Do Tuition Subsidies Raise Political Participation?*

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Abstract

Although civic externalities are a primary motivation for subsidizing education, little research estimates the civic returns to education subsidies. We use data on 16.4 million FAFSAs and a regression discontinuity (RD) design to estimate the impact of the United States' largest tuition-free 4-year college program on political participation. We find that each of the 2.6 million awards increases a student's voter turnout rate by 4 to 12 percentage points in the 2020 election, raising total citizen voter turnout by 1 percentage point and Joe Biden's margin of victory by 0.5 percentage points in the awarding state. Using detailed location data along with conservative assumptions, we calculate that 1 out of every 66 voters cast a ballot because of the tuition subsidy, with higher rates in politically competitive locations. Evidence from intermediate outcomes and time-variation in treatment effects point to peer socialization mechanisms. We externally validate our estimates with another RD design and data on 2.5 million students subject to a notch in Pell Grant generosity, showing that our results generalize to other policy settings. Our findings demonstrate that the civic externalities of education subsidies can exceed their labor market returns and are large enough to change the outcomes of recent national elections.

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1 Introduction

Identifying education's social returns is a fundamental economic question that requires estimates of both private labor market returns and externalities accruing to other parties. Extant research focuses on the health, crime, and innovation externalities of education policy but mostly overlooks civic externalities despite their historic importance.[1] Specifically, economists like Adam Smith and Milton Friedman mainly justified education subsidies by asserting that they produce informed, politically active citizens (Friedman, 2020; Frame and Schwarze, 2022). Policymakers often expanded education subsidies on a similar basis, explicitly citing effects on democratic participation rather than earnings (Mann, 1957). Omitting civic externalities from estimates of the social returns to education therefore both narrows the scope of public finance and risks understating education's marginal social benefits.

A well-documented positive association between education and political participation exists in almost every historical and national context (Gethin et al., 2021). Educated citizens are more likely to vote, follow the news, and hold public office. Existing work suggests preschool and primary school may raise political participation but is mixed on the impact of education later in life.[2] In particular, we know remarkably little about whether colleges improve political participation despite the largest participation gaps correlating with college and 235 billion dollars dispersed annually in US financial aid alone (College Board, 2023). Attempts to estimate the civic externalities of higher education have not reached a consensus due to the dearth of instruments that allow for a credible identification strategy.[3]

[1] For example, empirical work in leading general interest economic journals shows that education policies can have large impacts on incarceration and criminal activity (Deming, 2011; Billings et al., 2014; Bell et al., 2022; Anders et al., 2023; Gray-Lobe et al., 2023), estimates the impact of education on mortality and long-run health outcomes (Clark and Royer, 2013; Heckman et al., 2018; 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 and secondary schooling generally find positive effects (Dee, 2004; Sondheimer and Green, 2010; Wantchekon et al., 2015) but evidence on secondary schools is less consistent (Milligan et al., 2004; Tenn, 2007; Marshall, 2016, 2019; Cohodes and Feigenbaum, 2021; Willeck and Mendelberg, 2022).

[3] Most papers on this topic use propensity scores and matching methods, with conflicting results as a consequence (Kam and Palmer, 2008; Mayer, 2011; Willeck and Mendelberg, 2022). Three studies use instruments for enrollment, specifically distance to college and Vietnam conscription, and again arrive at different conclusions (Dee, 2004; Berinsky and Lenz, 2011; Doyle and Skinner, 2017).

In this paper, we use data on 16.4 million Free Applications for Federal Student Aid (FAFSAs) and a regression discontinuity (RD) design to provide the only evidence to date that tuition subsidies increase students' political participation. We make use of 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). This setting is advantageous because (1) extant evidence on the labor market returns to the Cal Grant are important context for understanding its civic externalities, (2) California is the largest setting in the democratic world where most voters self-report party preferences in administrative data, and (3) California has the highest retention rate of college students 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 56 percentage point baseline. This implies that the 2.6 million grants awarded over the 2010s led to an additional 259,000 votes (1 percentage point of the citizen voting eligible population) being cast in California during the 2020 general election alone. Effects are similar across proxies of race, socioeconomic status, and political environment of origin, but are larger among students with the highest high school GPAs. Our results are robust to multiple definitions of voter turnout as well as a number of RD implementation choices.

Using detailed location and partisanship records to track Cal Grant recipients and relying on conservative assumptions, we calculate the macro-level externalities of the program. Our estimates imply that 1 out of every 66 voters in California cast a ballot in 2020 because of the Cal Grant. The program accelerates geographic polarization in the electorate because its largest effects are in politically competitive locations that are home to public research universities. We find that almost 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 the state of

California by 0.5 percentage points in 2020. The magnitude of this effect in percentage points would be large enough to change the winner of presidential elections in 10 American states during the 21st century.

Evidence from intermediate outcomes and time variation highlight the importance of peer socialization and mechanisms related to in-person college attendance. Cal Grants increase peer socialization by raising the rate at which students enroll in 4-year colleges and live near campus. Effects on voter turnout materialize within two years of award receipt and accrue to measures of both the extensive and intensive margins of voter turnout across post-treatment elections. The null impact we find during COVID-19 remote instruction provides more support for the conclusion that in-person college socialization is a critical causal pathway.

We externally validate our findings with another RD design and data on 2.5 million students subject to a generosity notch in America's largest financial aid policy, the Pell Grant (Denning et al., 2019). Conservatively, the subsidy raises 2020 voter turnout by 0.5 percentage points per 1,000 dollars, mainly among center-left voters. We calculate that Pell Grants awarded since 2010 raised total American voter turnout in 2020 by 1,819,000 votes (1 out of every 87 voters) and Joe Biden's nationwide margin of victory by 1,182,000 votes, a large enough effect in percentage points to change the electoral college victor. The Pell and Cal Grant's analogous effects underscore our findings' generalizability across settings.

Our results demonstrate that tuition subsidies can have civic externalities exceeding their labor market returns. The program we study facilitates the production of an informed and active electorate by encouraging low income and college-educated youth with the highest GPAs to vote. Given our work on external validity, we conclude that omitting civic externalities from estimates of the social returns to education subsidies may significantly understate their marginal social benefits across policy settings. Our evidence also introduces challenging 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, like student loans or institution-specific grants.

The Cal Grant program assesses eligibility annually based on a number of criteria. For our analysis, we focus on the income eligibility thresholds for the Cal Grant A program from the 2017-2018 to 2019-2020 academic years. These income thresholds vary by family structure 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

[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.

[6]Expected Family Contribution or EFC is a measure of socioeconomic status that depends on family

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; Bettinger et al., 2019; Hendren and Sprung-Keyser, 2020). Bettinger et al. (2019) find that the Cal Grant has no significant effect on aggregate college enrollment for GPA-marginal students due to high baseline rates. However, at the *GPA threshold*, there are significant effects on 4-year college attendance three to four years after high school graduation, which are attributed to increased college persistence. The Cal Grant significantly increases the probability of earning a bachelor's or graduate degree among students local to the GPA threshold, with increases of 10 and 27 percent over the respective baselines. The Cal Grant also raises earnings by 3 to 4 percent between ages 28 and 32 among those at the GPA threshold and generates large fiscal externalities, resulting in an infinite marginal value of public funds (MVPF) estimated in Hendren and Sprung-Keyser (2020).[7]

At the *income threshold*, however, 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. The substitution between institutions is not associated with changes in college quality, but 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 MVPF 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 advantageous for estimating the impact of education spending

income, family assets, and the cost of attendance at the institution at which a student enrolls.

[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.

on political participation. Its standing as the largest market of higher education in the United States – and one of the largest worldwide – allows for a more precise estimation of causal effects. California further distinguishes itself with the highest in-state retention rate of college students in the US, offering unique advantages for longitudinally tracking students (Van Dam, 2022). Voter registration records in California are also highly detailed, showing that the political composition of Californian college-educated youth matches that of other American states and making California the largest setting on Earth where most 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.

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 FAFSA filer data provided by the California Student Aid Commission (CSAC). 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 FERPA and maintain anonymity, the names and dates of birth of FAFSA filers were never revealed to the authors.

The backbone of this data is sourced from CSAC, which administers the Cal Grant program. The sample provided by CSAC 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 reflected 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 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 (Columns "Main" and "Expanded").[9] Table 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,

[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.

[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.

and overwhelmingly favors the Democratic Party relative to the Republican Party. This is similar to the subsample of likely Cal Grant eligible FAFSA filers in Column 2, except for somewhat higher political participation and much lower family asset levels due to the Cal Grant's asset eligibility ceiling. Turning to 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 sample and expanded sample of near threshold FAFSA filers who were likely to be Cal Grant eligible are somewhat 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.

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 1).[10]

[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

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 students whose family incomes fall just below the eligibility threshold are similar to those whose income falls just above it except for the fact that the latter are 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.

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 sample by filing a FAFSA based on program eligibility. As demonstrated in Figure A.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 A.2 through A.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 A.5 through A.7. Third, we conduct placebo falsification tests to evaluate the design's validity.[12]

discontinuity at the threshold as Cal Grant C accounts for only 2 percent of Cal Grants awarded.

[11]The point estimates and confidence intervals are available for a range of potential bandwidths in Figures A.5 through A.7. 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.

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 may leave the state, which would make them unobservable in our outcome data. We note that National Student Clearinghouse and IRS tax data 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 a 11 years after FAFSA filing.[13] To be conservative, 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.[14]

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 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, which provides further evidence that this threat is unlikely to be driving our results.

[13] Bettinger et al. (2019) estimate an insignificant increase in out-of-state enrollment for each Cal Grant received. Because moving out of California could make a student's political participation records unobservable, a literal interpretation of the authors' estimates would bias our short run estimates *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).

[14] 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.

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.[15] COVID-19 is another potential threat, as it could have fundamentally altered the impact of the Cal Grant because college was entirely remote in the 2020-2021 academic year. To address this issue, we exclude the 2020-2021 cohort from our main analysis and only include them 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:

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

[15]These students represent roughly 6 percent of the sample within 10,000 dollars of the income ceiling.

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.

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).[16]

[16]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.

4 Results

4.1 Registration and Turnout

We begin by plotting our outcomes of interest against a student's normalized family income in Figure 2. The first outcome is voter registration in 2022, which we use as a broad and 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 is the voter turnout rate in all general elections that took place after the academic year in which students filed their FAFSA, which captures the full magnitude of participation in the elections that typically draw the highest levels of voter turnout. Finally, we interact the voter turnout rate with an indicator for being registered as a Democrat or independent[17], 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).[18] 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.

[17]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.

[18]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).

We test these outcomes formally in Table 2. 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 increases in aggregate voter registration. Our preferred specification in Column 1 yields a point estimate of 6.34 percentage points per grant awarded, which is significant at a 95 percent confidence interval. However, the estimates in Columns 2 through 6 are generally smaller or not significant, which suggests that the estimated impact of Cal Grant receipt on voter registration may be sensitive to model specification.

Turning to voter turnout in 2020 and general election turnout 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 model specifications and definitions of turnout.

Panel C rounds out the analysis 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.

To ensure that the estimates in Table 2 are robust, we re-estimate results varying a number of RD implementation choices. We present our findings in Figures 3 and B.1 through B.4, which show 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 B.1 that our main results are robust to the use of CCT bias-aware confidence intervals. Finally, we conduct falsification tests in Figure B.5, 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.[19] We find 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, providing further support for the validity of our RD design.

4.2 Heterogeneity and Intermediate Outcomes

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 3, 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

[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.

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 as our outcome of interest and repeat our preferred specification from Row 3, Column 1 of Table 2 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 neighbhorhoods, 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).

With our analysis on heterogeneous treatment effects complete, we pivot to intermediate outcomes, specifically campus housing choice. Because tuition subsidies like the Cal Grant may influence students' housing decisions, we present RD plots for these outcomes in Figure 4, using noisy CSAC records on housing intent for Cal Grant non-recipients and actual housing outcomes for recipients. The RD plots in Figure 4 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 show the impact of the Cal Grant on campus housing choice formally in Table 4. 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.

4.3 Time Variation

We now 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 this subsection, we first present the results for the 2010-2011 to 2019-2020 sample. Next, we examine the longitudinal impact of the Cal Grant on political participation. Finally, we investigate the impact of the COVID-19 pandemic on the Cal Grant's effect on political participation for the 2020-2021 cohort of students who entered college entirely online.

In Table 5, 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. We use the same model specifications and outcome measures as in Table 2, with the addition of an indicator variable for whether or not a student ever cast a ballot in a federal general election. The results are consistent with those of the more recent sample of FAFSA filers, with smaller magnitudes. In Panel A, we find that the Cal Grant raises voter registration rates over the expanded time period, although these results are somewhat noisy and sensitive to model specification.

Furthermore, we find increases in voter turnout between 2 and 9 percentage points across all definitions in Panel B, indicating that the Cal Grant raises both the intensive and extensive margins of political participation over the long-run. Lastly, we find in Panel C that over 80 percent of the effect on voter turnout accrues to registered Democrats and independents, consistent with our evidence from more recent cohorts.

Next, in Table 6, 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 7. 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.

5 Discussion

5.1 Mechanisms

We evaluate two potential mechanisms that could explain the observed results: exposure to the college campus environment and skin-in-the-game effects. Under the former causal pathway, the Cal Grant facilitates socialization in 4-year colleges by raising the rate at which students spend time with peers or interact with organizations on campus. Under the latter causal pathway, the effects of a government transfer payment like the Cal Grant are strictly due to students feeling more invested in election outcomes. We provide evidence aligned with socialization at 4-year colleges and generally contradicting skin-in-the-game effects.

We start with the idea that exposure to the college campus environment leads to increased socialization and, in turn, higher rates of voter turnout. The results of Table 4 demonstrate that Cal Grant reciept increases the rate at which students live on campus and reduces the rate at which they are housed with their family or guardians. Cal Grants may also reduce student labor hours and, based on extant research, cause a small amount of substitution from 2-year public colleges toward 4-year colleges in California, affording more opportunities for on-campus social engagement (Bettinger et al., 2019). Finally, evidence from time variation strongly supports the idea that some element of in-person exposure to a 4-year campus environment matters. As Table 6 illustrates, effects appear within the first two years of receiving a Cal Grant, the same time period over which a student is more likely to live on campus. Null effects for the 2020-2021 COVID-19 year in Table 7 likewise imply that online

learning does not generate similar effects.

The second mechanism we test is the idea that receiving a government transfer payment like the Cal Grant leads to increased "skin-in-the-game" and, in turn, higher rates of voter turnout. We find evidence that generally contradicts this hypothesis. Specifically, we show that higher turnout takes place even in presidential general elections that do not include elected officials who vote on state budgets, hold seats on public university governing boards, and have line-item vetoes over Cal Grant funding (see Tables 2 and 5). We also find null effects on turnout for the 2020-2021 students who received state financial aid but did not attend in-person schooling as a result of COVID-19, suggesting that skin-in-the-game alone is not enough to increase voter turnout. Finally, we note that the Cal Grant does not induce students to vote at higher rates in the alternating set of State Senate districts where legislators who vote on Cal Grant funding actually appear on the ballot.

Taken together, these findings suggest that the effect of the Cal Grant on voter turnout is primarily driven by increased exposure to the college campus environment and socialization, rather than by a sense of skin-in-the-game created by receiving financial aid. These findings have important implications for policymakers because they suggest that a wide set of policies that increase socialization during college, beyond tuition subsidies and other forms of financial aid, may have sizable civic externalities.

5.2 External Validity and the Pell Grant

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 1) who are local to a notch in 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 qualifies a student for zero EFC and increases Pell Grant generosity by 700 dollars.[20] Because we do not directly observe

[20] 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

total financial aid awarded, we assume a discontinuity of 1,000 dollars to be conservative.[21] 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 5 entails multiplying estimates in this section by 10.

We begin by plotting our main outcomes of interest against a student's centered AGI in Figure 5. Paralleling the results in Figure 2, the four panels of Figure 5 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, 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 across policy settings.

In Table 8, we formally estimate the intent to treat (ITT) effects of this notch in Pell Grant generosity. We replicate our outcomes and specifications from Table 5'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

voter turnout at the notch for Pell Grant generosity.

[21] 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.

size in Table 5, meaning that our results across the Pell Grant and Cal Grant imply similar per-dollar estimates of the civic externalities of 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.[22] Using conservative assumptions about the partisan composition of treated students[23], 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 winner in Arizona, Georgia, Wisconsin, and the overall electoral college.

5.3 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 from three angles, 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 selectorate 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

[22]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.

[23]See the middle column of Table 9 and Section 5.3 for a detailed explanation behind assumptions on partisanship.

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 a good proxy for the average treatment effect for all Cal Grant recipients (ATT). This assumption is likely reasonable, since we find little evidence of heterogeneity by race or socioeconomic status. However, if this assumption is inaccurate, we anticipate that the impact of the Cal Grant on political participation may be even greater for typical Cal Grant recipients, who are lower income on average. Moreover, we have used the presidential election as a 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 from Section 5.2. We also assume that there is no persuasion effect, 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 Table 9, 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 selectorate, and the aggregate number of people who received a Cal Grant

over the 2010-2011 to 2019-2020 cohorts.

In Step 1 of Table 9, 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.[24] 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[25], 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

[24]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.

[25]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.

and the aggregate voter turnout rate relative to the total citizen voting-eligible population (CVEP).

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 profound policy implications, especially given that they omit more than a decade's worth of Cal Grants and 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 6 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 6 out of the UC's 9 undergraduate campuses (See Figure C.1 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., 2021). 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.

At the macro-political level, the estimated impact on the partisan margin of victory (in percentage points) is large enough to change the winner of presidential races in 12 states since the 2000 election. At the individual level, Cal Grants' impact on voter turnout is greater than or equal to their impact on earnings and degree attainment for students local to the income threshold. From both perspectives, we conclude that higher education and tuition subsidies can generate large positive externalities on political participation, even when they have null or small effects on earnings and degree attainment. Other policies that increase peer socialization at 4-year colleges in other settings may have similar effects.

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. Policies like the Pell Grant or Cal Grant not only have the potential to increase earnings and degree attainment, but also raise political participation among a group that is disproportionately likely to support their candidates. Conversely, right of center politicians could face a tradeoff between 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 partisan cycles in subsidies for higher education (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 data on 16.4 million FAFSAs to estimate the impact of America's largest tuition-free 4-year 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 these voters make up in educated suburbs and small college towns.

A key policy implication of this study is that tuition subsidies may be especially beneficial to left-of-center political parties, in large part due to the leftward lean of college educated youth. This 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.

The Cal Grant is a compelling policy from the standpoint of external validity because it has clear analogs to tuition subsidies in other states and countries. Our results suggest that expanding eligibility for such programs has the potential to increase political participation, even among middle income students who do not earn more money and are no more likely to graduate as a result of the policy. Evidence on mechanisms suggests that other education policies that increase socialization in the 4-year college campus environment may have similar effects. Finally, we externally validate our results from the Cal Grant and demonstrate that America's largest tuition subsidy, the federal Pell Grant, has identical civic externalities.

Our analysis of the Cal Grant provides the clearest evidence to date that higher education and tuition subsidies have externalities on political participation. Since the proposal of public primary schools, policymakers and economists have argued that education enhances civic life by producing an informed citizenry and broader representation in the political process (Mann, 1957; Friedman, 2020). Our work shows that the Cal Grant program achieves both objectives: inducing low income and college-educated youth to vote, with effects concentrated among higher GPA students. Given that the Cal Grant's political impacts exceed its impact on earnings and degree attainment, we conclude that ignoring the civic externalities of higher

education subsidies could significantly understate their marginal social benefits (Finkelstein and Hendren, 2020; Hendren and Sprung-Keyser, 2020).

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Tables

Table 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.

Table 2: 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	123,744	123,744

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. "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 3: 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 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.

Table 4: 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	123,744	123,744

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.

Table 5: 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 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 6: 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 7: 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 8: 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 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 5.

Table 9: 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.

Figures

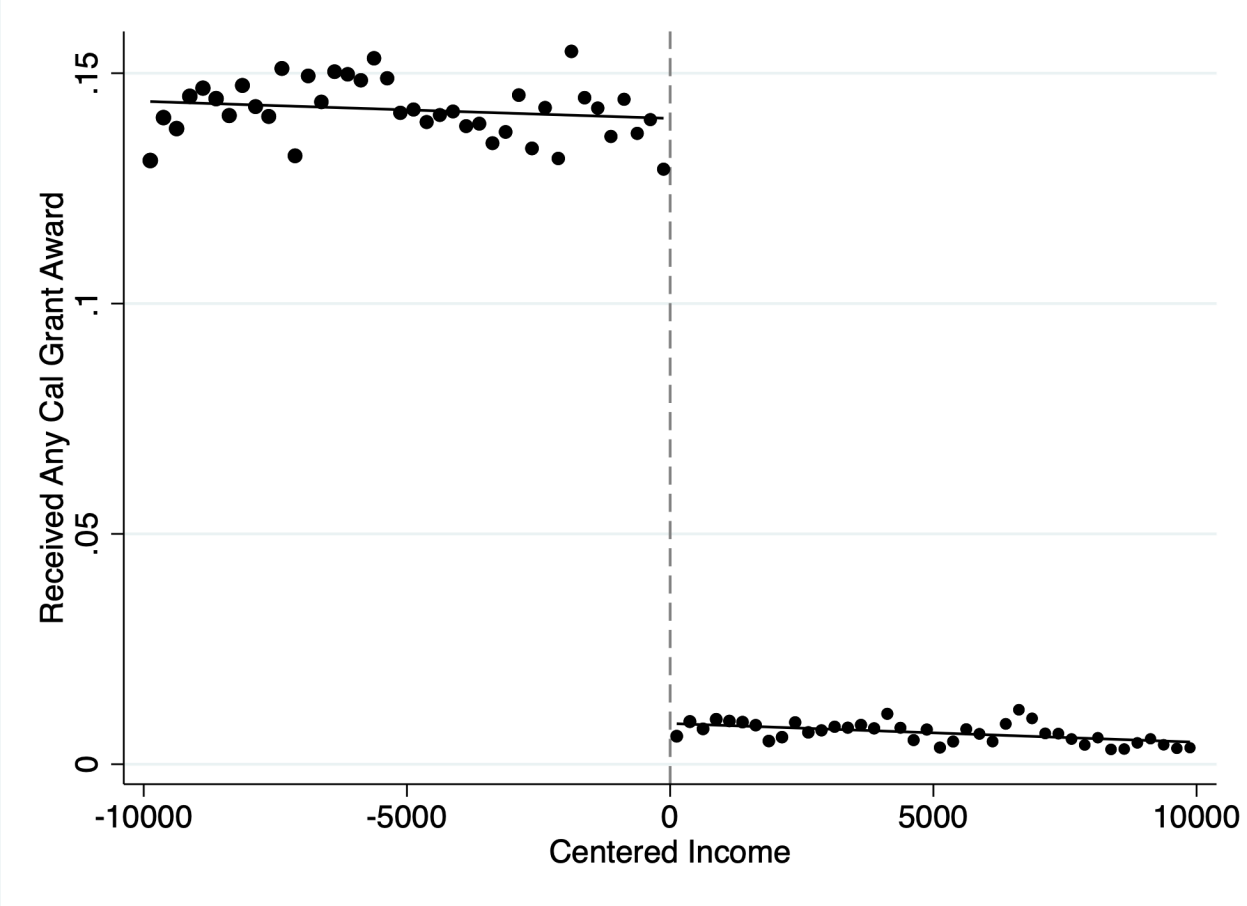

Figure 1: Regression Discontinuity Plot of Cal Grant Receipt

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.

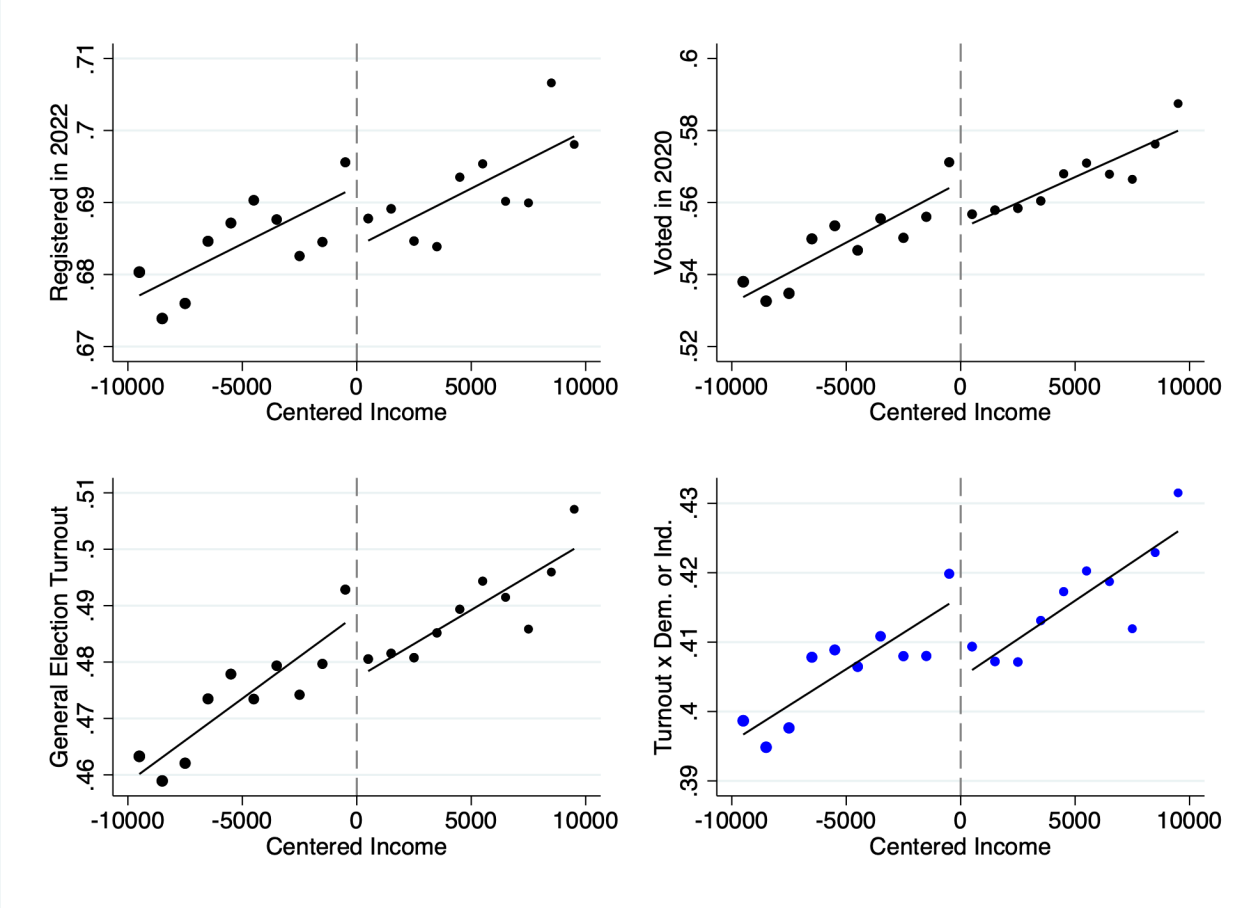

Figure 2: Regression Discontinuity Plots of Main Outcomes at Cal Grant Income Ceiling

Note: Centered income values are normalized to the income ceiling for Cal Grant A for a given individual. Outcomes correspond directly to those in Table 2.

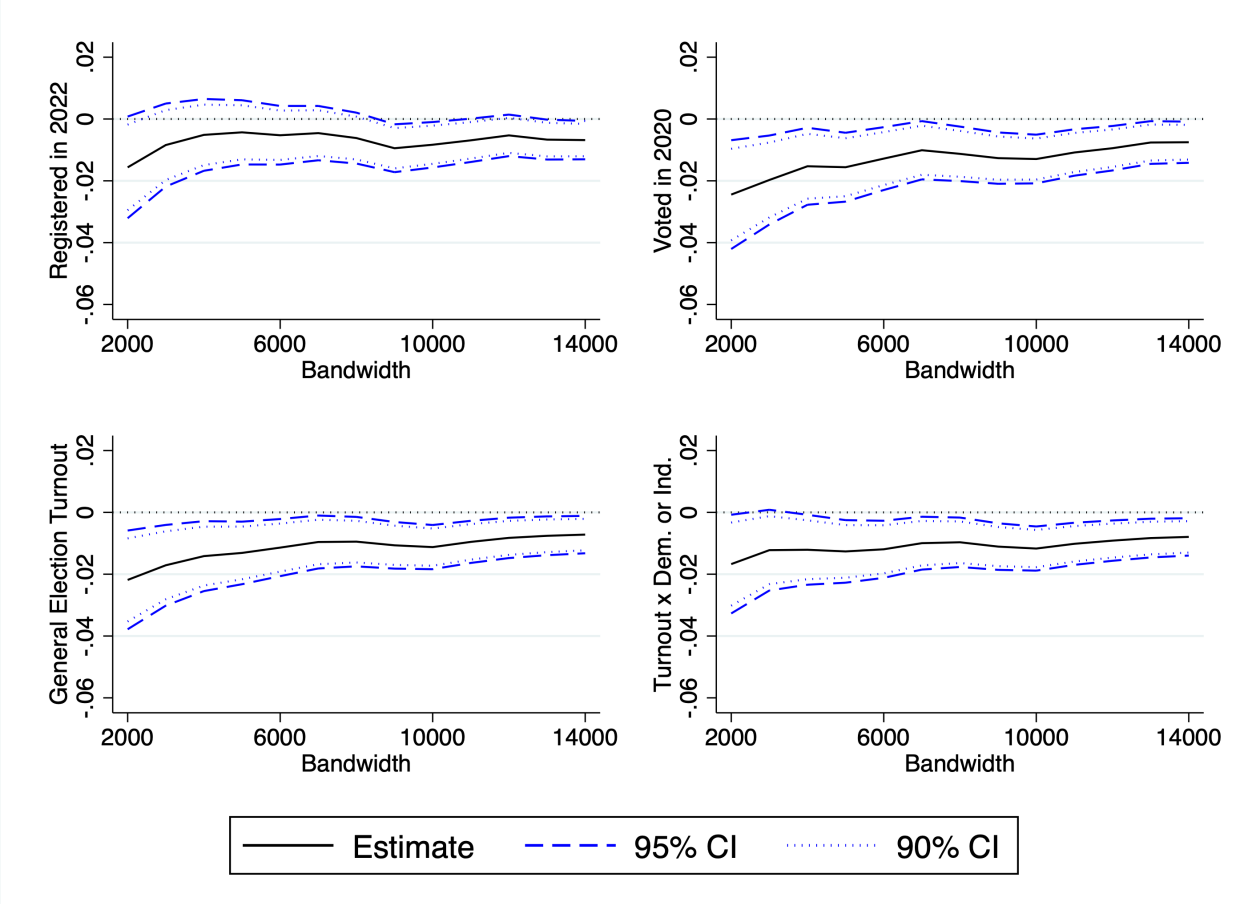

Figure 3: Bandwidth Robustness Tests for Main Outcomes

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 discontiuity in the outcome of interest. Outcomes correspond directly to those in Table 2.

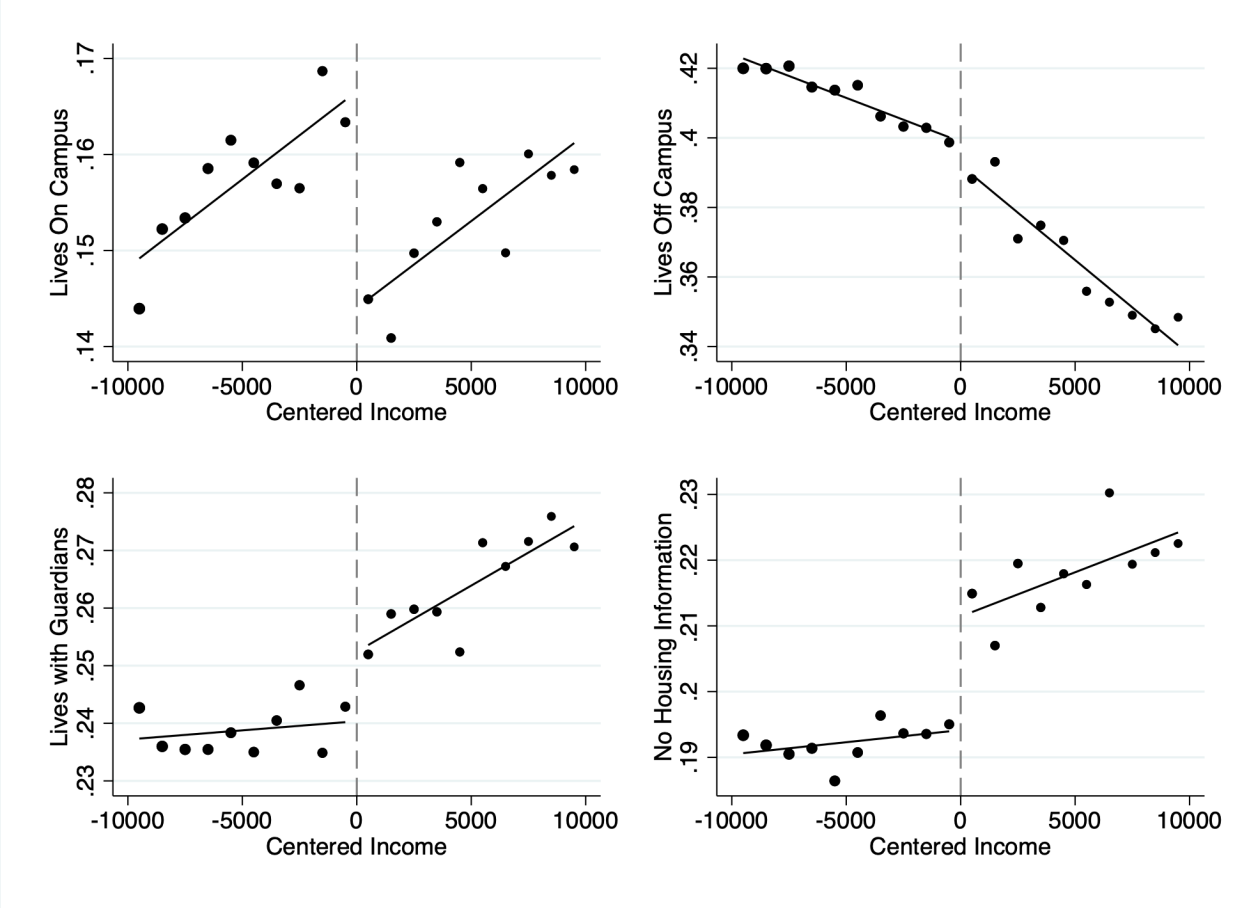

Figure 4: Regression Discontinuity Plots of Housing Choice and Intent

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 4.

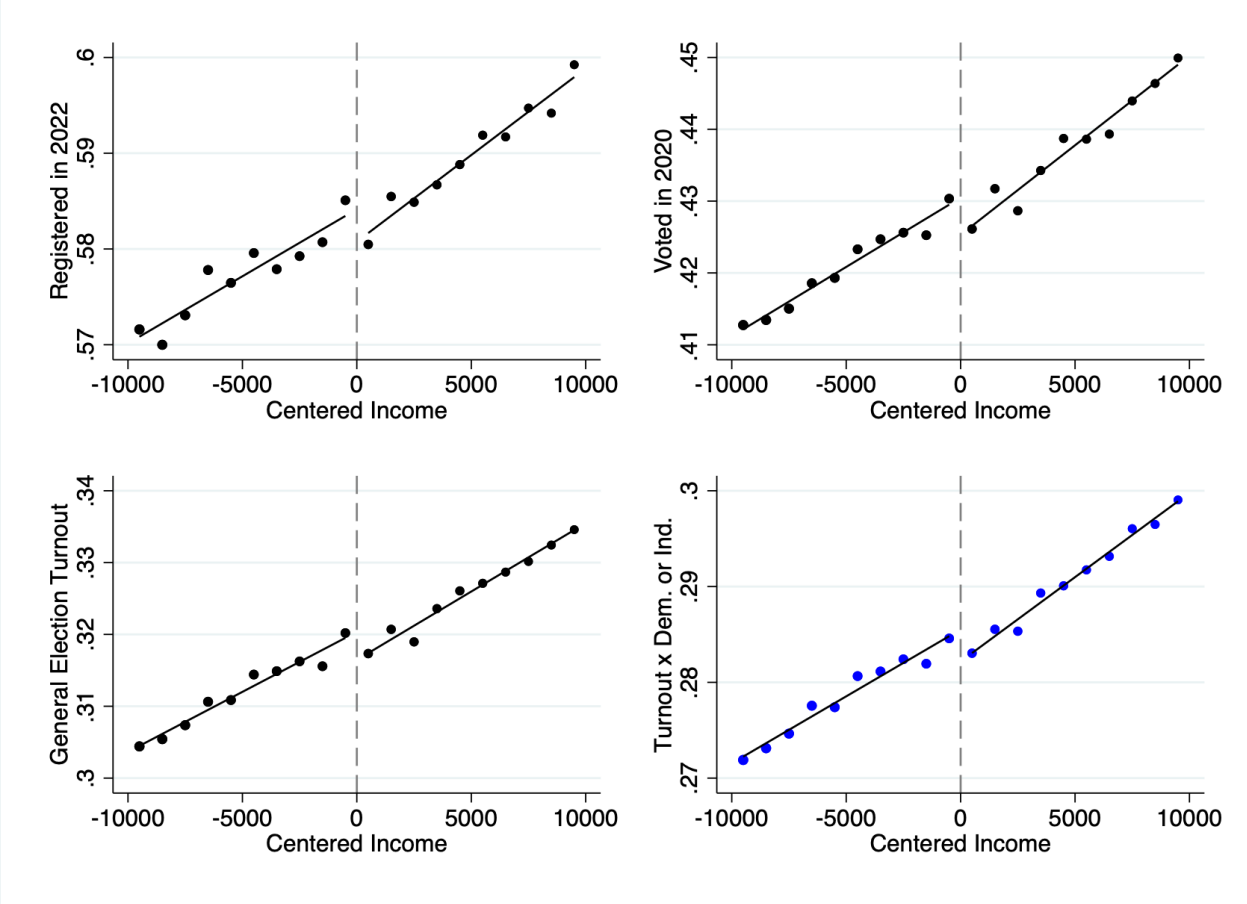

Figure 5: Regression Discontinuity Plots of Main Outcomes at Pell Grant Generosity Notch

Note: Centered income values are normalized to the Pell Grant's automatic zero expected family contribution threshold. Outcomes correspond directly to those in Table 8.

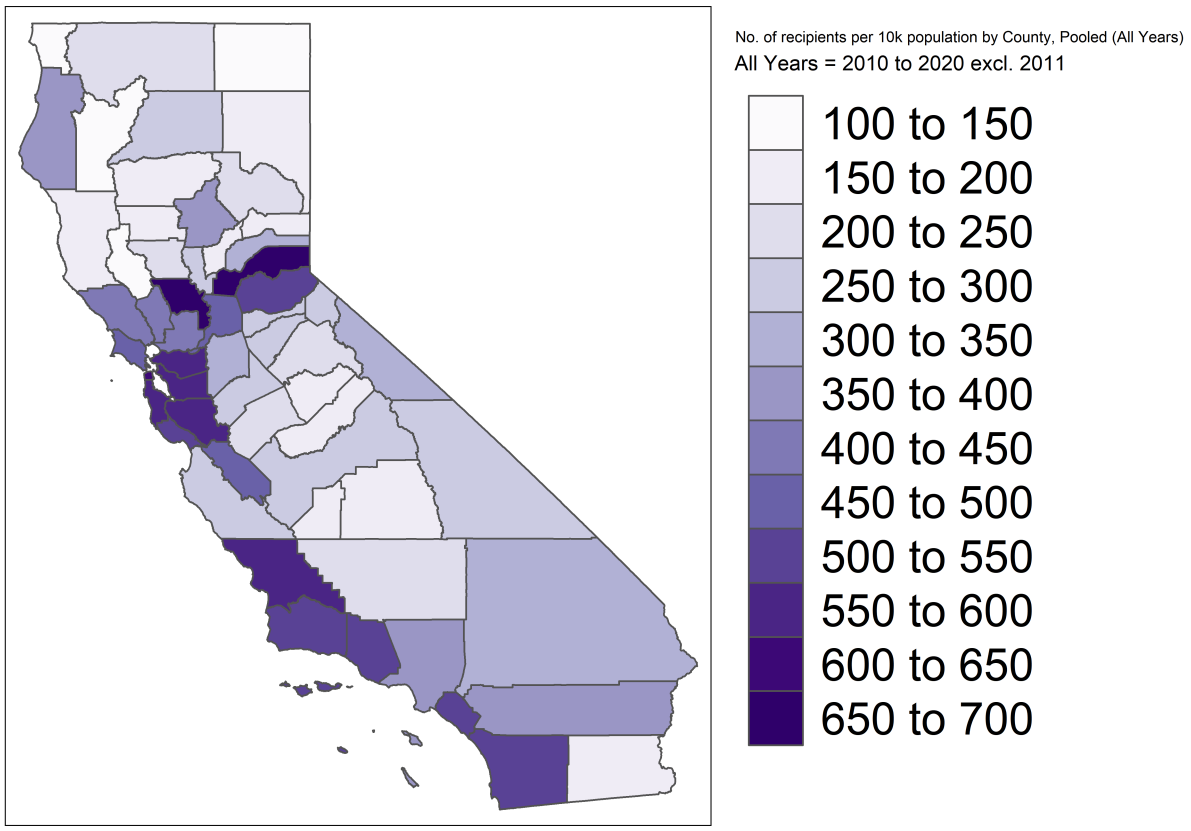

Figure 6: Heat Map of 2010-2020 Cal Grant Recipients in 2022

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 C.1 for a map of public university campuses.

Online Appendices

A RD Validation Appendix

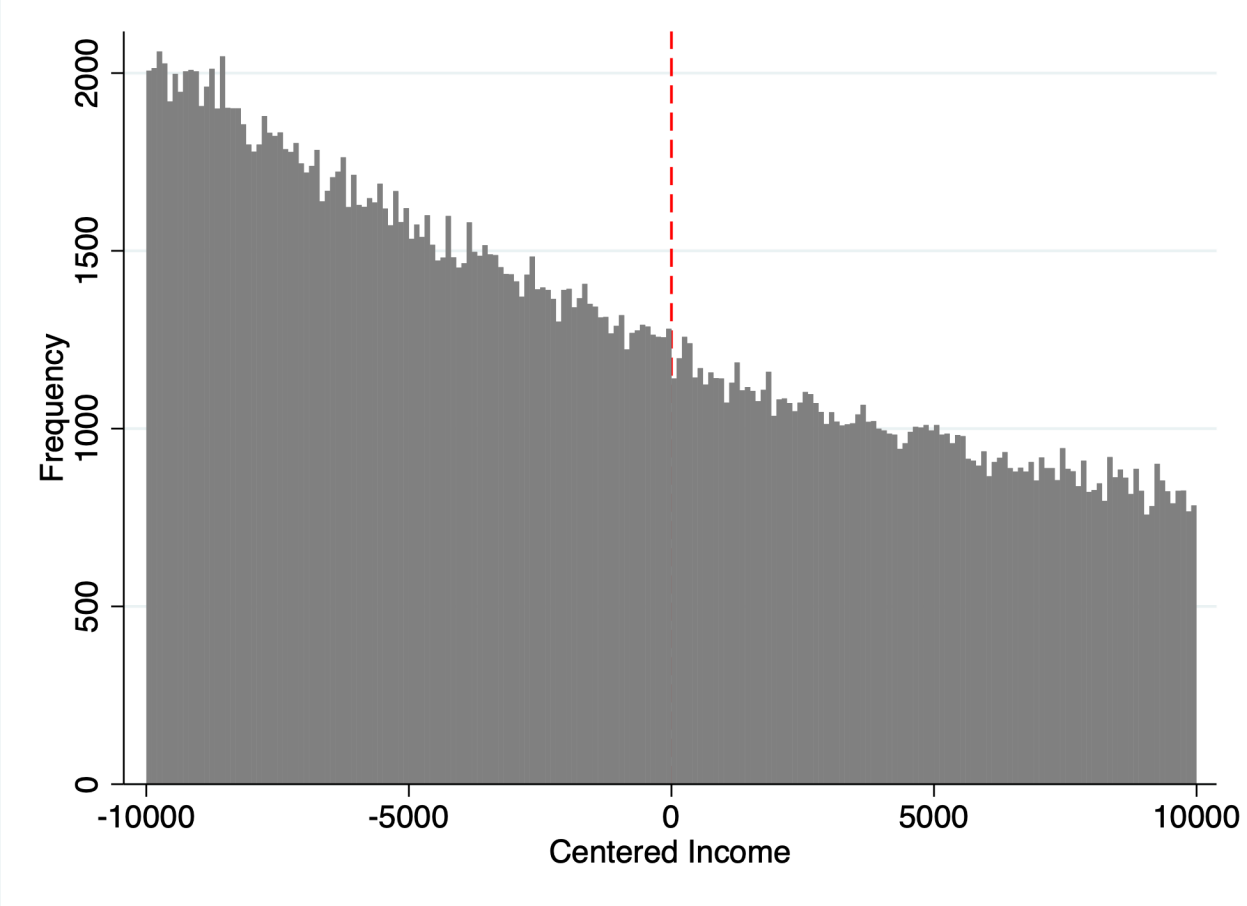

Figure A.1: McCrary Density Test

Note: Centered income values are normalized to the income ceiling for Cal Grant A for a given individual.

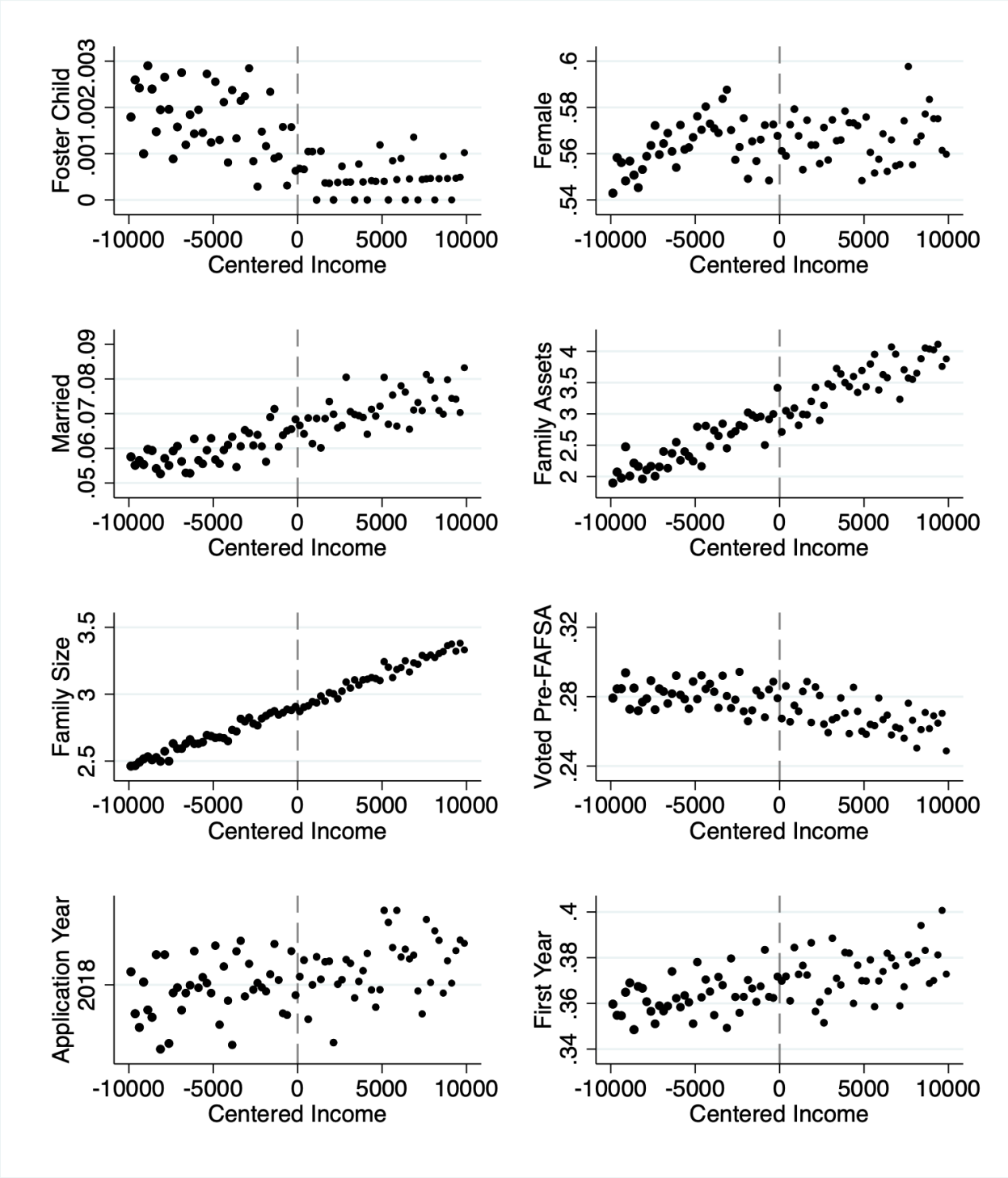

Figure A.2: Regression Discontinuity Plot for Covariates

Note: Centered income values are normalized to the income ceiling for Cal Grant A for a given individual.

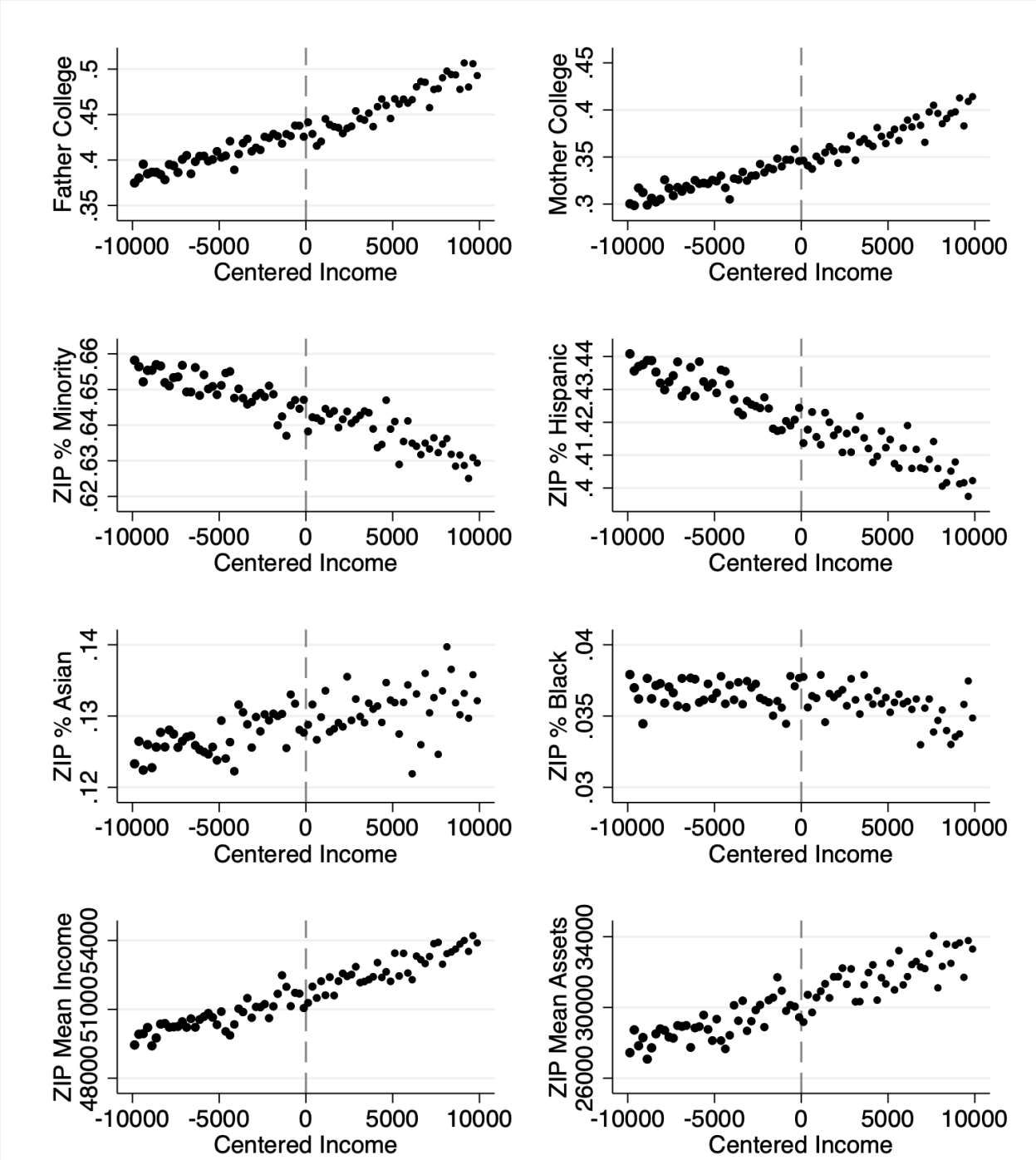

Figure A.3: Regression Discontinuity Plot for Covariates

Note: Centered income values are normalized to the income ceiling for Cal Grant A for a given individual.

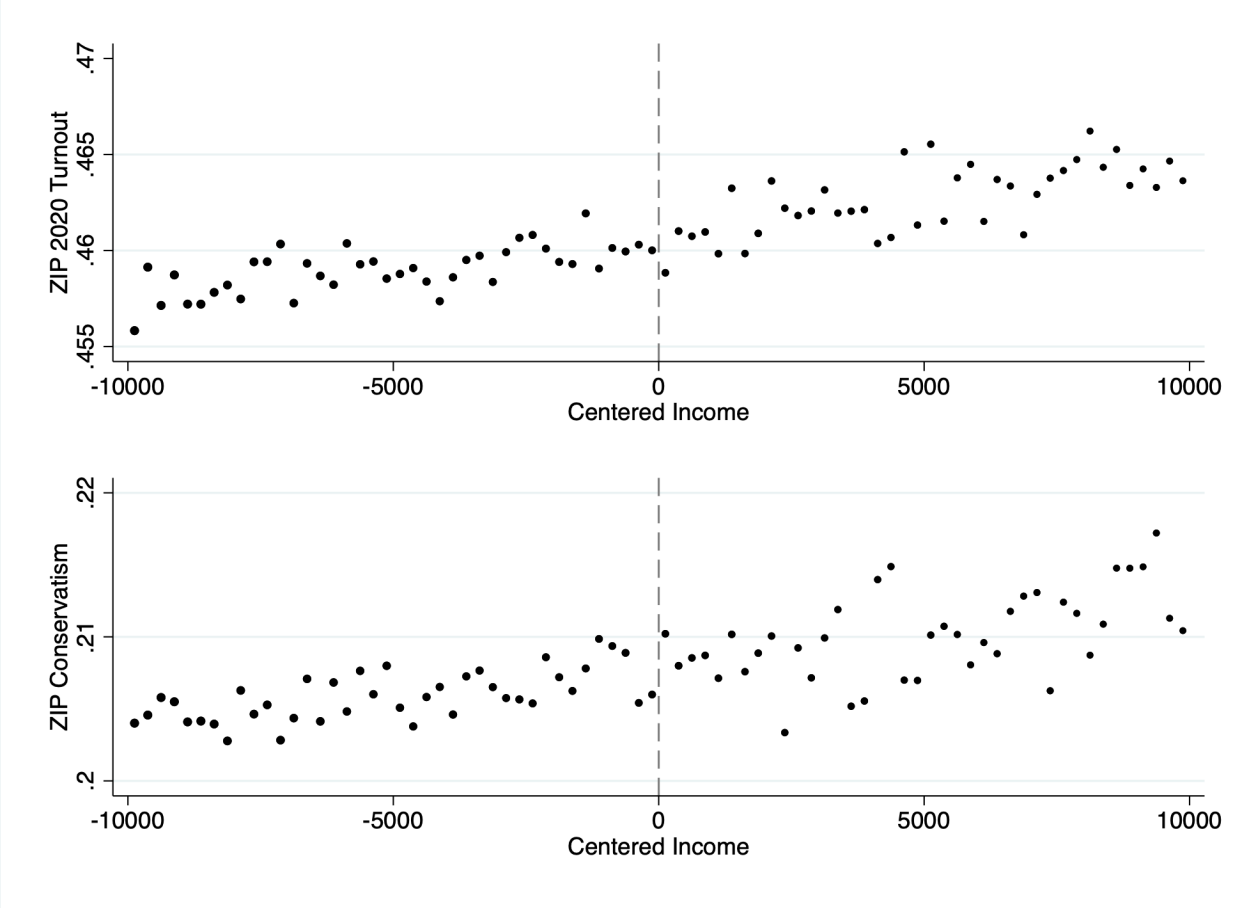

Figure A.4: Regression Discontinuity Plot for Covariates

Note: Centered income values are normalized to the income ceiling for Cal Grant A for a given individual.

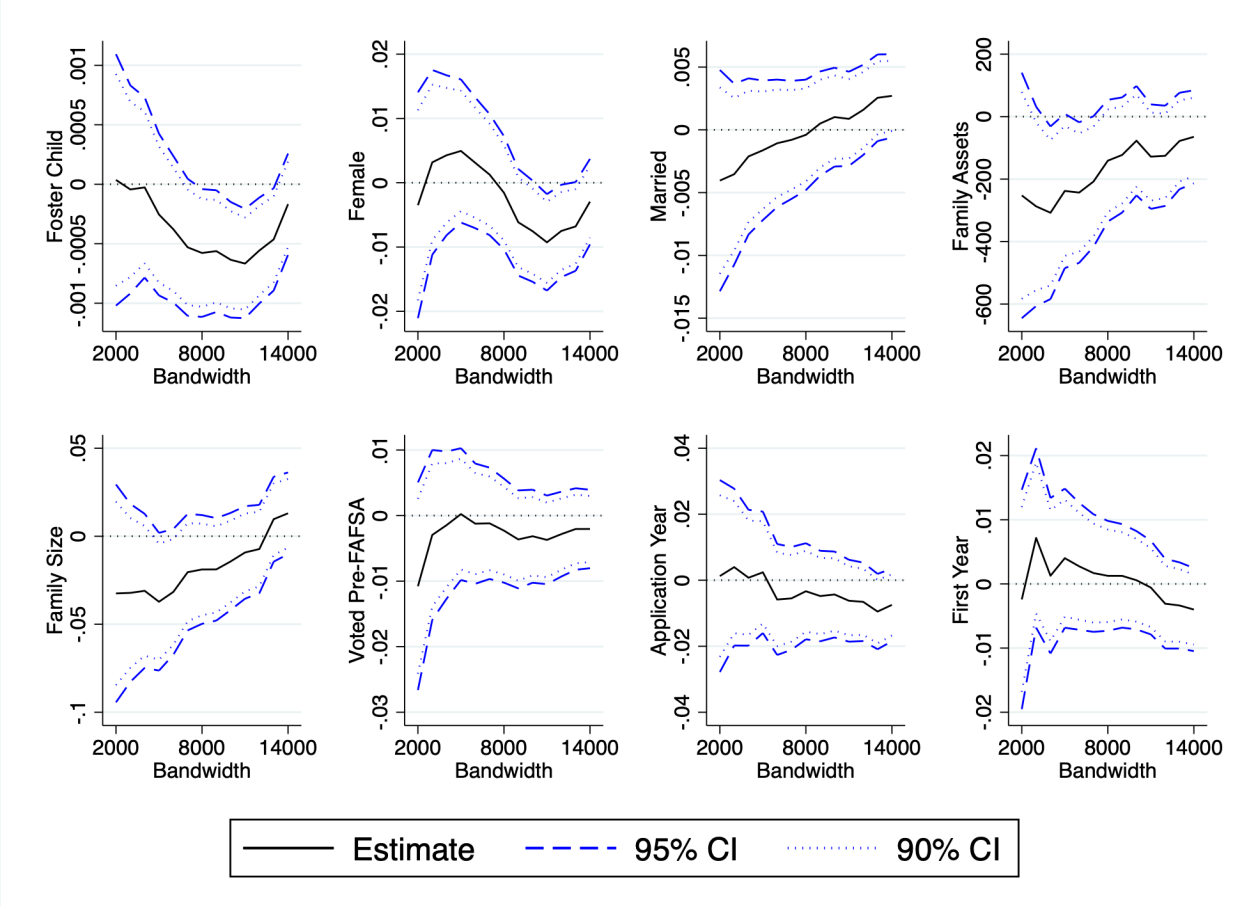

Figure A.5: Covariate Discontinuities by Bandwidth

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.

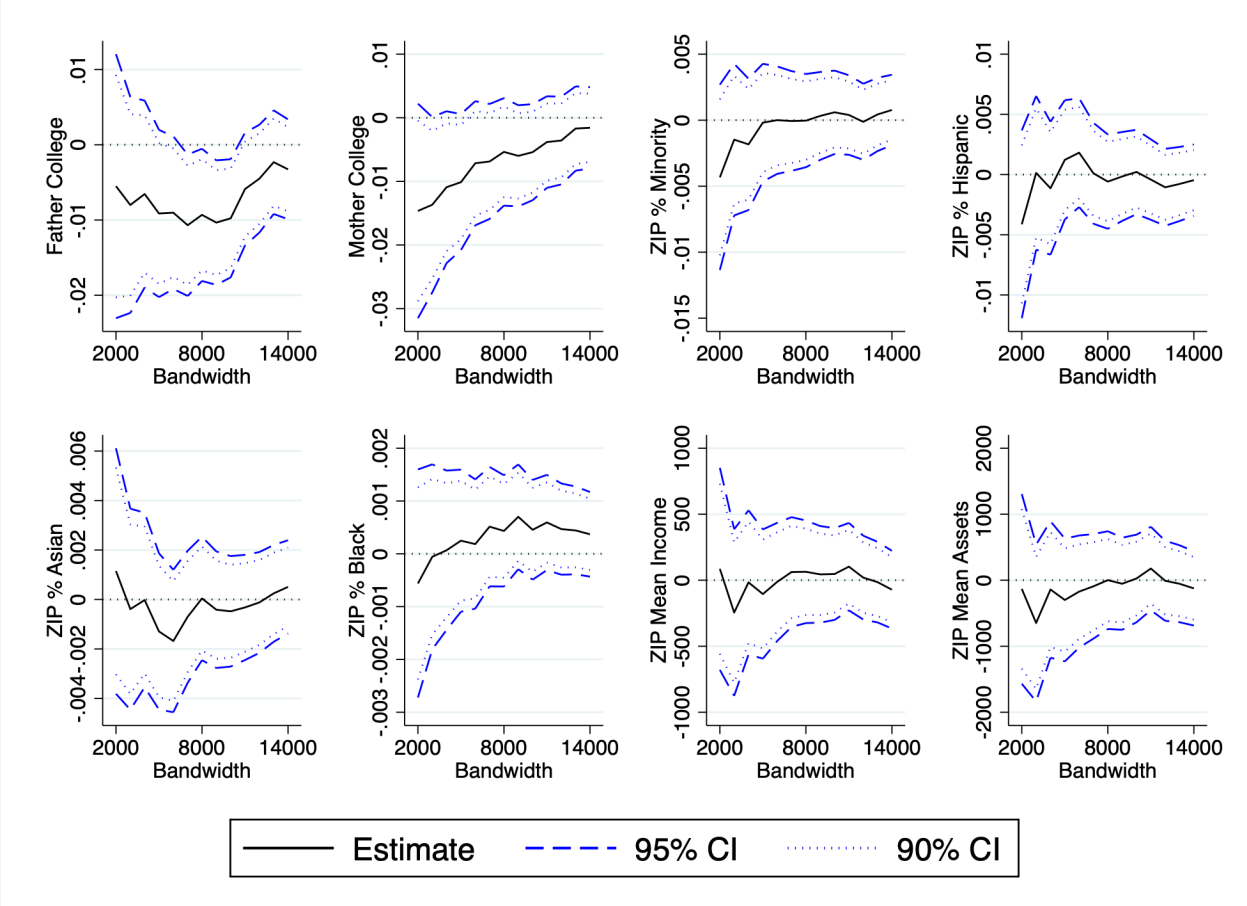

Figure A.6: Covariate Discontinuities by Bandwidth

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.

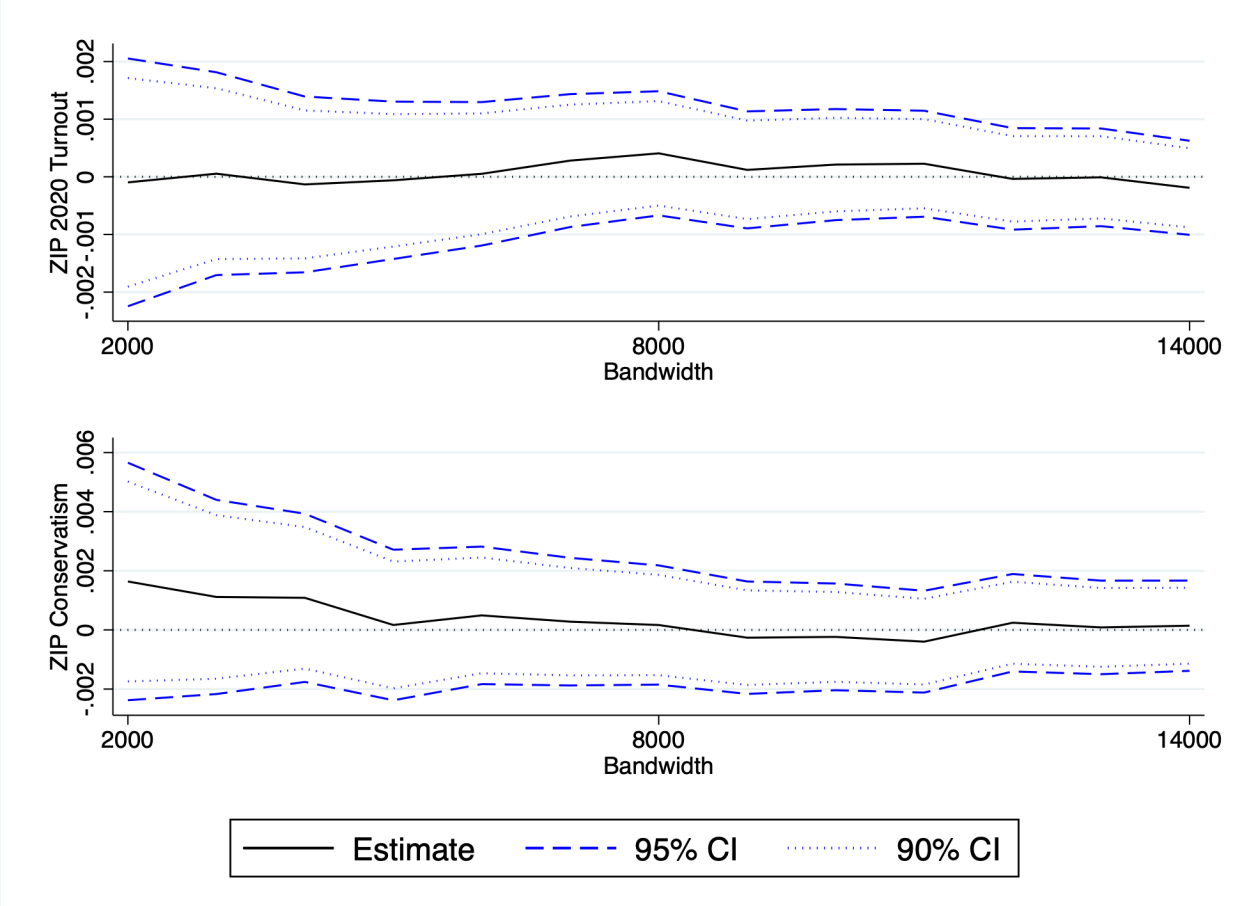

Figure A.7: Covariate Discontinuities by Bandwidth

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.

B Robustness Check Appendix

Table B.1: 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 2.

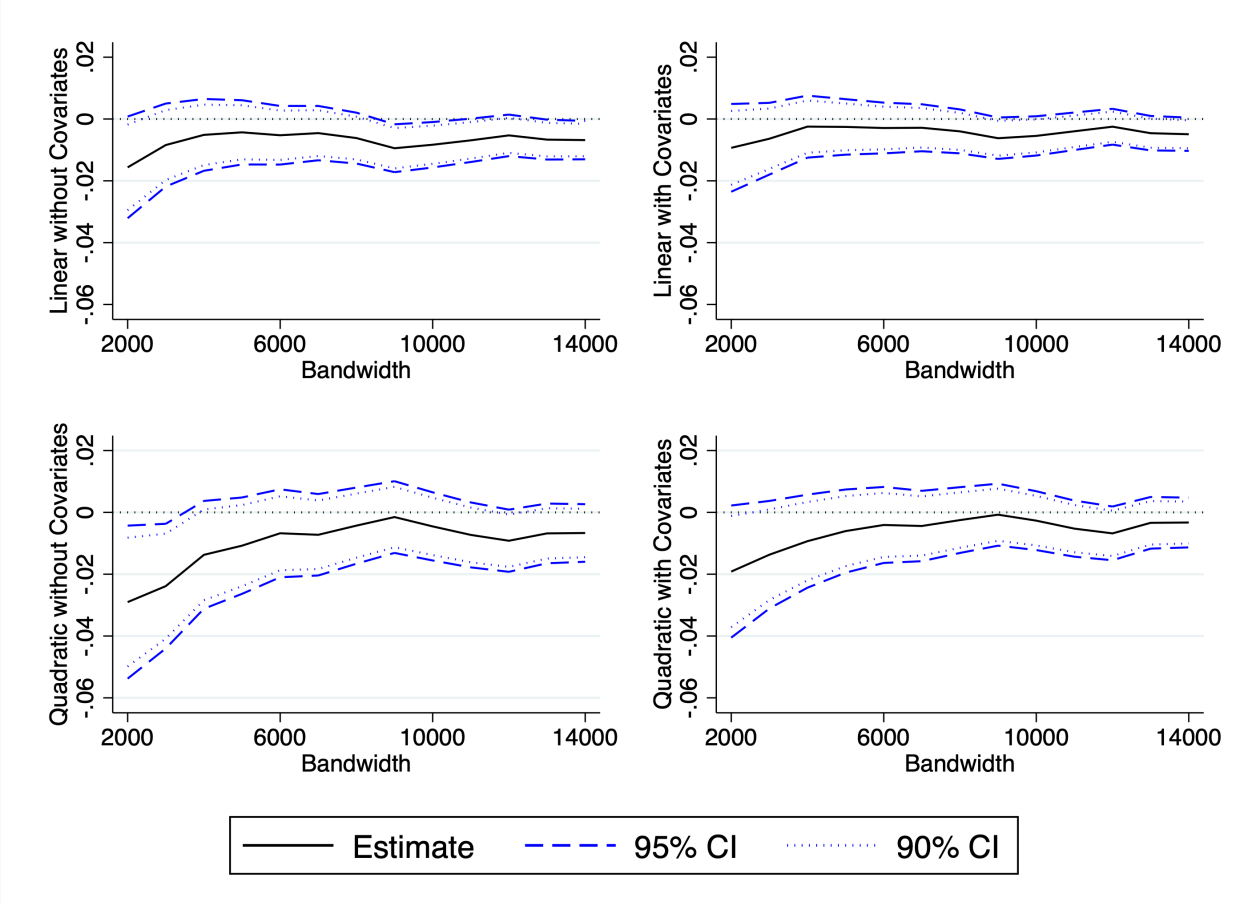

Figure B.1: Voter Registration in 2022 Bandwidth Robustness Tests

Note: The graphs reflect the point estimate, 95 percent confidence interval, and 90 percent confidence interval of the reduced form discontiuity in the outcome of interest. Each panel represents a different specification.

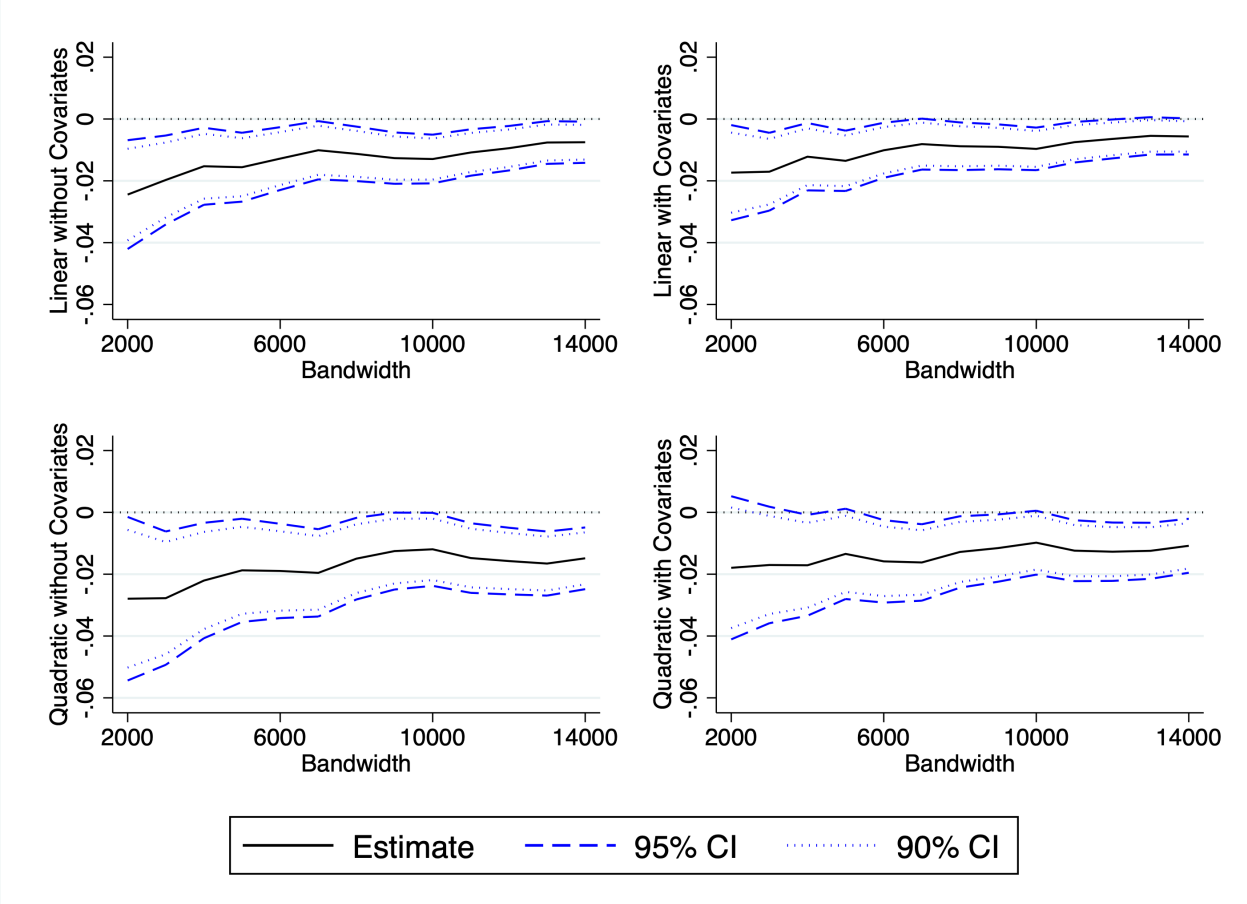

Figure B.2: 2020 General Election Turnout Bandwidth Robustness Tests

Note: The graphs reflect the point estimate, 95 percent confidence interval, and 90 percent confidence interval of the reduced form discontiuity in the outcome of interest. Each panel represents a different specification.

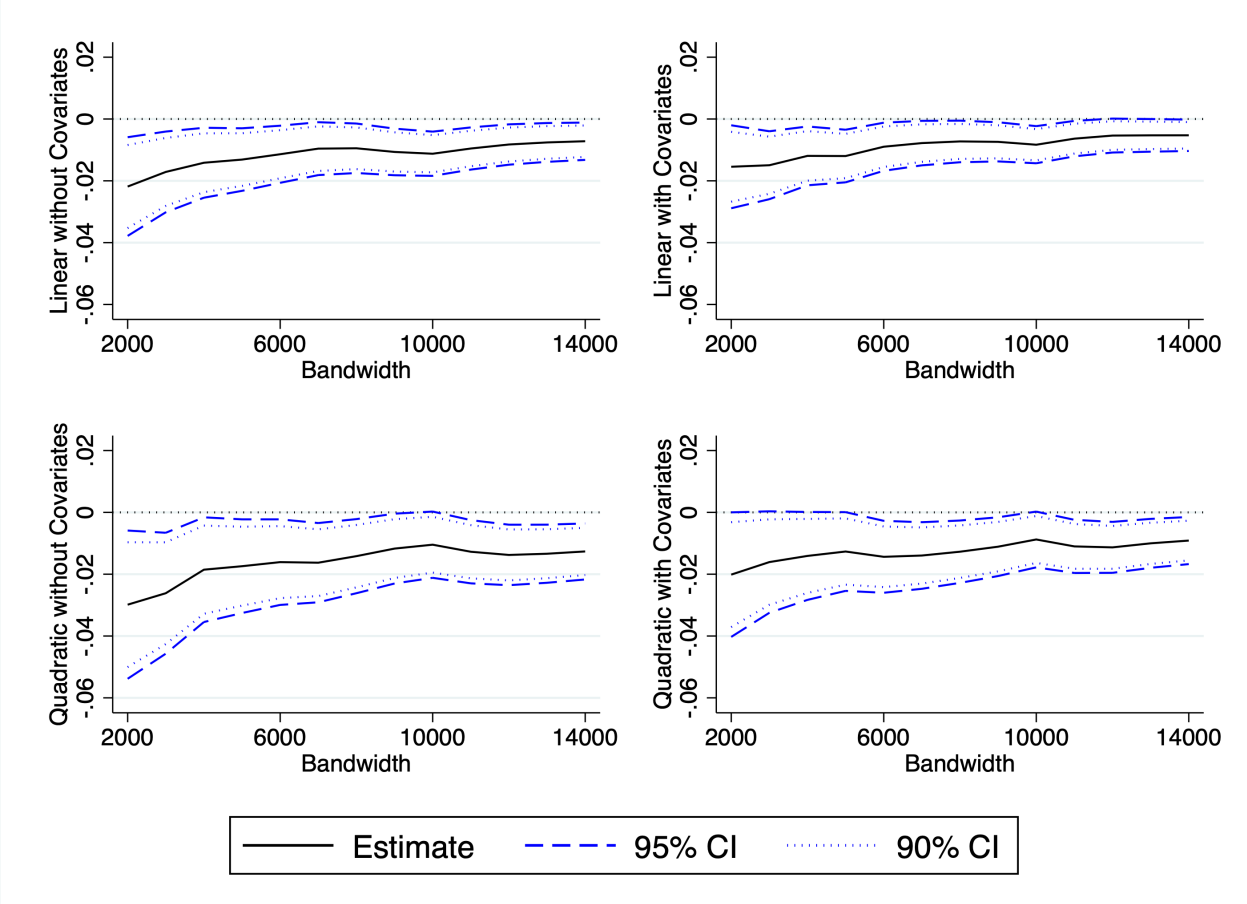

Figure B.3: General Election Turnout Bandwidth Robustness Tests

Note: The graphs reflect the point estimate, 95 percent confidence interval, and 90 percent confidence interval of the reduced form discontiuity in the outcome of interest. Each panel represents a different specification.

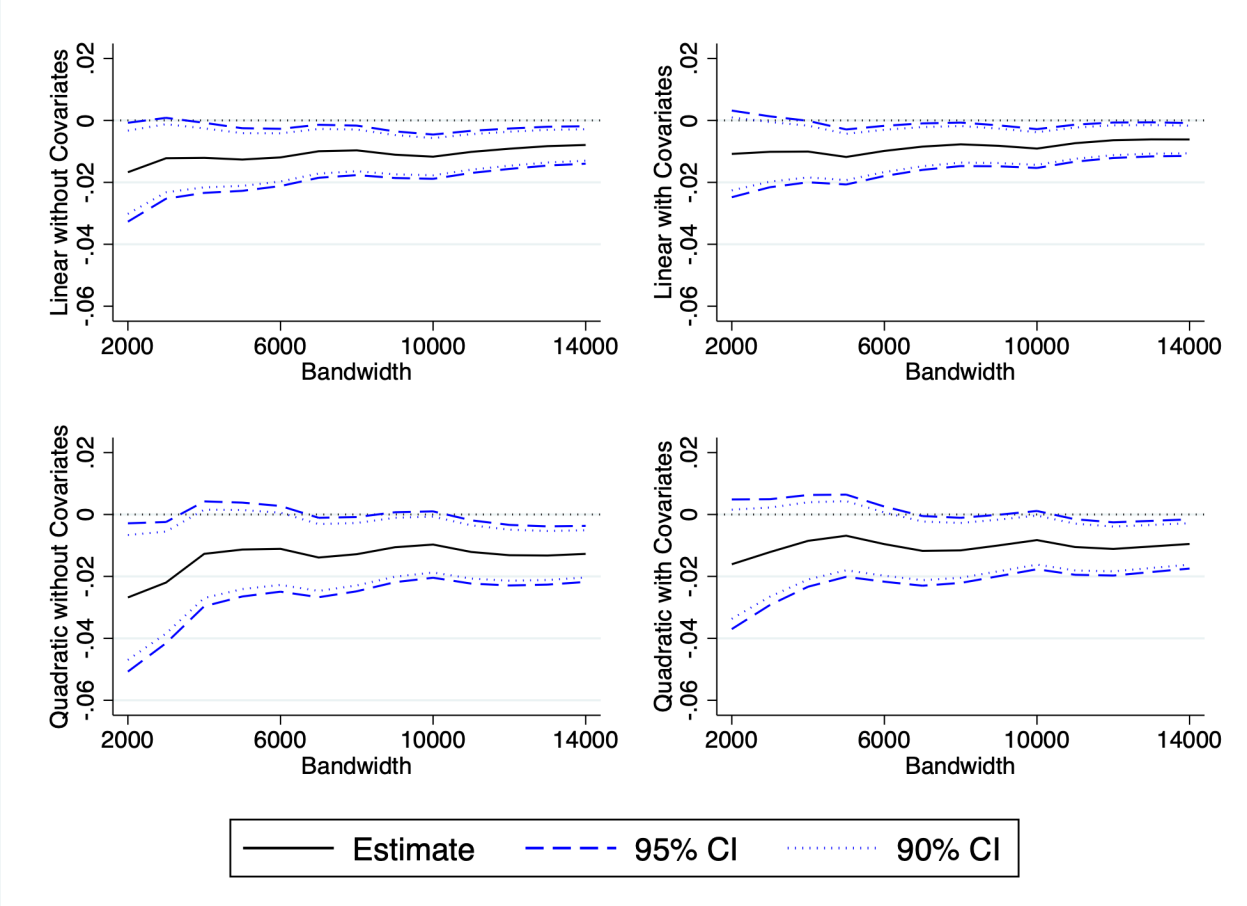

Figure B.4: Democratic or Independent Turnout Bandwidth Robustness Tests

Note: The graphs reflect the point estimate, 95 percent confidence interval, and 90 percent confidence interval of the reduced form discontiuity in the outcome of interest. Each panel represents a different specification.

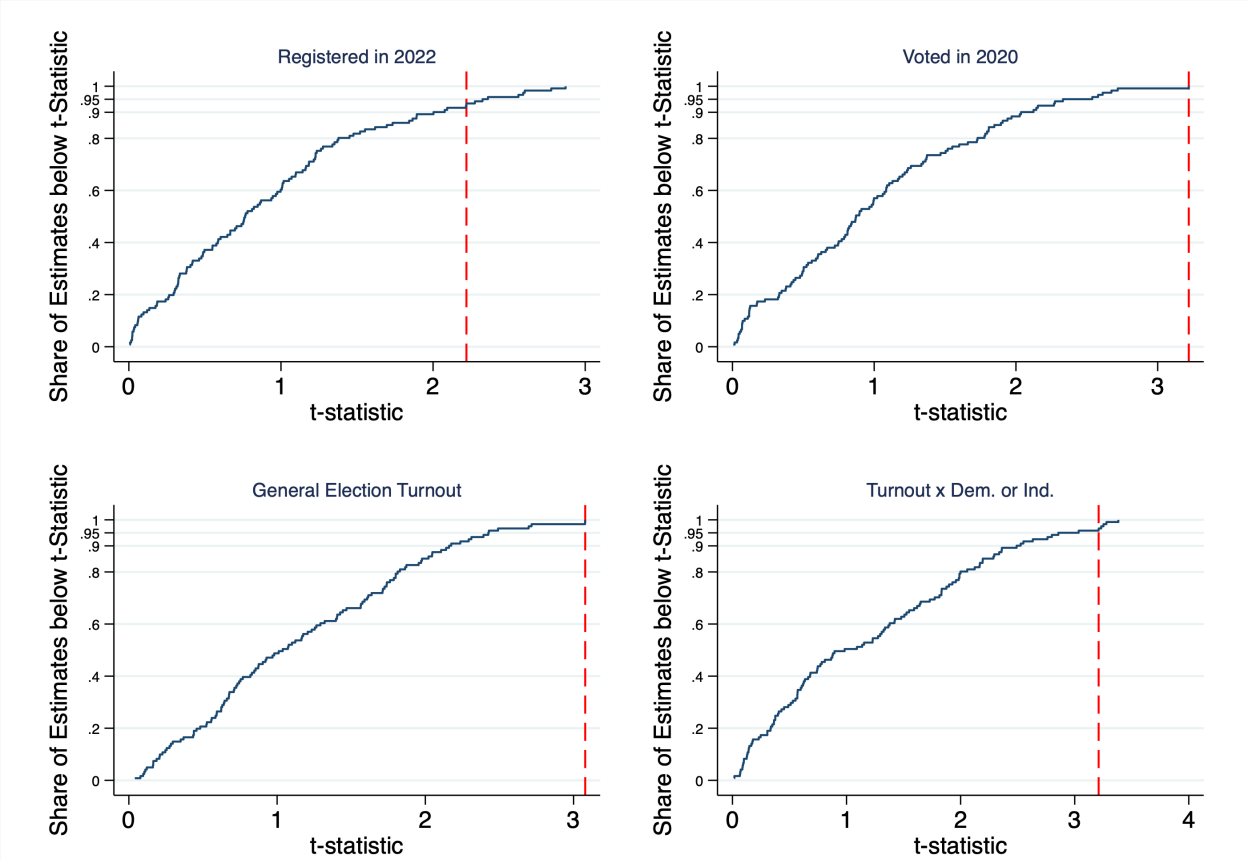

Figure B.5: Placebo Falsification Tests for Main Outcomes

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.

C Supplementary Information Appendix

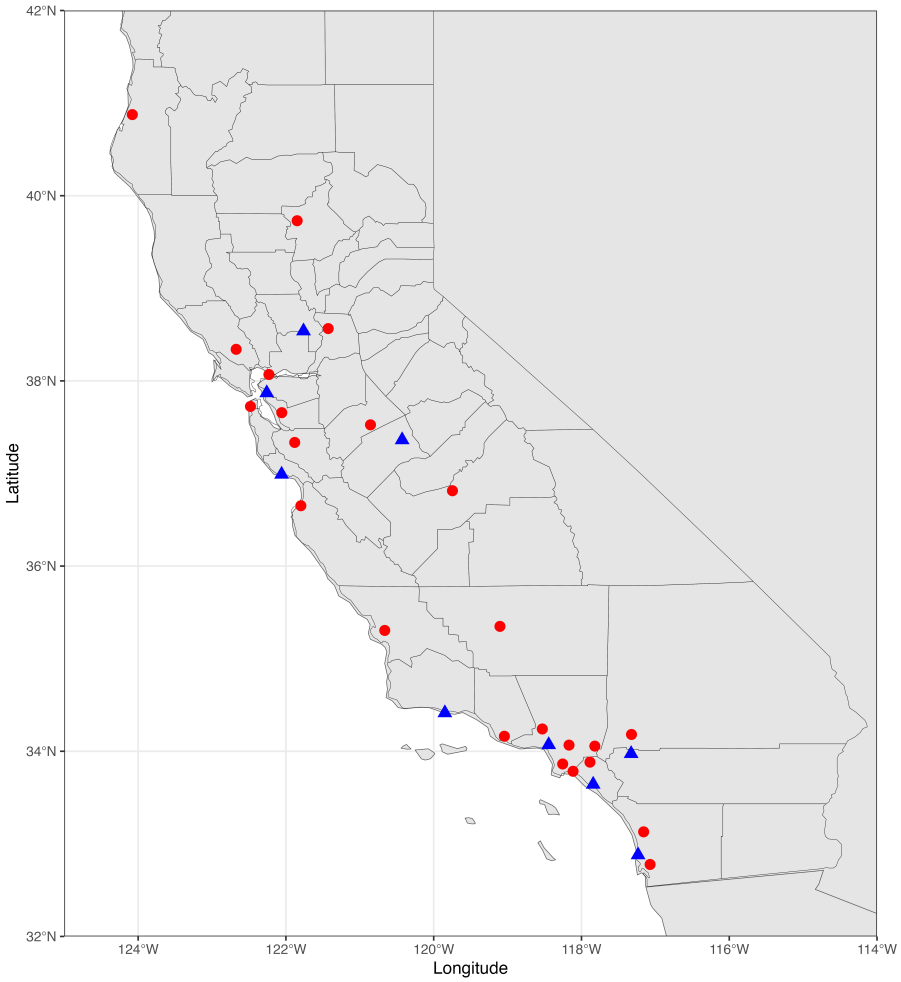

Figure C.1: Map of Public 4-Year Colleges in California

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 6 for a heat map of the Cal Grant's political impact by county.