Incentivizing the Green Vote?

The Role of California's Cap-and-Trade Investments on Electoral Outcomes

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Abstract

California's Cap-and-Trade program, launched in 2006, aimed to reduce greenhouse gas emissions while generating auction proceeds for the Greenhouse Gas Reduction Fund (GGRF). A portion of these funds is earmarked to fund grassroots-level environmental projects in disadvantaged communities. Funding eligibility is determined by the CalEnviroScreen (CES), which ranks communities on their level of environmental disadvantage. This paper investigates whether receiving these targeted environmental investments makes people more likely to vote in favor of pro-environmental (or "green") ballot measures. This paper contributes to the literature on environmental policy and political economy by examining whether targeted investments foster greater political engagement on environmental protection issues.

I would like to thank my thesis advisor, Dr. Réka Juhász for encouraging me, and motivating me to pursue novelty. I would like to thank my friends, who let me chat incessantly about this paper for months on end with few complaints. Lastly, I would like to thank my parents who raised me to value intellectual pursuit and rigor. **Replication package available on Github.**

1 Introduction

In 2006, California became the first U.S. state to launch the Cap-and-Trade program, in an effort to cost effectively achieve its emission reduction goals (California Air Resources Board, 2012).[1] At the outset, the program faced staunch opposition from environmental justice (EJ) organizations who demanded that monetary proceeds from Cap-and-Trade auctions should be directed towards disadvantaged communities who face disproportionate pollution and climate change burdens. These burdened communities consist of people located near industrial pollution zones, highways, and toxic waste disposal sites, often belonging to low-income and racialized groups (predominantly Black and Hispanic communities).

In response to EJ opposition to Cap-and-Trade fund usage, California State Bill (SB) 535 was signed into law in 2012, stipulating that at least 25% of the proceeds from Cap-and-Trade auctions (pooled in the Greenhouse Gas Reduction Fund) go toward funding projects that benefit disadvantaged and environmentally burdened communities, with at least 10% going to projects located within these communities (Legislature, 2014).

To decide which communities should benefit from the Greenhouse Gas Reduction Fund (GGRF), California's Environmental Protection Agency (CalEPA) built the CalEnviroScreen (CES), a screening tool to rate the universe of California's census tracts' based on their extent of "disadvantage", using a host of indicators that measure vulnerability to environmental hazards (California Office of Environmental Health Hazard Assessment (OEHHA), 2025). The indicators are based on geographic, socioeconomic, public health, and environmental

[1] The Cap-and-Trade Regulation establishes a declining limit on major sources of GHG emissions throughout California, and it creates a powerful economic incentive for significant investment in cleaner, more efficient technologies. The Program applies to emissions that cover approximately 80 percent of the State's GHG emissions. CARB creates allowances equal to the total amount of permissible emissions (i.e., the "cap"). One allowance equals one metric ton of carbon dioxide equivalent emissions (using the 100-year global warming potential). Each year, fewer allowances are created and the annual cap declines. An increasing annual auction reserve (or floor) price for allowances and the reduction in annual allowances creates a steady and sustained carbon price signal to prompt action to reduce GHG emissions. All covered entities in the Cap-and-Trade Program are still subject to existing air quality permit limits for criteria and toxic air pollutants (CARB).

hazard criteria, and each census tract is assigned a weighted score determining their level of environmental burden, which then determines their funding status under the program. A higher score on the CES indicates a higher level of environmental burden. CalEPA designates the top 25 percent scoring census tracts as disadvantaged communities eligible for GGRF funding – this is a sharp cut-off score, and all census tracts above the cut-off become eligible to apply for and receive funds under the program.

Since the program's inception in 2014, $8.1 billion of the GGRF corpus have been allocated to projects in disadvantaged communities, covering roughly 10 million California residents. The money is dispersed into disadvantaged communities in the form of community-led projects (e.g., community car sharing, composting), household-serving voucher programs (e.g., rebates for the purchase of cleaner cars, woodstove replacements), and large-scale infrastructure projects (e.g., affordable housing, transit). Funding via the GGRF is available year-round. Businesses, farmers, individuals, local governments, non-profits, public schools and school districts, transit agencies, tribal governments and universities are all eligible to apply for small grants that enable environmental resilience through affordable housing, transit, electric vehicle rebates, planning and technical assistance, land conservation and restoration, clean energy, climate smart agriculture, composting and recycling, and urban greening (California Air Resources Board, 2024a).

The GGRF funding program presents a unique setting to study environmental programs. First, it provides a wealth of information on grassroots level environmental initiatives, that have received a substantial amount of funding over time: eligible low-income, environmentally vulnerable communities have to self-select into applying for funding that may only be used for a designated environmental goal. The quirk of sharp RD eligibility at the census tract level near the cut-off score, combined with the self-selection into targeted funding programs form the setting for identification, and motivate the question: Are communities that have been beneficiaries of GGRH funding more likely to vote for pro-environmental protection legislation on state ballot measures? Here, pro-environmental voting is defined as votes cast in favor of California ballot measures that aim to preserve or protect the environment, reduce pollution or enforce taxes/issue bonds to generate revenue for environmental projects and infrastructure.

Environmental inequality and EJ have long documented the disproportionate impact of pollution and environmental hazards on the poor. The GGRF funding scheme was the first of its kind to let disadvantaged people self-select into receiving targeted funding to become environmentally resilient. While programs like Superfund cleanups focus on eradicating toxic waste from unduly exposed communities, literature that focuses on the impact of grassroots level funding is sparse. The lingering question remains: Can we design programs that generate popular support for environmental protection legislation? This project is motivated by the larger desire to understand what policies can bring people who face environmental burdens into the forefront of environmental policy design and popular support, and make environment an all-voter issue instead of a rich and educated voter issue.

California's Cap-and-Trade program has been studied in the past for its emissions reduction impact (Fowlie et al., 2012), pollution rights mechanisms (Fowlie and Perloff, 2013) and price effects (Borenstein et al., 2019). The literature studying the impact of the program's GGRF fund allocation, specifically in disadvantaged communities remains confined to sociology, epidemiology, and medicine.[2] This paper is motivated by a desire to fill that gap in the literature, while attempting to touch on a broader policy question: "What policies garner public favor for pro-environmental legislation?" by examining the effect that GGRH funding level treatments have on voter behavior.

It is important to note that political behavior is motivated by a host of complex factors, and pro-environmental voting outcomes at the county level are hard to attribute to just targeted environmental funding. This paper attempts to use California's census tract level RDD to

[2]Non-causal studies.

construct an instrument that predicts the amount of funding a county receives under the program, and measure the impact of funding treatment on voting in favor of environmental legislation.

This paper finds, in a surprising turn of events, that receiving targeted environmental funding makes voters less likely to vote in favor of upholding a plastic bag ban, less likely to vote in favor of upholding the fuel tax, and less likely to vote for rainwater capture systems being excluded from property taxes. Other results on pro-environmental voting are also negative, but insignificant.

2 Literature Review

Much of the literature on pro-environmental voting comes from Political Science. Mannoni (2025) defines pro-environmental voting as an instance of behavior that rests at the intersection of voting behavior and pro-environmental behavior and consists of voting for a party (or candidate) that stands for and significantly emphasizes environmental protection, regardless of whether that is the reason why the party was voted in. Interestingly, Mannoni finds that there's often a gap between concern and actual behavior – many who care about the environment don't vote accordingly. She also finds that women, millenials, Gen-Z, highly educated, and left-leaning people are more likely to vote pro-environment. Income is not a determinant in her findings. My paper borrows this definition of pro-environment voting (by looking at voting outcomes on specific *propositions* instead of the party elected), and includes similar demographic factors to check if those trends hold in my setting.

In economic literature, Nelson (2002) studies Green voting in the U.S. senate and finds that Democrats tend to vote more consistently in favor of environmental legislation than Republicans. The study is at the senator level and finds that green voting tends to be highly partisan and ideology driven. Since my paper looks at county level voter outcomes, it is hard to account for past ideology. California is historically a blue state for the most part, so this paper restricts itself to focusing on how funded populations react to environmental measures. This design is limited by a lack of data on county level ideological trends: further research (and this paper's design) would benefit greatly from being able to account for historical voter patterns at the county level, that can control for ideology trends.

The literature studying California's Cap-and-Trade program specifically is sparse. Prominent environmental program studies focus on the Superfund cleanup program in the United States, which invests clean-up money into hazardous waste sites across the country. Viscusi and Hamilton (1999) find that cleanup sites in areas with higher voter turnout, more environmental group membership, and pro-environment congressional voting records tended to receive more stringent cleanups and greater expenditure per cancer case averted. My paper looks at the opposite direction: what happens to pro-environmental voting in places where people receive more funding? My paper seeks to augment the literature on targeted environmental funding and pro-environmental voting by looking at the effects of Cap-and-Trade investments on voting in California counties.

As far as studies on *green* voting go, the economic literature is sparse, and virtually none of it addresses the impact of funding programs in environmentally disadvantaged communities. This paper seeks to fill a gap in the green voting literature by studying the impact of a targeted environmental funding program on pro-environmental voting outcomes in a novel setting, through the construction of an instrument that leverages a census-tract level sharp RD design to predict county-level outcomes.

3 Data

This paper uses data from several sources, all of which are accessible through the State of California and the United States Census Bureau. This project would not have been possible without California's commitment to open source data and reporting standards. This section is organized into four parts, each detailing the key data sources used by this paper and the transformations applied to them.

CalEnviroScreen The exogeneity of my instrument relies on CalEPAs census tract level scoring data called the CalEnviroScreen (CES). Each census tract is assigned a weighted score that indicates how environmentally disadvantaged the population residing in the census tract is.

The CES score is calculated as follows: each census tract is assigned a score on the following 19 indicators – Ozone, Particulate Matter 2.5, Diesel Particulate Matter, Drinking Water, Lead, Pesticides, Toxic Release, Traffic, Cleanup Sites, Groundwater Threats, Hazardous Waste, Important Water Bodies, Solid Waste, Pollution Burden, Asthma, Low Birth Weight, Cardiovascular Disease, Education, Linguistic Isolation, Poverty, Unemployment, Housing Burden. A higher score implies that the census tract suffers from a higher degree of environmental burden than lower scoring census tracts.

CalEPA groups the universe of 8057 census tracts into 5-percentile bins, and the top 25 percentile scoring tracts become eligible to receive funding under the GGRF program. I use CES versions 2.0 and 3.0 to determine the cut-off score at the 25th percentile threshold for each version, and use it to count the number of census tracts in each California county just above ("treated") and just below ("control") the cut-off score.

Figure 1b shows year-on-year variation in the calculated instrument, where each data point represents a California county. I leverage Regression Discontinuity Design assumptions to calculate a proportion of treated census tracts in each county, and use it as an instrument because tracts above and below are similar, and funding eligibility near the cut-off is as good as randomly assigned. I claim exogeneity based on past research by Huynh et al. (2024), who establish that the sharp cut-off at the 25th percentile is a valid RDD, and receiving "disadvantaged" designation leads to a 104% increase in funding at the census tract level.

The claim of exogeneity is further supplemented by the fact that all 19 indicators cannot be manipulated by census tracts in advance to become eligible for funding, and Huynh et al. (2024) find no evidence of a mass around the cut-off score. Note that the CES score is calculated using demographic indicators that play an important role in determining voting outcomes, namely: Education, Linguistic Isolation, Poverty, Unemployment, Housing Burden – this lends further credibility to my design, because census tracts just above the cut-off score should, in absence of GGRF funding, have similar demographic profiles.

I download CES scores from CalEPAs website as an excel sheet with 19 indicator level scores and final weighted scores for the universe of 8057 California census tracts. The CES framework is periodically revised to make the algorithm better suited to community needs. For the purpose of this paper, we use version 2.0 (released in 2014), and version 3.0 (released in 2018). The 25th percentile score for each iteration is different, so I build two versions of the instrument for each county – one based on the 2.0 and 3.0 scores respectively.

California Climate Investments The universe of GGRF funded projects since 2015 is available on the California Climate Investments (CCI) website. Each project is uniquely identified by a Project ID, and specifies the census tract where it was implemented. All project descriptions are at the census tract granularity, and contain details for the program name, agency, type of project, amount allocated via the GGRF corpus of funds, and date of completion.

I merge the CCI dataset with the CalEnviroScreen scoring data. Census tracts from the CES data that do not merge with the CCI universe ("never treated") are assigned a value of "0" funding – these are true zeroes. Note that many census tracts are funded repeatedly over the sample period, treated with varying amounts under each project. The CCI dataset contains projects funded under both version 2.0 and 3.0 of the CES data – based on the recorded version, I merge the CCI data with the CES calculated instrument accordingly to account for the change in sharp cut-off scores by scoring iteration.

I use the sharp CES cut-off to calculate the proportion of "treated" census tracts in each county-year pair. I then collapse the CCI dataset at the county level to calculate total GGRF funding received for every county-year pair, and also a cumulative funding measure. I choose cumulative funding to be my endogenous regressor because census tracts are treated multiple times over the years, and there are likely to be persistent impacts from each year of program funding that roll over into future years. As an illustrative example, this means that 2022 outcomes are regressed on the cumulative GGRF funding received by each county from 2015 to 2022. I log-transform both total GGRF funding and cumulative GGRF funding to scale and deal with outliers.

Figure 1a shows a histogram of the cumulative funding measure used in all main estimation equations for this paper. Figures 2 to 9 show the year-over-year variation in total county level funding under the GGRF program.

Secretary of State Election Outcomes The California Secretary of State (SoS) publishes county-wise voting data for general and statewide elections from each election year. The data is reported as an excel sheet which lists the name and number of the propositions up for vote on the ballot, and the number of "Yes" and "No" votes cast by each county. I use this to calculate the proportion of votes cast in favor of each proposition, which serves as my outcome variable.

I use election outcome data from 2016, 2018, and 2022, and focus on *green*, or pro-environmental ballot measures in each year. These propositions are: *Proposition 65* – Carryout Bag Charges, *Proposition 67* – Ban on Single-use Plastic Bags, *Proposition 68* – Natural Resources Bond, *Proposition 72* – Property Tax: New Construction: Rain-Capture, *Proposi-*

tion 3 - Bond for Water and Environmental Projects, *Proposition 6* - Repeals 2017's fuel tax and vehicle fee increases, *Proposition 30* - Tax to Fund ZEV/Wildfire Programs. The year 2020 is excluded because it does not include any green measures on the ballot.

I also use outcome data on green propositions from years 2012 and 2014 as balance checks to make sure that the proportion of treated census tracts in pre-GGRF funding years does not predict environmental voting outcomes. These propositions are: *Proposition 1* - Issues $7.12 billion in bonds for California's water system, *Proposition 39* - Requires out-of-state businesses to "support projects intended to improve energy efficiency and expand the use of alternative energy".

American Community Survey All data are downloaded from the U.S. Census Bureau and have no missing values/transformations that need to be addressed. I use the racial composition of each county for this paper to calculate the proportion on non-white people in each county-year pair – I exclude total Asians from the proportion of non-white people.[3] For all census data, I use 5-Year ACS estimates from 2013 to 2023. Note that the year 2020 does not report ACS estimates, and all 2020 variables are merged with 2019 census data.

4 Estimation Strategy

This paper employs a 2SLS strategy to measure the impact of GGRF funding on voting outcomes. This section is organized into three parts: Sub-section 4.1 walks through instrument construction, 4.2 defends instrument exogeneity and provides the identification assumptions, and 4.3 outlines the empirical design that is tested in this paper.

[3] I restrict my measure of non-white to Hispanic, Black and Indigenous people, who face higher levels of poverty and environmental burden as opposed to California Asians, who are middle to high income.

4.1 Instrument

The instrument relies on a census tract level sharp regression discontinuity, to calculate an exogenous measure, "proportion of treated census tracts". First, I determine the sharp cut-off score that makes census tracts eligible for GGRF funding. For CES version 2.0, the cut-off score is 32.66, and for version 3.0 the cut-off score is 38.68 – I use a Kalynaryaman-Imbens bandwidth of 3.86, as supported by the analysis in Huynh et al. (2024), and designate all tracts as "treated" or "control" based on the following:

For each tract with a CES score p, given a cut-off score c,

$$\mathbf{T} = 1\{p \in (c, c + 3.86]\} \tag{1}$$

$$\mathbf{C} = 1\{p \in [c - 3.86, c]\} \tag{2}$$

This design leverages the sharp RD assumptions, and assumes that control (not eligible for targeted GGRF funding) and treated (eligible for targeted GGRF funding) have virtually identical levels of environmental burden exposure (and do not vary significantly on the 19 indicators that determine CES scoring. This RD validity at the indicator level is rigorously established by Huynh et al. (2024)). I then sum the total number of treated and control census tracts in each county, for each year and calculate the following instrument:

$$Z_{i,t} = \frac{\sum^k \mathbf{T}}{\sum^k \mathbf{T} + \sum^{N-k} \mathbf{C}} \tag{3}$$

Where N is the total number of census tracts in each California county, and k is the total number of census tracts with a CES score in $(c, c+3.86]$ (the number of treated census tracts changes over time). [4]

Within-year variation in the instrument comes from the fact that not all counties are treated

[4] There is no re-calculation of census tract boundaries in my sample period, so the number of census tracts in each county stays constant from 2015 to 2023.

equally – some have a higher proportion of disadvantaged census tracts than others do. Figure 1b shows that there is also across-year variation in the proportion of treated census tracts for each county. Why does this happen? Note that there are two versions of the CES for which we compute the instrument value, so it is slightly counter-intuitive that the instrument would vary across years *and* scoring iterations both. This is a CCI quirk: if a project (in a census tract) is assigned to be funded under CES 2.0 anywhere between 2014 to 2018, then all subsequent funding is done under CES 2.0, even if the new rounds of funding are disbursed after scores for 3.0 is released.

Here is an illustrative example to make things clearer: Census Tract 6000657 has a CES 2.0 score in range $(c, c + 3.86]$, and is funded for a project in 2014. It appears in the CCI panel dataset in both 2019 and 2023 under the same project (after 3.0 is released), but it is still funded under version 2.0 for consistency. This means that in each year of data after 2018 (3.0 is released), projects are funded under both versions 2.0 and 3.0 of the CalEnviroScreen, so the instrument changes based on the split between 2.0 and 3.0 projects.[5]

There is another source of across-year variation in the instrument, and it is a bit more data-involved. The CES data scores the universe of census tracts in the state (8057), and the CCI dataset is a panel that runs from 2015 to 2023, but not all census tracts get funded. I merge the CES data with CCI and every census tract that doesn't merge with the CCI data is assigned a total funding value of "0" – these zeroes vary over the years in every county (there are tracts that were un-funded in 2015, that are funded in 2016), which causes the across year variation.

As an example, we zoom in on the control tracts in Alameda County: In 2014 there are 4 census tracts in the control score range that are zeroes, but in 2015 one of them gets funded, and this changes the calculated value of the instrument. This is best seen in Figure 1b and Figures 2 to 9, but the intuition is simple: program coverage changes every year, as more/less

[5]Census tract 6000657 does not exist. It is merely illustrative.

census tracts cycle in and out of GGRF funding.

My paper exploits both within and across-year variation in the proportion of treated census tracts in each county to predict the total amount of GGRF funding received by the county in each year.

Note that for some counties in each year of data, the instrument evaluates to missing "." – this happens when none of the census tracts in the county fall within the designated treated and control score ranges. I drop these counties from my sample, so each year has less than the universe of California counties (58). Secondly, for some counties in each year of data, the instrument evaluates to "0" – this happens when the county has a non-zero number of census tracts in the control range, but 0 tracts in the treatment range. I keep these in the dataset because they are true zeroes.

4.2 Identification Assumptions

This section argues that the constructed instrument measure satisfies the exclusion restriction. I present three reasons for why the instrument is exogenous to the proportion of votes cast in favor of a green ballot.

First, the construction of $Z_{i,t}$ relies on a sharp regression discontinuity design at the census tract level using CES scores. The RD uses a local window around a pre-determined cutoff c, so the treated and control tracts are as-good-as-randomly assigned within that bandwidth. Then by construction, census tracts just above and just below the threshold do not differ systematically in observed or unobserved characteristics relevant to treatment assignment or outcomes, except for their treatment status. As stated previously in this paper, Huynh et al. (2024) validate this assumption across all 19 environmental and socio-economic indicators that enter the CES score, supporting the local randomization assumption.

Second, the proportion of treated tracts at the county-year level aggregates only this quasirandom variation in treatment eligibility. Since treatment status is assigned at the tract level based on a deterministic cut-off score, and only local variation around that threshold is used to compute the proportion, the aggregate measure $Z_{i,t}$ borrows this exogeneity. The instrument then plausibly isolates exogenous variation in treatment intensity across county-year pairs.

Third, funding assignment through the California Climate Investments (CCI) reinforces the independence of the instrument from local economic or political factors because it uses the CES. The CCI assigns funding under fixed CES versions based on initial funding dates, not updated census tract-level conditions.

As outlined in 3, once a tract is treated under CES 2.0, all subsequent appearances in the funding dataset use CES 2.0, regardless of later updates to the CES version. This CCI quirk introduces across-year variation in treatment status that is mechanically driven by past assignment rules rather than updated demographic outcomes, further bolstering the argument that the instrument is independent of unobserved determinants for voter behavior at the county level. A formal proof of this assumption is provided in the Appendix A.

4.3 Empirical Design

Each of the following paragraphs breaks down the estimation design used in this paper.

First Stage:

$$F_{i,t} = \gamma_0 + \gamma_1 Z_{i,t} + \gamma_2 C_{i,t} + \upsilon_{i,t} \tag{4}$$

Where $F_{i,t}$ is Log(Cumulative Funding) in each county-year pair, $Z_{i,t}$ is the proportion of treated census tracts in each county-year pair, and $C_{i,t}$ is the proportion of non-white people in each county in year t, calculated based on ACS data. [6]

[6] I sum the total non-white population (excluding Asians) in each county and divide it by the total county population in that year.

Note that the proportion of non-white people is the only demographic control used because the CalEnviroScreen score uses a host of relevant demographic characteristics (Education, Linguistic Isolation, Poverty, Unemployment, Housing Burden) to calculate scores for census tracts, but excludes race due to political push-back. Demographic characteristics are included because environmental burden is often heavily correlated with poverty and low levels of education. The direction of causality is an open question, and the literature is inconclusive on whether these demographics are what cause "Tiebout-like" sorting into disadvantaged and polluted neighborhoods (Banzhaf and Walsh, 2008; Greenstone and Gallagher, 2008). Considering this, I do not include obvious controls that are absorbed in the CES to avoid collinearity, and only include race, which is correlated with voter outcomes and amount of funding disbursed (poorer and polluted areas are heavily funded under the program, and are also comprised heavily of Black/Hispanic populations).

Reduced Form and 2SLS:

$$Y_{i,t} = \alpha_0 + \alpha_1 Z_{i,t} + \epsilon_{i,t} \tag{5}$$

$$Y_{i,t} = \beta_0 + \beta_1 \hat{F}_{i,t} + \varepsilon_{i,t} \tag{6}$$

Where $Y_{i,t}$, is the proportion of votes in favor of an environmental proposition on the ballot in year t cast by county i, $Z_{i,t}$ is the instrument, and $\hat{F}_{i,t}$ is the predicted value of log cumulative funding from Equation 4. All equations are re-estimated with proportion of non-white people as a control for robustness.

Note that the proportion of non-white people is calculated from *census* data for the whole county population, as opposed to the unobserved racial composition of voters from each county. Since voter turnout is not 100 percent for each county, racial composition calculated on the total population does not align perfectly with the voter turnout population base. It is highly unlikely that a county is half non-white, and the voter base from that county that turns up to vote is also half non-white. Due to this measurement issue, all estimates with the control added are left as supplementary results in the robustness section.

Note: This design does not use year or county level fixed effects to preserve the variation in the instrument that is crucial to predicting funding amounts. See 1b for a graphical aid on how the instrument varies over time and across counties (each data point on the yearly plots represents one California county).

5 Results

5.1 First Stage

Table 1 shows the first stage, which regresses the log cumulative funding (total funding received by county i *until* year t) on the calculated instrument. The first stage is highly significant, and robust to inclusion of race as a control, though the significance falls.

The proportion of treated census tracts does have predictive power on the amount of funding that is dispersed in each county. I use cumulative funding as opposed to yearly funding because the effects of each round of GGRF funding should persist over the years.

Log(Cumulative Funding)	(1)	(2)
Z	1.920***	1.115*
	(0.647)	(0.636)
Proportion Non-white		5.941***
		(0.886)
Constant	16.45	14.68
Observations	268	267
R^2	0.081	0.315
Standard errors clustered at the county level.		

Table 1: First Stage Estimates

5.2 Reduced Form

We regress the proportion of "Yes" votes for each environmental propositions on each year's ballot (2016, 2018 and 2022) on the instrument. All estimates except proposition #3 and #65 are significant. The names of all propositions are listed below the tables, and the results are reported in Figure 10 of Appendix B.

5.3 2SLS Estimates

I present OLS and 2SLS estimates for each environmental proposition that was on the California ballot in years 2016, 2018 and 2022.

2016: Table 2 shows OLS and 2SLS estimates for green ballots in this year. There is consistent pattern for both estimates here: instrumenting the cumulative funding measure makes the coefficient negative.

Proposition 65, which proposed the Dedication of Revenue from Disposable Bag Sales to the Wildlife Conservation Fund Initiative, was defeated on the ballot that year, and we also see no significant results on program funding in garnering support for this initiative. The 2SLS estimate is negative, which is the opposite direction to the one hypothesized: the prior here is that a higher value of targeted environmental funding should make people more likely to vote in favor of pro-environmental propositions. However, the estimate is insignificant.

Proposition 67 is the California Plastic Bag Ban Veto Referendum, where a "Yes" vote supported upholding the contested legislation banning certain plastic bags, that was enacted by the California State Legislature as Senate Bill 270. The proposition passed in the state, however the 2SLS estimate is negative and significant, which is once again in the opposite direction to our prior.

Why would receiving targeted environmental funding make Californians less likely to vote in favor of upholding a plastic bag ban? The instrument captures exogenous variation in exposure to GGRF funding, but there may be a disconnect between the attitude towards the infrastructure centric projects that are funded by GGRF and consumer facing policies like plastic bags. Note that a key program feature is that funding is targeted to address disadvantaged communities with high levels of pollution burden, which so happen to be both less educated and poor (both components are captured in a high CES score).

A larger proportion of treated census tracts in a county implies that the county has a larger population of the poor, less educated and disadvantaged: if there exists an unobserved voting pattern among this group against consumer facing policies like plastic bag bans, it could help explain the results we see. These communities, despite being treated with environmental funding could harbor unobserved distaste towards prohibitory environmental regulations sanctioned by the government. It is also possible that program salience or visibility was low and did not translate into the model, biasing the estimates downward.

Proposition	65		67	
	OLS	2SLS	OLS	2SLS
Log(Cumulative Funding)	0.00591	-0.0161	0.0103	-0.0579*
	(0.00389)	(0.0141)	(0.00685)	(0.0313)
Constant	0.338	0.717	0.323	1.497
Observations	268	268	268	268
Standard errors clustered at the county level.				

Table 2: OLS and 2SLS Estimates for 2016. Estimates relate to *Proposition 65* (Carryout Bag Charges) and *Proposition 67* (Ban on Single-use Plastic Bags).

2018: Table 3 shows that for both propositions 3 and 6, the 2SLS estimates are larger than OLS and the coefficient on 6 is significant. **Proposition 3** was the California Water Infrastructure and Watershed Conservation Bond Initiative, which did not pass. A “Yes” vote supported this measure to authorize \$8.877 billion in general obligation bonds for water infrastructure, groundwater supplies and storage, surface water storage and dam repairs, watershed and fisheries improvements, and habitat protection and restoration. The 2SLS estimate is positive but insignificant, so the direction is as hypothesized.

Proposition 6, the Voter Approval for Future Gas and Vehicle Taxes and 2017 Tax Repeal Initiative was defeated. A "Yes" vote supported this initiative to: repeal fuel tax increases and vehicle fees that were enacted in 2017, including the Road Repair and Accountability Act of 2017 (RRAA) and require voter approval (via ballot propositions) for the California State Legislature to impose, increase, or extend fuel taxes or vehicle fees in the future. Given our prior that GGRF funding should make people more likely to vote pro-environmental, this estimate is again in the opposite direction – higher funding implies that people become more likely to vote in favor of *repealing* the fuel tax, which is a pro-environmental measure. Much of the reasons for this puzzling result are outlined in the previous paragraph that discusses another result that moves against the prior. Low program salience, voter composition of poor and less educated, and a general distaste for taxation could help explain this estimate.

Proposition	3		6	
	OLS	2SLS	OLS	2SLS
Log(Cumulative Funding)	0.00721**	0.0111	-0.0104	0.0442*
	(0.00329)	(0.00949)	(0.00681)	(0.0268)
Constant	0.372	0.305	0.631	-0.308
Observations	268	268	268	268
Standard errors clustered at the county level.				

Table 3: OLS and 2SLS Estimates for 2018. Estimates relate to *Proposition 3* (Bond for Water and Environmental Projects), *Proposition 6* (Repeals 2017's fuel tax and vehicle fee increases).

In Table 4 for propositions 68 and 72, the pattern is similar to 2016, where instrumenting funding leads to a negative coefficient on the 2SLS specification. **Proposition 68**, the Parks, Environment, and Water Bond was approved. A "Yes" vote supported this measure to authorize $4 billion in general obligation bonds for state and local parks, environmental protection projects, water infrastructure projects, and flood protection projects. The coefficient is both negative and insignificant.

Proposition 72, the Rainwater Capture Systems Excluded from Property Tax Assessments Amendment, was approved. A "Yes" vote supported this amendment to allow the state legislature to exclude rainwater capture systems added after January 1, 2019, from property tax reassessments. This estimate is again in the opposite direction – higher funding implies that people are less likely to vote for rainwater capture exclusion. The downward bias could be driven by low program salience, voter composition of poor and less educated, and a lack of understanding in these communities about what this ballot measure even entails. Poorer voters have low home ownership rates, and may not even care about this measure – an important observation that could help explain the significant downward estimate.

Proposition	68		72	
	OLS	2SLS	OLS	2SLS
Log(Cumulative Funding)	0.0147**	-0.0274	0.00333	-0.0267*
	(0.00596)	(0.0244)	(0.00250)	(0.0147)
Constant	0.286	1.010	0.766	1.284
Observations	268	268	268	268
Standard errors clustered at the county level.				

Table 4: OLS and 2SLS Estimates for 2018. Estimates relate to *Proposition 68* (Natural Resources Bond), and *Proposition 72* (Property Tax: New Construction: Rain-Capture).

2022: Table 5 shows results for **Proposition 30**, the Tax on Income Above $2 Million for Zero-Emissions Vehicles and Wildfire Prevention Initiative was defeated. A "Yes" vote supported increasing the tax on personal income above $2 million by 1.75% and dedicating the revenue to zero-emission vehicle subsidies; zero-emission vehicle infrastructure, such as electric vehicle charging stations; and wildfire suppression and prevention programs. The estimate is both negative and insignificant.

Proposition	30	
	OLS	2SLS
Log(Cumulative Funding)	0.00762	-0.0304
	(0.00514)	(0.0191)
Constant	0.283	0.937
Observations	268	268
Standard errors clustered at the county level.		

Table 5: OLS and 2SLS Estimates for 2022. Estimates relate to *Proposition 30* (Tax to Fund ZEV/Wildfire Programs).

6 Robustness

First, I add the control ”proportion non-white” to each of my estimating equations in the 2SLS framework to check for the heterogeneous impact of county-level racial disparity on voting outcomes. For reasons outlined previously in this paper, more obvious demographic controls that determine voting outcomes like level of education, poverty and housing burden are omitted because they are used in the calculation of census tract level CES scores, and would be collinear with the instrument if included in the estimation design.

This is especially salient as we zoom in on the tracts near the threshold (just near the top 25th percentile of scores), the level of education is lower, poverty is higher and the population is housing burdened. Then the calculated instrument measure, proportion of “treated” census tracts is highly correlated with education and poverty (more treated tracts implies lower levels of education and higher levels of poverty). This discussion relates to a potential threat to instrument exogeneity: if being poorer makes you more likely to be funded by the program, and also more likely to vote against pro-environmental measures, identification is threatened.

There are two things to note here: One, we cannot measure the poverty (or education) level of the people that do end up voting. Two, due to a lack of literature on pro-environmental voting, there is a dearth of estimates that conclusively show if poor (or less educated people) are more/less likely to vote in favor of environmental measures. In short, unless there exists a conclusive pro-environmental voting pattern, that is a product of being disadvantaged alone, the instrument is exogenous for my design, and the results remain interpretatble.

Tables 11 and 12 of Appendix B show all 2SLS specifications with proportion of non-white people in each county added as a control. None of the estimates are robust to the addition of race as a control, which is suggestive evidence for an unobserved racial voting pattern that could be biasing this design.

Additionally, to help explain the negative results for pro-environmental voting, I check for bandwidth sensitivity. Since the calculation of my instrument is dependent on a scoring bandwidth, I vary the bandwidth to check if the significance and direction of my results is sensitive to bandwidth selection. I toggle the 3.86 to be both 2 points higher and lower. Note that a lower bandwidth than 3.86 is sub-optimal because it significantly reduces the sample size – a large number of county instrument values either evaluate to "0" or missing, and the standard errors for all estimates are large – in light of this limitation, I only report estimates with a higher bandwidth. Toggling the bandwidth doesn't change the results. We see some added 2SLS significance for proposition 6 in the year 2018, and the direction is consistent with the results obtained with a lower bandwidth. None of the estimates are sensitive to bandwidth selection – barring proposition 6, the results remain similar and the direction does not change. Tables 14 and 15 show these estimates, and 13 shows the first stage with the toggled bandwidth. All tables are in Appendix B.

Lastly, I address the "pre-trends" or balance concern – does the instrument predict pro-environmental voting in pre-funding years? If the instrument has predictive power in years before the program is implemented, then we have support for an environmental voting pre-trend that is not captured. I choose environmental propositions from 2012 and 2014 as detailed in Section 3, and find that there is no first stage or reduced form for the instrument.

I also run the full 2SLS framework on old voting outcomes using post-funding data and find no significant results, thus lending credibility to instrument exogeneity, and a lack pf pre-trends. These estimates are reported in Table 16 of Appendix B.

7 Limitations

This paper attempts to measure the impact of targeted funding on voter behavior and uses a unique instrument to do so. In the process of writing this paper, I discovered why the body of work analyzing voter behavior is so limited: how people vote, and what they vote for is driven by a host of objectives that are hard to parse and vary exogenously. Within the specific context of my design, I outline some potential issues.

While the instrument does help isolate the variation to GGRF funding, the question of program salience remains unanswered. There is no data that measures public awareness about the Cap-and-Trade auctions or the provisions of GGRF funding. With any public program that requires a non-zero degree of initiative and self-selection from the public, the "first-stage" is always the level of awareness about the program itself, which is a variable we are unable to measure. A good smell-test for this paper would be high program awareness at the individual level. While the projects funded under GGRF are substantial and grow over time, the bulk of them are not at the individual level which makes it hard to parse if the ground-level beneficiaries know that the funding is a product of Cap-and-Trade, or that it is driven by pro-environmental objectives. Since the projects are always of a pro-environmental nature, it is not unreasonable to assume that the beneficiaries know that there is some (if not GGRF specifically) government program that is funding their solar panels, or watersheds, or EV subsidy, and that the receipt of this funding has the ability to change their pro-environmental attitudes, and I rely on this assumption when I discuss my results.

California is an interesting setting because it is canonically dubbed as the "green state" for its emphasis on climate and environmental policy. PPIC survey data shows that Californians are, on-average, both environmentally conscious and aware. This would signal the existence of pro-environmental voting trends in the state much before the onset of the GGRF policy in 2014. Pre-trends in voting (specifically in the context of the environment) are hard to measure and account for because they could be driven by partisanship (Democrats are more likely to push for environmental protection than republicans) and/or environmental disasters in election years (people are more likely to be in favor of the environment when impacted by disasters like wildfire). Disentangling these mechanisms is tricky, but valuable for any project that tries to measure public support in favor of the environment. Future research that aims to measure the impact of targeted programs on voting behavior would benefit greatly from understanding how people vote when the environment is at stake at the baseline.

8 Conclusion

This paper examines whether California's targeted environmental investments, funded through Cap-and-Trade auction proceeds and allocated to disadvantaged communities lead to increased support for pro-environmental ballot measures. Contrary to our priors that support would rise, the results consistently indicate either no effect or a significant negative effect of program funding on pro-environmental voting outcomes. These findings persist across multiple specifications and robustness checks, suggesting that increased environmental investment does not necessarily foster popular support for green measures.

The results point to a potential disconnect between the environmental benefits delivered by the state and the political attitudes of beneficiaries. This may be due to lack of program salience, limited political engagement or environmental literacy among target populations. There could also be a potentially unobserved pre-trend in environmental voting, or distaste for regulation that this paper's design could not capture.

Keeping in mind the limitations, if we do take the results at face value in light of a policy discussion, it raises deeper questions about the democratic legitimacy and distributional effects of market-based environmental policies. If targeted environmental spending—intended to redress environmental burden does not translate into political support or civic engagement, then the efficiency claims of such mechanisms should be scrutinized more closely. The Cap-and-Trade framework, while market-efficient in reducing emissions, may fall short in generating the necessary political support for long-term environmental progress, especially in communities most affected by pollution.

Two things follow as potential solutions. Firstly, measuring program salience and awareness is the missing link that could help explain the relationship between between targeted funding received and pro-environmental voting. The state could benefit from running surveys to measure the grassroots salience of a large scale program like the GGRF. Secondly, if funding does have a lukewarm/negative effect on pro-environmental measures (especially when driven by a larger proportion of disadvantaged communities), policymakers could stand to rephrase environmental goals and initiatives on the ballot in more equitable ways. Reforms cannot be voted in if the voter base feels disconnected from the measure at hand, or is unable to parse its significance.

While voting is a tough beast to tackle, future work could examine whether GGRF funding affects civic participation more broadly, such as political donations, local organizing, or turnout in environmental elections. In the specific context of attempting to understand what makes people (especially those who bear the brunt of polluting hazards) vote in favor of the environment, much work needs to be done. This paper is but an attempt in that direction.

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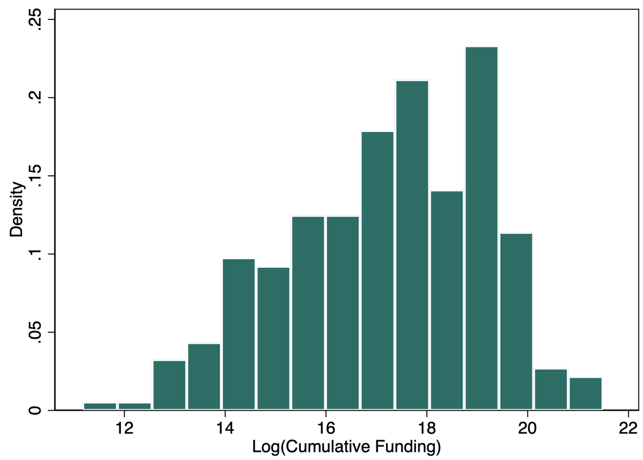
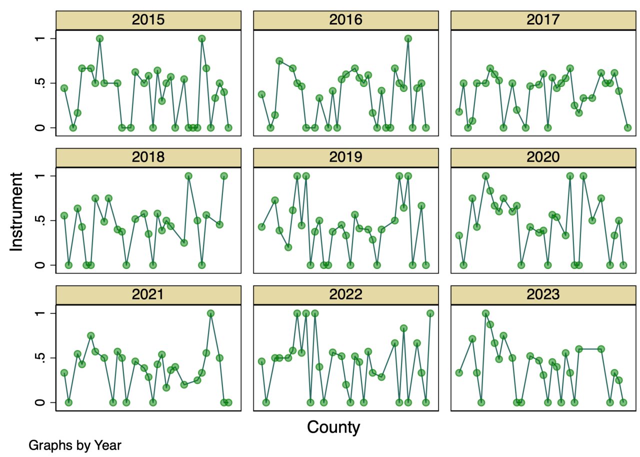
Figures



(a) Log Cumulative Funding



(b) Instrument Distribution

Figure 1: Visualizations of cumulative funding and instrument distribution.

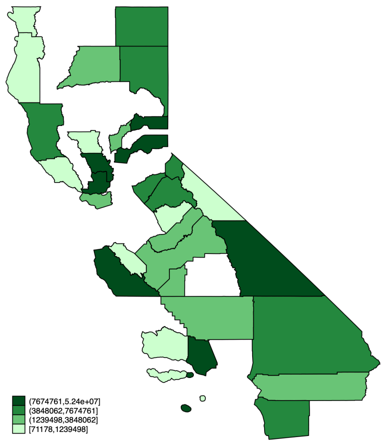


Figure 2: 2015

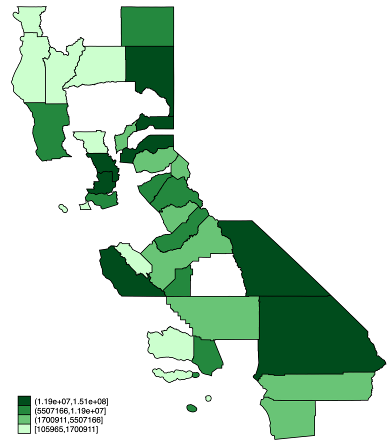


Figure 3: 2016

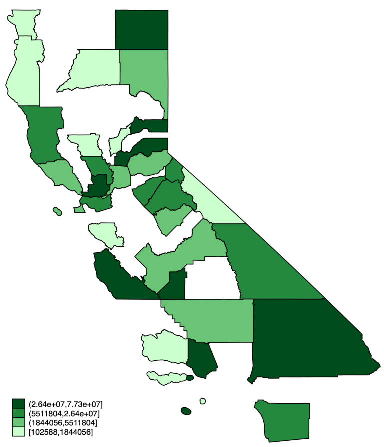


Figure 4: 2017

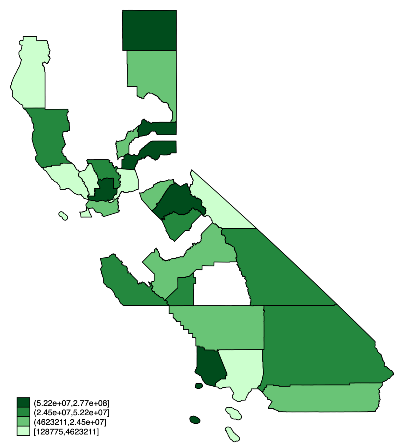


Figure 5: 2018

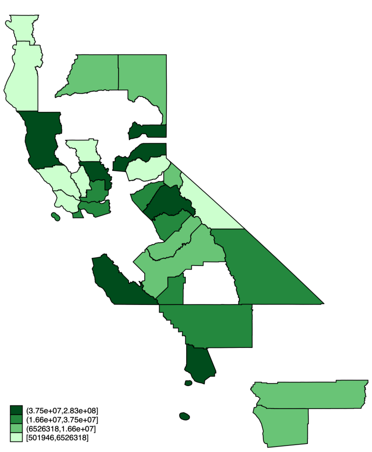


Figure 6: 2019

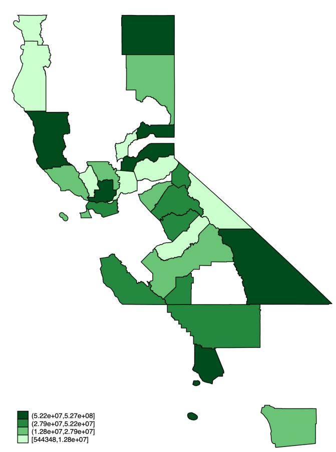


Figure 7: 2020

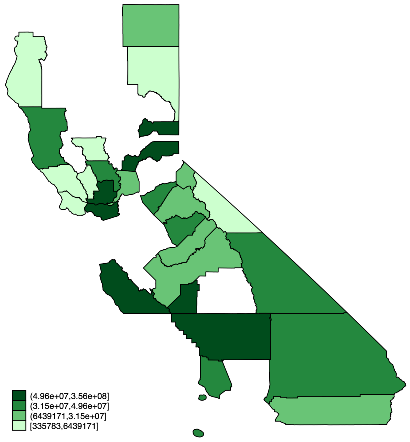


Figure 8: 2021

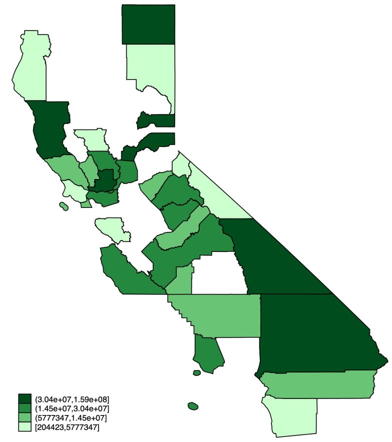


Figure 9: 2022

A Proof of IV Exogeneity

This section provides a short proof to show that the instrument is exogenous.

Proof. Consider $Z_{i,t}$, the instrument that is uncorrelated with the error term in the structural equation for the outcome variable. We state this assumption as:

$$\mathbb{E}[\varepsilon_{i,t} \mid Z_{i,t}] = 0$$

This implies that the instrument is as-good-as-randomly assigned, conditional on covariates. This exclusion restriction is defended in 4.2.

Let $Y_{i,t}$ be the outcome (proportion of votes cast in favor of a green ballot measure) in county i at time t, and let the estimated equation be:

$$Y_{i,t} = \beta_0 + \beta_1 F_{i,t} + \varepsilon_{i,t}$$

Where $F_{i,t}$ is the endogenous treatment variable (Log cumulative GGRF funding received by county i until time t), $Z_{i,t}$ is the instrument defined in Equation (3), and $\varepsilon_{i,t}$ is the error term.

Our design requires:

$$\mathbb{E}[\varepsilon_{i,t} \mid Z_{i,t}] = \mathbb{E}[\varepsilon_{i,t}] \quad \text{or} \quad \mathrm{Cov}(Z_{i,t}, \varepsilon_{i,t}) = 0$$

Now define the CES score for census tract j in county i at time t as $p_{i,j,t}$, and a deterministic cut-off score c such that:

$$T_{i,j,t} = \mathbf{1}\{p_{i,j,t} \in (c, c+h]\}$$
$$C_{i,j,t} = \mathbf{1}\{p_{i,j,t} \in [c-h, c]\}$$

for the chosen bandwidth $h = 3.86$. Under standard sharp RD assumptions (continuity and no manipulation of $p_{i,j,t}$ at cutoff c), we have:

$$\mathbb{E}[\varepsilon_{i,j,t} \mid T_{i,j,t} = 1] = \mathbb{E}[\varepsilon_{i,j,t} \mid C_{i,j,t} = 1]$$

This implies exogeneity at the census tract level: assignment to treatment around the cutoff is independent of unobserved determinants of the outcome.

We now define the instrument:

$$Z_{i,t} = \frac{\sum_{j=1}^{k_{i,t}} T_{i,j,t}}{\sum_{j=1}^{k_{i,t}} T_{i,j,t} + \sum_{j=1}^{N-k_{i,t}} C_{i,j,t}}$$

Which is the proportion of tracts in the RD bandwidth that are treated, at the county-year level.

Let $\varepsilon_{i,j,t}$ be the residual component of the outcome at the tract level, and assume county-level outcomes are aggregated from census tract-level outcomes. Then,

$$\varepsilon_{i,t} = \frac{1}{N_i} \sum_{j=1}^{N_i} \varepsilon_{i,j,t}$$

From census tract-level exogeneity, we get:

$$\mathbb{E}[\varepsilon_{i,j,t} \mid T_{i,j,t}] = \mathbb{E}[\varepsilon_{i,j,t}]$$

Using the law of iterated expectations and linearity of expectations:

$$\mathbb{E}[\varepsilon_{i,t} \mid Z_{i,t}] = \frac{1}{N_i} \sum_{j=1}^{N_i} \mathbb{E}[\varepsilon_{i,j,t} \mid Z_{i,t}] = \mathbb{E}[\varepsilon_{i,t}]$$

Then $Z_{i,t}$ is a function of $T_{i,j,t}$, and $T_{i,j,t}$ is independent of $\varepsilon_{i,j,t}$ by design.

Thus the instrument satisfies:

$$\text{Cov}(Z_{i,t}, \varepsilon_{i,t}) = 0$$

$\square$

B Supplementary Tables

	2016	
Proposition	65	67
Z	-0.0309	-0.111***
	(0.0238)	(0.0389)
Constant	0.452	0.545
Observations	268	268
R^2	0.021	0.064
Standard errors clustered at the county level.		

(a) 2016: *Prop 65* (Bag Charges), *Prop 67* (Plastic Bag Ban).

		2018		
Proposition	3	6	68	72
Z	0.0213	0.0848**	-0.0526	-0.0513**
	(0.0185)	(0.0356)	(0.0378)	(0.0193)
Constant	0.488	0.418	0.560	0.844
Observations	268	268	268	268
R^2	0.016	0.043	0.019	0.091
Standard errors clustered at the county level.				

(b) 2018: *Prop 3* (Water Bonds), *Prop 6* (Fuel Tax Repeal), *Prop 68* (Natural Resources), *Prop 72* (Rain-Capture Tax).

	2022
Proposition	30
Z	-0.0584**
	(0.0273)
Constant	0.437
Observations	268
R^2	0.039
Standard errors clustered at county level.	

(c) 2022: *Prop 30* (ZEV/Wildfire Tax).

Figure 10: Reduced Form Estimates for Propositions from 2016, 2018, and 2022.

Proposition	3		6	
	OLS	2SLS	OLS	2SLS
Log(Cumulative Funding)	0.00363	0.00731	-0.0114	0.0891
	(0.00356)	(0.0136)	(0.00790)	(0.0634)
Proportion Non-white	0.0787**	0.0552	0.0290	-0.613
	(0.0346)	(0.0727)	(0.0676)	(0.413)
Constant	0.406	0.351	0.637	-0.867
Observations	267	267	267	267
Standard errors clustered at the county level.				

(a) 2018: *Prop 3* (Water Bonds), *Prop 6* (Fuel Tax Repeal)

Proposition	68		72	
	OLS	2SLS	OLS	2SLS
Log(Cumulative Funding)	0.0157**	-0.0619	0.00729**	-0.0447
	(0.00676)	(0.0534)	(0.00296)	(0.0336)
Proportion Non-white	-0.0273	0.468	-0.0911**	0.241
	(0.0633)	(0.345)	(0.0350)	(0.219)
Constant	0.279	1.440	0.730	1.509
Observations	267	267	267	267
Standard errors clustered at the county level.				

(b) 2018: *Prop 68* (Natural Resources), *Prop 72* (Rain-Capture Tax).

Figure 11: OLS and 2SLS Estimates for 2018 (with controls)

Proposition	65		67	
	OLS	2SLS	OLS	2SLS
Log(Cumulative Funding)	0.00397	-0.0390	0.0139*	-0.109
	(0.00525)	(0.0290)	(0.00773)	(0.0756)
Proportion Non-white	0.0383	0.313*	-0.0864	0.698
	(0.0537)	(0.187)	(0.0763)	(0.497)
Constant	0.358	1.001	0.293	2.131
Observations	267	267	267	267
Standard errors clustered at the county level.				

(a) 2016: *Prop 65* (Bag Charges), *Prop 67* (Plastic Bag Ban).

Proposition	30	
	OLS	2SLS
Log(Cumulative Funding)	0.00804	-0.0624
	(0.00614)	(0.0443)
Proportion Non-white	-0.0158	0.434
	(0.0615)	(0.292)
Constant	0.281	1.336
Observations	267	267
Standard errors clustered at the county level.		

(b) 2022: *Prop 30* (ZEV/Wildfire Tax).

Figure 12: OLS and 2SLS Estimates for 2016 and 2022 (with controls)

Regression Results for Log Funding

Log(Cumulative Funding)	(1)	(2)
Z	1.810**	0.965
	(0.678)	(0.650)
Proportion Non-white		6.075***
		(0.902)
Constant	16.46	14.69
	(0.367)	(0.359)
Observations	279	278
R^2	0.070	0.302
Standard errors clustered at the county level.		

Figure 13: First Stage (toggled bandwidth)

Proposition	3		6	
	OLS	2SLS	OLS	2SLS
Log(Cumulative Funding)	0.00671**	0.00924	-0.00906	0.0511*
	(0.00317)	(0.00919)	(0.00692)	(0.0298)
Constant	0.381***	0.338**	0.603***	-0.428
	(0.0567)	(0.159)	(0.115)	(0.516)
Observations	279	279	279	279
Standard errors clustered at the county level.				

(a) 2018: *Prop 3* (Water Bonds), *Prop 6* (Fuel Tax Repeal)

Proposition	68		72	
	OLS	2SLS	OLS	2SLS
Log(Cumulative Funding)	0.0132**	-0.0345	0.00300	-0.0284*
	(0.00607)	(0.0269)	(0.00245)	(0.0156)
Constant	0.316	1.134	0.774	1.312
	(0.102)	(0.465)	(0.0414)	(0.267)
Observations	279	279	279	279
Standard errors clustered at the county level.				

(b) 2018: *Prop 68* (Natural Resources), *Prop 72* (Rain-Capture Tax).

Figure 14: OLS and 2SLS Estimates for 2018 (alternate bandwidth)

Proposition	65		67	
	OLS	2SLS	OLS	2SLS
Log(Cumulative Funding)	0.00532	-0.0161	0.00878	-0.0649*
	(0.00377)	(0.0142)	(0.00697)	(0.0343)
Constant	0.350	0.718	0.355	1.618
Observations	279	279	279	279
Standard errors clustered at the county level.				

(a) 2016: *Prop 65* (Bag Charges), *Prop 67* (Plastic Bag Ban).

Proposition	30	
	OLS	2SLS
Log(Cumulative Funding)	0.00698	-0.0343
	(0.00513)	(0.0211)
Constant	0.297	1.005
	(0.0870)	(0.366)
Observations	279	279
Standard errors clustered at the county level.		

(b) 2022: *Prop 30* (ZEV/Wildfire Tax).

Figure 15: OLS and 2SLS Estimates for 2016 and 2022 (alternate bandwidth)

Proposition	39	
	OLS	2SLS
Log(Cumulative Funding)	0.0119**	-0.0151
	(0.00529)	(0.0175)
Constant	0.383	0.847
Observations	268	268
Standard errors clustered at the county level.		

(a) 2012: *Proposition 39* - Requires out-of-state businesses to "support projects intended to improve energy efficiency and expand the use of alternative energy".

Proposition	1	
	OLS	2SLS
Log(Cumulative Funding)	0.0152***	-0.0148
	(0.00540)	(0.0194)
Constant	0.265	0.782
Observations	268	268
Standard errors clustered at county level.		

(b) 2014: *Proposition 1* - Issues $7.12 billion in bonds for California's water system.

Figure 16: OLS and 2SLS Estimates for 2012 and 2014