of the 2020 presidential election. Still, there are two notes of caution in generalizing our results to other settings. First, colleges may differ in the extent to which they facilitate peer socialization, with on-campus housing

and non-instructional time spent on campus appearing particularly important. Second, the magnitude of these turnout effects may be amplified or diminished by the extent to which an election cycle mobilizes young voters through youth-oriented issues.

Our evidence illustrates that tuition subsidies can have civic externalities, even when they do not generate meaningful changes in labor market outcomes. The Cal Grant and Pell Grant induce the participation of an informed and active electorate by encouraging low income and college-educated youth with high GPAs to vote. Given our conservative assumptions, we conclude that the civic externalities of education subsidies represent an important marginal external benefit. Our evidence also introduces challenging new questions for the political economy of education finance. Because the benefits of higher education's civic externalities are not symmetric between parties, partisans can have strong private incentives to distort funding levels relative to the social optimum.

2 California and the Cal Grant in Context

The Cal Grant is the largest tuition-free 4-year college program in the United States, with the California Student Aid Commission (CSAC) offering several awards that have provided over 2.7 million grants to eligible California residents since 2010 (Kane, 2003; Bettinger et al., 2019; Dickler, 2022; Scott-Clayton et al., 2022). High school graduates and continuing college students who meet income and academic requirements and enroll at any in-state public university are eligible for four years of tuition-free college under Cal Grant A or three years plus an annual living stipend under Cal Grant B.[4] The program also provides a benefit to attend an accredited private institution but cannot be used at community colleges.[5] These awards are "first dollar", meaning that they are provided without consideration of eligibility for most other forms of financial aid, such as student loans or institution-specific grants.

[4]Tuition in this context refers to mandatory systemwide tuition fees. It does not include campus-specific fees for services like student government events, athletics, campus health insurance, etc..

[5]The precise award amount varies based on the type of private institution, but for most of the time period and for the most popular private colleges it was worth roughly 9,000 dollars per year.

First-time college students who wish to apply for a Cal Grant must file a Free Application for Federal Student Aid (FAFSA) and must separately complete or have their high school submit a Cal Grant-specific form that assists in GPA or test score verification to validate their educational credentials in the December of the calendar year prior to their college enrollment. First-time students typically find out their grant status around the time of their college admission offers in the Spring before they begin college. Renewing students have a streamlined process consisting of filing another FAFSA for the upcoming academic year. For our analysis, we focus on the income eligibility thresholds for the Cal Grant A program between the 2017-2018 and 2019-2020 academic years. These income thresholds vary by family structure as well as applicant characteristics and are adjusted each year based on cost of living increases set by California's constitution. Families whose adjusted assets exceed certain limits, after excluding personal residence and retirement savings, are ineligible. CSAC's switch to "prior-prior year" income assessment in 2017-2018 combined with unpredictable cost-of-living adjustments results in several plausibly exogenous discontinuities in eligibility that we use for identification of causal effects.

Meeting the income and asset requirements is not sufficient for Cal Grant eligibility. Students must also have a 3.0 high school GPA if they are an entering freshman, not hold a bachelor's degree, meet in-state residence requirements, and have a sufficiently low "expected family contribution".[6] Hence, we restrict to the subset of students coded as in-state residents without a bachelor's degree who fall below the asset threshold. As we discuss at greater length in Section 3, we omit students whose family incomes are perfectly divisible by 1,000 dollars throughout our analysis and use the 2017 to 2019 cohorts of FAFSA filers in our preferred specification to address threats to our identification strategy.

The Cal Grant is an ideal policy setting for evaluating the social returns to higher education subsidies, in part, because extant evidence on the pecuniary returns to the program provide important context for assessing civic externalities and social returns (Kane, 2003;

[6]Expected Family Contribution or EFC is a measure of socioeconomic status that depends on family income, family assets, and the cost of attendance at the institution at which a student enrolls.

Bettinger et al., 2019; Hendren and Sprung-Keyser, 2020). At the *income threshold*, the Cal Grant has null effects on total college enrollment, but there is a shift in the type of college attended, with an increase in attendance at 4-year private institutions and a reduction in attendance at public 2-year and 4-year colleges and universities.[7] The substitution between institutions is not associated with changes in college quality but is associated with higher tuition costs and lower per-student expenditures. The Cal Grant has no impact on labor income for students at the income threshold, contributing to a marginal value of public funds (MVPF) estimated in Hendren and Sprung-Keyser (2020) of -0.69, and there is no evidence that it retains individuals in-state prior to 11 years after initially filing for a grant.

The state of California is ideal for estimating the impact of education spending on political participation. Its status as the most populous market of higher education in the United States allows for precise estimates of causal effects. California has the highest post-graduation retention rate of college students among the 50 American states, offering unique advantages for longitudinally tracking students (Van Dam, 2022). Voter registration records in California are detailed, showing that the political composition of Californian college-educated youth matches that of other American states and making California a setting where roughly three quarters of voters self-identify their political party preferences in administrative data (Firoozi, 2023). Notably, party membership is not a prerequisite for participating in any primary elections in California, except presidential primaries, meaning that voters have an incentive to register with the party that best reflects their policy views rather than to strategically register with the party that dominates state politics.

[7]We do not use the GPA threshold for identification in our paper. This is because over the 2010-2011 to 2020-2021 timespan in our dataset, the GPA cutoff was fixed at 3.0 and known ex ante, unlike previous work.

3 Research Design and Data

3.1 Data

The linked dataset used in this study is a de-identified file comprised of two data sources: the L2 California voter file and financial aid applicant (FAFSA) data provided by the California Student Aid Commission (CSAC) (Firoozi and Geyn, 2024). The original dataset contained approximately 16.4 million observations generated by merging the CSAC data and L2 voter data. This matching process was completed by providing CSAC with the L2 voter data, having CSAC match on full name and date of birth, and then receiving back a version of the CSAC data with L2 voter variables and without any personal identifying information. To comply with the Federal Education Rights and Privacy Act (FERPA) and maintain anonymity, the names and dates of birth of FAFSA filers were never revealed to the authors.

The sample provided by CSAC, which administers the Cal Grant program, includes records on all of the roughly 16.4 million FAFSA filers in California between the academic years 2010-2011 and 2020-2021, excluding 2011-2012.[8] We estimate that our data cover around three quarters of all Californian first time college applicants, returning college students, and transfers over this timeframe. CSAC's records include detailed information available from the FAFSA form, including family size and structure, adjusted gross income, Cal Grant receipt, housing intent, and ZIP code of origin.

The outcome data used in this study is sourced from L2 Inc., a non-partisan private vendor of political data. Specifically, we use L2 Inc.'s complete California VM2 voter file, which is a retrospective snapshot file that reflects the California voter rolls as of July 2022. This includes identified records on approximately 21 million Californians who are registered to vote, including their political party membership and participation in every election through the 2021 California gubernatorial recall. The file also contains commercial data, which provide

[8]Database errors at CSAC prevented retreival of 2011-2012 data. For brevity we refer to the sample excluding this cohort as the full sample in the remainder of the paper.

additional outcomes and detailed information on the locations where registrants live.

We use several different samples in various parts of this paper. For our main analyses, we focus on the set of students who were likely to be Cal Grant eligible within a 10,000 dollar bandwidth of the income cutoff.[9] Table A.1 shows summary statistics for each sample we use in our paper. We note in Appendix A.1 that our main sample is majority non-white, is much more likely to favor the Democratic Party than the Republican Party, has family asset levels (excluding retirement accounts and home equity) that are near zero, and has family income levels roughly around that of the median American family.

3.2 Regression Discontinuity Design

In this study, we use a fuzzy regression discontinuity design (RDD) to estimate the impact of the Cal Grant program on political participation. The Cal Grant program's main eligibility criterion is family income, which we use as the running variable in our RDD. Specifically, we standardize a student's family income against the income ceilings for Cal Grant A set by the California Student Aid Commission (CSAC). This approach identifies a clear discontinuity in the proportion of students who receive a Cal Grant (as shown in Figure A.1).[10]

The fundamental assumption of our fuzzy RDD is that the eligibility threshold serves as a clear cutoff for program participation over which students do not have perfect control. In the context of the Cal Grant program, this means that a student whose family income falls just below the eligibility threshold is similar to one whose income falls just above it except for the fact that the latter is ineligible for the program. This assumption implies that the distribution of observed and unobserved characteristics is continuous around the threshold, which is a critical prerequisite for the validity of the design.

[9] As mentioned in Section 2, we restrict to the subset of students coded as in-state residents without a bachelor's degree who fall below the asset threshold. As we discuss at greater length in Section 3, we omit students whose family incomes are perfectly divisible by 1,000 dollars throughout our analysis to address threats to our identification strategy.

[10] Note that there is also a lower income threshold for Cal Grant B eligibility, but it generates little variation in total Cal Grant receipt and leads a subset of students eligible for Cal Grant A to receive Cal Grant B instead. Cal Grant C has the same income eligibility ceiling as Cal Grant A, but there is near zero discontinuity at the threshold as Cal Grant C accounts for only 2 percent of Cal Grants awarded.

We assess the assumptions underlying our fuzzy RDD using several tests. First, we implement a McCrary test to confirm the fuzzy RDD's validity (McCrary, 2008; Cattaneo et al., 2018). This test assesses whether there is a discontinuity in the density of observations at the threshold, which could indicate that individuals are manipulating their reported income to become eligible for the Cal Grant program or selecting into the analysis sample by filing a financial aid application based on program eligibility. As demonstrated in Figure B.1 – and as we confirm through formal tests – there is no evidence of a density discontinuity at a 90 percent confidence interval (p-value=0.50). Second, we conduct balance tests to ensure that observable characteristics trend continuously across the policy threshold. In Figures B.2 through B.4, we present evidence that pre-FAFSA covariates including voting patterns are balanced, finding only three rejections out of 18 variables at a 90 percent confidence interval with a 10,000 dollar bandwidth.[11] Our balance tests' results are robust to varying the bandwidth around the income ceilings and we find only one rejection of the null hypothesis at a narrower (2,000 dollar) bandwidth, as demonstrated in Figures B.6 through B.8. Third, we conduct placebo falsification tests to evaluate the design's validity.[12]

We also address potential threats to identification that are specific to the Cal Grant policy by focusing our main results on the 2017-2018 to 2019-2020 cohorts of FAFSA filers before broadening our sample to the 2010-2011 to 2019-2020 filers. The first threat is that some students who barely miss out on the grant may leave the state, which would make them unobservable in the outcome data from L2's California voter file. To address this risk, we use four different approaches that we describe in detail in Appendix B.1. We start by pointing to the absence of out-of-state discontinuities in tax filings and college enrollment

[11] We also show an omnibus test for balance by predicting voter turnout rates using our full set of covariates in Figure B.5. The point estimates and confidence intervals are available for a range of potential bandwidths in Figures B.6 through B.8. Our results at a 10,000 dollar bandwidth are roughly consistent with a random rejection rate. However, it is worth noting that these measures likely overstate imbalance because receiving a Cal Grant directly compels the provision of variables like GPA and may make the reporting of other covariate information to CSAC more likely by increasing the rate at which students update their information and refile FAFSAs in future years.

[12] These tests entail assigning "placebo" thresholds and assessing the estimated treatment effect at the true threshold relative to the placebo thresholds. If the placebo test fails, it implies that the design may not be valid but, as we discuss in Section 4, the outcomes of these tests confirm the validity of our findings.

records. We then use a unique feature of the L2 dataset, in which out-of-state movers have their historical data pruned, and show the absence of a discontinuity in pre-treatment voter turnout. Next, we use a subset of our sample overlapping with the sample in Firoozi (2023) to directly illustrate that the Cal Grant is estimated to have null effects on voter registration outside of California. Finally, we choose to be conservative by beginning our results section with a focus on the 2017-2018 to 2019-2020 sample, who filed a FAFSA less than 5 years prior to our voter file snapshot, minimizing the possibility of selection bias due to out-of-state migration.

The second threat to our identification strategy is that some students may get married and change their legal last name after filing a FAFSA, making it difficult to match their voter registration records to CSAC data. We address this issue by focusing on the 2017-2018 to 2019-2020 sample because few people get married and change their names on the voter roll within 5 years of filing a FAFSA. We also find that women, a group more likely to change legal names, do not drive our estimated effects and note that there is little difference in match rates between female and not female-identifying students who filed a FAFSA for the 2015-2016 academic year or later.

The final threat to our fuzzy regression discontinuity design comes from time-specific concerns. Prior to 2017-2018, families could have seen the income thresholds before the end of the tax year and attempted to manipulate their reported income or selected into FAFSA filing based on unobservable characteristics. Relying on the post-2017 period is helpful, because "prior-prior year" income evaluation took effect, making it much harder to anticipate eligibility thresholds ex ante to filing a tax return. We also exclude students whose family incomes are bunched at perfect multiples of 1,000 dollars, as these families may have greater discretion to manipulate their reported income.[13] COVID-19 is another potential

[13]These students represent roughly 6 percent of the sample within 10,000 dollars of the income ceiling and come from families that are more likely to have salaried workers than hourly workers, are likely to have little to no capital gains income from stocks and bonds, and have fewer total sources of stochastic income relative to fixed income. As a consequence, they may be more capable of manipulating their income or are more likely to be aware of their eligibility because they can easily recall their income and opt-in to filing a FAFSA after finding out the thresholds.

threat, because Californian colleges were overwhelmingly remote in the 2020-2021 academic year, changing the mechanisms that were discontinuous across the threshold.[14] To address this issue, we use the 2020-2021 cohort solely to analyze mechanisms.

Taking continuity of the conditional expectations function as given, we estimate the following reduced-form RD equation:

$$Outcome_i = \phi_0 + \phi_1 Above_i + f(Income_i) + \mathbf{X}_i'\phi_2 + \nu_i, \tag{1}$$

where $Outcome_i$ is an outcome for student i, $Income_i$ is a student's centered income with the Cal Grant A income ceiling normalized to zero, $Above_i = \mathbb{I}[Income_i > 0]$ is a binary variable for a student being above the income ceiling specific to their cohort and family structure, $f(\cdot)$ is a continuous function, $\mathbf{X}_i$ is a vector of covariates, and ν_i is an idiosyncratic error term. Assuming the RD assumptions hold, our $-\hat{\phi}_1$ estimate identifies the average effect of being income-eligible for the Cal Grant among students local to the threshold.

We also use a fuzzy RDD approach to estimate the impact of Cal Grant receipt on political participation. Specifically, we treat equation 2 as a first-stage equation:

$$CalGrant_i = \gamma_0 + \gamma_1 Above_i + g(Income_i) + \mathbf{X}_i'\gamma_2 + u_i, \tag{2}$$

where $CalGrant_i$ is an indicator for individual i having received any Cal Grant award, $Above_i = \mathbb{I}[Income_i > 0]$ is a dummy variable indicating whether individual i's family income was above the ceiling for Cal Grant eligiblity, $Income_i$ is an individual's normalized family income, $g(\cdot)$ captures the relationship between normalized income and Cal Grant receipt, $\mathbf{X}_i$ is a vector of pre-FAFSA characteristics, and u_i is an idiosyncratic error term. We estimate $\hat{\gamma}_1$ as the first-stage impact of being above the income ceiling on Cal Grant receipt.

[14] As one example, peer socialization across the threshold would have been greatly curtailed due to extensive remote instruction and the sharp reduction of interaction in on-campus housing.

Next, we use equation 3 as an outcome equation to characterize the relationship between political participation and Cal Grant receipt:

$$Outcome_i = \beta_0 + \beta_1 CalGrant_i + h(Income_i) + \mathbf{X}_i'\beta_2 + \varepsilon_i, \tag{3}$$

where $Outcome_i$ is an outcome for student i, $h(\cdot)$ reflects the relationship between normalized family income and the outcome of interest, and ε_i is an idiosyncratic error term. We combine equations (2) and (3) and estimate β_1 using two-stage least squares with $Above_i$ as our excluded instrument. Our estimate $\hat{\beta}_1$ identifies the average effect of receiving a Cal Grant on each outcome among students local to the threshold. We then test our estimates for robustness to a number of different RDD implementation choices. We vary the order of a polynomial control for the running variable, include an expansive set of pre-FAFSA controls, flexibly change the bandwidth used for inference, and estimate bias-aware confidence intervals to demonstrate robustness (Calonico et al., 2014).[15]

4 Results

4.1 Main Results

We begin by plotting our outcomes of interest against a student's centered family income in Figure 1. The first outcome is voter registration in 2022, which we use as a low intensity measure of political participation. The second outcome is voter turnout in the 2020 general election, which is the first and only general election in which all students in our main sample could have participated assuming students enter college at 18 years of age. The third outcome, labeled "General Election Turnout", is the voter turnout rate in all general elections

[15]The controls we use include foster youth status, female self-identification, voter turnout in the general election prior to FAFSA filing, family income, family financial assets excluding personal residence and retirement saving, marital status, first year freshman status, father's college attainment, mother's college attainment, cohort fixed effects, family size, and ZIP code of origin measures of the minority, Black, Hispanic, and Asian population, voter turnout, voter conservatism, as well as mean income and asset levels.

between 2010 and 2020 that took place after the academic year in which students filed their FAFSA and received a grant. Finally, we interact the voter turnout rate with an indicator for being registered as a Democrat or independent[16], which captures the extent to which this turnout increase is attributable to center-left voters. Recent evidence from this setting shows that the interaction term is a strong predictor of support for Democratic candidates because, similar to other American states, roughly 75 percent of Californian, college-educated youth who are registered independents self-report favoring the Democratic Party over the Republican Party (Firoozi, 2023).[17] On balance, the results show that students who are below the income ceiling and are, therefore, income-eligible for the Cal Grant are more likely to participate in the political process, with essentially the entire increase among registered Democrats and independents.

In Panel A of Table 1, we formally estimate the effect of receiving a 2017-2019 Cal Grant on students' political outcomes. Each column reflects results for a different outcome of interest and each panel reflects estimated effects for a different policy and sample of students. The same RD specification is used in all estimates: a local linear estimate at a 10,000 dollar bandwidth of the income threshold with a uniform kernel and no pre-treatment covariate controls. The outcome in Column 1 is a binary indicator for being registered to vote in the state of California in 2022. Column 2 shows results for a binary indicator for casting a ballot in the 2020 presidential general election. Column 3 reflects the total voter turnout rate for a student across all post-treatment elections in which they could have participated.[18] Column

[16]In California, independent refers to voters who are either registered to vote with no stated party preference or with a minor political party like the Green Party, the Peace and Freedom Party, the Libertarian Party, or the American Independent Party.

[17]Firoozi (2023) finds that registered independents in this setting are as likely to support Democrats as registered Republicans are to support Republicans, are more than twice as ideologically close to registered Democrats as registered Republicans on economic policy and sociocultural issues, and favor Democratic and liberal candidates over Republicans and conservatives by more than a 5 to 1 margin in their political donations. Data from election returns in college campus precincts in both California and other American states illustrate that Republican candidates receive vote totals approximately equal to their share of the registered voters who cast a ballot, corroborating the idea that young, college-educated independents disproportionately vote for Democrats (Firoozi, 2023).

[18]For these more recent cohorts of students, only the 2020 and/or 2018 election are included as post-treatment elections.

4 shows results for a binary indicator for having ever boted in a federal or state general election. Column 5 tests the results for an interaction term between an indicator for being registered as a Democrat or independent and the voter turnout rate in Column 3. Column 6 tests results for an interaction term between being registered as a Republican and the voter turnout rate in Column 3.

Receiving a Cal Grant sharply increases political participation among these students. Although results for voter registration are noisy and sensitive to RD specification choices in our robustness checks, we find that voter turnout rates rise between 8 and 10 percentage points across the various measures of this outcome in Columns 2 through 4. This indicates that the Cal Grant raises both the intensive and extensive margins of political participation. The entirety of the increase is attributable to voters who are likely to support Democratic Party candidates, with the point estimate on center-left turnout in Column 5 exceeding the point estimate for total turnout in Column 3. There are no detectable effects of receiving a Cal Grant on voter turnout rates among voters registered with the Republican Party, with a negative point estimate in Column 6.

To ensure that the estimates in Panel A are robust, we re-estimate results varying a number of RD implementation choices. We first estimate results across different bandwidths, varying the inclusion of pre-treatment covariate controls and the order of a polynomial control for the runnning variable (See Appendix C.1 and Table C.1). We then estimate results separately by gender identity in Table C.2 to show that our results are not driven by women changing their last names after marriage. We then present findings in Figures C.1 through C.5, highlighting that the estimated reduced-form discontinuities in our outcome variables are relatively stable across a wide range of bandwidths, the inclusion of pre-FAFSA covariates, and the use of a quadratic control for the running variable. Next, we show in Table C.3 that our main results are robust to the use of CCT bias-aware confidence intervals. Finally, we conduct falsification tests in Figure C.6, in which we assign a dummy variable for Cal Grant income-eligibility based on placebo income thresholds and then compare the

results to those derived from the true income ceiling, finding that our results at the true policy threshold are larger than the 95th percentile of results in these placebo tests for all measures of voter turnout.[19]

4.2 Time Variation

We extend our analysis to include FAFSA filers from the 2010-2011 to 2020-2021 academic years to examine time variation in treatment effects. While we acknowledge the identification risks that come from earlier and later cohorts (i.e. from potential income manipulation in the earlier time period and from remote instruction in the latter time period), we believe that these results are useful for understanding the causal mechanisms underlying the Cal Grant's impact on political participation.

In Panel B of Table 1, we show the impact of the Cal Grant on political participation for the 2010-2011 to 2019-2020 sample of likely Cal Grant-eligible FAFSA filers using the same set of outcomes and RD specification as Section 4.1. The results are consistent with those of the more recent sample of FAFSA filers, but with smaller magnitudes. Although the impact of Cal Grants on voter registration in Column 1 is again somewhat noisy and sensitive to model specification (See Table D.1), we find significant increases in voter turnout between 4 and 7 percentage points across the definitions in Columns 2 through 4. In Columns 5 and 6, we again find that over 85 percent of the effect on voter turnout accrues to registered Democrats and independents, consistent with our evidence from more recent cohorts. Each of these results are robust to changes in RD specification choices like the inclusion of pre-treatment controls, varying the order of a polynomial control for the running variable, and changing the bandwidth around the threshold used for inference (See Table D.1).

[19] We generate a "placebo threshold" at each 500 dollar increment along centered family income, and compare the estimated reduced form impact of these synthetic policies relative to the true policy. Placebo thresholds are bounded between -20,000 and +60,000 dollars relative to the true income ceiling, because this avoids false positives from capturing discontinuities taking place at family incomes of zero at the lower bound and this spans up to the 98th percentile of centered income on the upper bound. A 10,000 dollar bandwidth is used to remain consistent with our preferred specification. We exclude discontinuities within a 10,000 dollar bandwidth of the true cutoff to avoid generating false positives by including the actual policy discontinuity in our placebo estimates.

In Appendix D.1, we use other dimensions of time variation to understand the Cal Grant's impact on political participation. We find that point estimates of these effects are largest within one or two years of receiving a Cal Grant and appear to shrink over longer time windows, but we cannot reject the null hypothesis that they are flat over time. The 2020 COVID-19 student cohort[20] have null effects, in stark contrast to the large effects observed for the 2017-2019 cohorts of students. Lastly, our point estimates are largest for the 2020 election and are generally larger in presidential cycles than midterm cycles, suggesting that campaigns in which youth-focused issues are salient may amplify the effects we observe.

4.3 External Validity, Heterogeneity, and Intermediate Outcomes

We are also able to assess the external validity of our findings for the Cal Grant by estimating the impact of the Pell Grant, the largest nationwide tuition subsidy in the United States. We use an income threshold that generates variation in the generosity of Pell Grants to identify the impact of the intensive margin of financial aid generosity, as opposed to the extensive margin of financial aid receipt we tested with the Cal Grant. We implement an RD design comparing students just above and below a an income ceiling that hovers in the 20,000 to 30,000 dollar per year family income range, and provide a detailed analysis and description of our approach in Appendix C.2. Figure 2 displays each of the same outcomes of interest as Figure 1 from Section 4.1, but plots students based on their family income normalized to the threshold for receiving a more generous Pell Grant. Matching the Cal Grant's pattern, the students just below the income limit to receive a more generous Pell Grant are more likely to register to vote, have higher post-treatment voter turnout rates, and are more likely to cast a ballot as either a Democrat or an independent than their counterparts just above the income limit.

In Panel C of Table 1 we estimate the intent to treat effect of this increase in Pell Grant generosity, using the same outcomes and specifications as we did for Cal Grants and

[20]These are students whose instruction was almost entirely online prior to election day and had essentially no opportunities for peer socialization.

noting that the dollar value of this increase in Pell Grant aid is roughly one tenth that of receiving a Cal Grant. Pell Grants have noisy, if any, positive effects on voter registration rates in Column1. Similar to the Cal Grant, the Pell Grant induces clear increases in voter turnout across each definition of the variable in Columns 2 through 4, with over 80 percent of the increase attributable to Democratic and independent voters in Column 5 rather than Republican voters in Column 6. We note that each of these outcomes, with the exception of voter registration, are robust to the use of different RD implementation choices (See Table C.4).

In Appendix D.2 we evaluate the heterogeneous effects of Cal Grants on political participation across various demographic and socioeconomic dimensions. The analysis looks at treatment effect heterogeneity by race, ethnicity, socioeconomic status, political background, and high school GPA. While we find little variation in the impact of Cal Grants based on racial or ethnic background, socioeconomic factors, or political environment of a student's home location., there are stronger effects on political participation among students with higher high school GPAs, above 3.41 or roughly a B+ on the American grading scale.

After examining heterogeneity we turn to housing choices in Figure 3, using noisy CSAC records on students' reported housing intent for Cal Grant non-recipients and actual housing outcomes for recipients. The RD plots in Figure 3 show a clear and significant increase in the share of Cal Grant recipients living on campus, accompanied by sharp declines in the share of students living off campus with family or legal guardians or who did not report their housing intent. The decrease in the latter group is due to the fact that receipt of a Cal Grant requires students to disclose their housing choice *ex post*. We note that, because of the housing choice variable's definition, our estimates will be subject to some amount of bias in the direction of students' underestimated housing expectations.

The large changes we observe in reported housing status and intent nonetheless point to large impacts from financial aid. We show the impact of the Cal Grant on campus housing choice formally in Table 2. Our results indicate a 17.17 percentage point increase in the share

of students living on campus as a result of the Cal Grant under our preferred specification in Column 1, with corresponding decreases of 13.17 percentage points in the share of students with no stated housing intent and 9.20 percentage points in the share of students living off campus with family or guardians. This suggests that the Cal Grant induces some students who did not have a strong preference or who planned to live with family to instead live on campus. We note that this effect could be driven in small part by the fact that Cal Grants reduce enrollment at community colleges, which do not offer on-campus housing, and increase enrollment at 4-year colleges, which house a high proportion of students on campus.

5 Discussion

5.1 Mechanisms

We present evidence on four plausible causal mechanisms that could operate independently or collectively: campus-based socialization, electoral reciprocity, civic trust, and income effects. While this is not an exhaustive list of all possible explanations for the civic returns to tuition subsidies, we believe these mechanisms are the most compelling ones in our setting. Moreover, we note that several other sets of mechanisms can effectively be ruled out as meaningful explanations of these externalities, given that they rely on first-stage effects that do not occur or that are small in our policy setting.

Because the 10 percentage point effect of the Cal Grant on voter turnout exceeds the 0.2 percentage point impact on college enrollment and the 6.8 percentage point substitution from 2-year to 4-year colleges, changes in college enrollment are insufficient to fully account for the causal effects of the Cal Grant (Bettinger et al., 2019). Even assuming that switching from a 2-year college to a 4-year college increases voter turnout by 20 percentage points due to mechanisms like voter registration drives or other differences between campuses, changes in enrollment could account for no more than 15 percent of our total estimated externalities.[21]

[21] A 20 percentage point estimate is larger than what is suggested by the extant literature and is intended

As Appendix Table E.1 shows, voter turnout rises even conditional on voter registration, suggesting that plausible mechanisms must raise voter turnout beyond their impact on voter registration status or enrollment choice.

Given that changes in enrollment choices are not sufficiently large for differences between campuses to explain the majority of our observed effects, we turn to our four main plausible mechanisms: on-campus socialization, electoral reciprocity, civic trust, and income effects. First, Cal Grants may facilitate on-campus socialization in 4-year colleges by extending the amount of time students spend at college campuses and increasing the share of students living on-campus independently of their enrollment choice. Second, students may think about the social transfers they received and reciprocate support from Democratic politicians by casting votes for the same politicians. Third, receiving a large conditional cash transfer early in adulthood may increase young people's trust in civic institutions and government. Fourth, financial aid programs may be similar to an increase in total family income, raising voter turnout and shifting students toward the political right.

5.1.1 Peer Socialization

We start with the idea that exposure to the college campus environment increases socialization and, in turn, raises voter turnout (Persson, 2015; Firoozi, 2023). The results of Table 2 demonstrate that Cal Grant receipt increases the rate at which students live on campus by 15 to 20 percentage points and reduces the rate at which they are housed with their family or guardians. Cal Grants cause substitution from 2-year to 4-year colleges in California and generate imprecise but positive increases in the share of students completing 4-year degrees and graduate degrees, extending the amount of time students spend in higher education (Bettinger et al., 2019). Evidence from time variation also supports the idea that some element of in-person exposure to a 4-year campus environment matters. As Table D.2 illustrates, effects appear within the first two years of receiving a Cal Grant, the same time

to represent a strict upper bound (Bell et al., 2024).

period over which a student is more likely to live on campus and to remain enrolled in higher education. Null effects for the 2020-2021 COVID-19 year in Table D.3 may likewise suggest that online instruction does not generate similar effects.

5.1.2 Voter Reciprocity

The second mechanism we test is the idea that students reward government transfer payments like the Cal Grant with their votes, a pattern of reciprocity observed among conditional cash transfers and stimulus spending in other contexts (Manacorda et al., 2011; Pop-Eleches and Pop-Eleches, 2012; Huet-Vaughn, 2019; Firoozi, 2024). We find evidence that is generally mixed on this hypothesis. Contrary to the hypothesis, we show that the largest increase in turnout takes place in the 2020 presidential general election, when California did not vote on state government offices that are critical for tuition policy (see Tables C.1, D.1, and D.4).[22] The effects we observe for the intensive margin of financial aid generosity in Section C.2 suggests that the salience of receiving any financial aid is not the primary driver of these effects. Furthermore, null effects on turnout for the 2020-2021 students who received financial aid but did not attend in-person college as a result of COVID-19 suggest that reciprocity alone may not be sufficient to generate our observed results.

While these findings are inconsistent with reciprocity, they do not conclusively rule it out. For example, Cal Grant recipients may have voted for Democratic politicians at the federal level in 2020 because federal Democrats are co-partisans who support national financial aid programs. In Appendix Table E.2, we test whether Cal Grants change students' party registration or vote choice.[23] To the extent that Cal Grants have any impact on registration, the whole of the increase occurs among Democrats and independents with insignificant, negative effects for the Republican Party, shifting partisanship to the left. To the extent that such changes reflect partisan persuasion and differ from an even split between parties,

[22]For context, elected officials including the Governor and Superintendent of Public Instruction hold seats on university governing boards, where they can and do directly vote on tuition policy and hold line-item vetoes over Cal Grant funding.

[23]We show the same exercise for the federal Pell Grant in Table E.3.

this evidence may be consistent with a voter reciprocity mechanism.

5.1.3 Civic Trust

The third mechanism, called civic trust, posits that receiving in-kind transfer payments like financial aid early in adulthood fosters greater trust in civic institutions and government, which then leads to higher voter turnout (Alesina and Wacziarg, 2000; Wang, 2016). This mechanism is supported by the rapid rise in post-treatment voter turnout and the disproportionate share of newly registered voters affiliating with Democrats, who are generally perceived as favoring more government spending. However, the seemingly short-lived treatment effects in Section D.1 are not especially consistent with a durable change in civic trust and support for government. Furthermore, despite predictions by Alesina and Wacziarg (2000) that differences in pre-treatment civic trust based on race and income should lead to heterogeneous treatment effects of financial aid by race and socioeconomic status, our data does not support significant racial or socioeconomic heterogeneity in these effects (see Table D.5 and Section C.2).

5.1.4 Income Effects

The final mechanism we evaluate is that financial aid induces higher voter turnout through income effects if voting is a normal good and reducing the cost of college attendance reduces student labor hours.[24] Under this mechanism we should expect short-run increases in voter turnout that dissipate over time, effect sizes that are roughly in line with the association between income and voting, a partisan shift toward the political right, and larger effects for students who start off with lower family incomes (Romer, 1975; Meltzer and Richard, 1981). To the first point, we are able to show in Table D.2 that there is some evidence that effects are largest shortly after receiving the grant.

However, evidence on the latter three points are inconsistent with income effects. We

[24] Although the Cal Grant does not generate an increase in long-run earnings at the income threshold we study, it may still function similarly to a short-run increase in family income (Bettinger et al., 2019).

note that the estimated impact of both the Cal Grant and Pell Grant on voter turnout are conservatively in the range of 0.4 to 0.8 percentage points per thousand dollars, which significantly exceed the roughly 0.2 percentage point association between turnout and income. Moreover Tables E.2 and E.3 show that financial aid programs do not shift registration toward the political right. We also do not find evidence that effect sizes are considerably larger for lower income students either in Table D.5 or through the comparison of Pell Grant and Cal Grant effect sizes in Section C.2, which are identified at different points of the income distribution.[25] The similar per-dollar estimates we find for both the intensive and extensive margin of financial aid across a wide range of family income runs contrary to the diminishing marginal returns we would otherwise expect from an income effects mechanism.

5.1.5 Significance

On balance, our results suggest that the Cal Grant's effects are likely driven by peer socialization, with plausible contributions from voter reciprocity and increased civic trust. The implications of these findings on mechanisms are twofold. First, because our work suggests that in-person socialization within colleges can play an important role in the formation of political behavior, our work contributes to the extant literature on political cleavages by education. By highlighting the role of dormitories and on-campus non-instructional time, our work could help explain the puzzle of why higher education is associated with political engagement in rich and middle-income democracies, but not other contexts (Przeworski, 2008; Gallego, 2010; Gethin et al., 2021b).[26] Second, by raising the possibility that Cal Grants simultaneously change partisanship and voter turnout, this paper poses challenging questions about the incentives elected officials have when setting tuition subsidies.

[25]The typical Pell Grant near-threshold student has a family income of roughly 25 thousand dollars per year, whereas the typical Cal Grant near-threshold student has a family income of roughly 73 thousand dollars per year.

[26]Low-income countries typically have universities and colleges that house a much smaller proportion of students on-campus and in dormitories. Non-democracies do not typically allow for a robust political and activist culture to be established on college campuses.

5.2 Macro-Level Impact and Policy Implications

Having tested the generalizability of our findings with the federal Pell Grant and seeing suggestive evidence of large impacts on the political system, a natural question arises about the extent to which the Cal Grant itself has changed California's political landscape. We approach this question in three ways, using the full sample of 16.4 million FAFSAs and 21 million registered California voters. First, we evaluate the aggregate increase in voter turnout induced by the Cal Grant relative to total participation in California to provide a sense of scale. Second, we consider the electoral implications of these results based on the partisan leanings of new voters who are induced to enter the electorate as a result of the policy. Third, we analyze the geographic distribution of Cal Grant recipients after voter registration to identify the types of community that receive the bulk of these effects.

We have made a deliberate effort to err on the side of conservative assumptions throughout this analysis. Our goal is to present an accurate picture of the impact of tuition subsidies on California's political outcomes, but we note that our assumptions will likely understate the full extent of this impact.

To estimate the impact of the Cal Grant on political participation, we assume that the local average treatment effect (LATE) for compliers near the income ceiling is the same as the average treatment effect for all Cal Grant recipients (ATT), including those with family incomes well below the income threshold we study. We view this assumption as reasonable because we find little evidence of heterogeneity by race or socioeconomic status in Table D.5 and because estimated treatment effects of the Pell Grant estimates in Section C.2 are similar on a per-dollar basis for a pool of students who have roughly 1/3rd as much income.[27] However, if this assumption is inaccurate, we anticipate that the impact of the Cal Grant on political participation may be plausibly greater for inframarginal Cal Grant recipients, who are lower income on average. Moreover, we have used the presidential election as a

[27]The typical Pell Grant near-threshold student has a family income of roughly 25 thousand dollars per year, whereas the typical Cal Grant near-threshold student has a family income of roughly 73 thousand dollars per year.

benchmark outcome to increase the denominator of our estimates, as it had the highest turnout rate of any election in 2020.

It is worth noting that our analysis is narrow in defining the people we consider treated by the Cal Grant. We assume that Cal Grants issued prior to 2010-2011 and in 2020-2021 have no effect on political outcomes, which excludes more than a decade of treated students. Furthermore, we assume that no other financial aid programs, including the federal Pell Grant, have impacted students' political participation despite our strong evidence to the contrary from Section C.2. We also assume that there is no persuasion effect despite suggestive evidence in Table E.2, meaning that students who would have voted regardless of Cal Grant receipt are not more likely to cast their vote for Democrats to reward them for the policy. Finally, we assume that there are no spillover effects on the voter turnout rates or party preferences of Cal Grant recipients' parents, families, or any other individuals.

In Appendix A.2 and Table A.2, we show our calculations of the estimated impacts of the Cal Grant on the 2020 presidential election results in California. Across three columns, we generate a lower bound estimate of the impact on partisanship and turnout, an upper bound estimate, and our best estimate using preferred specifications and the most accurate data available. We vary three assumptions across the columns to capture the full range of plausible results: the estimated treatment effect on 2020 voter turnout, the partisan composition of new voters who enter the electorate, and the aggregate number of people who received a Cal Grant over the 2010-2011 to 2019-2020 cohorts.

Under our best estimate from our preferred specifications, we find that Cal Grants issued between 2010-2011 and 2019-2020 increased aggregate 2020 general voter turnout in the state of California by approximately 259,000 votes and Joe Biden's margin of victory over Donald Trump by 168,000 votes. These figures correspond to a roughly 1 and 0.5 percentage point increase in the California CVEP turnout rate and the Democratic statewide margin of victory. Stated another way, 1 out of every 66 Californian voters in 2020 voted because of the Cal Grant program along with 1 out of every 55 Californian Biden voters. The magnitude

of these effects has important policy implications, especially given that they omit all other forms of financial aid. The increases occur among low-income youth – one of the most underrepresented groups in the electorate – and college-goers with high GPAs, who would plausibly contribute to an informed citizenry.

Figure A.2 provides insight into the geographic distribution of the Cal Grant's impact on voter turnout. Specifically, we show the number of Cal Grants received per capita in each county based on the 2022 voter registration address of registered Cal Grant recipients. Our findings are noteworthy for two reasons. First, although the sample is disproportionately Hispanic and Asian youth that originate from low income cities and rural areas, we observe that the effects are most prominent in affluent, suburban counties that are dominated by public research universities. The top quartile of California's 58 counties by per capita Cal Grant impact are home to six out of the UC's nine undergraduate campuses (See Figure A.3 for campus locations). Second, the new voters' concentration in well-educated suburbs is likely to accelerate the suburbs' leftward shift, which was already happening due to the growing education cleavage in Western democracies (Gethin et al., 2021a). For example, the largest per capita impacts in Southern California are not concentrated in urban Los Angeles, but Orange County and San Diego, which are each home to one UC and two CSU campuses and have recently supported Republicans running for statewide office.

Given the stark leftward lean of college-educated youth across democratic countries, there are clear partisan incentives in the expansion of higher education and tuition subsidies. For left of center politicians, the results are unambiguous. Tuition subsidies can raise political participation among a group that is disproportionately likely to support their candidates, regardless of its impact on earnings and degree attainment. This poses a risk that left of center candidates could support tuition subsidies to increase their vote share, even when such policies have no benefits for labor market outcomes. Conversely, right of center politicians could face a tradeoff between potentially higher earnings, degree attainment, and political participation on the one hand and a net loss in votes on the other. At worst, this tension

may lead to misaligned incentives for policymakers and partisan cycles in subsidies for higher education observed in other contexts (Ortega, 2020). Binding budget formulas that peg spending to the youth population and price levels or supermajority rules to change tuition subsidies may be welfare enhancing under such circumstances.

6 Conclusion

We use a regression discontinuity design and 16.4 million financial aid applications to estimate the impact of a tuition-free college program, the Cal Grant, on political participation among college students. The results show that Cal Grant receipt sharply increased voter turnout, almost entirely among left-leaning voters. Under conservative assumptions, our preferred specification suggests that the 2.6 million grants awarded over the 2010s induced an additional 259,000 people to vote in the 2020 general election in California, raising the voter turnout rate and the Democratic margin of victory by 1 and 0.5 percentage points respectively. These effects are amplified by the disproportionate share of the electorate such voters represent in educated suburbs and small college towns.

This paper contributes to a notable shift in the literature on education spending's civic externalities, which increasingly posits socialization rather than non-cognitive skills as a key mechanism (Persson, 2015; Mendelberg et al., 2017). Our evidence suggests an important role for in-person time and on-campus residence in higher education, particularly in recent elections like the 2020 US presidential election. Time variation in treatment effects and evidence on mechanisms pose important questions about how our results will apply to elections in future time periods and other settings. The magnitude and generalizability of these externalities will likely hinge on (1) the extent to which educational institutions facilitate socialization through in-person, non-instructional time, and (2) the salience of youth-oriented, socio-cultural issues and platforms that may be responsible for the growing political-education gradient (Przeworski, 2008; Gethin et al., 2021b)

A key policy implication of this study is that tuition subsidies benefit the electoral prospects of left-of-center political parties, despite recent evidence suggesting that higher education shifts students toward the economic right (Mendelberg et al., 2017; Scott, 2022). This is in large part due to the pre-existing cultural liberalism of college educated youth, but there are plausible contributions from the persuasive effects of socialization as well (Apfeld et al., 2022; Firoozi, 2023). The partisan externalities of higher education could act as a barrier to the expansion of these programs, as partisan policymakers have a private incentive to distort the level of financial aid provision relative to the social optimum. Such motivations present tough questions for the political economy of education finance because they may encourage elected officials to expand or reduce education subsidies with little regard to beliefs about their impact on earnings or social welfare.

The Cal Grant is a compelling higher education policy from the standpoint of external validity because it has clear analogs to tuition subsidies and tuition-free college programs in other settings. Our findings highlight an important example of how financial aid can increase political participation, even among middle income students who do not earn more money and are no more likely to attend college as a result of the program. Evidence from the Pell Grant helps externally validate these results with an alternative program that targets different students, subsidizes attendance at a different set of institutions, and impacts policy through a different margin of treatment.

Policymakers and economists have asserted for several centuries that public education expenditures enhance civic life by producing an informed citizenry and broader representation in the political process (Mann, 1957; Friedman, 2020). Our work shows that large-scale tuition subsidies can do both: inducing middle and low-income and college-educated youth to vote, with effects concentrated among high GPA students. Given that the Cal Grant's effects take place even absent any labor market returns, we conclude that the civic externalities of education expenditures are an important part of their marginal social benefits (Finkelstein and Hendren, 2020; Hendren and Sprung-Keyser, 2020).

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Tables

Table 1: Estimated Impact of College Tuition Subsidies on Political Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Registered	2020 Vote	Turnout	Ever Voted	Left Vote	Right Vote
A. Estimated Impact of Receiving a 2017-2019 Cal Grant						
Cal Grant	0.0634*	0.0985**	0.0855**	0.0848**	0.0891**	-0.0036
	(0.0285)	(0.0305)	(0.0277)	(0.0304)	(0.0277)	(0.0150)
Mean	[0.6914]	[0.5661]	[0.4884]	[0.5819]	[0.4152]	[0.0731]
N	258,329	258,329	258,329	258,329	258,329	258,329
B. Estimated Impact of Receiving a 2010-2019 Cal Grant						
Cal Grant	0.0522**	0.0615**	0.0443**	0.0611**	0.0388**	0.0055
	(0.0175)	(0.0180)	(0.0146)	(0.0180)	(0.0143)	(0.0073)
Mean	[0.6292]	[0.5111]	[0.3863]	[0.5515]	[0.3294]	[0.0569]
N	738,046	738,046	738,046	738,046	738,046	738,046
C. Estimated Impact of Increasing 2010-2019 Pell Grant Generosity						
Pell Grant	0.0034**	0.0052**	0.0039**	0.0042**	0.0033**	0.0006
	(0.0012)	(0.0012)	(0.0010)	(0.0013)	(0.0010)	(0.0004)
Mean	[0.5894]	[0.4372]	[0.3256]	[0.4802]	[0.2906]	[0.0349]
N	2,561,537	2,561,537	2,561,537	2,561,537	2,561,537	2,561,537
Bandwidth	$10,000	$10,000	$10,000	$10,000	$10,000	$10,000
Kernel	Uniform	Uniform	Uniform	Uniform	Uniform	Uniform
Polynomial	1	1	1	1	1	1
Controls	No	No	No	No	No	No

Note: $^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$. Heteroskedasticity robust standard errors in parentheses. Control group mean values of each outcome variable are shown in brackets below each estimate. "N" refers to the sample size used in each regression. "Registered" refers to a binary indicator for being registered to vote in California in 2022. "Turnout" refers to the share of all post-treatment federal general elections between 2010 and 2020 in which a student voted after the academic year in which they filed a FAFSA, which includes up to two elections in 2018 and 2020 for students in the 2017 to 2019 cohorts. "Ever Voted" refers to a binary indicator variable for ever having cast a ballot in a federal or state general election. "Left Vote" refers to the interaction between voter turnout in Column 3 and an indicator for whether the student was a registered Democrat or independent, following Firoozi (2023). "Right Vote" refers to the interaction between voter turnout in Column 3 and an indicator for being registered with the Republican Party.

Table 2: Estimated Impacts of 2017-2019 Cal Grant Receipt on Student Housing Choice

Outcome	(1)	(2)	(3)	(4)	(5)	(6)
On Campus	0.1717**	0.1584**	0.1646**	0.1427**	0.1921**	0.1691**
	(0.0215)	(0.0203)	(0.0333)	(0.0317)	(0.0304)	(0.0289)
Off Campus	0.0520^{+}	0.0707**	-0.0051	0.0300	0.0243	0.0645^{+}
	(0.0302)	(0.0264)	(0.0467)	(0.0411)	(0.0429)	(0.0376)
With Guardians	-0.0920**	-0.1031**	-0.0597	-0.0781*	-0.0987**	-0.1197**
	(0.0267)	(0.0249)	(0.0414)	(0.0388)	(0.0379)	(0.0354)
No Housing Info	-0.1317**	-0.1260**	-0.0998**	-0.0946*	-0.1177**	-0.1139**
	(0.0247)	(0.0247)	(0.0385)	(0.0385)	(0.0351)	(0.0351)
Bandwidth	$10,000	$10,000	$10,000	$10,000	$5,000	$5,000
Kernel	Uniform	Uniform	Uniform	Uniform	Uniform	Uniform
Polynomial	1	1	2	2	1	1
Controls	No	Yes	No	Yes	No	Yes
Sample Size	258,329	258,329	258,329	258,329	123,774	123,774

Note: $^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$. Heteroskedasticity robust standard errors in parentheses. "On Campus" refers to students who received a Cal Grant and lived on campus or who did not receive a Cal Grant and stated an intent to live on campus."Off Campus" refers to students who received a Cal Grant and lived off campus at a residence without family or guardians or who did not receive a Cal Grant and stated an intent to live off campus at a residence without family or guardians. "With Guardians" refers to students who received a Cal Grant and lived off campus at a residence with family or guardians or who did not receive a Cal Grant and stated an intent to live off campus at a residence with family or guardians. "No Housing Info" refers to students who did not receive a Cal Grant and did not express a preference for their housing intent.

Figures

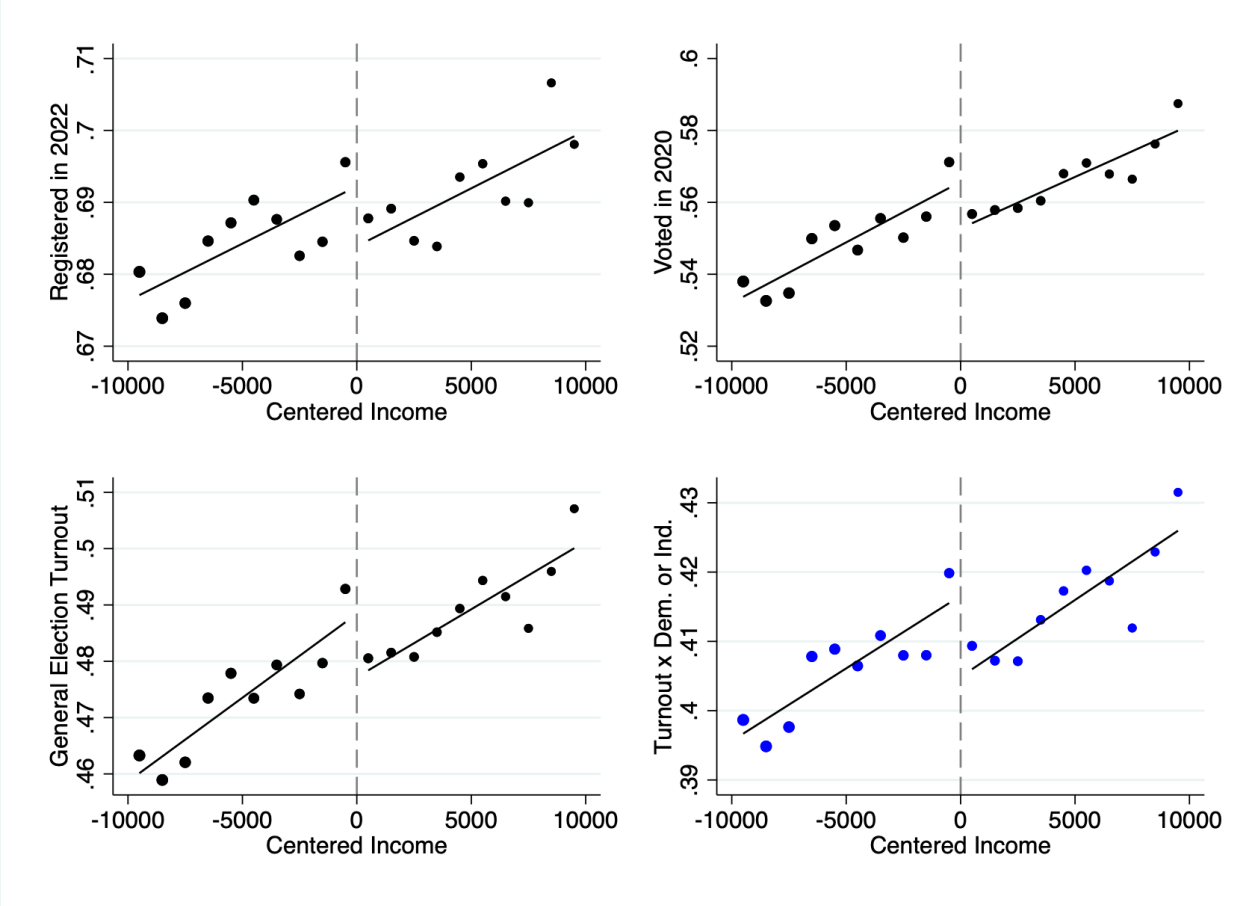

Figure 1: Regression Discontinuity Plots of Main Outcomes at Cal Grant Income Ceiling

Note: Centered income values are normalized to the income ceiling for Cal Grant A for a given individual. Outcomes correspond directly to those in Tablse 1 and C.1. The sample includes Cal Grant students in the 2017 through 2019 entering student cohorts.

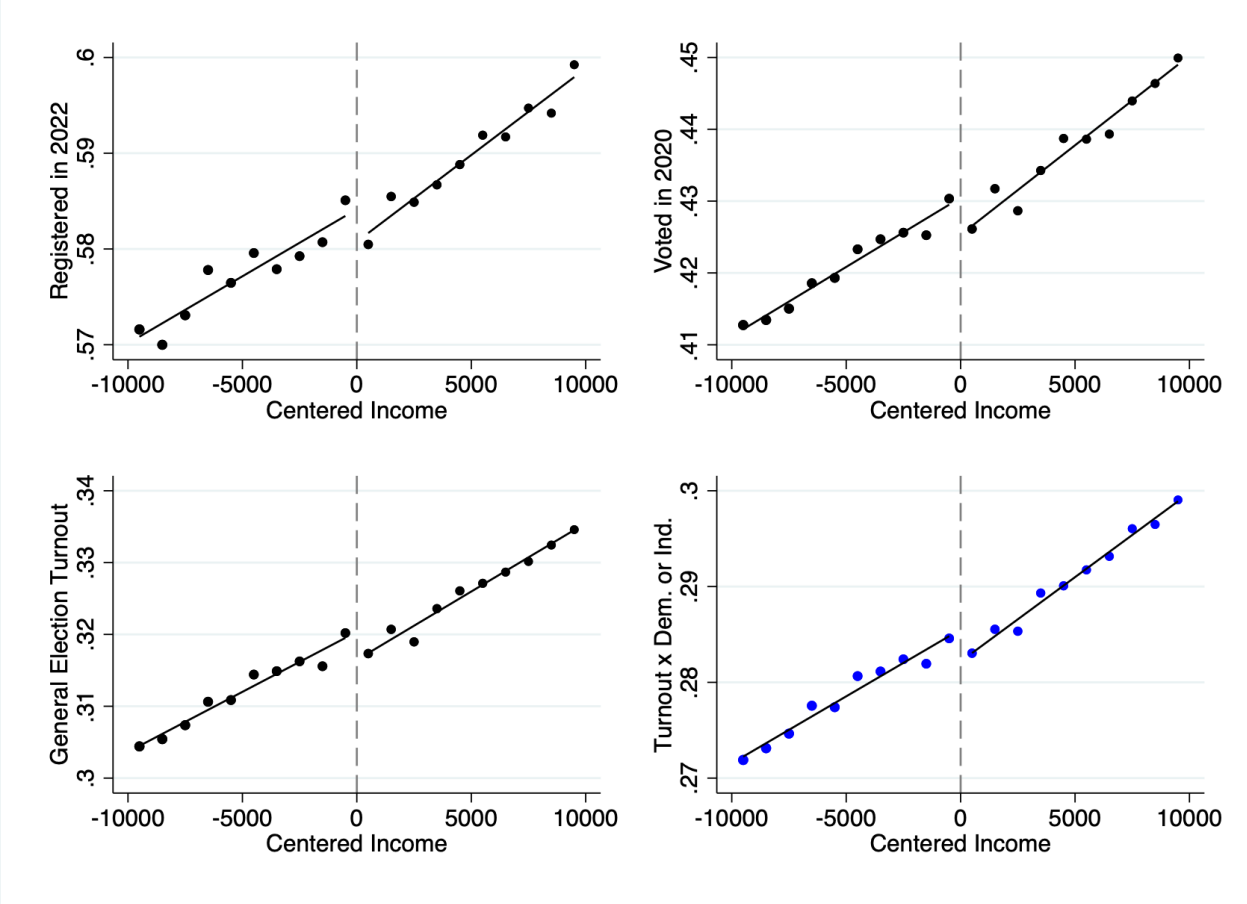

Figure 2: Regression Discontinuity Plots of Main Outcomes at Pell Grant Generosity Notch

Note: Centered income values are normalized to the Pell Grant's automatic zero expected family contribution threshold. Outcomes correspond directly to those in Tables 1 and C.4. The sample includes Pell Grant students in the 2010 to 2019 entering student cohorts.

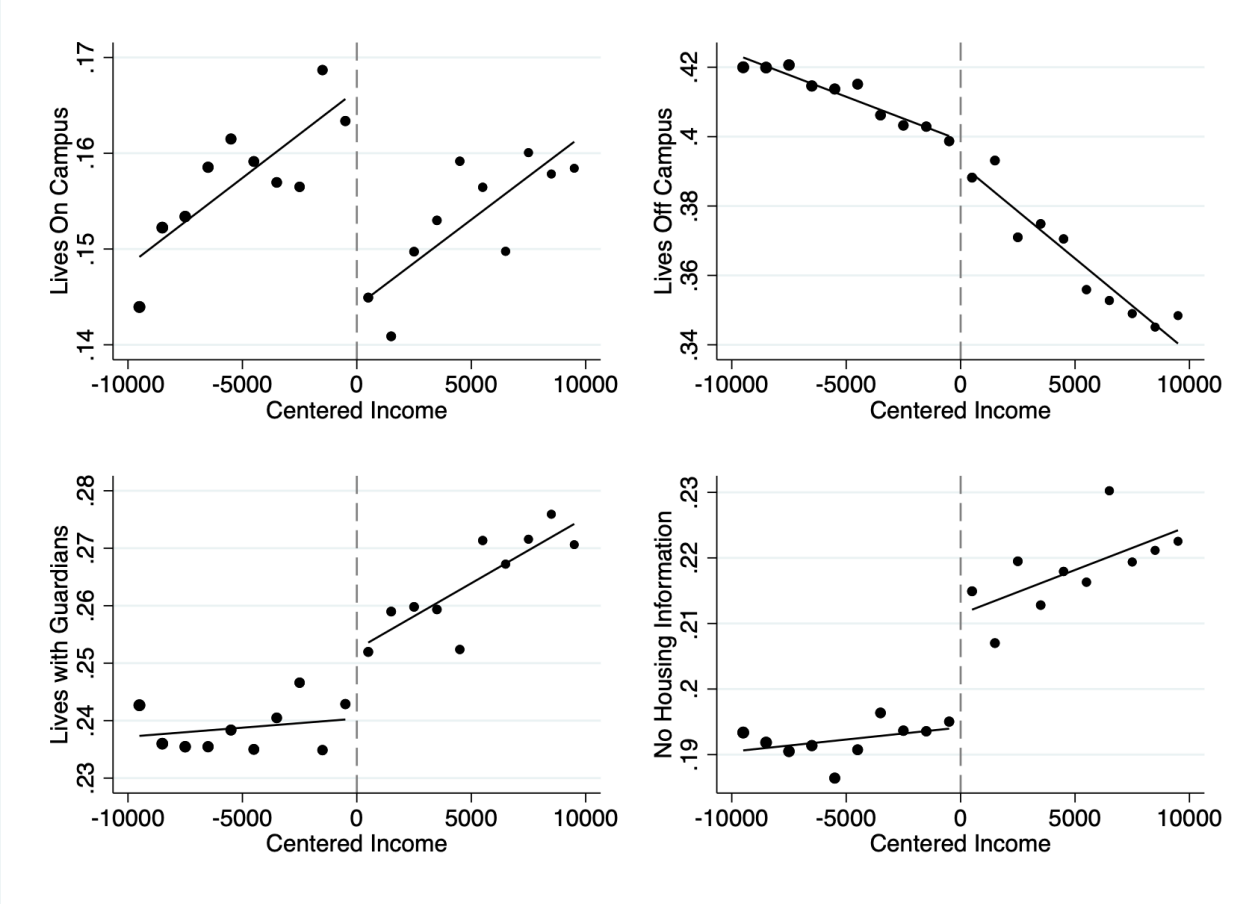

Figure 3: Regression Discontinuity Plots of Housing Choice and Intent

Note: Centered income values are normalized to the income ceiling for Cal Grant A for a given individual. Outcomes correspond directly to those in Table 2. The sample includes Cal Grant students in the 2017 through 2019 entering student cohorts.

Online Appendices

A Summary and Descriptive Statistics Appendix

A.1 Descriptive Statistics

Table A.1 shows summary statistics for the full sample of all FAFSA filers from 2010-2011 to 2020-2021 in Column 1, likely Cal Grant eligible FAFSA filers from 2010-2011 to 2020-2021 in Column 2, likely Cal Grant eligible FAFSA filers from 2017-2018 to 2019-2020 in Column 3, near-threshold and likely eligible FAFSA filers from 2017-2018 to 2019-2020 in Column 4, and near-threshold and likely eligible FAFSA filers for the 2010-2011 to 2019-2020 cohorts in Column 5.

Beginning with Column 1, we note that the sample of FAFSA filers is likely to be majority Hispanic and Asian, has family income and asset levels of 53,000 dollars and 39,000 dollars, and overwhelmingly favors the Democratic Party relative to the Republican Party.[28] This is similar to the subsample of likely Cal Grant eligible FAFSA filers in Column 2, except for higher political participation and much lower family asset levels due to the Cal Grant's asset eligibility ceiling. In Column 3, we show that the characteristics of likely Cal Grant eligible FAFSA filers in the analysis sample does not change much in the time period after prior-prior year income assessment took effect, suggesting that restricting the sample to those less able to manipulate their income does not change the observable demographics of the sample.

Finally, in Columns 4 and 5, we note that our main and expanded samples of near threshold FAFSA filers who were likely to be Cal Grant eligible are more likely to be white, have higher incomes and lower family assets, and are more likely to be politically active at baseline than their peers in the full FAFSA filer sample. The intuition behind these differences is that we are focusing on students local to the maximum allowable income to remain eligible for financial aid, while excluding students who are ineligible based on other characteristics. We note that our main sample is a policy-relevant group because debates over the expansion of financial aid often center on extensions of eligibility to higher quantiles of the family income distribution.

[28] Conditional on observing race, a majority of students are Hispanic or Asian American. We expect that consistent with general patterns in the American electorate, non-registered voters are more likely to be Hispanic and Asian American than registered voters. This suggests that our racial and ethnic composition descriptive statistics conditional on registration understate the share of FAFSA filers who are racial or ethnic minorities.

Table A.1: Descriptive Statistics for In-Sample FAFSA Filers

Sample	Full	Eligible	Analysis	Main	Expanded
Sample Size	16,393,526	8,563,732	3,318,159	258,329	738,046
A. Race and Ethnicity (Missing if Not Registered to Vote)					
Share Latino	0.225	0.280	0.307	0.277	0.243
Share White	0.161	0.166	0.174	0.240	0.224
Share Asian	0.068	0.067	0.065	0.060	0.060
Share Black	0.019	0.020	0.020	0.020	0.019
Share Other	0.028	0.029	0.031	0.031	0.029
B. FAFSA Household Characteristics					
Family Income	53,494	47,607	51,493	72,787	67,237
Family Assets	39,109	1,533	1,772	2,833	2,451
Family Size	3.184	3.442	3.444	2.839	2.904
FAFSA Year	2015.68	2015.31	2018.01	2018.00	2015.05
Share Married	0.085	0.086	0.083	0.064	0.067
C. Political Characteristics					
Democratic Party	0.294	0.327	0.348	0.355	0.326
Republican Party	0.069	0.077	0.081	0.102	0.093
Voted in 2020	0.413	0.460	0.494	0.555	0.502
Eligiblity Limits	No	Yes	Yes	Yes	Yes
Earliest Year	2010	2010	2017	2017	2010
Latest Year	2020	2020	2019	2019	2019
Bandwidth Limit	None	None	None	$10,000	$10,000

Note: Each column shows the characteristics of a different sample of our FAFSA filer data. Labels for each of these samples are displayed in the first row of the table. Race and ethnicity are only available for registered voters and are missing for all people who are not registered to vote, because race and ethnicity data are only recorded through the voter file. Registered voters may choose to register as a Democrat, a Republican, a member of another political party, or as no party preference.

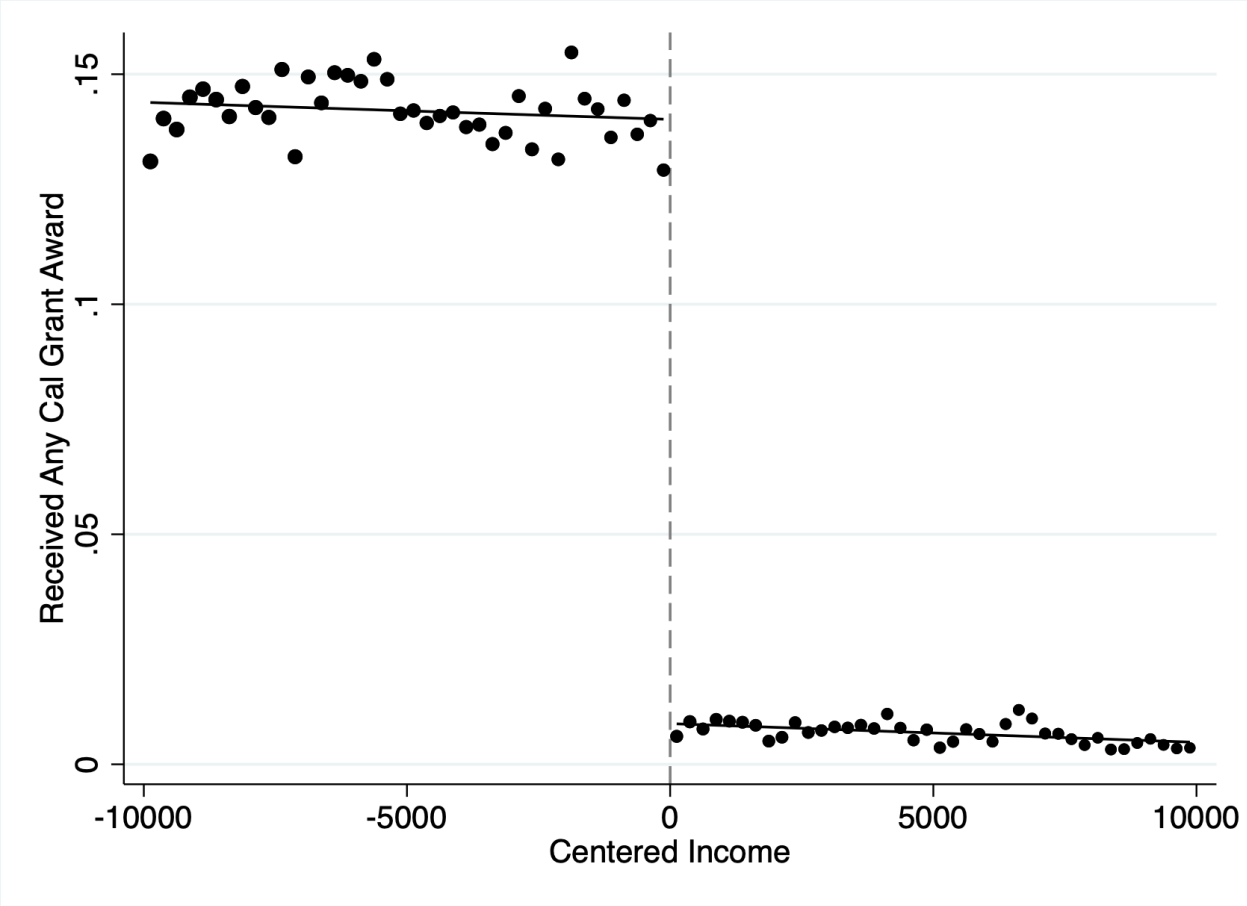

Figure A.1: Regression Discontinuity Plot of Cal Grant Receipt

Note: Centered income values are normalized to the income ceiling for Cal Grant A for a given individual. "Received Any Cal Grant Award" refers to an indicator for having received any Cal Grant award in the academic year following FAFSA filing. Observations includes all main sample 2017-2019 Cal Grant eligible students within a 10,000 dollar bandwidth of a Cal Grant income threshold.

A.2 Calculating Macro-level Outcomes

In Step 1 of Table A.2, we explicitly state the assumptions for each column. The lower bound assumes that the smallest point estimate for voter turnout in the 2020 general election is true, despite the fact that this point estimate relies on data that is susceptible to potential bias from manipulation of the running variable, students exiting the state, and legal name changes after marriage. The lower bound also relies on the lowest ratio of new center-left to new center-right voters across our specifications and assumes that 80 percent of center-left voters support Biden and 0 percent of registered Republicans support Biden.[29] Lastly, the lower bound estimate assumes that the number of Cal Grant recipients in 2011-2012 was the same as 2010-2011, despite the recipient totals monotonically increasing each year.

Our best estimate uses the results of our preferred specification for 2020 turnout and the partisan composition of new voters, making the same assumptions on vote by party as the lower bound column. It also assumes that the number of Cal Grant recipients in 2011-2012 was the average of the 2010-2011 and 2012-2013 cohorts. Finally, the upper bound uses the largest point estimate on the treatment effect for 2020 turnout, assumes the full net increase accrues to Democrats[30], and that the 2011-2012 Cal Grant awards were equal to the 2012-2013 total.

In Step 2, we multiply out the assumptions from Step 1 and compare them to the actual results of the 2020 presidential election in California across each column. In Step 3, we deduct the estimated impacts of 2010-2019 Cal Grants from the actual results and show the projected vote totals if the grants had not been awarded. In Step 4, we show the net impact of the 2010-2019 Cal Grants on the Biden-Trump margin of victory in the state of California and the aggregate voter turnout rate relative to the total citizen voting-eligible population (CVEP).

[29] Based on actual election returns and surveys of California college students that matched party registration records to policy preferences, this is likely to be an underestimate. At both private and public 4-year college campus precincts in California and other states, between 80 and 95 percent of voters cast a ballot for Democratic candidates. Likewise, a survey of Californians who applied to college between 2007 and 2011 that was fielded in 2022 found that approximately 75 percent of registered third party and non-partisan voters, 94 percent of registered Democrats, and 23 percent of registered Republicans reported that they favored the Democratic Party over the Republican Party (Firoozi, 2023). Evidence from political contribution data suggest even higher rates of support for Democratic candidates.

[30] This is mathematically feasible because some specifications find a net decrease in GOP turnout, with more than the whole of the net increase accruing to registered independents and Democrats.

Table A.2: Estimated Impact of 2010-2019 Cal Grants on the 2020 Election in California

	Lower Bound	Best Estimate	Upper Bound
Step 1: Assumptions			
1.a) Average Treatment Effect	+3.86 pp	+9.85 pp	+11.93 pp
1.b) Partisan Assignment	70% D 25% R 5% O	80% D 15% R 5% O	100% D 0% R 0% O
1.c) Total Grants Awarded	2,612,744	2,626,306	2,639,867
Step 2: Actual 2020 Outcomes and Calculated Effects			
2.a) Actual 2020 Biden Votes		11,110,250 (63.48 pp)	
Impact of Cal Grants	+70,596	+206,953	+314,936
2.b) Actual 2020 Trump Votes		6,006,429 (34.32 pp)	
Impact of Cal Grants	+25,213	+38,803	+0
2.c) Actual 2020 Other Votes		384,202 (2.20 pp)	
Impact of Cal Grants	+5,043	+12,935	+0
2.d) Actual 2020 Total Votes		17,500,881 (100.00 pp)	
Impact of Cal Grants	+100,852	+258,691	+314,936
Step 3: Estimated Outcomes without 2010-2019 Cal Grants			
3.a) Projected 2020 Biden Votes	11,039,654 (63.45 pp)	10,903,297 (63.24 pp)	10,795,314 (62.81 pp)
3.b) Projected 2020 Trump Votes	5,981,216 (34.37 pp)	5,967,626 (34.61 pp)	6,006,429 (34.95 pp)
3.c) Projected 2020 Other Votes	379,159 (2.18 pp)	371,267 (2.15 pp)	384,202 (2.24 pp)
3.d) Projected 2020 Total Votes	17,400,029 (100.00 pp)	17,242,190 (100.00 pp)	17,185,945 (100.00 pp)
Step 4: Estimated Impacts of 2010-2019 Cal Grants			
4.a) Impact on Biden-Trump Margin	+45,383 +0.08 pp	+168,150 +0.53 pp	+314,936 +1.30 pp
4.b) Impact on Voter Turnout Rate	+0.40 pp	+1.03 pp	+1.26 pp

Note: The citizen voting eligible population (CVEP) was 25,090,517 in California for the 2020 presidential general election.

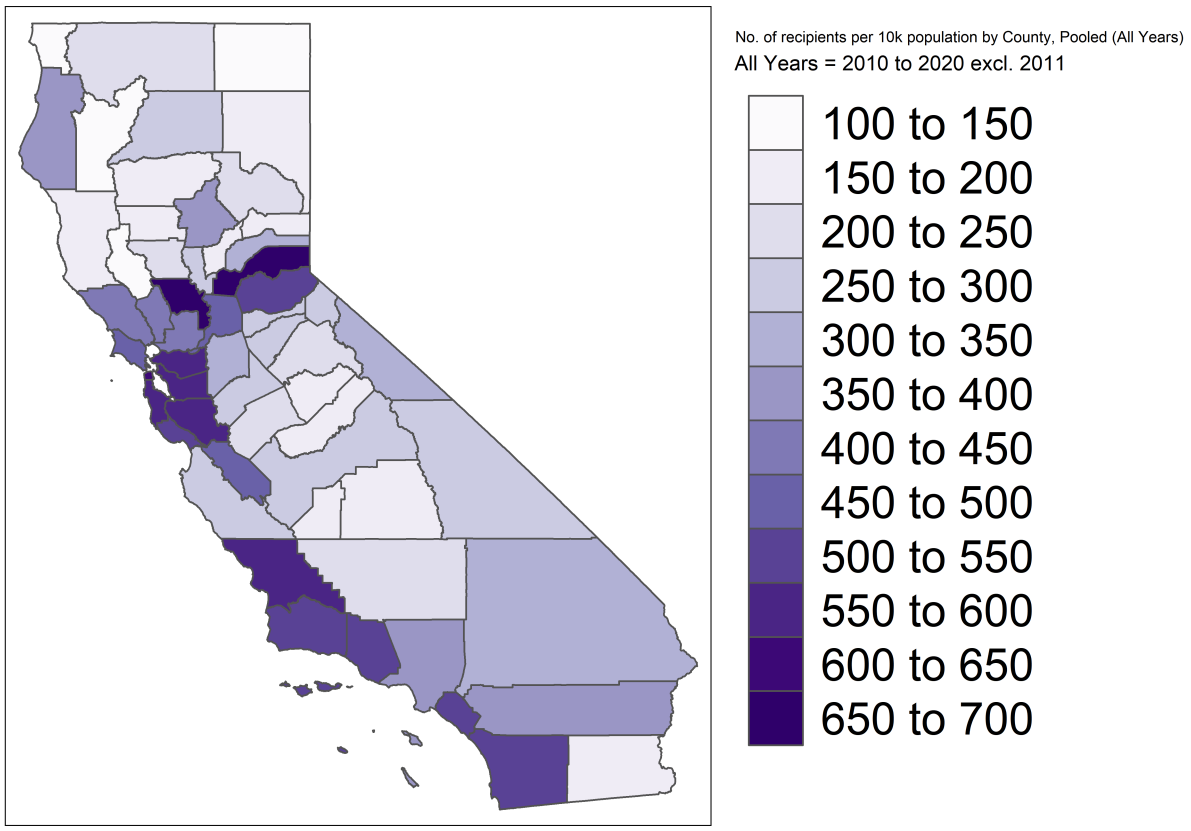

Figure A.2: Map of 2010-2020 Cal Grant Recipients in 2022

Note: The map above shows the number of Cal Grant recipients who were registered to vote per capita in each of California's counties. Locations are assessed based on the 2022 voter registration address of Cal Grant recipients. See Figure A.3 for a map of public university campuses.

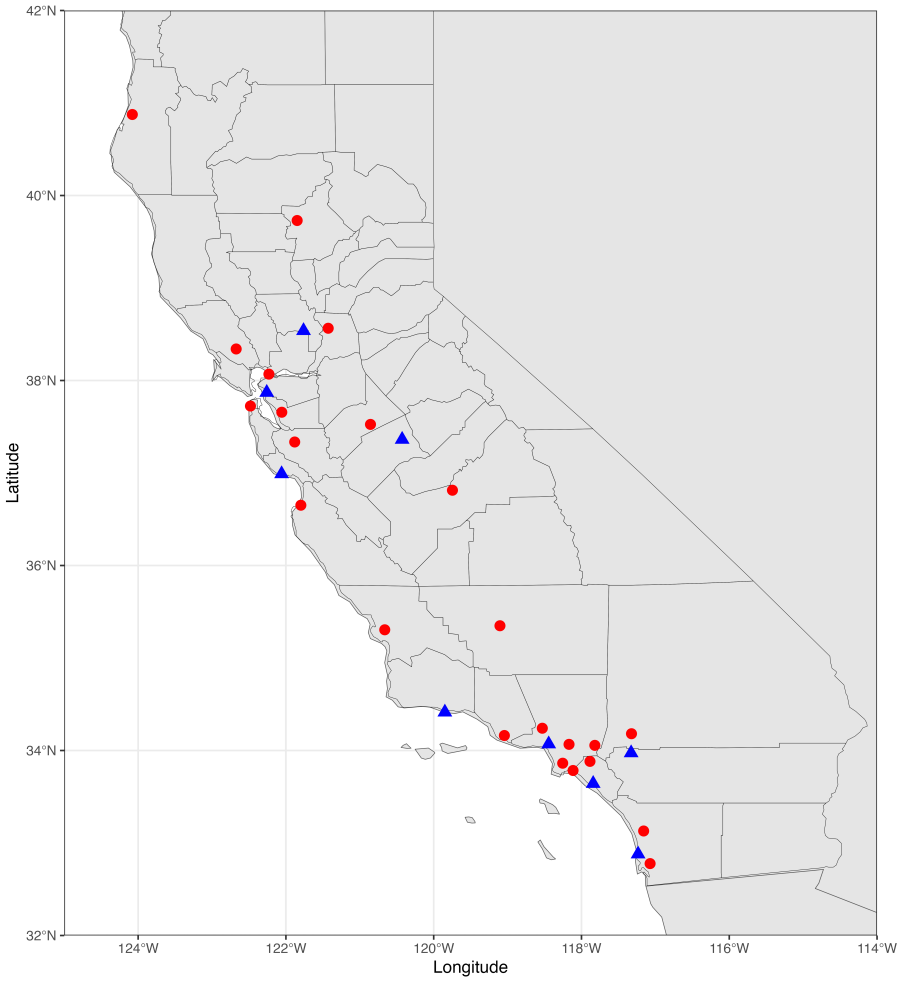

Figure A.3: Map of Public 4-Year Colleges in California

Note: California's public universities are plotted with symbols and county lines in the map above. Blue triangles represent the University of California's (UC) undergraduate campuses, which are highly selective research universities. Of the 9 UC campuses, 8 are categorized as R1 research universities. Red circles represent the California State University's (CSU) campuses, which are selective local comprehensive universities that primarily focus on teaching. See Figure A.2 for a heat map of the Cal Grant's political impact by county.

B RD Validation Appendix

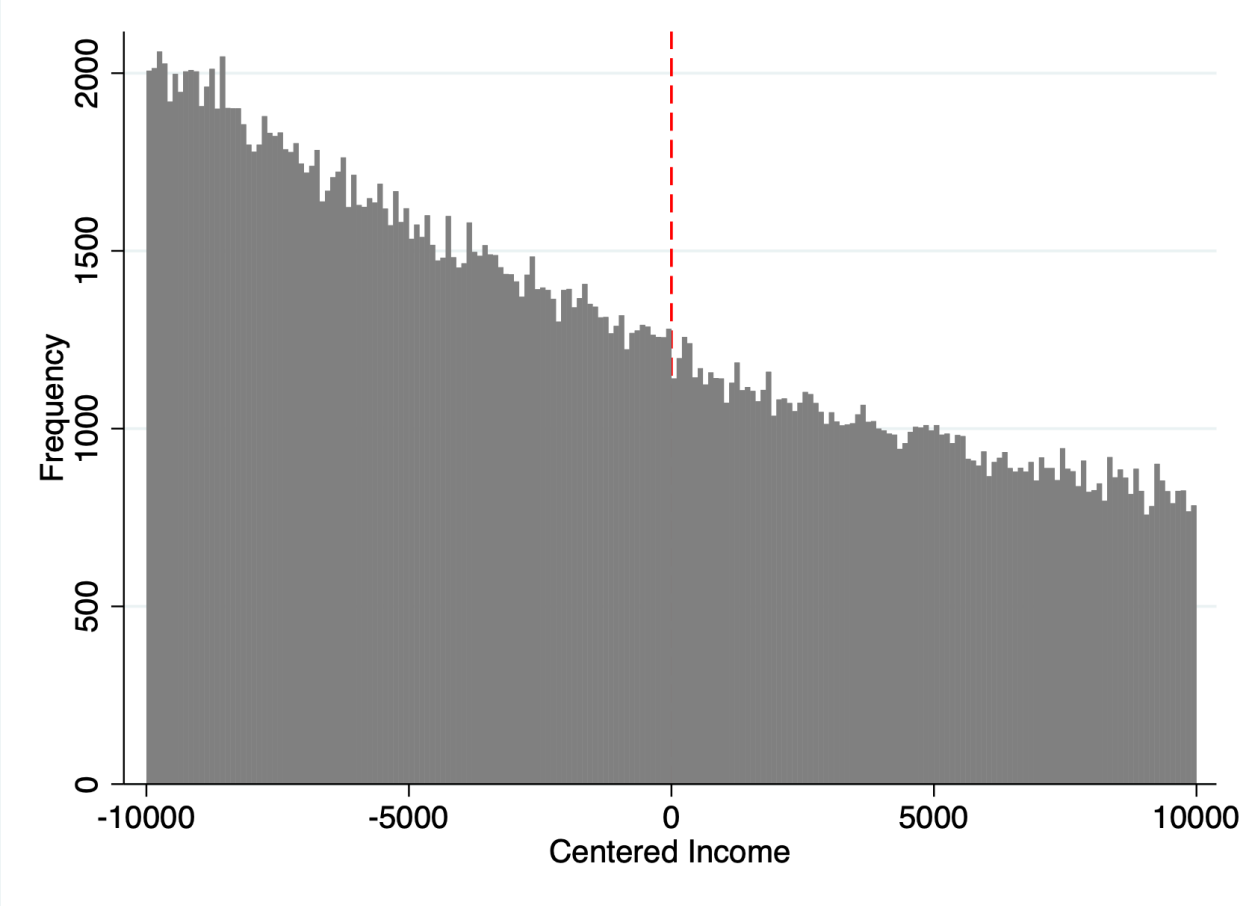

Figure B.1: McCrary Density Test

Note: Centered income values are normalized to the income ceiling for Cal Grant A for a given individual. Observations include all main sample 2017-2019 Cal Grant eligible students within a 10,000 dollar bandwidth of a Cal Grant income threshold.

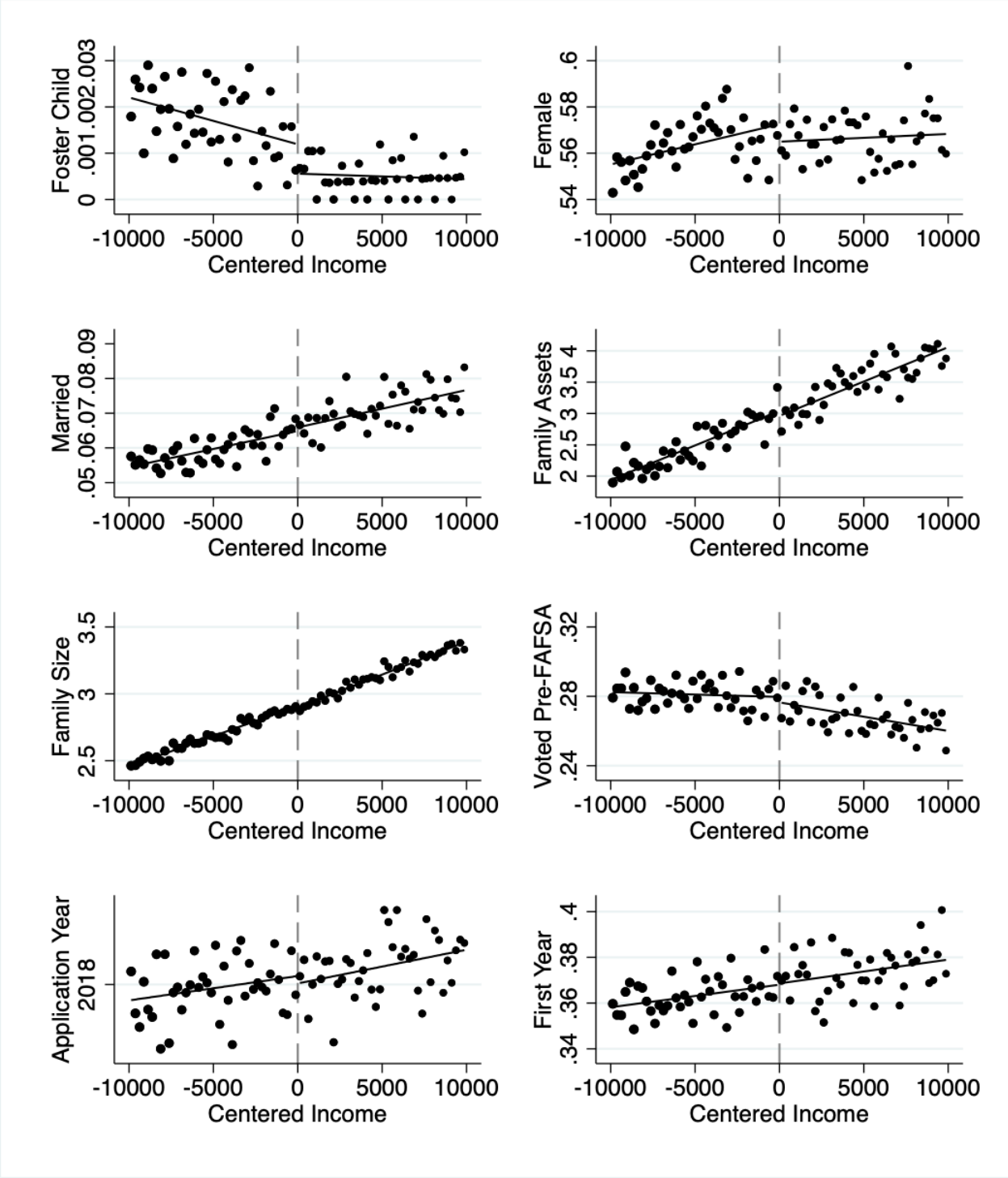

Figure B.2: Regression Discontinuity Plot for Covariates

Note: Centered income values are normalized to the income ceiling for Cal Grant A for a given individual. Observations include all main sample 2017-2019 Cal Grant eligible students within a 10,000 dollar bandwidth of a Cal Grant income threshold.

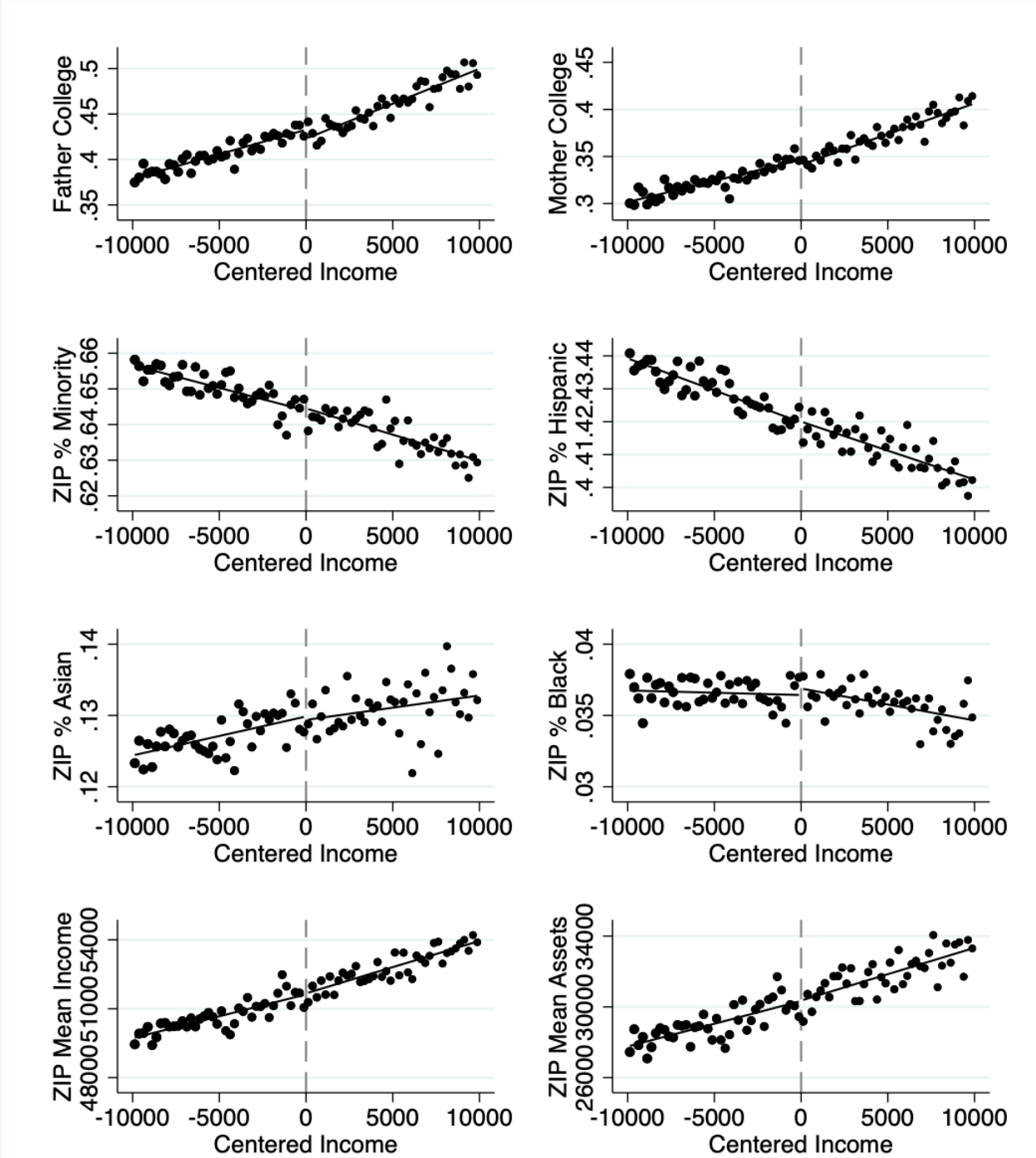

Figure B.3: Regression Discontinuity Plot for Covariates

Note: Centered income values are normalized to the income ceiling for Cal Grant A for a given individual. Observations include all main sample 2017-2019 Cal Grant eligible students within a 10,000 dollar bandwidth of a Cal Grant income threshold.

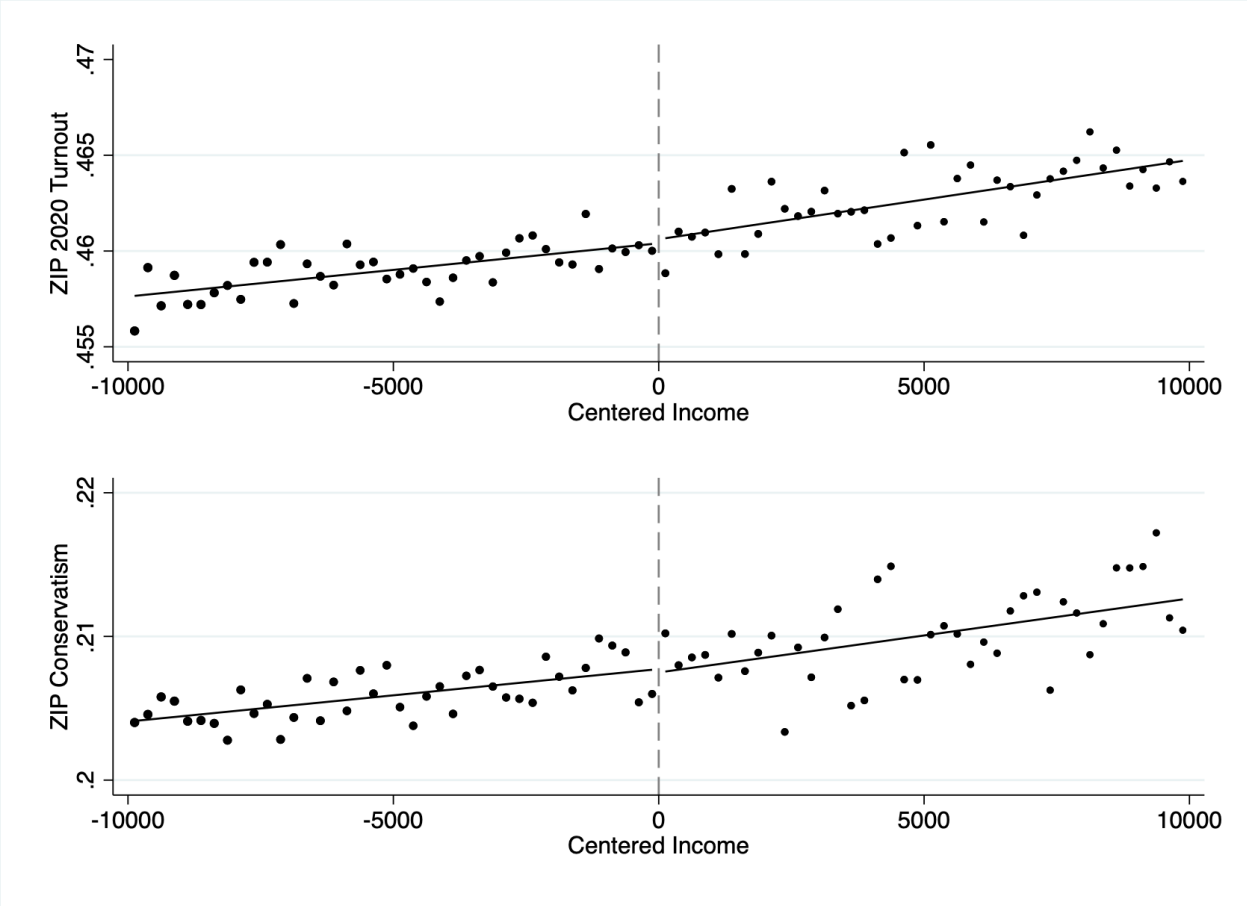

Figure B.4: Regression Discontinuity Plot for Covariates

Note: Centered income values are normalized to the income ceiling for Cal Grant A for a given individual. Observations include all main sample 2017-2019 Cal Grant eligible students within a 10,000 dollar bandwidth of a Cal Grant income threshold.

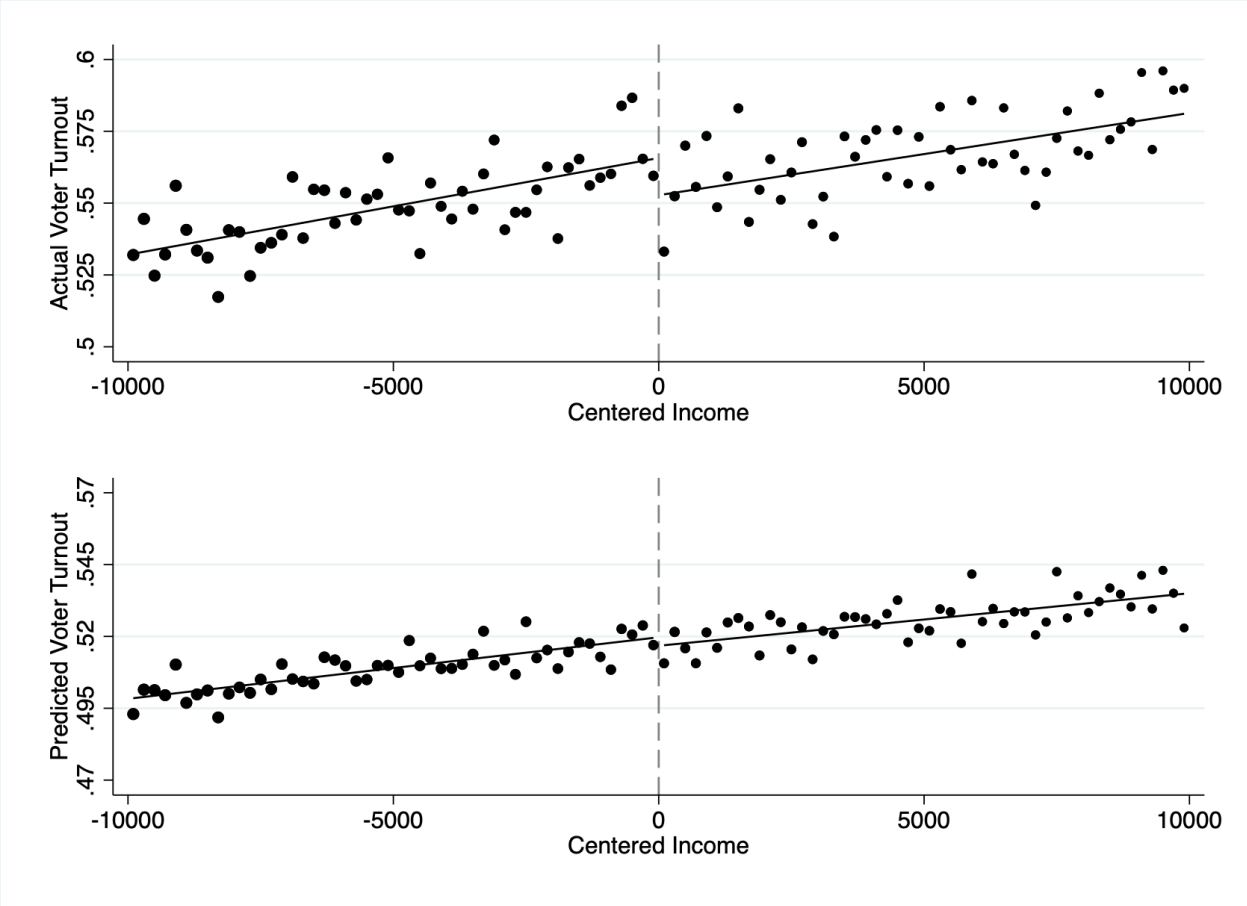

Figure B.5: Omnibus Covariate Balance Test

Note: Centered income values are normalized to the income ceiling for Cal Grant A for a given individual. Observations include all main sample 2017-2019 Cal Grant eligible students within a 10,000 dollar bandwidth of a Cal Grant income threshold.

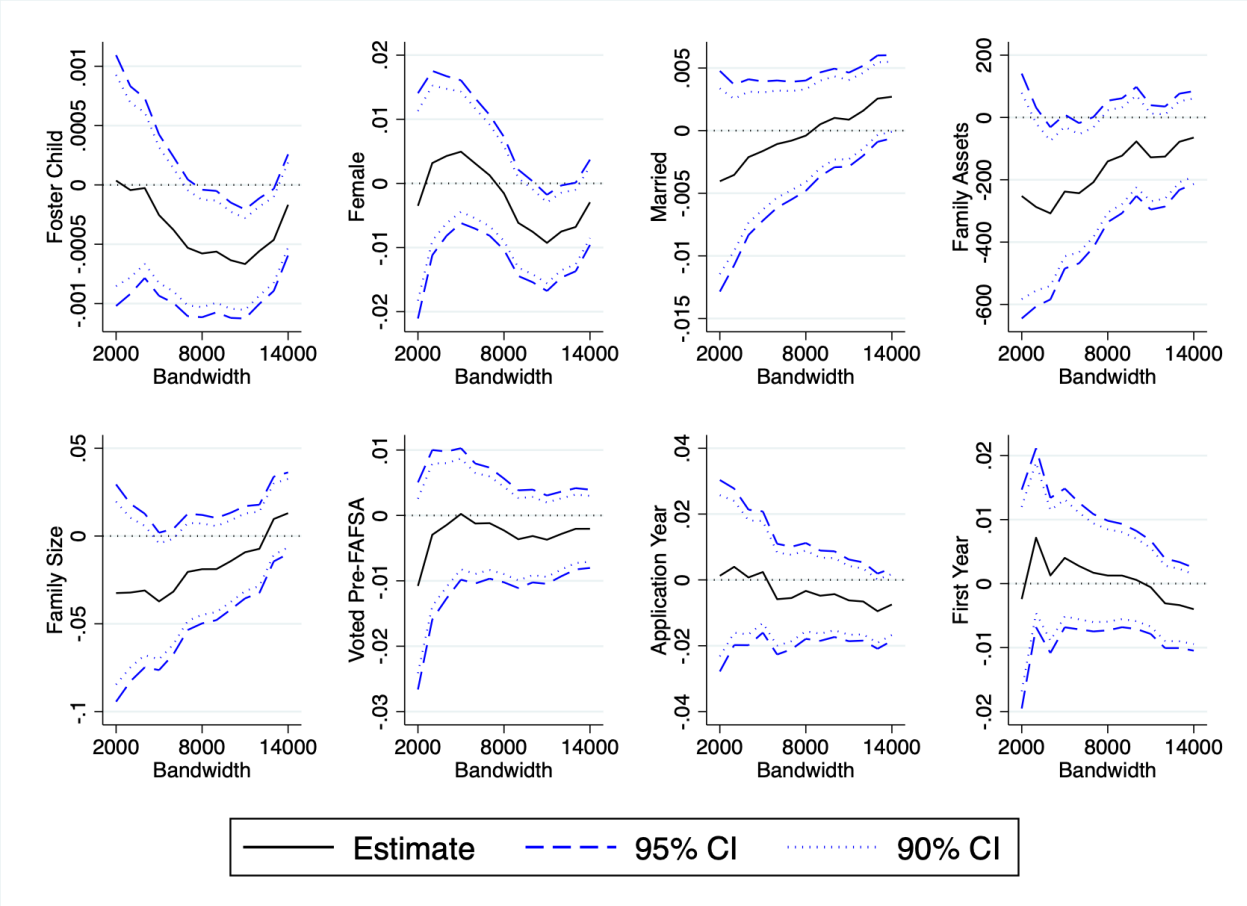

Figure B.6: Covariate Discontinuities by Bandwidth

Note: Each panel shows the reduced form disconitinuity in a coviarate across a range of potential bandwidths using a local linear specification and a uniform kernel without covariates. The graphs reflect the point estimate, 95 percent confidence interval, and 90 percent confidence interval. Observations include all main sample 2017-2019 Cal Grant eligible students within a given bandwidth of a Cal Grant income threshold.

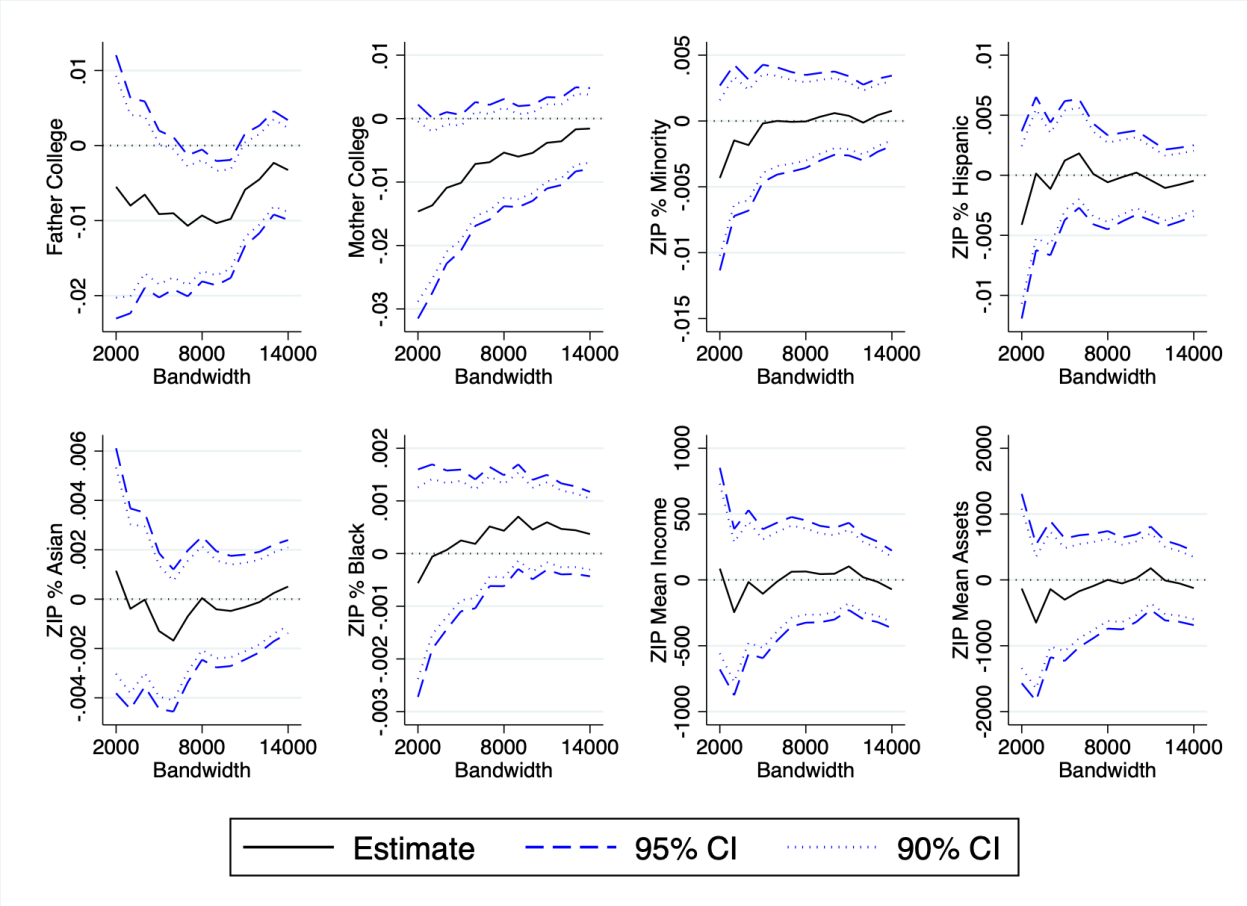

Figure B.7: Covariate Discontinuities by Bandwidth

Note: Each panel shows the reduced form disconitinuity in a coviarate across a range of potential bandwidths using a local linear specification and a uniform kernel without covariates. The graphs reflect the point estimate, 95 percent confidence interval, and 90 percent confidence interval. Observations include all main sample 2017-2019 Cal Grant eligible students within a given bandwidth of a Cal Grant income threshold.

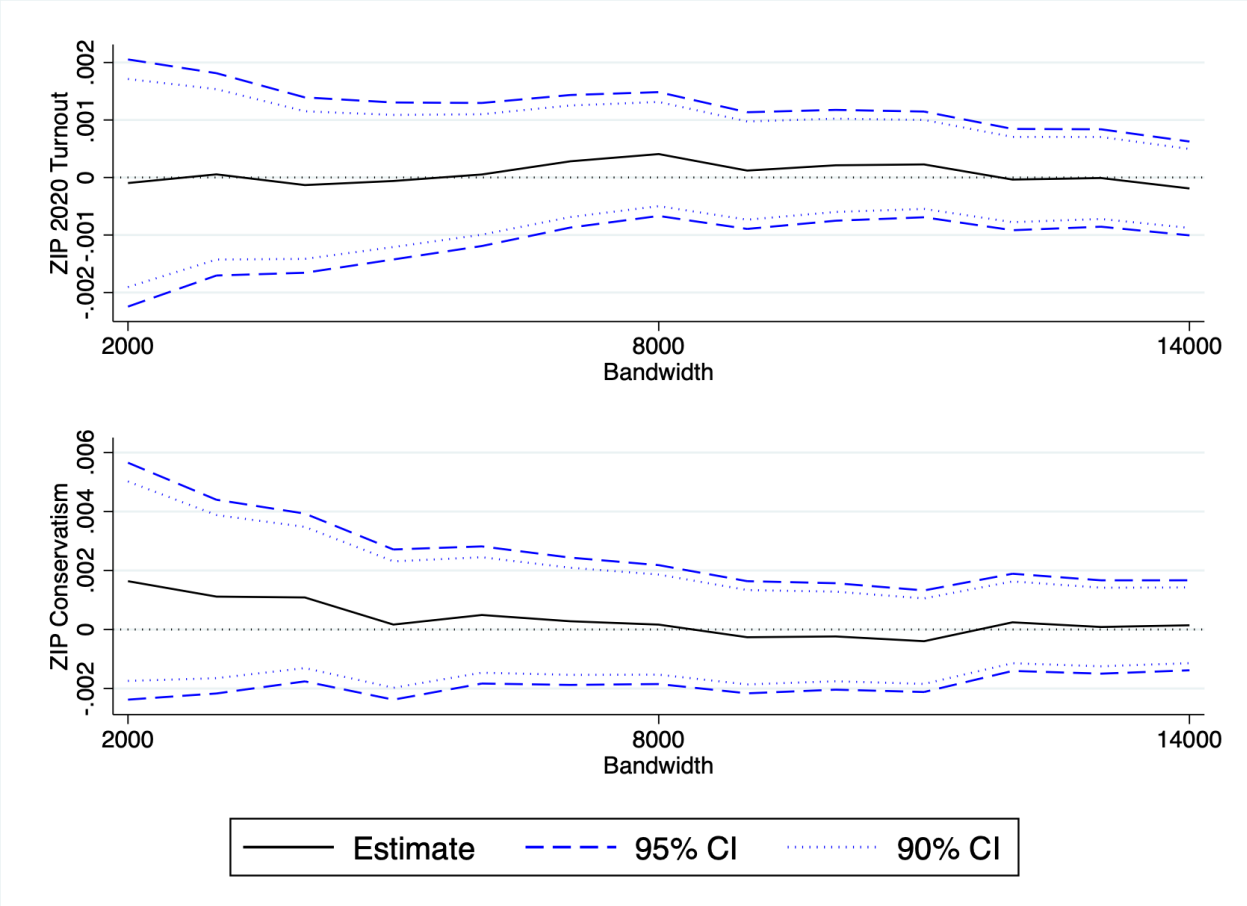

Figure B.8: Covariate Discontinuities by Bandwidth

Note: Each panel shows the reduced form disconitinuity in a coviarate across a range of potential bandwidths using a local linear specification and a uniform kernel without covariates. The graphs reflect the point estimate, 95 percent confidence interval, and 90 percent confidence interval. Observations include all main sample 2017-2019 Cal Grant eligible students within a given bandwidth of a Cal Grant income threshold.

B.1 Interstate Migration

We note that there is a match rate of 50 to 60 percent, depending on the particular sample used, between FAFSA files from CSAC and L2's California voter file. Because the merged dataset is de-identified, it is not possible to know the precise registration rate for unique individuals within the dataset. Non-matches between these two datasets are attributable to students moving to another state/country, lack of interest in voting or registering to vote while living in California, or disenfranchisement (for example, due to incarceration or death). Based on the descriptive statistics we observe for the tables generated in this appendix, we estimate that approximately three quarters of the non-matches between datasets are attributable to Californian residents who simply lack interest in registration or voting, with the remaining quarter attributable to migration out of California.

We use four approaches to address the risk that out-of-state migration may bias our estimates.

First, we note that National Student Clearinghouse data and IRS tax records have been used to track movers at the Cal Grant income ceiling, confirming the validity of our approach. Receiving a Cal Grant has insignificant effects on the share of students enrolling at in-state colleges in the short-run and null effects on IRS tax filings in California until 11 years after FAFSA filing. Bettinger et al. (2019) formally estimate an insignificant increase in *out-of-state* enrollment for each Cal Grant received. Because moving out of California makes a student's political participation records unobservable in our dataset, a literal interpretation of the authors' estimates would mean that our short run estimates are biased *toward zero* by roughly 2 percentage points (assuming a 50 percent turnout rate). The paper also uses IRS data to show null effects of the Cal Grant on California residence (<2 pp) followed by a significant increase in retention within the state of California of approximately 2 percentage points after 11 years have elapsed since FAFSA filing. Because the falling share of students who reside in California over time should partially offset the selective retention induced by the Cal Grant after 11 years, we expect relocation to be a minor source of upward bias of approximately 1 percentage point in estimates after 11 years have elapsed (again assuming a 50 percent turnout rate).

Second, we use the subsample of our dataset in which we can observe out-of-state migration to confirm that our results match those of earlier work on the Cal Grant. Specifically, we use the subset of students that overlap between our main dataset and the main dataset used by Firoozi (2023), which would be UC applicants filing a financial aid application in anticipation of the 2010 and 2011 academic years. We show that receiving a Cal Grant has no measured impact on out-of-state voter registration in Table B.1 for this sample and that, under most specifications, the point estimate is positive. Each specification reflects a result

that is both small in absolute magnitude and in economic terms. The observation that most specifications generate a positive point estimate suggests that out-of-state migration may very slightly bias our estimates toward zero, which is consistent with previous work on the Cal Grant's impact on migration.

Third, we use a unique feature of our L2 dataset to illustrate that attrition has not made the sample unbalanced. In the L2 dataset, people who move out of the state of California will have no California voter record in either pre-treatment or post-treatment elections. This is because L2, which observes all state voter files, prunes historical voting records of interstate migrants from the dataset of the origin state. Therefore, if ambitious students with a high propensity to vote who are barely too rich to receive the Cal Grant attrit to other states, we should observe a discontinuity in pre-treatment voter turnout mirroring the results for post-treatment voter turnout. As we show in Table B.2, there are null effects of receiving a Cal Grant on pre-treatment turnout.

Fourth, we choose to be conservative in our approach to identifying causal effects. We begin our results section with a focus on the 2017-2018 to 2019-2020 sample, who filed a FAFSA less than 5 years prior to our voter file snapshot, minimizing the possibility of selection bias due to out-of-state migration. We also externally validate our results with a discontinuity in the generosity of the federal Pell Grant, showing that our findings generalize to policies that subsidize tuition at colleges outside of California. For our results to be explained by out-of-state migration, the Cal Grant's effect on short-run out-of-state migration would need to have changed from a negative value in the early 2000s to a present day effect size of roughly 20 percentage points per grant awarded and the Pell Grant would need to induce selection into California at a similar per dollar rate.

Table B.1: Estimated Impacts of 2010-2011 Cal Grants on Out-of-State Registration

Outcome	(1)	(2)	(3)	(4)	(5)	(6)
Non-CA Registered	-0.0136	-0.0134	0.0016	0.0027	0.0009	0.0010
	(0.0154)	(0.0153)	(0.0212)	(0.0212)	(0.0202)	(0.0203)
Bandwidth	$10,000	$10,000	$10,000	$10,000	$5,000	$5,000
Kernel	Uniform	Uniform	Uniform	Uniform	Uniform	Uniform
Polynomial	1	1	2	2	1	1
Controls	No	Yes	No	Yes	No	Yes
Sample Size	6,589	6,589	6,589	6,589	3,318	3,318

Note: $^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$. Heteroskedasticity robust standard errors in parentheses. "Non-CA Registered" refers to the proportion of students who were observed as registered to vote in a state other than California. Data is from a sample of UC applicants between the 2010-2011 and 2011-2012 academic years who filed a FAFSA (Firoozi, 2023).

Table B.2: Placebo Test of 2017-2019 Cal Grant Receipt on Pre-Treatment Voter Turnout

Outcome	(1)	(2)	(3)	(4)	(5)	(6)
Voted Pre-FAFSA	0.0240	0.0258	0.0053	0.0078	-0.0017	-0.0004
	(0.0276)	(0.0269)	(0.0428)	(0.0420)	(0.0392)	(0.0384)
Bandwidth	$10,000	$10,000	$10,000	$10,000	$5,000	$5,000
Kernel	Uniform	Uniform	Uniform	Uniform	Uniform	Uniform
Polynomial	1	1	2	2	1	1
Controls	No	Yes	No	Yes	No	Yes
Sample Size	258,329	258,329	258,329	258,329	123,774	123,774

Note: $^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$. Heteroskedasticity robust standard errors in parentheses. "Voted Pre-FAFSA" refers to having voted in the general election immediately prior to filing a FAFSA and potentially having received a Cal Grant.

C Robustness Check Appendix

C.1 Effects of 2017-2019 Cal Grants

We test our main outcomes formally in Table C.1. Each row of the table reflects the IV estimate of the effect of receiving a Cal Grant on the aforementioned outcomes and each column represents a different specification. Column 1 begins with our preferred specification, which uses local linear estimation with a uniform kernel at a 10,000 dollar bandwidth without covariates. Column 2 adds a set of pre-FAFSA covariate controls. Columns 3 and 4 increase the order of the polynomial control for the running variable to a quadratic functional form, with and without covariates. Columns 5 and 6 use local linear estimation at a much narrower 5,000 dollar bandwidth, again varying the inclusion of pre-FAFSA covariates.

Beginning with voter registration in Panel A, we find that the Cal Grant generates noisy, if any, increases in aggregate voter registration. Our preferred specification in Column 1 yields a point estimate of 6.34 percentage points per grant awarded. However, the estimates in Columns 2 through 6 are smaller and not significant, which suggests that the estimated impact of Cal Grant receipt on voter registration are sensitive to model specification and could be null or positive and small.

In Panel B, we present evidence that the rate at which students actually cast a ballot rises sharply as a result of California's tuition-free college program. For each grant awarded by the state, a student's odds of casting a ballot in the 2020 general election rose by 9.85 percentage points, which is significant at a 99 percent confidence interval. For all general elections held after the academic year in which students filed their FAFSA, we estimate a similar increase of 8.55 percentage points per grant awarded. These findings are consistent across each successive column and remain significant at a 90 percent confidence interval, suggesting that the effect of Cal Grants is robust to different specifications and definitions of turnout.

Panel C concludes by interacting voter turnout with measures of student partisanship. Row 4 begins with an indicator for Democratic or independent registration status interacted with a student's general election turnout rate. We find that, for each grant awarded, the rate at which students turnout for elections as a Democrat or independent increases by 8.91 percentage points, which is significant at a 99 percent confidence interval. This result is much larger than the effect on Republican turnout in Row 5, which is estimated to be -0.36 percentage points per grant awarded. Taken together, these results suggest that the Cal Grant program substantially raises political participation, exclusively among left-leaning voters.

Table C.1: Estimated Impacts of 2017-2019 Cal Grant Receipt on Political Participation

Outcome	(1)	(2)	(3)	(4)	(5)	(6)
A. Voter Registration						
Registered in 2022	0.0634*	0.0419+	0.0358	0.0214	0.0329	0.0197
	(0.0285)	(0.0249)	(0.0443)	(0.0388)	(0.0405)	(0.0355)
B. Voter Turnout						
Voted in 2020	0.0985**	0.0743**	0.0942*	0.0783+	0.1193**	0.1047**
	(0.0305)	(0.0270)	(0.0474)	(0.0421)	(0.0434)	(0.0385)
Voter Turnout	0.0855**	0.0639**	0.0824+	0.0700+	0.1004*	0.0927**
	(0.0277)	(0.0235)	(0.0430)	(0.0367)	(0.0394)	(0.0336)
C. Voter Turnout by Partisanship						
Center-left Turnout	0.0891**	0.0698**	0.0765+	0.0661+	0.0966*	0.0914**
	(0.0277)	(0.0245)	(0.0431)	(0.0383)	(0.0394)	(0.0351)
Center-right Turnout	-0.0036	-0.0059	0.0059	0.0039	0.0037	0.0013
	(0.0150)	(0.0147)	(0.0233)	(0.0230)	(0.0213)	(0.0210)
Bandwidth	$10,000	$10,000	$10,000	$10,000	$5,000	$5,000
Kernel	Uniform	Uniform	Uniform	Uniform	Uniform	Uniform
Polynomial	1	1	2	2	1	1
Controls	No	Yes	No	Yes	No	Yes
Sample Size	258,329	258,329	258,329	258,329	123,774	123,774

Note: $^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$. Heteroskedasticity robust standard errors in parentheses. "Voter Turnout" refers to the share of all federal general elections between 2010 and 2020 in which a student voted after the academic year in which they filed a FAFSA, which includes up to two elections in 2018 and 2020 for these student cohorts. "Center-left Turnout" refers to the interaction between voter turnout and an indicator for whether the student was a registered Democrat or independent, following Firoozi (2023). "Center-right Turnout" refers to the interaction between voter turnout and an indicator for being registered with the Republican Party.

Table C.2: Impacts of 2017-2019 Cal Grants on Political Participation by Gender

Outcome	(1)	(2)	(3)	(4)	(5)	(6)
A. Non-Female						
Registered in 2022	0.0765^{+}	0.0616	0.0899	0.0367	0.0876	0.0507
	(0.0464)	(0.0415)	(0.0748)	(0.0670)	(0.0678)	(0.0608)
Voted in 2020	0.1494**	0.1359**	0.2113**	0.1562*	0.2378**	0.2004**
	(0.0502)	(0.0452)	(0.0811)	(0.0730)	(0.0736)	(0.0664)
Voter Turnout	0.1293**	0.1264**	0.1909**	0.1471*	0.2078**	0.1841**
	(0.0451)	(0.0390)	(0.0730)	(0.0631)	(0.0662)	(0.0574)
Sample Size	112,527	112,527	112,527	112,527	53,478	53,478
B. Female						
Registered in 2022	0.0557	0.0279	0.0008	0.0101	-0.0029	-0.0013
	(0.0361)	(0.0308)	(0.0546)	(0.0470)	(0.0503)	(0.0432)
Voted in 2020	0.0618	0.0334	0.0224	0.0278	0.0448	0.0420
	(0.0384)	(0.0333)	(0.0580)	(0.0509)	(0.0534)	(0.0467)
Voter Turnout	0.0531	0.0227	0.0164	0.0208	0.0332	0.0335
	(0.0351)	(0.0293)	(0.0530)	(0.0448)	(0.0488)	(0.0411)
Sample Size	145,802	145,802	145,802	145,802	70,296	70,296
Bandwidth	\$10,000	\$10,000	\$10,000	\$10,000	\$5,000	\$5,000
Kernel	Uniform	Uniform	Uniform	Uniform	Uniform	Uniform
Polynomial	1	1	2	2	1	1
Controls	No	Yes	No	Yes	No	Yes

Note: $^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$. Heteroskedasticity robust standard errors in parentheses. "Voter Turnout" refers to the share of all federal general elections between 2010 and 2020 in which a student voted after the academic year in which they filed a FAFSA.

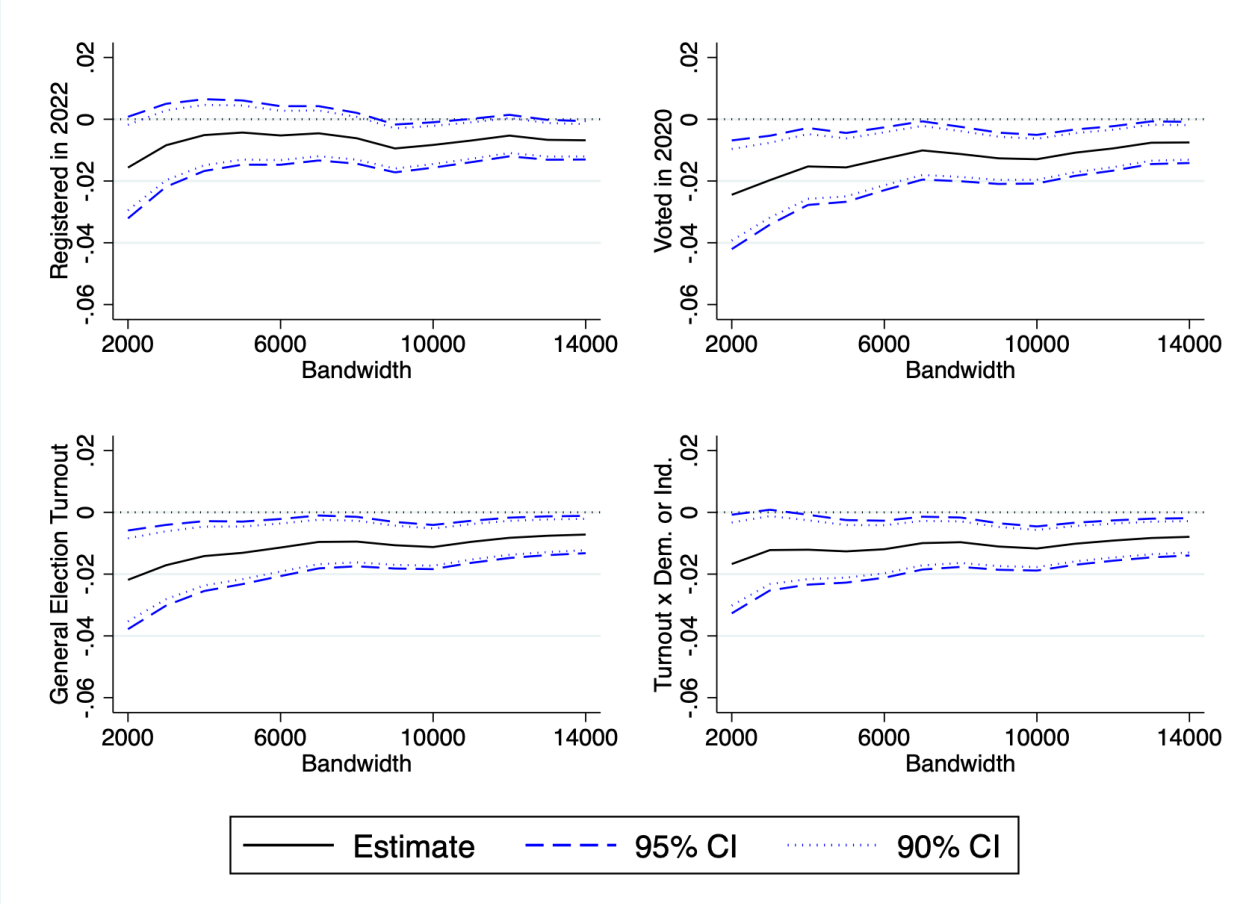

Figure C.1: Bandwidth Robustness Tests for Main Outcomes

Note: Each panel shows the reduced form discontinuity in an outcome variable across a range of potential bandwidths using a local linear specification and a uniform kernel without covariates. The graphs reflect the point estimate, 95 percent confidence interval, and 90 percent confidence interval of the reduced form discontinuity in the outcome of interest. Outcomes correspond directly to those in Table C.1.

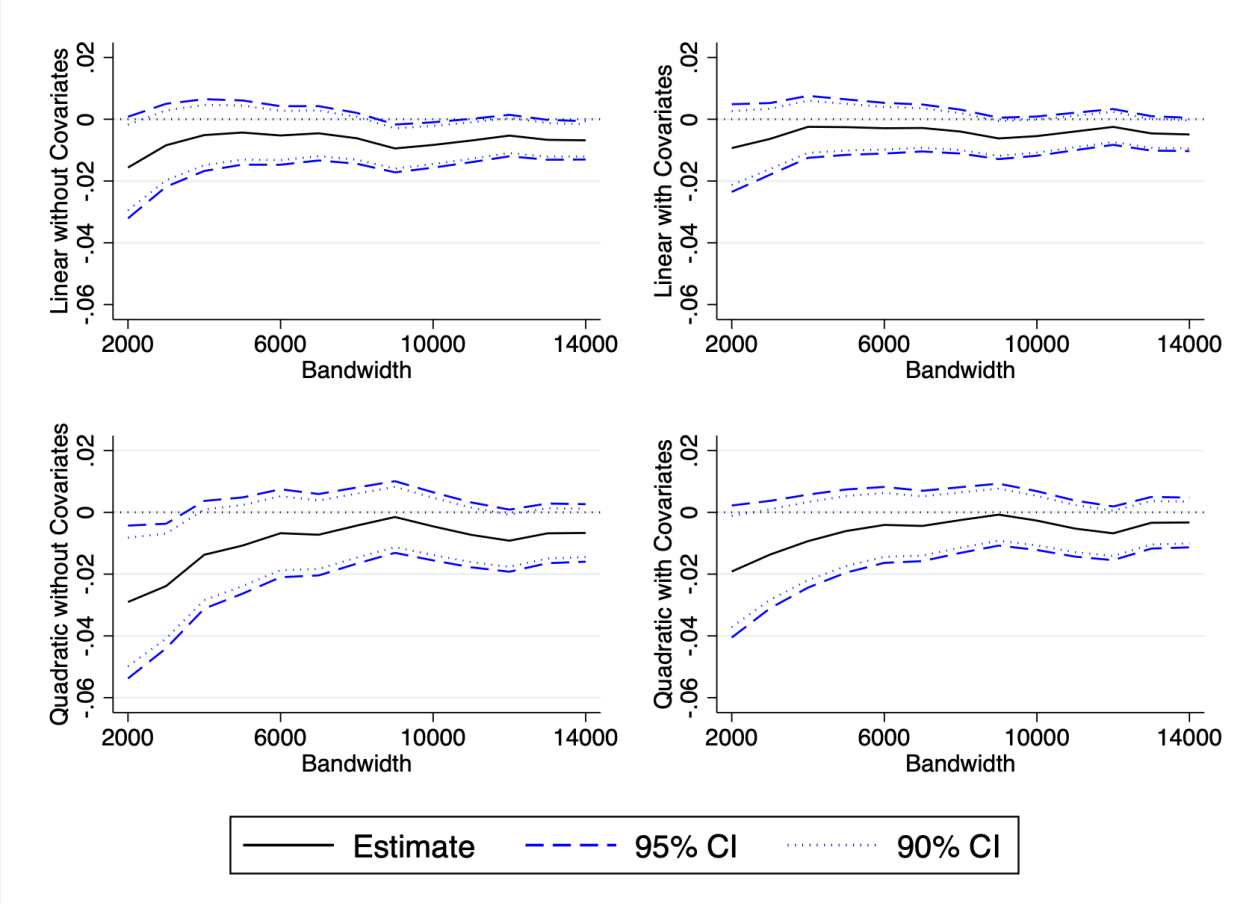

Figure C.2: Voter Registration in 2022 Bandwidth Robustness Tests

Note: The graphs reflect the point estimate, 95 percent confidence interval, and 90 percent confidence interval of the reduced form discontinuity in the outcome of interest. Each panel represents a different specification. Observations include all main sample 2017-2019 Cal Grant eligible students within a 14,000 dollar bandwidth of a Cal Grant income threshold.

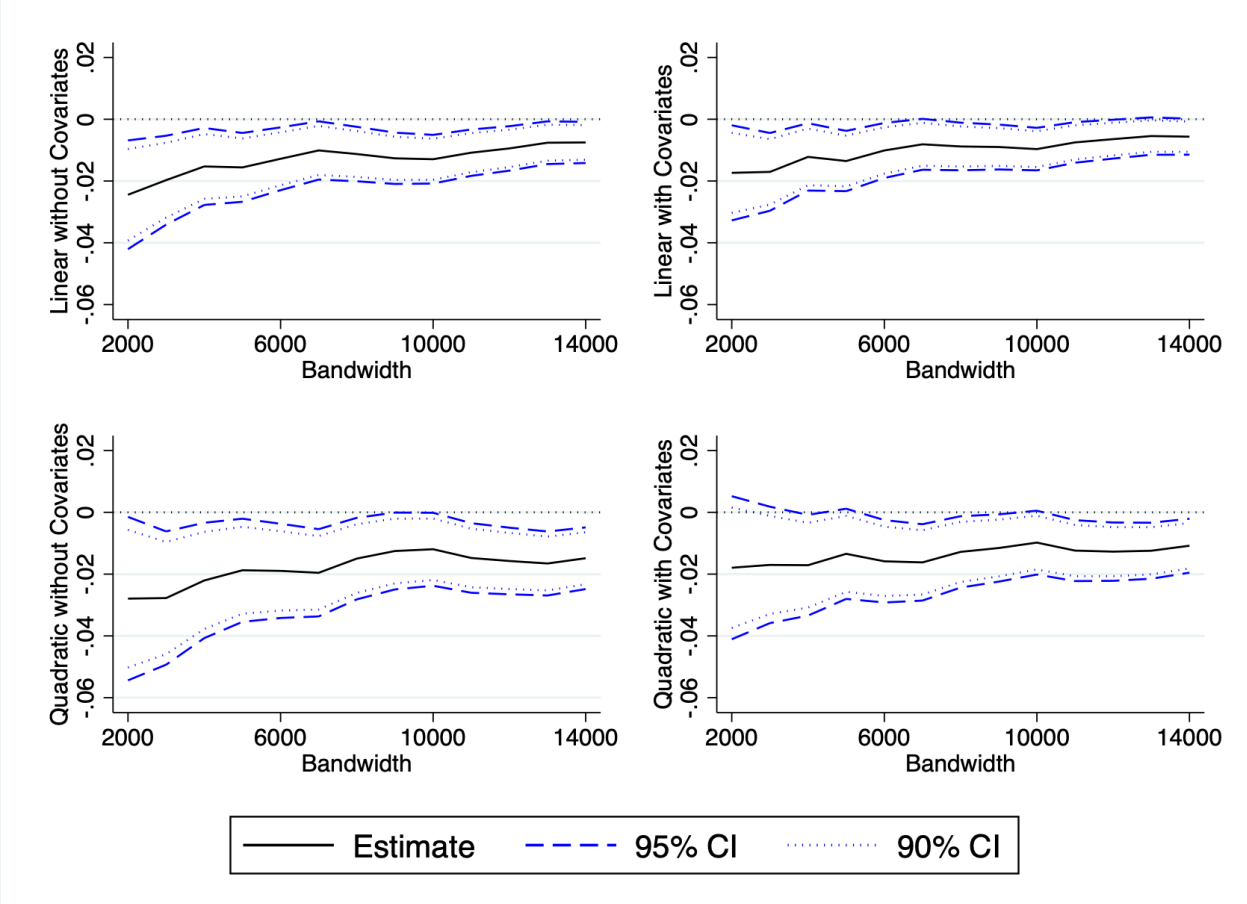

Figure C.3: 2020 General Election Turnout Bandwidth Robustness Tests

Note: The graphs reflect the point estimate, 95 percent confidence interval, and 90 percent confidence interval of the reduced form discontinuity in the outcome of interest. Each panel represents a different specification. Observations include all main sample 2017-2019 Cal Grant eligible students within a 14,000 dollar bandwidth of a Cal Grant income threshold.

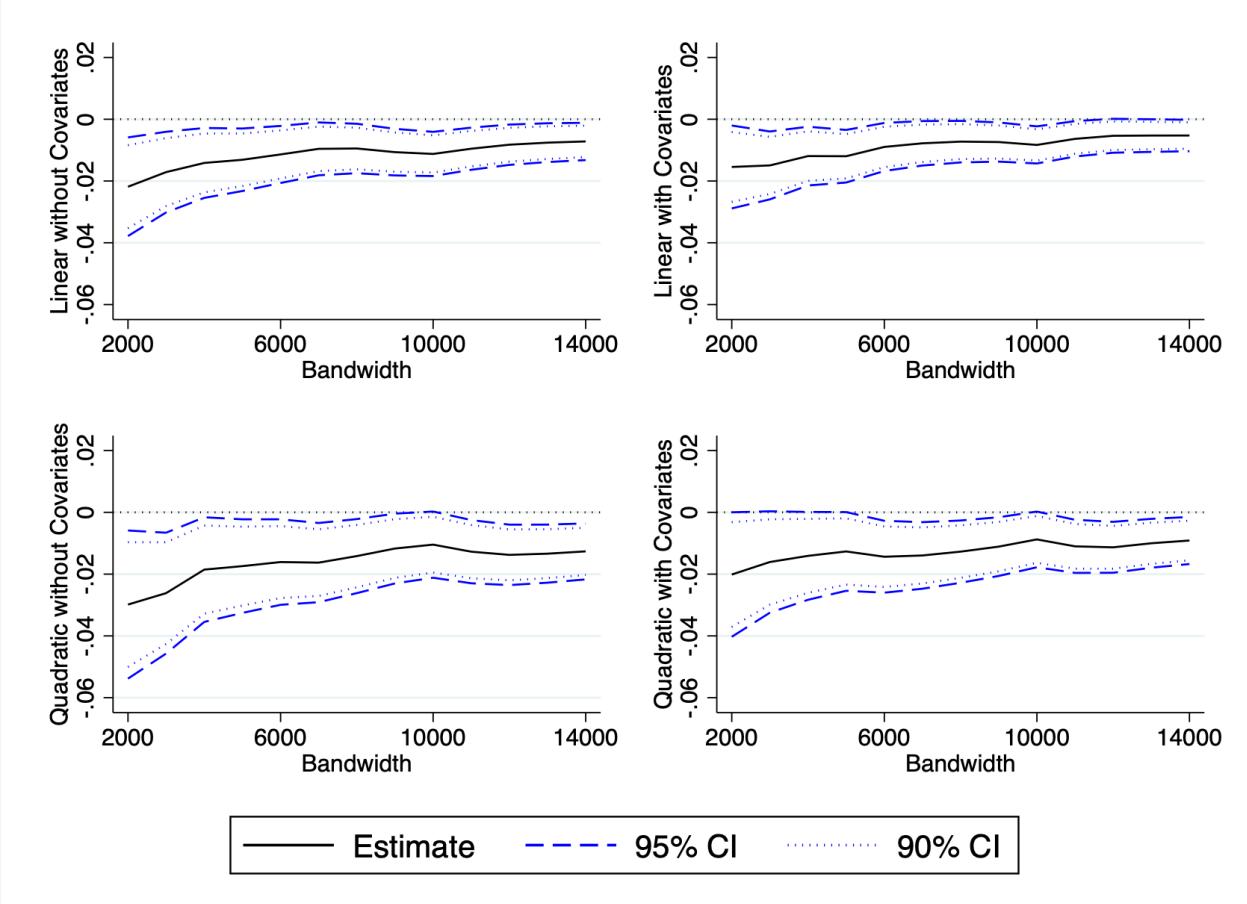

Figure C.4: General Election Turnout Bandwidth Robustness Tests

Note: The graphs reflect the point estimate, 95 percent confidence interval, and 90 percent confidence interval of the reduced form discontinuity in the outcome of interest. Each panel represents a different specification. Observations include all main sample 2017-2019 Cal Grant eligible students within a 14,000 dollar bandwidth of a Cal Grant income threshold.

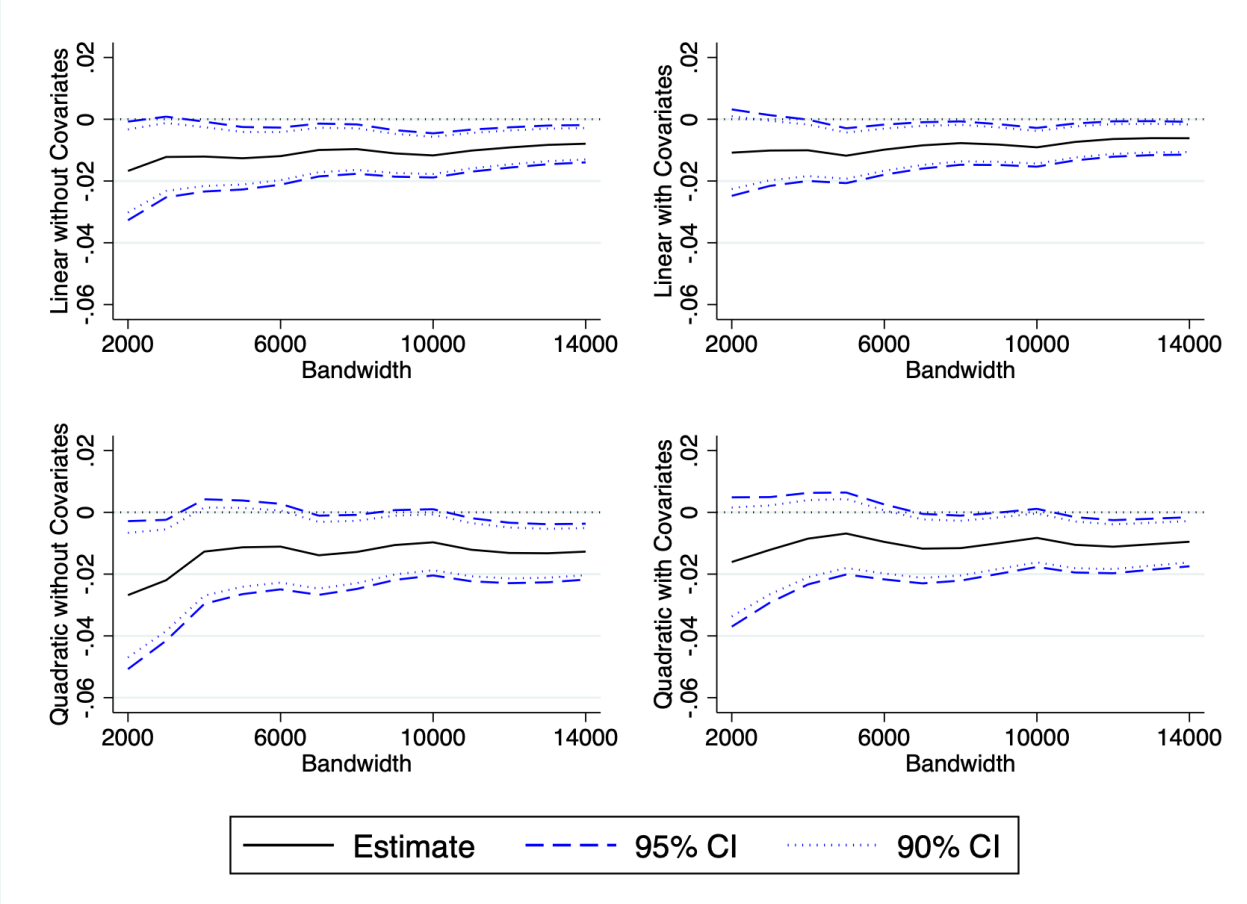

Figure C.5: Democratic or Independent Turnout Bandwidth Robustness Tests

Note: The graphs reflect the point estimate, 95 percent confidence interval, and 90 percent confidence interval of the reduced form discontinuity in the outcome of interest. Each panel represents a different specification. Observations include all main sample 2017-2019 Cal Grant eligible students within a 14,000 dollar bandwidth of a Cal Grant income threshold.

Table C.3: Main Results for 2017-2019 with CCT Bias-Aware Confidence Intervals

Outcome	(1)	(2)
Voter Registration		
RD_Estimate	0.0525^{+} (0.0316)	0.0531 (0.0446)
Robust 95% CI	[-.05 ; .131]	[-.006 ; .251]
Robust p-value	0.386	0.061
Voted in 2020		
RD_Estimate	0.0968** (0.0338)	0.1299** (0.0477)
Robust 95% CI	[.023 ; .216]	[.045 ; .319]
Robust p-value	0.016	0.009
Voter Turnout		
RD_Estimate	0.0843** (0.0307)	0.1144** (0.0433)
Robust 95% CI	[.019 ; .195]	[.042 ; .291]
Robust p-value	0.017	0.009
Center-Left Turnout		
RD_Estimate	0.0842** (0.0307)	0.0933* (0.0435)
Robust 95% CI	[.003 ; .18]	[0 ; .25]
Robust p-value	0.042	0.049
Center-Right Turnout		
RD_Estimate	0.0001 (0.0166)	0.0211 (0.0235)
Robust 95% CI	[-.032 ; .063]	[-.025 ; .108]
Robust p-value	0.522	0.223
Polynomial	1	1
Kernel	Triangular	Triangular
Bandwidth	$10,000	$5,000

Note: $^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$. Each row titled "RD Estimate" shows the conventional point estimate and standard errors in parentheses for a given outcome variable. These are calculated using a triangular kernel for a local linear specification without covariates. The rows "Robust 95% CI" and "Robust p-value" show the bias-corrected confidence interval and the bias-corrected p-value for the same outcome variable (Calonico et al., 2014). These outcomes correspond to those in Table C.1.

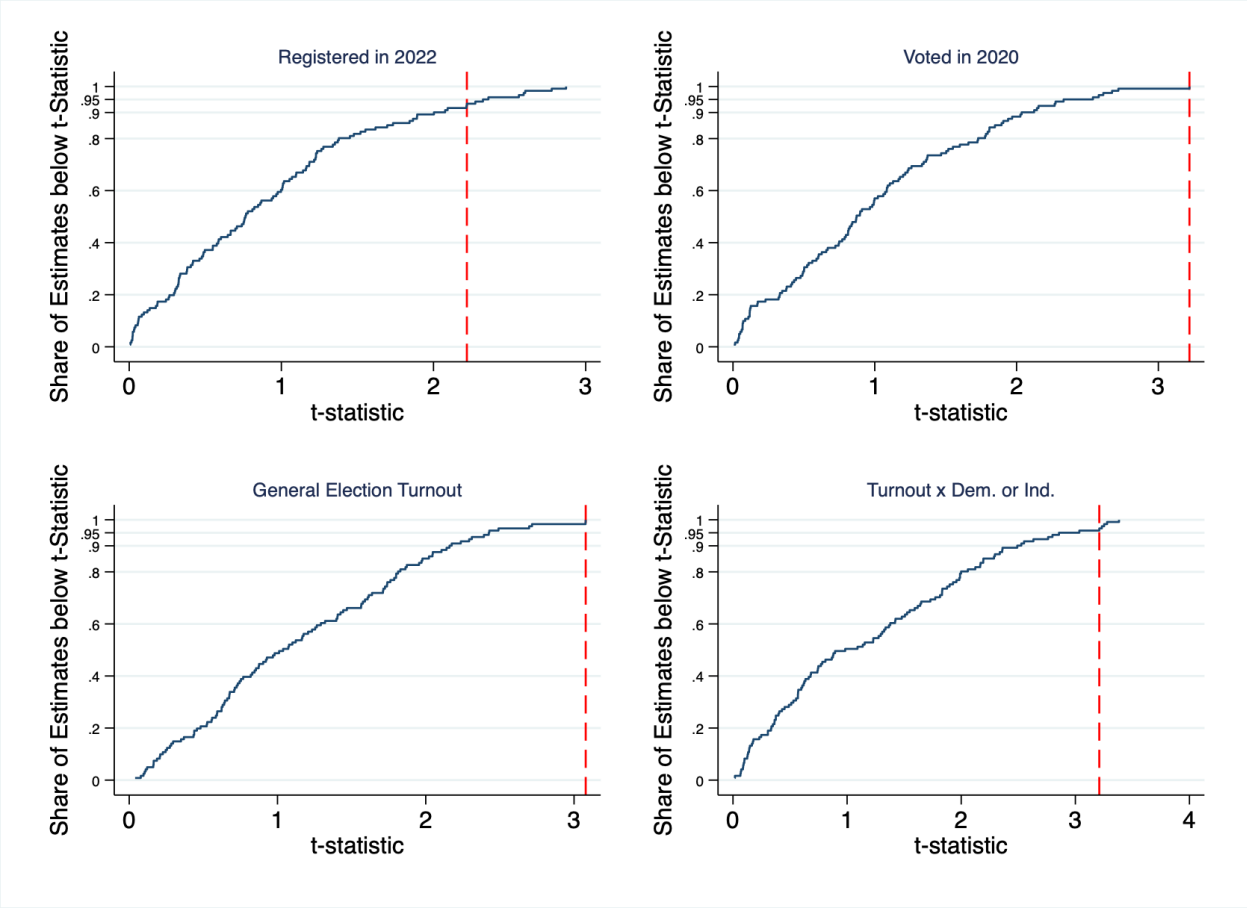

Figure C.6: Placebo Falsification Tests for Main Outcomes

Note: Each panel shows the cumulative distribution of t-statistics estimated at placebo thresholds for an outcome of interest with a red dashed line indicating the estimated t-statistic at the true policy threshold. We generate a "placebo threshold" at each 500 dollar increment along centered family income, and compare the estimated reduced form impact of these synthetic policys relative to the true policy. Placebo thresholds are bounded between -20,000 and +60,000 dollars relative to the true income ceiling, because this avoids false positives from capturing discontinuities taking place at family incomes of zero at the lower bound and this spans up to the 98th percentile of centered income on the upper bound. A 10,000 dollar bandwidth is used to remain consistent with our preferred specification. We exclude discontinuities within a 10,000 dollar bandwidth of the true cutoff to avoid generating false positives by including the actual policy discontinuity in our placebo estimates. Observations include all main sample 2017-2019 Cal Grant eligible students.

C.2 External Validity and the Pell Grant

We externally validate our findings from the Cal Grant using a notch in the generosity of the United States' largest tuition subsidy, the Pell Grant. The Pell Grant is a federal need-based tuition subsidy awarded to between 6 and 10 million students per year with identical eligibility and selection criteria in all American states.[31] The Pell Grant is less generous than the Cal Grant, covering at most half the cost of tuition and fees at University of California campuses as opposed to full tuition and fees. Students submit a FAFSA to apply for a Pell Grant and eligibility is determined by a mix of characteristics including family income, household size, family assets, and a college's cost of attendance. The program targets a much poorer subset of students than the Cal Grant, with few families with incomes over 60,000 dollars per year receiving a Pell Grant award as opposed to the upper bound of roughly 120,000 dollars per year for the richest Cal Grant students. The Pell Grant also subsidizes a much wider subset of post-secondary institutions with close to half of recipients attending 2-year colleges or vocational programs that are essentially excluded from tuition subsidies under the Cal Grant. We view the Pell Grant as an effective context to assess the generalizability of our findings because, relative to the Cal Grant, it is less generous, it subsidizes a wider and much less selective set of post-secondary institutions, and its eligibility criteria are both broader than state aid and consistent throughout the United States.

To identify the impact of the Pell Grant, we exploit a notch in the intensive margin of subsidy generosity rather than the extensive margin of subsidy receipt. When a students' family income falls below a certain level, they are automatically assigned an expected family contribution (EFC) level of zero dollars toward their cost of college attendance. This means that the generosity of the federal Pell Grant as measured through the size of the financial aid grant awarded to the student rises sharply below the zero EFC income threshold (Denning et al., 2019). Our estimate of the change in political participation at the Pell Grant income threshold will compare students who receive marginally more financial aid dollars to people who receive marginally fewer dollars from the same tuition subsidy.[32]

We test the external validity of our findings formally with data on 2.5 million FAFSA filers from our eligible sample (see Column 2 of Table A.1) who are local to the income notch used to determine Pell Grant generosity. Following the strategy in Denning et al. (2019), we center students' adjusted gross family income (AGI) relative to the level that automatically

[31]The Pell Grant does not have a strict 3.0 GPA cutoff like the Cal Grant. Roughly 1 out of 6 Pell Grant recipients are located in the state of California in any given year.

[32]It is worth bearing in mind that RD designs estimate the average treatment effects for compliers near some eligibility threshold. The notch at the zero EFC threshold we study takes place for family incomes in roughly the 20,000 to 30,000 dollar per year range whereas our Cal Grant thresholds are usually in the 40,000 to 120,000 dollar per year range for most students.

qualifies a student for zero EFC and increases Pell Grant generosity by 700 dollars.[33] Because we do not directly observe total financial aid awarded, we assume a discontinuity of 1,000 dollars to be conservative.[34]

We begin by plotting our main outcomes of interest against a student's centered AGI in Figure 2. Mirroring the results in Figure 1, the four panels of Figure 2 display the share of students who were registered to vote in 2022, the share of students who voted in the 2020 general election, the total voter turnout rate across all post-treatment general elections betwen 2010 and 2020, and the interaction between the voter turnout rate across post-treatment general elections and an indicator for being registered as a member of the Democratic Party or an independent. There is clear evidence of a discontinuity in each outcome and the results are identical to those of the Cal Grant. Receiving more generous tuition subsidies from America's largest financial aid program, the federal Pell Grant, increases voter registration and turnout, largely among left-leaning students. We take this figure to provide strong evidence that the civic externalities of the Cal Grant and Pell Grant generalize to tuition subsidies that cover different types of post-secondary institutions, target different populations of students, and have different levels and margins of generosity.

In Table C.4, we formally estimate the intent to treat (ITT) effects of this notch in Pell Grant generosity. We replicate our outcomes and specifications from Table D.1's estimates of the Cal Grant and find similar results. Our preferred specification suggests that raising Pell Grant generosity by 1,000 dollars at this notch increases voter turnout in the 2020 election by 0.52 percentage points, with around four fifths of the effect occurring among left-of-center voters. These estimates are robust to alternative definitions of political participation and a number of RD implementation choices like changing the bandwidth used for inference, including pre-treatment covariates, and changing the order of a polynomial control for the running variable. Each point estimate is just under one tenth of the corresponding effect size in Table D.1, meaning that our results across the Pell Grant and Cal Grant imply similar per-dollar estimates of the civic externalities of tuition subsidies. We find the similarity especially notable given the programs target different populations, subsidize different postsecondary institutions, and represent differences in the extensive versus intensive margins of treatment

[33]A small part of this increase in generosity is due to crowding-in of state-level financial aid. While we find that the zero EFC threshold crowds in Cal Grant awards at a 0.3 percentage point rate, we know from Section 4 of our paper that this crowd-in is unable to explain more than a 0.03 percentage point increase in voter turnout at the notch for Pell Grant generosity.

[34]The size of this discontinuity was generally decreasing over time, so this is likely to be an overestimate (Denning et al., 2019). Assuming a larger first-stage discontinuity deflates the estimated effect per dollar awarded. For perspective, a typical Cal Grant recipient receives around ten times as many dollars over our sample timeframe. Hence, a quick point of comparison to our results for the Cal Grant in Table D.1 entails multiplying estimates in this section by 10, which we discuss in greater detail in Section 5.2.

with tuition subsidies.

These estimates suggest that Pell Grants *issued during the 2010s alone*, which disbursed 349.8 billion dollars, increased voter turnout in the 2020 American elections by 1,819,000 votes and were responsible for the turnout of 1 out of every 87 American voters.[35] Using conservative assumptions about the partisan composition of treated students[36], the Pell Grant increased the Democratic Party's lead in the national popular vote by 1,182,000 votes (0.74 percentage points of total 2020 ballots) in the 2020 presidential election, with large enough effects to change the 2020 winner in Arizona, Georgia, Wisconsin, and the overall Electoral College.

[35]This is calculated by dividing 349.8 billion by 1,000 dollars and then multiplying by the effect size of 0.0052 votes per 1,000 dollars awarded.

[36]See the middle column of Table A.2 and Section 5.2 for a detailed explanation behind assumptions on partisanship. We note that we find similar results when using ANES 2020 survey data for the types of voters who are likely to receive financial aid and are similar to recipients of the Pell and Cal Grants. Specifically, we limit the sample to young voters aged 18 to 28 in 2020, who have family incomes below 100,000 dollars per year, and report tertiary education or postgraduate education. Approximately 77 percent of these voters nationwide report supporting Biden, compared to 17 percent who report supporting Trump and the remainder supporting minor party candidates. For voters in states outside of California, the comparable figures are 76 percent for Biden and 18 percent for Trump. In each case, the results are similar to our imputation of 80 percent of our sample favoring Biden and 15 percent favoring Trump, which we estimated using survey data from Firoozi (2023) and the partisan registration of in-sample students.

Table C.4: Estimated Impacts of 2010-2019 Pell Grant Generosity on Political Participation

Outcome	(1)	(2)	(3)	(4)	(5)	(6)
A. Voter Registration						
Registered in 2022	0.0034**	0.0025*	0.0038*	0.0022	0.0034*	0.0017
	(0.0012)	(0.0011)	(0.0018)	(0.0017)	(0.0017)	(0.0016)
B. Voter Turnout						
Voted in 2020	0.0052**	0.0049**	0.0051**	0.0041*	0.0051**	0.0040*
	(0.0012)	(0.0011)	(0.0019)	(0.0017)	(0.0017)	(0.0016)
Ever Voted	0.0042**	0.0042**	0.0052**	0.0041*	0.0048**	0.0036*
	(0.0013)	(0.0011)	(0.0019)	(0.0017)	(0.0018)	(0.0016)
Voter Turnout	0.0039**	0.0031**	0.0036*	0.0024+	0.0035*	0.0023+
	(0.0010)	(0.0008)	(0.0015)	(0.0013)	(0.0014)	(0.0012)
C. Voter Turnout by Partisanship						
Center-left Turnout	0.0033**	0.0025**	0.0028*	0.0016	0.0026+	0.0013
	(0.0010)	(0.0008)	(0.0014)	(0.0013)	(0.0013)	(0.0012)
Center-right Turnout	0.0006	0.0006	0.0008	0.0008	0.0009	0.0009+
	(0.0004)	(0.0004)	(0.0006)	(0.0006)	(0.0006)	(0.0006)
Bandwidth	$10,000	$10,000	$10,000	$10,000	$5,000	$5,000
Kernel	Uniform	Uniform	Uniform	Uniform	Uniform	Uniform
Polynomial	1	1	2	2	1	1
Controls	No	Yes	No	Yes	No	Yes
Sample Size	2,561,537	2,561,537	2,561,537	2,561,537	1,279,637	1,279,637

Note: $^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$. Heteroskedasticity robust standard errors in parentheses. Sample excludes 2011-2012 FAFSA filers, because the data for that cohort was not available. "Ever Voted" refers to the extensive margin of ever having participated in a general election in the academic year after a student filed a FAFSA. "Voter Turnout" refers to the share of all federal general elections between 2010 and 2020 in which a student voted after the academic year in which they filed a FAFSA. "Center-left Turnout" refers to the interaction between voter turnout and an indicator for whether the student was a registered Democrat or independent, following Firoozi (2023). "Center-right Turnout" refers to the interaction between voter turnout and an indicator for being registered with the Republican Party. Outcomes correspond to those in Table D.1.

D Heterogeneity and Time Variation Appendix

D.1 Time Variation

In Table D.2, we examine the longitudinal impact of the Cal Grant on voter turnout, standardizing election outcomes relative to the year in which a student would have received a Cal Grant. In Row 1, we show the impact on voter turnout in the first general election that takes place after a student would have started receiving a Cal Grant. Row 2 shows the same outcome for the next general election, taking place after another 2 years. Row 3 shows results for the extensive margin of casting a ballot in any federal general election taking place 4 or more years after the first general election after Cal Grant receipt.

Our preferred specification in Column 1 shows that receiving a Cal Grant increases voter turnout by 4.96 percentage points in the first general election after Cal Grant receipt. This result is between 2 and 6 percentage points and significant at a 95 percent confidence interval across most RD implementation choices. While there are positive point estimates of meaningfully large magnitudes for elections taking place later on, we lack the precision to detect them and can reject neither the null hypothesis that there is no impact, nor the null hypothesis that the effects are the same as in the first election after Cal Grant receipt.

To make further use of time variation, we display results for the 2020-2021 cohort, whose instruction was fully online due to the COVID-19 pandemic in the year of their Cal Grant receipt, and compare them to earlier cohorts in Table D.3. Panel A shows results for the earlier 2017-2018 to 2019-2020 cohorts, while Panel B shows results for the 2020-2021 COVID-19 cohort. We find that there are not obvious differences in the impact on voter registration in 2022, but there are large gaps in the impact on voter turnout in the 2020 general election. Students whose schooling took place in person in the year in which they received their Cal Grant have significant positive effects on 2020 election turnout between 7 and 12 percentage points, whereas the COVID-19 cohort has null effects between -4 and 3 percentage points. Although we lack the statistical power under most specifications to reject the null hypothesis that the results are the same, we nonetheless view this as suggestive evidence consistent with on-campus residence and other campus-based socialization activities as a key mechanism in increasing student political participation.

Finally, in Table D.4 we estimate the impact of the Cal Grant and Pell Grant across the four different elections in which we have sufficiently large sample sizes to match that of our main specification from Table 1, using all students within the four most recent entering cohorts. We find larger point estimates on the effect of subisidies in the 2020 election than previous elections, growing point estimates over time for the California-based Cal Grant, and a pattern of larger effects in presidential cycles for the federal Pell Grant. However, we

caution against reading too much into heterogeneous point estimates over different election cycles because our confidence intervals remain too wide to reject the null hypothesis that these effects are consistent over time.

Table D.1: Estimated Impacts of 2010-2019 Cal Grant Receipt on Political Participation

Outcome	(1)	(2)	(3)	(4)	(5)	(6)
A. Voter Registration						
Registered in 2022	0.0522**	0.0268+	0.0618*	0.0208	0.0497+	0.0163
	(0.0175)	(0.0155)	(0.0273)	(0.0243)	(0.0253)	(0.0225)
B. Voter Turnout						
Voted in 2020	0.0615**	0.0386*	0.0849**	0.0456+	0.0827**	0.0514*
	(0.0180)	(0.0164)	(0.0281)	(0.0257)	(0.0262)	(0.0238)
Ever Voted	0.0611**	0.0359*	0.0838**	0.0420+	0.0764**	0.0427+
	(0.0180)	(0.0161)	(0.0280)	(0.0253)	(0.0261)	(0.0234)
Voter Turnout	0.0443**	0.0270*	0.0597**	0.0301	0.0566**	0.0345+
	(0.0146)	(0.0125)	(0.0228)	(0.0197)	(0.0211)	(0.0182)
C. Voter Turnout by Partisanship						
Center-left Turnout	0.0388**	0.0244+	0.0542*	0.0272	0.0540**	0.0350+
	(0.0143)	(0.0128)	(0.0224)	(0.0202)	(0.0208)	(0.0186)
Center-right Turnout	0.0055	0.0025	0.0055	0.0029	0.0026	-0.0005
	(0.0073)	(0.0073)	(0.0115)	(0.0114)	(0.0107)	(0.0106)
Bandwidth	$10,000	$10,000	$10,000	$10,000	$5,000	$5,000
Kernel	Uniform	Uniform	Uniform	Uniform	Uniform	Uniform
Polynomial	1	1	2	2	1	1
Controls	No	Yes	No	Yes	No	Yes
Sample Size	738,046	738,046	738,046	738,046	354,091	354,091

Note: $^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$. Heteroskedasticity robust standard errors in parentheses. Sample excludes 2011-2012 FAFSA filers, because the data for that cohort was not available. "Ever Voted" refers to the extensive margin of ever having participated in a general election in the academic year after a student filed a FAFSA. "Voter Turnout" refers to the share of all federal general elections between 2010 and 2020 in which a student voted after the academic year in which they filed a FAFSA. "Center-left Turnout" refers to the interaction between voter turnout and an indicator for whether the student was a registered Democrat or independent, following Firoozi (2023). "Center-right Turnout" refers to the interaction between voter turnout and an indicator for being registered with the Republican Party.

Table D.2: Estimated Impacts of 2010-2019 Cal Grant Receipt on Voter Turnout

Outcome	(1)	(2)	(3)	(4)	(5)	(6)
Voted 1st Chance	0.0496**	0.0303*	0.0513*	0.0248	0.0485*	0.0270
	(0.0163)	(0.0143)	(0.0256)	(0.0226)	(0.0237)	(0.0209)
	[738,046]	[738,046]	[738,046]	[738,046]	[354,091]	[354,091]
Voted 2nd Chance	0.0202	0.0120	0.0414	0.0151	0.0388	0.0198
	(0.0185)	(0.0162)	(0.0289)	(0.0255)	(0.0269)	(0.0236)
	[651,212]	[651,212]	[651,212]	[651,212]	[312,477]	[312,477]
Voted 3rd+ Chance	0.0337	0.0092	0.0652+	0.0121	0.0484	0.0065
	(0.0223)	(0.0205)	(0.0349)	(0.0323)	(0.0327)	(0.0300)
	[479,717]	[479,717]	[479,717]	[479,717]	[230,317]	[230,317]
Bandwidth	$10,000	$10,000	$10,000	$10,000	$5,000	$5,000
Kernel	Uniform	Uniform	Uniform	Uniform	Uniform	Uniform
Polynomial	1	1	2	2	1	1
Controls	No	Yes	No	Yes	No	Yes

Note: $^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$. Heteroskedasticity robust standard errors in parentheses. Sample size in brackets. Estimates exclude 2011-2012 FAFSA filers, because the data for that cohort was not available. "Voted 1st Chance" refers to an indicator for participating in the first general election taking place after a student filed their FAFSA, between one and two years later. "Voted 2nd Chance" refers to an indicator for participating in the second general election taking place after a student filed their FAFSA, between three and four years later. "Voted 3rd+ Chance" refers to an indicator for ever participating in any general election taking place subsequent to the second general election after a student filed their FAFSA.

Table D.3: Estimated Impacts of Recent Cal Grant Receipt on Political Participation

Outcome	(1)	(2)	(3)	(4)	(5)	(6)
A. Sample: Academic Years 2017-2018 to 2019-2020						
Registered in 2022	0.0634*	0.0419+	0.0358	0.0214	0.0329	0.0197
	(0.0285)	(0.0249)	(0.0443)	(0.0388)	(0.0405)	(0.0355)
	[258,329]	[258,329]	[258,329]	[258,329]	[123,744]	[123,744]
Voted in 2020	0.0985**	0.0743**	0.0942*	0.0783+	0.1193**	0.1047**
	(0.0305)	(0.0270)	(0.0474)	(0.0421)	(0.0434)	(0.0385)
	[258,329]	[258,329]	[258,329]	[258,329]	[123,744]	[123,744]
B. Sample: Academic Year 2020-2021 (COVID-19 Cohort)						
Registered in 2022	-0.0377	-0.0333	0.0410	0.0320	0.0156	0.0091
	(0.0530)	(0.0479)	(0.0771)	(0.0714)	(0.0730)	(0.0673)
	[83,664]	[83,664]	[83,664]	[83,664]	[40,290]	[40,290]
Voted in 2020	-0.0350	-0.0383	0.0160	0.0066	0.0234	0.0127
	(0.0579)	(0.0518)	(0.0843)	(0.0771)	(0.0797)	(0.0727)
	[83,664]	[83,664]	[83,664]	[83,664]	[40,290]	[40,290]
Bandwidth	$10,000	$10,000	$10,000	$10,000	$5,000	$5,000
Kernel	Uniform	Uniform	Uniform	Uniform	Uniform	Uniform
Polynomial	1	1	2	2	1	1
Controls	No	Yes	No	Yes	No	Yes

Note: $^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$. Heteroskedasticity robust standard errors in parentheses. Sample size in brackets.

Table D.4: Impacts of Cal Grants and Pell Grants by Election

	(1)	(2)	(3)	(4)
Election Year	2014	2016	2018	2020
Cal Grant Receipt	0.0062	0.0149	0.0343	0.0985**
	(0.0168)	(0.0249)	(0.0244)	(0.0305)
Sample Size	253,196	330,899	337,420	258,329
Pell Grant Notch	0.0010	0.0040*	-0.0004	0.0049*
	(0.0011)	(0.0017)	(0.0015)	(0.0020)
Sample Size	828,132	1,181,103	1,261,717	1,003,200
Bandwidth	$10,000	$10,000	$10,000	$10,000
Kernel	Uniform	Uniform	Uniform	Uniform
Polynomial	1	1	1	1
Controls	No	No	No	No

Note: $^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$. Heteroskedasticity robust standard errors in parentheses. Each column reflects the results for students entering in the respective calendar year or any of the three preceding years, excluding missing data and sample restrictions. The heterogeneity in estimated treatment effects for different election years may capture: changes in the composition of near threshold students over time as the income thresholds move around and eligibility criteria change, sharp changes in the generosity of financial aid at these thresholds over time, changes in the composition of college going cohorts of students across years, random noise given that the confidence intervals of many of these estimates overlap, and unobserved biases in the estimated treatment effects for pre-2017 entering student cohorts for whom income thresholds for eligibility were visible ex ante to filing a tax return.

D.2 Heterogeneity

Having demonstrated the Cal Grant's strong overall impact on political participation, we pivot to heterogeneous treatment effects and intermediate outcomes. The estimated results of our heterogeneity analysis are displayed in Table D.5, which includes four panels that show heterogeneity by the racial or ethnic composition of a student's home ZIP code in Panel A, the socioeconomic composition of their ZIP code in Panel B, the political composition of their ZIP code in Panel C, and the high school GPA of GPA-eligible students for whom this data is available in Panel D. Each column of the table represents the results for one quartile of the distribution of the respective variable for which we are assessing heterogeneity. We use total general election voter turnout in all post-treatment elections between 2010 and 2020 as our outcome of interest and repeat our preferred specification from Row 3, Column 1 of Table C.1 to provide a common point of comparison.

We start with heterogeneity by race and ethnicity in Panel A, using the racial and ethnic composition of a student's home region to proxy for these characteristics. Specifically, we use L2 voter file data to calculate the racial and ethnic shares of registered California voters who filed a FAFSA between 2010-2011 to 2020-2021 and collapse this data on ZIP code of origin. Rows 1 through 4 show the estimated impact of the Cal Grant on 2017-2018 to 2019-2020 FAFSA filers by the probability of being of a non-European ethnicity, Hispanic, Asian, and Black, respectively. We see little heterogeneity in the treatment effects of Cal Grants across these dimensions and are unable to detect significant differences by quartile of racial or ethnic composition. This suggests that the impact of higher education spending on political participation is unlikely to be driven by a single racial or ethnic group.

In terms of heterogeneity by socioeconomic composition in Panel B, we do not identify any significant differences in the treatment effects of Cal Grants on political participation. We repeat our methods from Panel A and find that there are no obvious patterns across quartiles of mean ZIP code income or mean ZIP code asset levels. We acknowledge that two limits to this analysis are the reality that Cal Grant recipients must themselves have low assets and that we are examining impacts for students local to an income eligibility ceiling. Our interpretation is that the absence of a clear pattern by neighborhood SES nonetheless provides suggestive evidence that there is unlikely to be large SES heterogeneity at the individual level.

Panel C repeats these methods again to examine whether effects are concentrated among students who originate from politically different households or neighborhoods, using the political composition of a FAFSA filer's ZIP code of origin as a proxy. Specifically, we collapse the 2020 voter turnout rate and Republican to Democratic ratio at the ZIP code of origin level, defining these as ZIP Code Voter Turnout and ZIP Code Conservatism, respectively.

We find no detectable heterogeneity along either of these dimensions, suggesting that the political climate of one's upbringing is relatively unimportant in determining the magnitude of Cal Grants' impact on future political participation.

Finally, we examine heterogeneity in the Cal Grant's impact on voter turnout by a student's high school GPA, restricting to the subset of students for whom this data is available. We note that Cal Grant receipt increases the share of students reporting a GPA across the policy threshold, as GPA verification is required to determine the eligibility of high school students who are first-time college applicants. Because of potential issues with selection into reporting GPA, mixed listing of high school and community college GPAs, and the minimum 3.0 GPA eligibility limit for Cal Grants, we restrict our sample in this row to students with GPAs above 3.0 for whom we can identify a high school of origin. Our findings suggest that Cal Grants have a stronger impact on political participation among students with the highest high school GPAs, with the largest effects observed among students with GPAs above 3.41 (roughly a B+ average).

Table D.5: Estimated Impacts of 2017-2019 Cal Grant Receipt on General Election Turnout

Dimension of Heterogeneity	(1)	(2)	(3)	(4)
A. Heterogeneity by Racial and Ethnic Composition of Home ZIP Code				
ZIP Code Share Minority	0.0557	0.0756	0.1323*	0.0967
	(0.0487)	(0.0500)	(0.0592)	(0.0712)
ZIP Code Share Hispanic	-0.0018	0.1193*	0.1670**	0.0736
	(0.0497)	(0.0501)	(0.0600)	(0.0676)
ZIP Code Share Asian	0.0212	0.1085*	0.1700**	0.0192
	(0.0689)	(0.0547)	(0.0505)	(0.0522)
ZIP Code Share Black	0.0289	0.1606**	0.0444	0.0962
	(0.0543)	(0.0499)	(0.0534)	(0.0674)
B. Heterogeneity by Socioeconomic Composition of Home ZIP Code				
ZIP Code Mean Income	0.0788	0.2002**	0.0037	0.0953*
	(0.0914)	(0.0668)	(0.0509)	(0.0405)
ZIP Code Mean Assets	0.1294	0.1291*	0.0399	0.0857+
	(0.0815)	(0.0646)	(0.0489)	(0.0442)
C. Heterogeneity by Political Composition of Home ZIP Code				
ZIP Code 2020 Turnout	0.1440+	0.0052	0.0802	0.1197**
	(0.0752)	(0.0621)	(0.0516)	(0.0436)
ZIP Code Conservatism	0.1122+	0.1266*	0.0551	0.0658
	(0.0653)	(0.0580)	(0.0534)	(0.0485)
D. Heterogeneity by High School GPA (Subset of Full Sample)				
High School GPA	-0.0442	0.0171	0.1060*	0.0936*
	(0.0825)	(0.0648)	(0.0526)	(0.0371)
Bandwidth	$10,000	$10,000	$10,000	$10,000
Kernel	Uniform	Uniform	Uniform	Uniform
Polynomial	1	1	1	1
Controls	No	No	No	No
Quartile	1	2	3	4

Note: $^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$. Heteroskedasticity robust standard errors in parentheses. The outcome for all regressions in this table is "General Election Turnout", which refers to the share of all federal general elections between 2010 and 2020 in which a student voted after the academic year in which they filed a FAFSA. "ZIP Code Voter Turnout" refers to the 2020 voter turnout rate in a student's home ZIP code among FAFSA filers who originated from their home ZIP code. "ZIP Code Conservatism" refers to the share of FAFSA filers originating from a student's home ZIP code that registered with a major political party and joined the Republican Party.

E Mechanisms

Table E.1: Turnout Effects of 2017-2019 Cal Grants Conditional on Voter Registration

Outcome	(1)	(2)	(3)	(4)	(5)	(6)
Voter Turnout	0.0501*	0.0424+	0.0704+	0.0710*	0.0937**	0.0945**
	(0.0234)	(0.0218)	(0.0367)	(0.0343)	(0.0333)	(0.0312)
Bandwidth	$10,000	$10,000	$10,000	$10,000	$5,000	$5,000
Kernel	Uniform	Uniform	Uniform	Uniform	Uniform	Uniform
Polynomial	1	1	2	2	1	1
Controls	No	Yes	No	Yes	No	Yes
Sample Size	177,356	177,356	177,356	177,356	85,147	85,147

Note: $^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$. Heteroskedasticity robust standard errors in parentheses. "Voter Turnout" refers to the share of all federal general elections in which a student voted after the academic year in which they filed a FAFSA. Outcomes directly correspond to those in Table C.1.

Table E.2: Effects of 2017-2019 Cal Grants on Partisanship and Vote Choice

Outcome	(1)	(2)	(3)	(4)	(5)	(6)
A. Total Registration						
Registered in 2022	0.0634*	0.0419+	0.0358	0.0214	0.0329	0.0197
	(0.0285)	(0.0249)	(0.0443)	(0.0388)	(0.0405)	(0.0355)
B. Registration by Partisanship in 2022						
Republican	-0.0056	-0.0076	-0.0060	-0.0080	-0.0108	-0.0143
	(0.0189)	(0.0186)	(0.0293)	(0.0290)	(0.0268)	(0.0265)
% of New Registrants	[-9%]	[-18%]	[-17%]	[-37%]	[-33%]	[-73%]
Democrat/Independent	0.0690*	0.0496+	0.0418	0.0294	0.0437	0.0341
	(0.0303)	(0.0277)	(0.0471)	(0.0432)	(0.0431)	(0.0395)
% of New Registrants	[109%]	[118%]	[117%]	[137%]	[133%]	[173%]
C. Imputed 2020 Vote Probabilities						
Biden Vote 2020	0.0855**	0.0652**	0.0746+	0.0620+	0.0971**	0.0867**
	(0.0262)	(0.0234)	(0.0408)	(0.0366)	(0.0373)	(0.0335)
Trump Vote 2020	0.0130	0.0091	0.0196	0.0163	0.0221	0.0180
	(0.0131)	(0.0128)	(0.0204)	(0.0199)	(0.0187)	(0.0182)
No Vote 2020	-0.0985**	-0.0743**	-0.0942*	-0.0783+	-0.1193**	-0.1047**
	(0.0305)	(0.0270)	(0.0474)	(0.0421)	(0.0434)	(0.0385)
Bandwidth	$10,000	$10,000	$10,000	$10,000	$5,000	$5,000
Kernel	Uniform	Uniform	Uniform	Uniform	Uniform	Uniform
Polynomial	1	1	2	2	1	1
Controls	No	Yes	No	Yes	No	Yes
Sample Size	258,329	258,329	258,329	258,329	123,744	123,744

Note: $^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$. Heteroskedasticity robust standard errors in parentheses. Outcomes directly correspond to those in Table C.1. "Biden Vote 2020" and "Trump Vote 2020" refer to the probability that a person cast a ballot for the respective candidate in the 2020 general election using actual data on whether or not they voted and Cal Grant voters' probabilities of favoring Democrats and Republicans match those of a sample of young, college-educated independents in recent survey data of California college applicants that were linked to voter registration records (Firoozi, 2023).

Table E.3: Effects of 2010-2019 Pell Grant Generosity on Partisanship and Vote Choice

Outcome	(1)	(2)	(3)	(4)	(5)	(6)
A. Total Registration						
Registered in 2022	0.0034**	0.0025*	0.0038*	0.0022	0.0034*	0.0017
	(0.0012)	(0.0011)	(0.0018)	(0.0017)	(0.0017)	(0.0016)
B. Registration by Partisanship in 2022						
Republican	-0.0002	-0.0001	0.0004	0.0005	0.0001	0.0002
	(0.0006)	(0.0006)	(0.0009)	(0.0009)	(0.0009)	(0.0009)
% of New Registrants	[-6%]	[-4%]	[11%]	[23%]	[3%]	[12%]
Democrat/Independent	0.0036**	0.0026*	0.0034+	0.0017	0.0033+	0.0015
	(0.0013)	(0.0012)	(0.0019)	(0.0017)	(0.0018)	(0.0016)
% of New Registrants	[106%]	[104%]	[89%]	[77%]	[97%]	[88%]
C. Imputed 2020 Vote Probabilities						
Biden Vote 2020	0.0041**	0.0037**	0.0039*	0.0029*	0.0038*	0.0027+
	(0.0011)	(0.0010)	(0.0016)	(0.0015)	(0.0015)	(0.0014)
Trump Vote 2020	0.0011*	0.0012**	0.0012+	0.0012+	0.0013*	0.0013*
	(0.0004)	(0.0004)	(0.0007)	(0.0006)	(0.0006)	(0.0006)
No Vote 2020	-0.0052**	-0.0049**	-0.0051**	-0.0041*	-0.0051**	-0.0040*
	(0.0012)	(0.0011)	(0.0019)	(0.0017)	(0.0017)	(0.0016)
Bandwidth	$10,000	$10,000	$10,000	$10,000	$5,000	$5,000
Kernel	Uniform	Uniform	Uniform	Uniform	Uniform	Uniform
Polynomial	1	1	2	2	1	1
Controls	No	Yes	No	Yes	No	Yes
Sample Size	258,329	258,329	258,329	258,329	123,744	123,744

Note: $^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$. Heteroskedasticity robust standard errors in parentheses. Outcomes directly correspond to those in Table C.1. "Biden Vote 2020" and "Trump Vote 2020" refer to the probability that a person cast a ballot for the respective candidate in the 2020 general election using actual data on whether or not they voted and Pell Grant voters' probabilities of favoring Democrats and Republicans match those of a sample of young, college-educated independents in recent survey data of California college applicants that were linked to voter registration records (Firoozi, 2023).