Green Industrial Policy in the Voting Booth: The Electoral Effects of the Inflation Reduction Act*

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June 19, 2026

Abstract

The Inflation Reduction Act is the most significant climate mitigation policy ever adopted by the United States. We analyze the electoral effects of this large-scale green industrial policy in the 2024 election. Because the policy promises near-term benefits with few direct costs, it is a most likely case for generating electoral support and avoiding voter backlash. Using a difference-in-differences approach and entropy balancing, we compare vote outcomes in counties that receive IRA-induced private sector investment to observationally similar counties without investment. Looking across presidential, congressional, and gubernatorial elections, we find that the policy did not generate consistent electoral rewards or punishment. We provide several potential explanations for the results. The findings stand in contrast to previous studies that find incumbents are punished for significant climate reforms.

*For their helpful feedback on previous drafts of this paper, we thank Mats-Philip Ahrenshop, Lucy Barnes, Tim Hicks, Michael Jacobs, Roland Kristo, Moritz Marbach, and audiences at the Stockholm School of Economics, University of Oxford, Royal Holloway University, Environmental Politics and Governance Online Workshop, PSA Environment and EPOP Workshop on Elections and Climate Change, and the London Environment and Public Policy (LEAPP) Workshop.

1 Introduction

On August 16, 2022, the United States enacted the Inflation Reduction Act (IRA), the most significant climate change legislation in its history. It represented not only a domestic breakthrough after many climate policy failures and reversals, but also an international one. The US is the second largest emitter of greenhouse gases (GHG), and the largest cumulative emitter. Its actions have a significant impact on whether global climate change temperature targets can be met. More broadly, the IRA represented a high-profile ideational shift in climate policy paradigms. Instead of focusing primarily on emissions reductions, as carbon pricing policies might do, it was green industrial policy that sought to achieve a decrease in GHG emissions alongside other domestic policy goals, especially increased economic competitiveness and onshoring of supply chains.

Importantly, the IRA also embodied a distinct political logic. Climate mitigation policies vary in their distributional impacts, which in turn structure their political economy. Carbon pricing, while long favored by economists, has proven difficult to adopt, implement, and sustain politically (Rabe, 2018). By concentrating visible short-term costs on households and businesses, and distributing diffuse benefits globally over the long term, carbon pricing often generates strong constituencies of opposition and weak ones of support. This dynamic has contributed to policy reversal in Australia and Canada (Crowley, 2017), resistance in France and South Africa (Driscoll, 2023; Rennkamp, 2019), and repeated policy failures in the US (Mildenberger, 2020; Rabe, 2010). For example, President Clinton failed to enact an energy tax in 1993, federal gasoline taxes have remained unchanged for over thirty years, and bills for establishing a nationwide emissions trading scheme repeatedly failed in Congress throughout the 2000s. The political challenges associated with carbon pricing help explain why only around 3.2% of global emissions are sufficiently priced (World Bank, 2025).

Green industrial policy (GIP) on the other hand reflects a different distributional dynamic. We define GIP as policies that stimulate and facilitate the development of low-carbon industries (Allan et al., 2021). It can be designed to generate short-term economic benefits through subsidies, tax credits, preferential finance, local content requirements, and targeted investments, among other instruments, while diffusing, obscuring, or delaying short-term costs. In the case of the IRA, federal tax credits, loans, and direct expenditure channeled near-term benefits to households, firms, workers, and communities, creating concentrated winners. Crucially, the IRA imposed no immediate costs on most actors, especially voters. Instead, costs were primarily imposed on very large companies through a corporate alternative minimum tax.

Existing studies find that politicians often lose popular support when imposing visible and concentrated costs associated with climate goals, such as coal phaseouts (Egli et al., 2022; Stutzmann, 2025), polluting car bans (Colantone et al., 2024), and stringent mitigation policies (Furceri et al., 2023); but gain support when implementing policies that provide targeted benefits to affected communities (Bolet et al., 2024). By front-loading visible and concentrated benefits, green industrial policies may better avoid voter opposition and backlash. Additionally, such policies may better promote the development of a constituency of beneficiaries with a stake in their continuation. If so, green industrial policies could become self-reinforcing over time as a result of policy feedback dynamics and path dependence (Meckling et al., 2015, 2017; Pahle et al., 2018; Zysman and Huberty, 2014).

We shed light on these questions by examining voters' response to the IRA. Our identification strategy relies on the geographically staggered implementation of the policy's tax credits. Driven by tax incentives, private sector actors invested in some, but not all, US counties. We use a difference-in-difference approach with entropy balancing to compare electoral outcomes during the 2024 election in counties that received IRA-induced private sector investment to observationally similar counties that did not receive investment. We analyze presidential, congressional (house and senate) and gubernatorial vote shares.

Our findings suggest that IRA-induced private investments did not consistently generate electoral reward or punishment for the party that enacted it – the Democrats. Similarly, we find no consistent evidence of reward or punishment for incumbents, regardless of party. A wide range of heterogeneity analyses that examine variation in treatment effects by the scale of investment and project status yields similar results.

We consider several explanations for our findings. Data suggests that neither employment nor wages increased in treated counties after the IRA's passage, indicating that IRA projects may not have produced visible material gains for voters before the election. At the same time, and plausibly as an effect of limited material benefits, surveys suggest that voters knew little about the IRA, reducing its salience. Even if material benefits were larger and the policy was more salient, voters may not have been able to attribute the policy to the Democrats given the contested credit claiming around IRA-related projects, especially at the state level where Republican governors actively sought to claim credit. Relatedly, deep political polarization may have prevented voters, especially in Republican districts, from rewarding the Democrats for the policy, even if they benefited from it.

The fact that such a large scale climate policy in a deeply polarized country did not generate significant electoral backlash is noteworthy. Overcoming opposition and containing backlash is a key theme in climate change politics research (Aklin and Mildenberger, 2020; Colgan et al., 2021; Finnegan et al., 2025; Finnegan, 2022a,b; Meckling et al., 2022; Patterson, 2023; Rabe, 2018; Ross, 2025; Stokes, 2020), and as mentioned, previous studies on the electoral effects of climate policies have generally found voter discontent. Importantly, our findings highlight how green industrial policy may better reduce voter opposition compared to other types of policy instruments. That said, even though the

IRA did not generate electoral punishment, parts of it have still been successfully rolled back by the second Trump administration, suggesting that the policy did not generate a support coalition quick enough to resist reversal.

The article contributes to a growing literature on the politics of green industrial policy. We complement recent work on the strategies and motivations behind governments' use of green industrial policy (Allan and Nahm, 2024; Cullenward and Victor, 2020; Kupzok and Nahm, 2024; Meckling, 2021; Meckling and Strecker, 2023; Nahm, 2021) by turning attention to its electoral politics. Scholars have hypothesized that such policies can generate voter support, and surveys suggest that individuals often prefer them (Beiser-McGrath, 2021; Bergquist et al., 2020; Dechezleprêtre et al., 2022; Drews and Bergh, 2015), however no study to our knowledge has evaluated their electoral effects. Knowing how voters actually respond to green industrial policy has major consequences for the political feasibility of this increasingly utilized policy approach.

Additionally, the paper contributes to a broader literature on voters and climate policy. Existing studies have tended to focus on the losers of climate reforms (Colantone et al., 2024; Stokes, 2016; Egli et al., 2022; Furceri et al., 2023). We instead examine the effects of a policy that promises near-term benefits and few costs. In this way, our study is related to Bolet et al. (2024) who examine a just transition policy in Spain and find that the incumbent is electorally rewarded. Additionally, it builds on work that examines how political parties that are seen as issue owners can benefit from climate policy (Otteni and Weisskircher, 2022; Urpelainen and Zhang, 2022).

More broadly, our paper contributes to work on public investment and voter support. To our knowledge, only one study has examined the electoral effects of industrial policy. Hong and Park (2016) analyze South Korea in the 1970s and 80s, finding that voters reward the incumbent for industrial policy. There is a larger literature on government spending and voter support (Stein and Bickers, 1994; Kriner and Reeves, 2012; Lazarus and Reilly, 2010), though scholars have tended to examine policies that afford politicians control over the geographic targeting of benefits (e.g., pork barrel spending). Our study provides additional evidence on the link between industrial policy and elections, but in a context where politicians do not have the power to target benefits.

2 The Electoral Politics of Green Industrial Policy

Generally speaking, we expect the electoral politics of a climate policy instrument to be driven by its underlying distributive profile. Here we describe the distributive profile of green industrial policy and how it may generate electoral support, contrasting it with conventional mitigation policies like carbon pricing.

The goals of GIP are two fold: economic (i.e., increase national economic growth and competitiveness) and environmental (i.e., reduce emissions).[1] To achieve these, a variety of policy instruments can be used, including, for example: direct public spending through tax credits and subsidies, concessional loans and grants, public procurement, and local content requirements (Rodrik, 2014; Meckling, 2021). Importantly, these instruments can be designed to distribute costs and benefits in a politically advantageous way along three dimensions: temporal, geographic and sectoral. Temporally, visible economic benefits can be generated over the short- to medium-term in the form of increased investment, accelerated project siting and factory construction, employment, and income growth. Additionally, benefits can flow directly to voters in the form of subsidies for the adoption of clean technology, such as rooftop solar, heat pumps, and electric vehicles. If GIP is financed through deficit spending, general taxation, or tax increases in other parts of the economy, policies can impose little to no visible short-term costs on households. Geographically and sectorally, policies can be designed to generate targeted economic benefits in politically-relevant communities and industries. Taken together, GIP's distributive profile has the potential to create broad policy winners across the economy, as well as concentrated winners clustered in targeted regions and sectors. Electorally, it has the potential to generate a coalition of beneficiaries to support and defend the policy. While the electoral effects of GIP are understudied, existing literature suggests that concentrated economic benefits can generate electoral support, such as in the cases of just transition policy (Bolet et al., 2024), wind energy investments (Urpelainen and Zhang, 2022), and investments in industrial complexes (Hong and Park, 2016).

In contrast, conventional mitigation policies like carbon pricing tend to have less politically favorable distributive profiles. Temporally, standard carbon pricing imposes visible short-term costs on voters today, primarily in exchange for the long-term and diffuse global public good of climate change mitigation. Furthermore, these short-term costs are unevenly distributed across regions and industries, with communities that are dependent on fossil fuel production and emissions-intensive sectors bearing the highest costs, threatening local economies and employment (Raimi et al., 2022; Vona, 2019). Electorally, the combination of visible short-term costs, concentrated in particular communities and industries, and diffuse, delayed gains tends to mobilize strong coalitions of opposition and weak ones of support.[2] Existing studies support this expectation. Implementing a carbon price has generated voter backlash for the incumbent party in carbon-intensive and fossil-fuel dependent constituencies (Pepin-Proulx, 2025), especially in coal-mining towns (Egli et al., 2022; Gazmararian, 2025; Weber, 2020). Rising household energy prices have also boosted support for radical-right parties (Voeten, 2025), and carbon taxes have sparked large-scale protests among car-dependent rural workers (Driscoll, 2023; Royall,

[1] Additionally, governments may have national security and geopolitical goals.

[2] To be sure, carbon pricing instruments can be designed with a view to generating short-term benefits, especially via revenue recycling schemes. However, even these designs tend to face political resistance (Mildenberger et al., 2022).

2020). For this reason, evidence shows that politicians are more likely to adopt carbon taxes when they are electorally secure (Finnegan, 2022a).

Our broad expectation of GIP's positive electoral effects is consistent with several theoretical traditions in political science. Scholars of retrospective voting argue that voters reward or punish incumbents based on their observed performance (Healy and Malhotra, 2013). Applied to GIP, visible local job creation, wage growth, and community revitalization provide both "pocketbook" (improvements in individual material wellbeing) and sociotropic (improvements in community wellbeing) benefits, and these gains should translate into electoral rewards for incumbents who deliver them. Additionally, theories of prospective voting theory emphasize that voters are also forward-looking, basing their choices on expectations about future outcomes (Lockerbie, 1992, 2008; Rosema, 2006). In the case of GIP, recipients of policy benefits may support the party most likely to sustain or expand the program, ensuring continued investment and employment growth. Policy feedback theory further suggests that new policies can create a coalition of supporters, whose material gains can increase their political participation and generate durable support for the policy's sponsors (Campbell, 2012; Larsen, 2019; Pierson, 1993; Jordan, 2013). Existing work on GIP has already highlighted its potential to foster such coalitions (Allan et al., 2021; Meckling et al., 2015; Meckling, 2021). Taken together, they suggest that when GIP's benefits are visible and attributable, it can increase and strengthen electoral support for the parties responsible for enacting it.

3 The Case of the Inflation Reduction Act

To investigate the electoral effects of green industrial policy we utilize the case of the US Inflation Reduction Act (IRA). At its initial adoption in August 2022[3], the IRA included provisions related to climate change, healthcare, and corporate taxation. In the case of climate, it included tax credits and direct expenditure. Tax credits provided incentives to businesses and individuals to adopt and deploy zero-emissions technologies. Businesses could receive tax credits for: clean electricity generation and storage; zero-emission nuclear power production; clean fuel production, including sustainable aviation fuels and second-generation biofuels; purchase of commercial clean vehicles; carbon capture and sequestration; building energy-efficient homes; advanced energy projects related to critical minerals and facility-level emissions reductions; and producing components along the supply chain for clean technologies, such as batteries, critical minerals, and solar and wind components (Bistline et al., 2023). In the case of individuals, they could receive credits for adopting clean energy technologies, including solar panels, heat pumps, and electric vehicles, and improving the energy efficiency of their homes. Most tax credits

[3]Here we describe the Inflation Reduction Act as it was enacted and before changes made by the second Trump administration.

were legislated to expire by 2032.

Importantly, businesses could earn bonus credits by satisfying certain conditions related to labor (by paying prevailing wage and providing apprenticeships), local content (by using US-produced materials), and location (by siting investments in low income communities or energy communities–those with brownfield sites, fossil fuel dependence, or closed coal mines). Also, IRA tax credits were transferrable. Businesses who generated credits but did not have enough tax liability to use them could sell them to other unrelated businesses that did. Additionally, because tax credits were based on eligible technologies (apart from bonus credits for low-income and energy communities), they could not, to our knowledge, be geographically targeted by politicians. All businesses with eligible projects could receive credits. Last, by supporting a wide range of technologies rather than specific firms, the credits did not 'pick winners' per se.

The Congressional Budget Office and Joint Committee on Taxation estimated that the cost of the tax credits to the federal government was around $271 billion for the period 2022-2031 (CBO, 2022; Bistline et al., 2023). However, most tax credits were uncapped, which meant the ultimate cost would depend on demand for them. Some independent estimates put the cost as high as $1 trillion (Bistline et al., 2023).

In terms of direct expenditure, the IRA included $121 billion in grants and loans for energy efficiency, industrial decarbonization, reducing emissions from agriculture, replacing emissions-intensive infrastructure, and other programs (CBO, 2022; Bistline et al., 2023). Summing the cost of the tax credits and the direct expenditure results in the widely-cited $392 billion cost estimate for the climate provisions in the bill. In terms of emissions reductions, studies suggest that the bill would contribute to reducing US economywide emissions by 33-40% below 2005 levels by 2030 and 43-48% below 2005 levels by 2035 (Bistline et al., 2023). While substantial, the country would still require additional policies to meet its 2030 goal under the Paris Agreement of a 50–52% reduction below 2005 levels.

The bill was paid for by establishing a corporate alternative minimum tax rate of 15% on firms with earnings over $1 billion, imposing a 1% excise tax on stock repurchases by publicly-traded companies, and imposing an excise tax on drug manufacturers who failed to enter into drug pricing agreements with the federal government. In total, these reforms were estimated to raise around $300 billion over ten years with around $220 billion coming from the corporate alternative minimum tax alone (CBO, 2022).

In terms of its distributive profile, the IRA generated concentrated and visible short-term financial benefits for voters and businesses that took advantage of the tax credits. Additionally, it generated medium- and long-term concentrated economic benefits for individuals and communities that enjoyed IRA-induced private sector investment and/or direct federal spending, for example in the form of jobs and growth in sectors supported by the tax credits, as well as the long-term and diffuse global benefit of climate change mitigation. Regarding costs, it imposed no costs on voters and relatively modest ones on a limited number of very large corporations (only around 150 companies were estimated to be subject to the corporate alternative minimum tax (Barthold, 2022)).[4] Given this distributive profile of short-term benefits with virtually no short-term costs, the IRA is a most likely case for generating electoral support and a least likely one for electoral punishment.

Before moving on, it is important to note that the IRA has been amended by the second Trump administration's budget bill, signed into law on July 4, 2025. Among other things, the policy terminates tax credits to households related to electric vehicles and home energy efficiency upgrades, phases out wind, solar, and green hydrogen credits by 2027, increases domestic content requirements for manufacturing, and implements new foreign entity of concern restrictions. These changes and reversals are likely to increase the country's emissions (Jenkins et al., 2025). They could also affect our analysis if firms anticipated them, and in response, paused or reduced their investments in the lead up to the 2024 election. To be sure, this would not affect our identification strategy. However, any reduced investment would reduce treatment intensity.

4 Data and Research Design

Ideally, we would estimate the total electoral effect of all of the IRA's climate provisions. However, this is challenging given available data. Data on federal expenditure and tax credits to individuals are only observed at the state level, and there is no treatment variation across states as all states receive some level of this spending. Private sector investment on the other hand can be observed at the county level, providing meaningful heterogeneity in treatment across counties, enabling us to identify how differential exposure to IRA-induced investments shapes electoral outcomes. Our analysis therefore exploits geographic variation in the private investment flows created by the policy to estimate electoral effects.

Data.—To identify the causal effect of private investments on voter behavior, we use panel data on votes cast in 3,082 counties in US elections held between 2000 to 2024.[5] More specifically, we analyze vote shares for the two main parties, the Democrats and Republicans, and the electoral data are votes cast in presidential, Senate, House, and gubernatorial elections. The election data is gathered from the MIT Election Data and Science Lab and Dave Leip's Atlas of US Presidential Elections (MIT Election Data

[4] Though if the costs were to rise higher than the CBO estimated, as a result of higher demand for tax credits, additional costs could be imposed.

[5] Due to missing data on covariates for 61 counties, our sample does not include all 3,144 counties in the US.

Table 1: Technology Investment Summary

Technology	Freq.	Percent	Mean Project Capex
Solar	1,630	61.00	96
Energy storage	515	19.30	177
Batteries	120	4.49	751
Wind	106	3.97	322
Hydrogen	51	1.91	755
Zero emission vehicles	49	1.83	377
Other	38	1.42	59
Fueling equipment	36	1.35	34
Clean fuels	31	1.16	46
Sustainable aviation fuels	25	0.94	1,395
Carbon management	25	0.94	609
Critical minerals	23	0.86	311
Electrolyzers	13	0.49	135
Cement	5	0.19	506
Iron and steel	4	0.15	721
Pulp and paper	1	0.04	199
Total	2,672	100.00	184

Note: Projects included here are announced between August 16, 2022 and November 5, 2024. Mean project capital expenditures (Capex) show values in million USD (in 2023 USD).

and Science Lab, 2025; Leip, 2025). We use data on IRA-induced private investments to pinpoint the treated and control counties, respectively. This data is provided by the Clean Investment Monitor database, and gives us 1,083 treated and 1,999 control counties (Rhodium Group, 2025).

We define treatment at the county level using the timing of the first IRA-linked private investment announcement. A county is coded as treated if it receives at least one such announcement between August 16, 2022 (when the IRA was enacted) and November 5, 2024 (election day). In our main analysis we study electoral outcomes in the 2024 election only, so the post-treatment outcome is measured at a common point in time for all treated counties. As a result, while treatment timing varies across counties within the 2022–2024 window, we are not estimating event-time (dynamic) effects.

Table 1 summarizes the set of IRA-linked private-sector investments that we use to define treated counties. It highlights both the technological breadth of the sample and the concentration of projects in a few technology categories. Of the 2,672 projects announced between August 16, 2022 and November 5, 2024, solar and energy storage together account for about four-fifths of all observations. At the same time, the table shows sub-

Table 2: Project Status Summary

Project Status	Freq.	Percent
Operating	1,261	47.20
Announced	738	27.62
Under construction	604	22.60
Canceled prior to operation	68	2.54
Retired	1	0.04
Total	2,672	100.00

Note: Projects included here are announced between August 16, 2022 and November 5, 2024. Project status refers to the operational stage of the IRA investment project as of Dec 31, 2024.

stantial heterogeneity in project size across technologies. Average capital expenditures for solar and energy storage are below the overall mean ($184 million), whereas projects related to, for example, sustainable aviation fuels, batteries, and hydrogen involve much larger investments on average. This distinction matters for the interpretation of our results: a technology's project count is not necessarily informative about its local economic impact – and thus its potential electoral effects – because relatively infrequent categories can correspond to disproportionately large capital outlays. For this reason, we distinguish between the extensive margin (whether a county receives any IRA-linked project) and the intensive margin (differences in investment intensity implied by project-level Capex).

Table 2 shows that IRA-linked investments also differ markedly in their stage of implementation. As of Dec 31, 2024, around half of all projects are already operating, while the other half are either under construction or remain at the announcement stage. Only a small share were canceled prior to operation or already retired. This variation is important for our empirical design and interpretation: projects that are operating or under construction are more likely to generate visible local effects – such as employment, contracting activity, or increased tax revenues – compared to projects that have only been announced. Consistent with this, prior work finds that projects in the construction phase and those with larger capital investments generate greater employment and, on average, higher wages (Cai et al., 2017; Garrett-Peltier, 2017; Wei et al., 2010). Taken together, Tables 1 and 2 underscore that 'treatment intensity' is not homogeneous: counties differ not only in whether they receive a project, but also in the type of technology, the scale of capital investment, and the degree to which the investment has materialized. We exploit this variation in project status and investment intensity in our heterogeneity analysis

The location of the treated and control counties are shown in the map in Figure 1. IRA-induced private investments are observed in every state in the continental US, but are more densely clustered in the Northeast, the South, the West, and the Midwest.

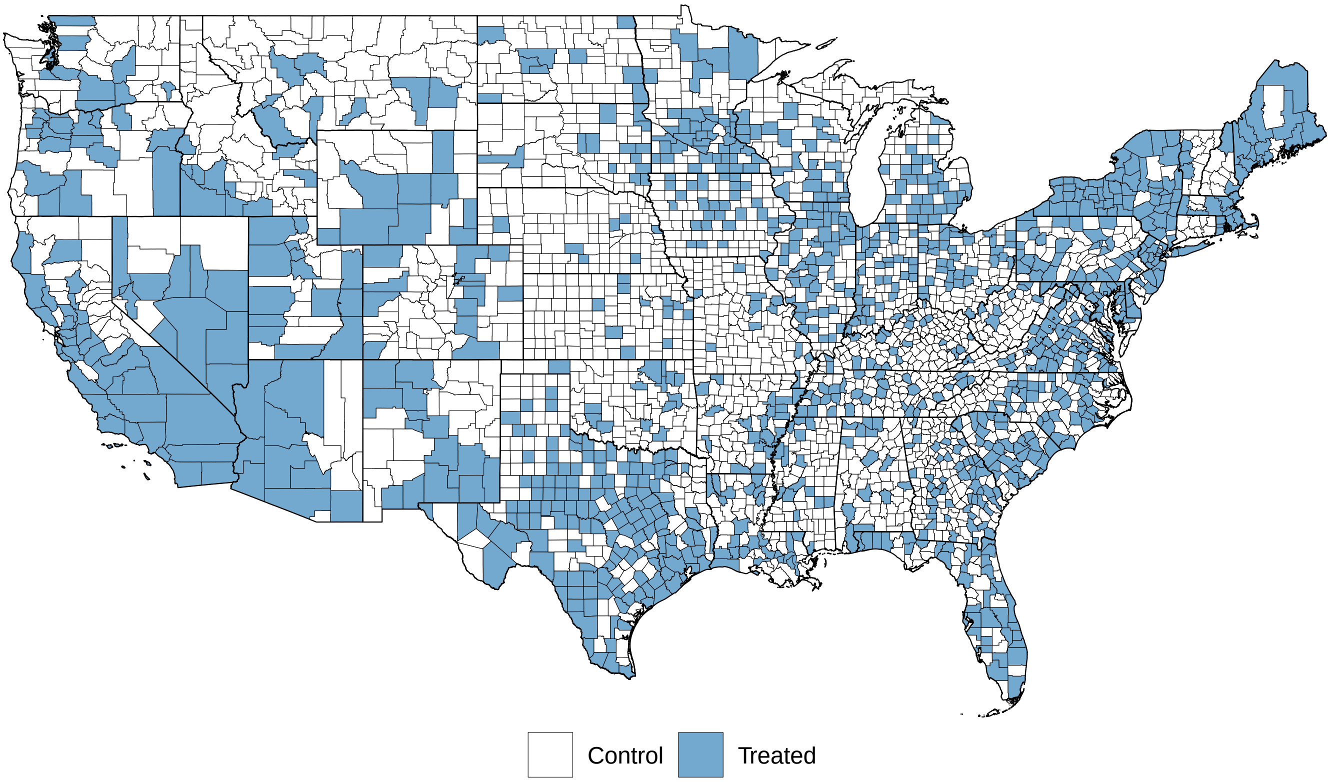


Figure 1: Map of Treated and Control Counties

Note: The figure displays counties in the contiguous United States; Alaska and Hawaii are omitted for readability but are included in our analysis. Counties shaded in blue received at least one IRA-linked private investment announcement during the study window (August 16, 2022 - November 5, 2024), while white counties received none.

DiD Model.—Using our vote-share data, we estimate the causal effect of IRA-induced private investment on Democratic vote shares with the following difference-in-differences (DiD) specification, estimated separately for each election type $i \in \{\text{Presidential, House, Senate, Gubernatorial}\}$:

$$Y_{ct} = \alpha + \beta\, \text{Treat}_c \times \text{Post}_t + \gamma_c + \delta_t + \varepsilon_{ct} \tag{1}$$

where Y_{ct} denotes the Democratic vote share (0-100 percent) in county c at election year t; Treat_c is a binary indicator for whether county c ever receives IRA-induced private investment before the 2024 election, and Post_t is an indicator defined as $1[t = 2024]$ and 0 in other election years; γ_c are county fixed effects; δ_t are time fixed effects; and ε_{ct} is an error term.

Although the IRA was enacted in August 2022, we do not treat the November 2022 midterm election as post-treatment in our main specification because the exposure window is arguably too short to meaningfully affect voting.[6]

Entropy Balancing.—Because IRA-induced private investments are not randomly distributed across counties (see Figure 1), treated and control counties may differ systematically in ways that also affect vote shares, potentially biasing our DiD estimates. We therefore complement the DiD design with entropy balancing (Hainmueller, 2012), which reweights the control group to match the treated group on pre-treatment covariates. Specifically, we choose weights for control counties such that selected covariates have the same first moment (mean) in the reweighted control group as in the treated group. We then use these time-invariant weights in equation (1) to estimate weighted DiD effects. By improving balance on observed characteristics, this approach increases the plausibility that treated and control counties would have followed similar trends absent treatment and reduces selection concerns. Moreover, when the reweighted control group closely resembles the treated group along a rich set of pre-treatment covariates, it becomes more plausible that the groups are also similar along relevant unobserved dimensions. Our identification strategy relies on the assumption that, absent IRA-induced investment, treated and control counties would have followed parallel trends in Democratic vote share (conditional on fixed effects and entropy-balancing weights).

We balance on two categories of covariates, one set that predicts investments and another set that predicts voter behavior. The investment predictors at the county level include population size, corporate tax rates, right-to-work status, union membership, union coverage, solar radiation, public support and opposition for renewable energy, and the proportion of the working population employed in manufacturing, transportation, warehousing, utilities. The voting behavior predictors are both economic indicators – local unemployment, poverty rate, and wage rate – and demographics – age, sex, race, and education. Lastly, to these lists of covariates we also add lags of the outcome variable – Democratic vote share. For the presidential, House, and gubernatorial analyses, we include Democratic vote share for the relevant office in the 2016 and 2020 elections (e.g., for the presidential analysis we include vote shares from previous presidential elections). For the Senate, we use Democratic vote share in the 2018 Senate election. Matching on pre-treatment outcomes helps capture otherwise unobserved determinants of voting, including those with potentially time-varying effects.[7] Additional details on all variables and data sources are provided in Appendix A1.

To find the specific weights, w_i, for each control county, the entropy balancing method minimizes the loss function $\sum_{i:D_i=0} w_i \log\left(\frac{w_i}{q_i}\right)$, subject to the balance constraints on

[6]As a robustness check, Appendix A5.3 reports DiD estimates for the 2022 election for house, senate, and gubernatorial elections for the small group of counties that received project announcements before this election. The results are not substantively different from our main findings.

[7]See Abadie et al. (2010) and Abadie (2021) for a similar discussion of the role of lagged outcomes in improving balance on unobservables in synthetic control frameworks.

the moments, and where q_i are uniform base weights. The method picks a vector of nonnegative control weights that sum to n_1 (the sample size of the treated) while deviating as little as possible from uniform base weights q_i (Hainmueller and Xu, 2013).

Given the panel data structure, a key question is *when* to balance the covariates. We use 2019 values. Many covariates, especially economic indicators, could be 'bad controls' (Angrist and Pischke, 2009) in that they are likely affected by IRA investments, so their post-treatment values should not be used for balancing. Furthermore, 2019 is recent enough to reflect contemporary county environments. It also precedes the pandemic, thereby avoiding any potentially differential pandemic-related impacts.

Lastly, two threats to identification merit discussion. First, IRA-induced investments may generate compositional change if workers move into treated counties (or away from controls), which could affect vote shares independently of changes in individual voting behavior. We probe this channel by estimating effects separately by project status (announced vs. under construction/operating) with the assumption that mere announcement does not trigger migration and real economic changes to the same extent as construction and operation. The second confounder is spill-over effects from investments into voter behavior in control counties. Since the IRA is a national policy it affects voters in all counties, and if voters in control counties dislike that investments and tax credits flow into other regions, possibly due to a zero-sum view of the economy (Chinoy et al., 2023), they may express their dislike with the IRA by punishing the Democrats in the voting booth. This potential spill-over effect will make it hard to disentangle if the treatment effect is driven by changes in voter behaviour in the treated counties or control counties, or both. To the extent that the IRA is not highly salient to many voters, especially in control counties, such spillovers may be limited (and we provide evidence of such low salience in Section 6); nonetheless, they would bias our estimates toward a relative (treated vs. untreated) effect rather than a full general-equilibrium effect.

5 The Electoral Effects of the IRA

Figure 2 plots average county-level Democratic presidential vote share in treated counties and in the unweighted control group from 2000 to 2024. Treated counties are consistently more Democratic: before the IRA, the average Democratic vote share was 40.3 percent in treated counties versus 35.5 percent in control counties, a difference of 4.8 percentage points (p=0.027, one-tailed).[8] While the groups track broadly similar movements over time, the gap begins to widen after 2012 as Democratic vote share declines more sharply in control counties. These baseline level differences and emerging divergences motivate our use of entropy balancing to reweigh the control group and improve the credibility of

[8]Because these averages are unweighted by county population, smaller – typically more Republican – counties are overrepresented, which pulls average Democratic vote shares downward.

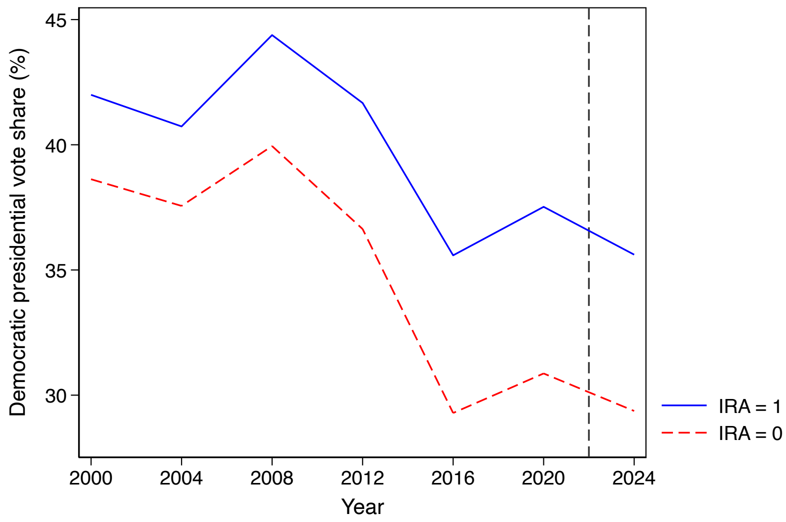


Figure 2: Democratic Presidential Vote Share by Treatment Status

Note: Solid (dashed) line shows the average Democratic presidential vote share in treated (control) counties. Both series are simple (unweighted) county averages.

the counterfactual in the DiD analysis.[9]

5.1 Main Results

Figure 3 shows the comparison between the average Democratic presidential vote share in treated counties with that of the weighted control group, where entropy balancing is used to account for pre-treatment differences. The weighted controls track the treated counties closely across the pre-treatment period (both level and trend). The mean difference in the pre-treatment periods is only 0.07 percentage points ($p = 0.49$, one-tailed), providing a substantially improved fit and stronger support for the parallel-trends assumption. Figure 3 lends supports to the use of the entropy-balanced DiD model as our main specification.

Table 3 presents the main weighted DiD estimates of IRA-linked private investment on Democratic vote share across our four election types. The point estimates are generally small but positive: 0.084 percentage points in the presidential election (column 1), 0.831 in House elections (column 2), and 0.995 for Class 1 Senate races comparing outcomes in 2018 to 2024 (column 3).[10] For gubernatorial elections, the estimate is 0.488 for the 2016–2020–2024 elections cohort (column 4). These results indicate that the Democratic vote share is slightly larger in treated counties in the posttreatment period compared to

[9]Figure A1 in Appendix A3 shows that entropy balancing was successful: covariate means are nearly identical between the treatment and control groups.

[10]Class 1 Senate races consist of the 33 Senate seats that were running for election in 2024, which took place in the following states: Arizona, California, Connecticut, Delaware, Florida, Hawaii, Indiana, Maine, Maryland, Massachusetts, Michigan, Minnesota, Mississippi, Missouri, Montana, Nebraska, Nevada, New Jersey, New Mexico, New York, North Dakota, Ohio, Pennsylvania, Rhode Island, Tennessee, Texas, Utah, Vermont, Virginia, Washington, West Virginia, Wisconsin, and Wyoming.

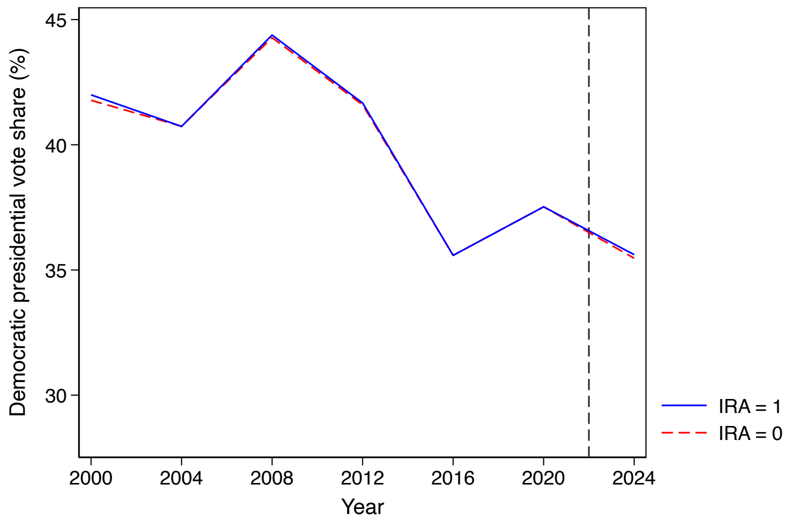


Figure 3: Democratic Presidential Vote Share by Treatment Status (Weighted Controls)

Note: Solid (dashed) line shows the average Democratic presidential vote share in treated (control) counties. The control series is a weighted average using the county-weights from the entropy balancing.

the counterfactual control group, with treatment effects larger for the more local elections (House, Senate, and governor) compared to the presidential election. That said, only the estimate for Senate elections reaches conventional levels of statistical significance. Though this result is sensitive and loses significance in our robustness checks (see below). Furthermore, we do not recover statistically significant coefficients in any of our other Senate-related analysis.

In addition to examining Democratic vote share, we carry out the same analysis for incumbent performance in Senate and gubernatorial races in Appendix A5.2. We find no statistically significant effects.

5.2 Heterogeneity Analysis

We turn next to investigating heterogeneous treatment effects. If IRA investments influenced voting through local economic benefits, we should arguably observe stronger electoral effects where projects are more developed and larger in scale compared to counties with merely announced projects. We therefore tested whether implementation stage matters by iteratively restricting the treatment sample in our DiD model (equation 1) to counties with projects at three separate phases: merely announced, under construction only, and operational only. A bit more than 40 percent of the treated counties (460 out of 1083) fall into one of these three categories while the rest has mixtures of projects at all stages of implementation. In this section we present results for presidential elections only. Results for House, Senate, and gubernatorial elections are in Appendix A5.1. They

Table 3: Treatment Effects on Democratic Vote Share by Election Type

	President (1)	House (2)	Senate (3)	Governor (4)
Post × Treated	0.084	0.831	0.995*	0.488
	(0.361)	(0.449)	(0.493)	(0.568)
Pretreatment Elections	2000-2020	2016-2022	2018	2016-2020
Observations	21,573	15,342	3,482	1,698
Treated Counties	1,083	1,082	726	145
Control Counties	1,999	1,999	1,015	421
R^2 (within)	0.315	0.145	0.436	0.276

Note: Difference-in-differences estimates with entropy-balanced weights of IRA-linked investment effects on Democratic vote shares across election types. Column 1 presents results for presidential elections; column 2 for House elections; column 3 for Senate elections (Class 1 seats, comparing 2024 to 2018); and column 4 for gubernatorial elections (counties voting in 2024, 2020, and 2016). All columns include outcome lags of Democratic vote share in the 2016 and 2020 elections, except for column (3) which includes an outcome lag for the 2018 election. Number of observations are measured as county × election-year. All specifications include county and time fixed effects. Standard errors clustered at the county level in parentheses; $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table 4: Treatment Effects on Presidential Vote Share by Project Implementation Stage

	Treatment Group Definition			
	Announced Only (1)	Construction Only (2)	Operating Only (3)	All Projects (4)
Post × Treated	−0.227	0.072	0.161	0.084
	(0.429)	(0.749)	(0.340)	(0.361)
Observations	15,176	14,294	15,728	21,574
Treated Counties	169	43	248	1,083
Control Counties	1,999	1,999	1,999	1,999
R^2 (within)	0.482	0.472	0.347	0.315

Note: Difference-in-differences estimates with entropy-balanced weights of IRA-linked private investment effects on Democratic presidential vote share. All specifications include county and time fixed effects. Columns 1-3 restrict treatment to counties with projects at a single implementation stage as of Dec 31 2024. Column 4 replicates the main specification from Table 3 for comparison. Standard errors clustered at the county level in parentheses; $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

are not substantively different to our findings here.

The results shown in Table 4 indicate that the further along the projects are in

Table 5: Treatment Effects on Presidential Vote Share by Investment Intensity

	Median Investment (1)	75th Percentile (2)	90th Percentile (3)	Mean Investment (4)	All Projects (5)
Post × Treated	−0.184	−0.255	−0.338	−0.184	0.084
	(0.310)	(0.427)	(0.588)	(0.516)	(0.361)
Observations	17,787	15,890	14,756	15,197	21,574
Treated Counties	542	271	109	172	1,083
Control Counties	1,999	1,999	1,999	1,999	1,999
R^2 (within)	0.471	0.516	0.573	0.544	0.315

Note: Difference-in-differences estimates with entropy-balanced weights comparing high-investment counties to all control counties. All specifications include county and time fixed effects. Columns (1)–(4) use alternative definitions of CAPEX investment intensity: median, 75th percentile, 90th percentile, and mean CAPEX thresholds. Column (5) reports the "All Projects" specification from the main results for comparison. Standard errors clustered at the county level in parentheses; $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

their development, the more positive is the effect on presidential Democratic vote share. Restricting the sample to counties with only operating projects (column 3) show a point estimate of 0.161 percentage points, around twice the size of the full sample (column 4). Counties with only projects under construction (column 2) show an estimate of 0.072, which is close to the coefficient for all projects, while counties with only announced projects (column 1) instead show a negative point estimate of -0.227. But, similar to the presidential results above, none of the estimates approach statistical significance, with large standard errors relative to the point estimates.

Furthermore, the specification in column (1) speaks to a potential confounder: compositional change through migration. Announcements alone are arguably less likely to trigger substantial labor inflows or other population sorting compared to projects that enter construction or operation. If migration matters, we would expect larger positive effects on Democratic vote share in counties with active projects, because workers relocating in response to construction and operation may be more supportive of the IRA and thus more inclined to reward Democrats. Consistent with this intuition, the point estimate is more positive in columns (2)-(4) than in the "announced only" sample (column 1). However, the estimates are imprecise and the difference between them is not statistically meaningful, so we find no clear evidence that migration-driven compositional change is impacting our results.

Table 5 examines whether investment scale matters. If larger capital outlays generate proportionally larger electoral benefits, we would expect counties with greater per-capita investment to show stronger positive effects on Democratic vote share. Presidential results for counties with above-median per-capita investment ($2,287 in 2023 USD), above-75 percentile per-capita investment (\$12,059), above-90 percentile investment (\$45,651), and above-mean investment (\$22,945) are shown in columns 1-4, respectively. Counterintuitively, high-investment counties display negative point estimates – directionally opposite from the full-sample estimate – though, again, the estimates are not statistically different from zero.

Taken together, the heterogeneity analyses reveal no evidence that either project maturity or investment intensity systematically affects Democratic vote share in presidential, Senate, House, or gubernatorial elections. The absence of detectable effects even among operating projects and high-investment counties – where economic impacts should be most visible – strengthens our conclusion from the main analysis that IRA-linked investments generate no consistent electoral impact. This pattern of results across specifications suggests the explanation lies not in measurement or sample composition, but in other factors that shaped the IRA's impact on voters, which we explore in Section 6 below.

5.3 Robustness Tests and Additional Analyses

To check the robustness of our results, we re-estimate the main results using an alternative lag structure and an alternative specification – event study analysis (see Appendix A4). The results do not change. Furthermore, our main result for Senate elections from Table 4 above loses statistical significance.

We then undertake additional analyses to further explore the relationship between IRA-related investment and vote outcomes (see Appendix A5). First, we investigate heterogeneous treatment effects (by project status and investment intensity) for House, Senate, and gubernatorial elections. Second, we examine whether investments had an effect on incumbent vote share, regardless of party. Third, we rerun the analysis and classify the 2022 election as a post-treatment election. Fourth, we examine whether NIMBY-related concerns about wind and solar projects generated backlash against these projects in particular (Jarvis, 2025; O'Neil, 2021; Stokes, 2016; Wolsink, 2007). Fifth, we examine politically competitive settings by restricting the sample to swing states and swing counties (counties where the Democratic vote share in the 2020 General Election lies within 5 percentage points of 50 percent). Across all models, the results are the same. We find no statistically significant electoral effects.

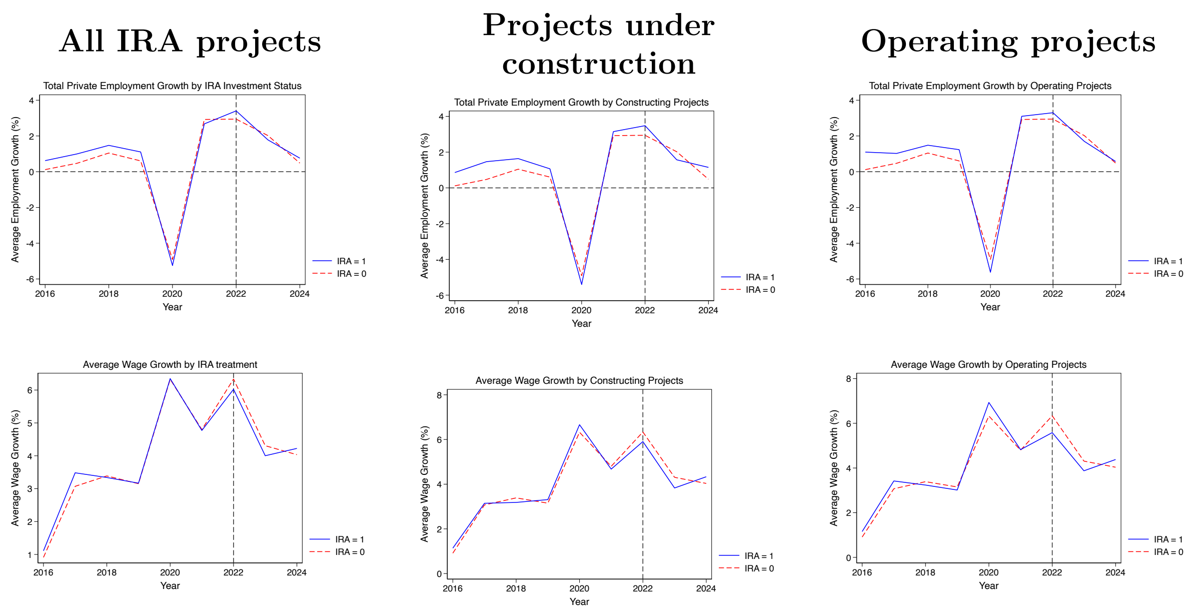
6 Explaining Our Findings

In this section, we consider several non-mutually-exclusive hypotheses that can explain our findings, including the extent to which: the IRA produced local economic benefits, voters were aware of the policy and could attribute benefits to politicians, partisan polarization prevented Republican voters from rewarding the Democrats, and voters believed that the promised future benefits of the policy would actually materialize. We end by discussing the significance of our findings in the context of previous studies that tend to find politicians are punished for ambitious mitigation policies.

6.1 Local Economic Benefits

We turn first to assessing the extent to which the IRA generated local economic benefits. Figure 4 compares average employment and wage growth in treated versus control counties since 2016. Across all plots, treated and control trajectories are nearly indistinguishable from 2016 to 2024. Neither employment nor wage growth accelerated after the IRA's passage in 2022 in the treated counties. If anything, both measures slowed down at similar rates in the post-treatment period. This pattern suggests that private IRA-related investment may not have produced material gains in treated counties before the 2024 election that were large enough to affect voter support.

Figure 4: Average employment and wage growth by IRA investment



Notes: Blue solid lines represent counties with IRA projects, red dotted lines represent counties without IRA projects. The top row in each column shows average private employment growth (year-over-year percent change in private-sector employment). The bottom row shows average private wage growth (year-over-year percent change in private-sector average pay). Column 2 compares counties with projects under construction with control counties; and column 3 compares counties with operating projects with control counties. All values are unweighted. Source of data: US Bureau of Labor Statistics.

One potential reason for the lack of employment and wage growth could be the relatively low labor- and capital-intensity of solar projects. As Tables 1 and 2 show, the majority of IRA projects were related to solar energy, which also has among the lowest average capital expenditures relative to other project types. Prior studies suggest that solar energy generates the most employment during construction, installation, and manufacturing, while ongoing operations and management require relatively few workers (Cai et al., 2017; Wei et al., 2010). In our sample, around a third of IRA projects (916 of the 2,672) were operating solar projects at the time of the election, which may help to explain why we do not observe larger economic effects in Figure 4.

6.2 Policy Awareness and Salience

In addition to, and plausibly as a consequence of, a lack of economic benefits, voters may not have known about the IRA or linked it to local economic activity. Existing polling data suggests that public awareness of the policy was consistently low in the run up to the election. In August 2023, one year after the policy was enacted, seven in ten Americans had heard little or nothing about it (Romm et al., 2023). By 2024, more of the public seemed to know about the policy, but large segments were still unfamiliar with it. A poll in April found that roughly 35 to 45 percent of US adults were unfamiliar with the IRA and its impacts (AP-NORC, 2024). Similarly, in June, a study found that 61% of registered voters had heard nothing or little about the policy (Leiserowitz et al., 2024).

Even in the absence of broad policy awareness, voters might still reward incumbents if they observed new IRA-related projects in their local communities. Yet recognition of such investments appears uneven. Recent evidence suggests that visibility depends on both physical proximity and the distinctiveness of projects relative to the local economic landscape. People living close to new IRA facilities were more likely to notice and correctly identify them, while projects in dense urban areas were often overlooked and less salient than facilities in rural counties (Gazmararian et al., 2025). Thus, if IRA-related investments were not sufficiently visible or distinctive in many communities, voters may not have recognized them as new economic opportunities, let alone attributed them to the Democratic party.

6.3 Credit Claiming and Attribution

Since the IRA's passage, credit-claiming has been diffuse and contested. In addition to Democratic incumbents, Republican governors, in particular, have frequently sought to associate themselves with IRA-facilitated projects, even while criticizing the legislation itself. For instance, Georgia's Republican Governor Brian Kemp publicly condemned the IRA while attending a groundbreaking ceremony for a company that had received more than $100 million in federal funding from the IRA (Associated Press, 2023). Similar patterns are evident elsewhere: Tennessee's Republican Governor Bill Lee participated in the opening of an LG battery plant (LG Chem, 2023), Oklahoma's Republican Governor Kevin Stitt welcomed Enel's new solar panel factory (Reuters, 2024), and South Carolina's Republican Governor Henry McMaster celebrated Redwood Materials' new battery facility (Mejia, 2022). Beyond these symbolic appearances, a number of GOP lawmakers also urged Congress to retain key IRA tax credit provisions before and after the election (Brugger, 2025; Budryk, 2024; Siegel and Bikeles, 2025). This contested credit claiming may have made it difficult for voters to attribute the policy to the Democrats and reward them for it.

Beyond attributing the IRA to a political party, recent studies suggest that voters tended to credit their governors for new green projects, rather than the president or the federal government (Gazmararian et al., 2025). This may further explain why we do not find significant effects for presidential or congressional elections. Yet, we also do not find significant effects for gubernatorial elections, indicating that even this attribution may not have translated into consistent electoral rewards.

6.4 Partisan Polarization

Partisan polarization has been a long-standing feature of climate policymaking and politics in the US. Republican elites have tended to downplay the importance of climate change and oppose climate action, and Republican voters have tended to reflect these positions in their opinions (Karol, 2019; Leiserowitz et al., 2024; McCright and Dunlap, 2011).

This context of polarization matters for understanding the electoral effects of the IRA. Its passage followed a strictly partisan path, with every Democratic member of Congress voting in favor and every Republican opposed, culminating in Vice President Kamala Harris casting the decisive tie-breaking vote in the Senate. Additionally, there was a clear partisan divide in terms of voter knowledge and support. Even though the majority of IRA projects fell within Republican states and districts (see figure 1), polling data shows that Republican voters were much less likely to have heard about the IRA, support it, or think it would help them or their family, or help with jobs or the US economy (Leiserowitz et al., 2024). Given partisan polarization on the issue, Republican voters may have been unwilling to support the IRA and cross party lines to reward Democrats for it, even if they experienced tangible material benefits.

6.5 Expectations of Future Benefits

Another possibility is that voters did not believe that the IRA would credibly deliver on its promises of economic benefits and climate mitigation. To be sure, the IRA had already delivered billions of dollars in federal spending and private investment by the time of the election (Blaeser and Tamborrino, 2024), and the Democrats were showing no sign that they would reverse course under a Kamala Harris administration. Rather it was the Trump campaign that was signaling their preference to repeal it (Tamborrino, 2024).

That said, voters may still not have believed that the IRA's benefits would materialize in the future, regardless of the government's formal commitments and past performance in delivering it. Such perceptions are likely to be shaped by trust in government (Fairbrother et al., 2021; Jacobs and Matthews, 2012), which was at historic lows in the US (Pew Research Center, 2025). As a result, voters across the board may have been skeptical of the policy. Additionally, partisan identification may have moderated these perceptions, as voters tend to be more inclined to trust promises made by co-partisan politicians (Bianco, 1994; Goldstein and Wiedemann, 2022). Democratic voters may have been more willing to believe in the IRA's long-term benefits, while Republicans remained unconvinced.

6.6 The Absence of Voter Backlash

While we find no consistent evidence of electoral reward, we also do not find evidence of electoral punishment. This matters for the political economy of climate policy, where political backlash has often dominated prior studies of electoral effects (Colantone et al., 2024; Egli et al., 2022; Furceri et al., 2023; Gazmararian, 2025; Pepin-Proulx, 2025; Stokes, 2016; Weber, 2020) and scholars have focused on strategies for overcoming voter opposition to policy change, such as insulating the policymaking process and compensating policy losers (Finnegan et al., 2025; Finnegan, 2022b,a; Meckling et al., 2022). Crucially, our findings indicate that the adoption of significant mitigation policies does not necessarily portend electoral pain for incumbents. Indeed, the absence of electoral backlash for a mitigation policy on the scale of the IRA suggests optimism that GIP, when designed in a way that minimizes short-term costs for voters and business, can be a politically-viable path forward for urgently needed decarbonization.

That said, it is also true that elements of the IRA have been successfully rolled back by the second Trump administration. The lack of widespread electoral support may have made these efforts politically easier. Still, groups of supporters did publicly defend the policy, including Democratic lawmakers, several Republican legislators (Brugger, 2025; Budryk, 2024; Siegel and Bikeles, 2025), firms in renewable energy and clean manufacturing (PBS News, 2025; Volcovici, 2024), labor unions (Gunn, 2025; Prescod, 2025; Zhu, 2025), and advocacy groups (ACP, 2025; Volcovici, 2025). While their actions did not prevent policy change, they did likely prevent wholesale policy reversal. Furthermore, these groups suggest that a constituency of supporters is emerging to sustain the policy, which may be evidence of policy feedback (Meckling et al., 2015, 2017), even in the absence of widespread voter support.

7 Conclusion

Governments around the world are increasingly turning to green industrial policy as a way to reduce emissions and increase economic prosperity. By generating economic benefits over the short- to medium-term and minimizing short-term costs, GIP has the potential to generate greater political support compared to conventional mitigation policies with higher and more visible short-term costs, such as carbon pricing. Yet despite the increasing use of GIP, surprisingly little is known about its real world political effects.

We assess the impact of green industrial policy on electoral outcomes using the case of the US Inflation Reduction Act. The IRA is the most ambitious climate change policy ever enacted by the US. Moreover, because it seeks to generate both economic benefits and reduce emissions, it is an important case of large-scale green industrial policy. The policy utilizes tax credits to mobilize investment in the production and deployment of a broad array of clean technologies, while imposing no costs on voters or most businesses. Given this distributive profile, we argue that the IRA is a most likely case for generating voter support and limiting backlash.

Our identification strategy relies on the fact that IRA tax credits incentivized private sector investment in some, but not all, US counties. Employing a difference-in-differences design with entropy balancing, our analysis leverages geographic variation in private investment flows at the county level to identify how differential exposure to IRA-induced investment impacted electoral outcomes for presidential, congressional (House and Senate), and gubernatorial elections in 2024. The results suggest that IRA-induced private investments generated neither electoral reward nor punishment for the Democrats. Similarly, we find no evidence of reward or punishment for incumbents, regardless of party. Examining a wide range of heterogeneousness treatment effects yields similar findings.

Several factors are likely driving our results. Evidence suggests that the policy produced limited economic benefits in treated counties in the run up to the election. At the same time, and plausibly as a consequence of limited material benefits, polls consistently showed limited voter awareness of the IRA or its effects. To the extent visible material benefits and policy awareness are critical to voter support for green industrial policy, these appear to be absent in the case of the IRA before the election. Furthermore, contested credit claiming may have made it difficult for voters to attribute the policy to the Democrats. Deep political polarization may have also limited voters' willingness—particularly in Republican districts—to reward Democrats for the policy, even if they stood to benefit from it. Last, it is important to emphasize that our analysis is limited to the business tax credit component of the IRA. It could be that the consumer tax credits and federal expenditure components generated clearer electoral rewards.

While the IRA did not produce an electoral windfall, it also did not generate voter discontent. This is especially noteworthy given the scale of the policy and the polarized and contentious climate policymaking context in the US. Furthermore, it stands in stark contrast to existing studies on the electoral effects of mitigation policy that tend to find voter opposition (Colantone et al., 2024; Egli et al., 2022; Furceri et al., 2023; Pepin-Proulx, 2025; Stokes, 2016). Our findings highlight how green industrial policy, when designed in a way that minimizes short-term costs, may better reduce electoral punishment compared to other types of policy instruments. In this way, our study underscores the importance of a climate policy's distributive profile in driving its electoral effects.

Additional research on the electoral effects of green industrial policy is needed. Findings may be different in contexts where, for example, economic benefits are larger and more visible, the policy is more salient and voters have greater awareness of it, and voters are less polarized and more willing to reward incumbents, regardless of party. Furthermore, GIP can take different forms. Scholars should examine the electoral effects of a wide variety of GIP policy mixes and distributive profiles.

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Appendix

A1 Data Description and Sources

This section describes our data. Table A1 summarizes our outcome, treatment, and control variables and their sources and table A2 shows covariate means for the treated, control, and full samples.

We obtained data on private investment facilitated by the IRA from the Clean Investment Monitor dataset (Rhodium Group, 2025), which is a joint project of Rhodium Group and MIT's Center for Energy and Environmental Policy Research (CEEPR). Our election results data are gathered from the MIT Election Data and Science Lab and Dave Leip's Atlas of US Presidential Elections (Leip, 2025; MIT Election Data and Science Lab, 2025). Our sociodemographic control variables are gathered from the American Community Survey of the US Census Bureau (US Census Bureau, 2025). The annual average employment and wages data are collected from the Quarterly Census of Employment and Wages dataset by the US Bureau of Labor Statistics (US Bureau of Labor Statistics, 2025). The data on which states have enacted Right to Work laws are gathered from the National Right to Work Committee (National Right To Work Committee, 2024). Public opinion on renewable energy is collected from the Yale Climate Opinion Maps (Howe et al., 2015). Finally, data on solar radiation, measured as the annual average global horizontal irradiance, in kilojoules per square meter, at the county level, is collected from the National Environmental Public Health Tracking Network (National Environmental Public Health Tracking Network, 2024).

Table A1: Data Description and Sources

Variable Name	Description	Time Range	Source(s)
Treated	The treatment assignment variable. Equals 1 if the county has IRA-related private investment after the IRA was signed into law, and 0 otherwise.	2022 - 2024	Clean Investment Monitor
Election results	Vote share proportion for a party or candidate (candidate votes divided by total cast votes per county). Presidential elections data is available from 2000; Senate, House, and gubernatorial data is available from 2016.	2000 - 2024; 2016 - 2024; 2016 - 2024	MIT Election Data and Science Lab; Dave Leip's Atlas of U.S. Presidential Elections
Total population	Estimated total population residing in a county in a given year.	2019	
Median household income	Income of all members in the household in the past 12 months. Median based on total number of households.	2019	
Unemployment rate	Labor force data annual average; Unemployed population divided by total employees.	2019	
Race	The proportion of white residents in the county.	2019	
Gender	The proportion of male residents in the county.	2019	
Education	The proportion of residents with a bachelor's degree and above.	2019	US Census Bureau: American Community Survey
Household poverty rate	The proportion of households that lived in poverty in the past 12 months.	2019	
Median age	The age that divides the population into two equal-size groups.	2019	
Employed Workers in TWU	The proportion of the working population that works in transportation, warehousing, or utilities.	2019	
Employed Workers in Manufacturing	The proportion of the working population that works in manufacturing.	2019	
Annual average employment	Annual average of monthly employment levels for a given year based on employment levels for a given year.	2016 - 2024	US Bureau of Labor Statistics: Quarterly Census of Employment and Wages

Continued on next page

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Table A1 continued

Variable Name	Description	Time Range	Source(s)
Annual average wages	Average annual pay based on wage levels for a given year.	2016 - 2024	
Union membership	The percentage of employed workers who are members of a union. We have data on the whole private industry, in manufacturing, and in the construction industry.	2019	Macpherson & Hirsch (2023)
Union coverage	The percentage of employed workers who are covered by a union. We have data on the whole private industry, in manufacturing, and in the construction industry.	2019	
Corporate tax rates	The top corporate tax rate a state charges on a company's revenues for that county.	2019	Tax Foundation
Right to work status	A dummy variable that indicates if a county is located in a state that has a "right to work" law. Equals "1" if the state has "right to work" law and "0" if not.	2019	National Right to Work Committee
Public support for renewable energy	Estimated percentage of who somewhat/strongly support funding research into renewable energy sources.	2019	Yale Climate Opinion Maps (Howe et al., 2015)
Public opposition for renewable energy	Estimated percentage of who somewhat/strongly oppose funding research into renewable energy sources.	2019	
GHI	A time-invariant covariate that records the annual average global horizontal irradiance (GHI), in kilojoules per square meter, level at that county in 2020.	2020	The National Environmental Public Health Tracking Network

Note: All variables are measured at the county level apart from union membership, right to work status, and corporate tax rates, which are measured at the state level.

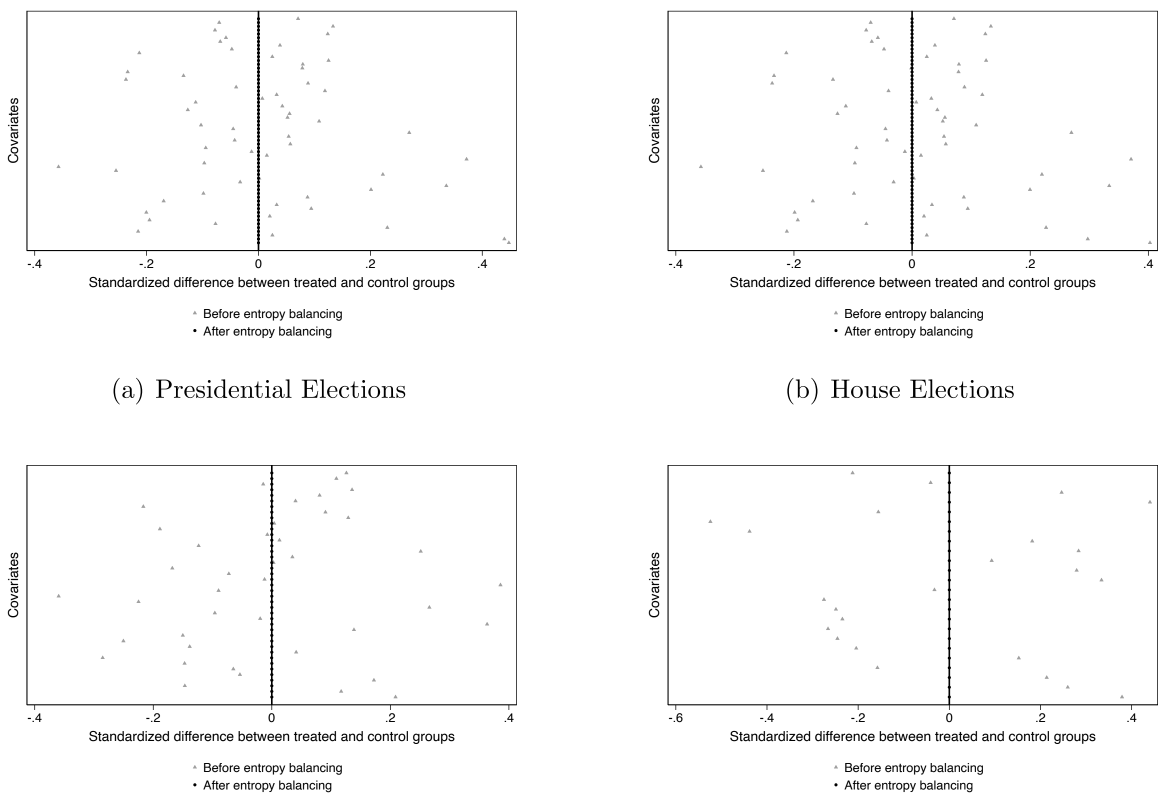
Table A2: Descriptive statistics (means) by treatment status

Variables	Control (Unweighted)	Treatment	Full sample
Total Population	54,043.551	195,000	103,000
Male Population (%)	50.121	49.926	50.053
Median age	42.095	40.297	41.468
White Population (%)	84.650	80.680	83.264
Residents with Higher Education (%)	21.240	23.212	21.928
Employed Workers in Manufacturing (%)	12.311	12.349	12.324
Employed Workers in TWU (%)	5.596	5.548	5.579
Median Income	51,658.318	56,297.906	53,277.613
Unemployment Rate (%)	2.872	3.106	2.953
Household Poverty Rate (%)	10.998	10.464	10.812
Union-Member Workers in Private Industry (%)	5.110	5.382	5.205
Union-Member Workers in Manufacturing (%)	8.483	7.757	8.230
Union-Member Workers in Construction (%)	11.145	11.564	11.291
Union-Covered Workers in Private Industry (%)	5.892	6.196	5.998
Union-Covered Workers in Manufacturing (%)	9.546	8.623	9.224
Union-Covered Workers in Construction (%)	12.161	12.470	12.269
Right to Work Status	0.736	0.644	0.704
Corporate Tax Rates	5.786	5.543	5.701
Public Support for Renewable Energy (%)	80.719	81.300	80.922
Public Opposition for Renewable Energy (%)	18.850	18.254	18.642
GHI (kJ /m^2)	3,644.303	3,655.850	3,648.360

A2 Covariate Balance

Figure A1 shows covariate balance plots for our main results (Table 3 of the main text). Panel (a) shows covariate balance for presidential elections, (b) shows the covariate balance for House elections, (c) for Senate elections and (d) for gubernatorial elections. All covariates are balanced using the covariates we describe in Section 4 *Entropy Balancing* of the main paper.[11] All covariance balance plots include their election-specific democratic outcome lags in 2016 and 2020, except for Senate results which includes the Senate outcome lag in 2018. Across all election types, good covariate balance has been achieved.

Figure A1: Covariate balance plots



(a) Presidential Elections

(b) House Elections

(c) Senate Elections

(d) Gubernatorial Elections

Note: Covariate names are omitted because the large number of covariates would make the y-axis labels unreadable. Standardized difference for each covariate is calculated as $\frac{m_t - m_c}{\sqrt{(v_t + v_c)/2}}$, where m_t and v_t are the means and variance of a covariate for treated counties, and m_c and v_c are the means and variance of a covariate for control counties.

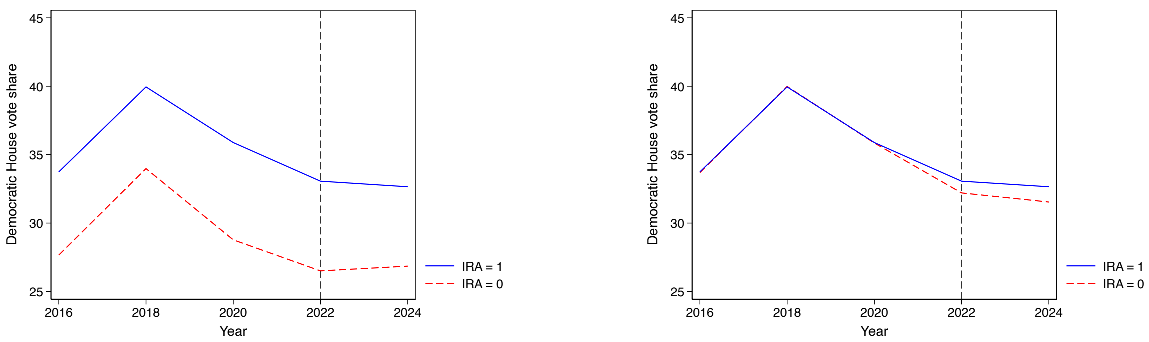
[11]These are: corporate tax rates, union membership, solar radiation, public support for funding renewable energy, sectoral composition of the local economy, local unemployment, poverty rate, wage rate, age, sex, race, and education.

A3 Parallel Trends Plots

In the main article we show the parallel trends plot for vote shares in presidential elections. Here we provide parallel trends plot for House and gubernatorial election results, which are shown in figures A2 and A3. We do not show the same plots for Senate elections because for Class 1 Senate elections, data are available only in 2018 and 2024, leaving us with one pre-treatment and one post-treatment observations. As a result, we cannot assess or plot pre-treatment parallel trends for the Senate results.

Plots are shown for the unweighted and weighted samples. The weighted samples are based on entropy balancing using the same set of covariates we use for the main results (see the first footnote in the Appendix above) and vote share lags in 2016 and 2020.

Figure A2: Parallel trends plot for House election results



(a) Unweighted (b) Weighted

Note: Solid (dashed) line shows the average Democratic House vote share in treated (control) counties. On the left, both series are simple unweighted county averages. On the right, the control series is a weighted average using the county weights from the entropy balancing.

Figure A3: Parallel trends plot for gubernatorial election results

(a) Unweighted (b) Weighted

Note: Solid (dashed) line shows the average Democratic gubernatorial vote share in treated (control) counties. On the left, both series are simple unweighted county averages. On the right, the control series is a weighted average using the county weights from the entropy balancing.

A4 Robustness Checks

A4.1 Main Results without Balancing on Lags of the Outcome

We reproduce our main results but this time without balancing on lags of the outcome (vote share in previous elections). We otherwise balance on the same covariates as the main results (see the first footnote in the Appendix above). Table A3 shows the effects of IRA private investment on presidential, House, Senate, and gubernatorial elections. We see positive, but statistically insignificant, coefficients.

Table A3: Treatment Effects on Democratic Vote Share by Election Type without Lags of the Outcome

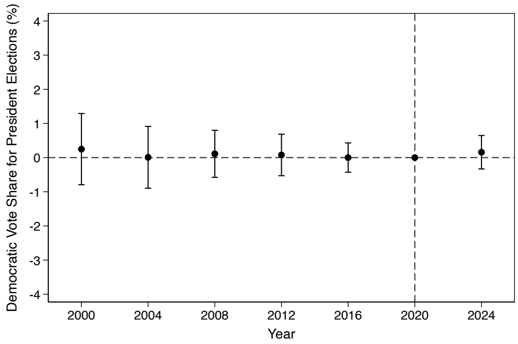
	President (1)	House (2)	Senate (3)	Governor (4)
Post × Treated	0.667	0.972	0.802	0.425
	(0.405)	(0.537)	(0.501)	(0.569)
Pretreatment Elections	2000–2020	2016–2022	2018	2016–2020
Observations	21,573	15,342	3,482	1,698
Treated Counties	1,083	1,082	726	145
Control Counties	1,999	1,999	1,015	421
R^2 (within)	0.350	0.143	0.428	0.277

Note: Difference-in-differences estimates with entropy-balanced weights of IRA-linked private investment on Democratic vote shares. No outcome lags are included in the entropy balancing procedure. Column 1 reports presidential elections; column 2 for House elections; column 3 for Senate elections (Class 1 seats, comparing 2024 to 2018); and column 4 or gubernatorial elections (counties voting in 2024, 2020, and 2016). Number of observations are measured as county × election-year. All specifications include county and time fixed effects. Standard errors are clustered at the county level and shown in parentheses; $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

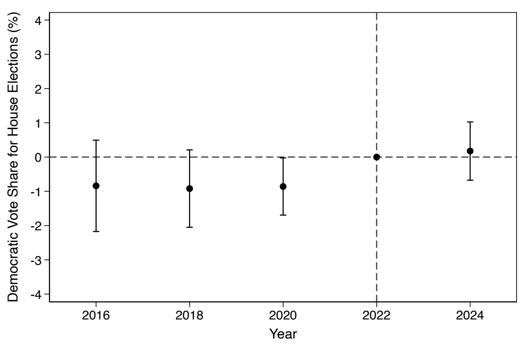
A4.2 Alternative Specification: Event-study Analysis

Our main results rely on a DiD framework. Here we reproduce our main results for presidential, House and gubernatorial elections using an alternative specification - event-study analysis, which also provides a direct check of the parallel trends assumption via placebo tests in pre-treatment periods. We exclude Senate elections here because the two Senate election periods are too short for an event-study, which requires at least one pre-treatment reference period. As figure A4 shows, the results are similar to our main findings: IRA-induced investments have little effect on Democratic vote shares.

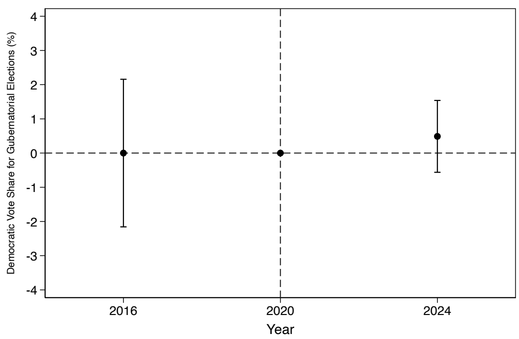
Figure A4: IRA investment effects event-study



(a) President



(b) House



(c) Governor

Note: The outcome is the vote share received by Democratic candidates. Panels (a) - (c) present results for the US presidential, House, and gubernatorial elections, respectively. Vertical dotted lines represent the last election period before the adoption of the IRA. All event-study specifications use entropy-balanced weights from covariates we describe in Section 4 and include their election-specific outcome lags in 2016 and 2020. Estimates are from event-study models with 95% confidence intervals.

A5 Additional Analyses

A5.1 Additional analyses of heterogeneous treatment effects

In the main text we report heterogeneous analysis by project implementation stage and investment intensity for presidential elections only. Here we provide results of the same heterogeneous analysis for House, Senate, and gubernatorial elections in 2024. Tables A4 and A5 show the results for House elections, tables A6 and A7 for Senate elections, and tables A8 and A9 for gubernatorial elections.

Similar to the findings for presidential elections in the main text, we find little evidence of heterogeneous treatment effects for House, Senate, or gubernatorial elections.

Table A4: Treatment Effects on House Democratic Vote Share by Project Implementation Stage

	Treatment Group Definition			
	Announced Only (1)	Construction Only (2)	Operating Only (3)	All Projects (4)
Post × Treated	0.111	0.236	−0.164	0.831
	(0.660)	(1.085)	(0.438)	(0.449)
Pretreatment Elections	2016–2022	2016–2022	2016–2022	2016–2020
Observations	10,800	10,173	11,191	15,342
Treated Counties	169	43	248	1,082
Control Counties	1,999	1,999	1,999	1,999
R^2 (within)	0.160	0.179	0.098	0.145

Note: Difference-in-differences estimates with entropy-balanced weights of IRA-linked private investment effects on Democratic *House* vote share. All specifications include county and time fixed effects. Columns (1)-(3) restrict treatment to counties with projects at a single implementation stage as of Dec 31 2024. Column (4) reports the "All Projects" specification from the main results for comparison, it is equivalent to column 2 in Table 3. All columns include outcome lags for House democratic vote share in 2016 and 2020. Standard errors clustered at the county level in parentheses; $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table A5: Treatment Effects on House Democratic Vote Share by Investment Intensity

	Median Investment (1)	75th Percentile (2)	90th Percentile (3)	Mean Investment (4)	All Projects (5)
Post × Treated	0.439	1.082	1.632	1.310	0.831
	(0.509)	(0.640)	(0.888)	(0.776)	(0.449)
Pretreatment Elections	2016–2022	2016–2022	2016–2022	2016–2022	2016–2020
Observations	12,657	11,309	10,503	10,815	15,342
Treated Counties	542	271	109	172	1,082
Control Counties	1,999	1,999	1,999	1,999	1,999
R^2 (within)	0.159	0.167	0.248	0.203	0.145

Note: Difference-in-differences estimates with entropy-balanced weights comparing high-investment counties to all control counties. All specifications include county and time fixed effects. Columns (1)–(4) use alternative definitions of CAPEX investment intensity: median, 75th percentile, 90th percentile, and mean CAPEX thresholds. Column (5) reports the "All Projects" specification from the main results for comparison, it is equivalent to column 2 in Table 3. All columns include outcome lags for House democratic vote share in 2016 and 2020. Standard errors clustered at the county level in parentheses; $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table A6: Treatment Effects on Senate Democratic Vote Share by Project Implementation Stage

	Treatment Group Definition			
	Announced Only (1)	Construction Only (2)	Operating Only (3)	All Projects (4)
Post × Treated	0.222	−0.193	0.525	0.995*
	(0.636)	(0.992)	(0.415)	(0.493)
Pretreatment Elections	2018	2018	2018	2018
Observations	2,226	2,080	2,338	3,482
Treated Counties	98	25	154	726
Control Counties	1,015	1,015	1,015	1,015
R^2 (within)	0.450	0.585	0.484	0.436

Note: Difference-in-differences estimates with entropy-balanced weights of IRA-linked private investment effects on Democratic *Senate* vote share. All specifications include county and time fixed effects. Columns (1)-(3) restrict treatment to counties with projects at a single implementation stage as of Dec 31 2024. Column (4) reports the "All Projects" specification from the main results for comparison, it is equivalent to column 3 in Table 3. All columns include the lag of Senate Democratic vote share in 2018. Standard errors clustered at the county level in parentheses; $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table A7: Treatment Effects on Senate Democratic Vote Share by Investment Intensity

	Median Investment (1)	75th Percentile (2)	90th Percentile (3)	Mean Investment (4)	All Projects (5)
Post × Treated	0.167	−0.201	−0.227	−0.220	0.995*
	(0.363)	(0.466)	(0.592)	(0.554)	(0.493)
Pretreatment Elections	2018	2018	2018	2018	2018
Observations	2,724	2,400	2,214	2,280	3,482
Treated Counties	347	185	92	125	726
Control Counties	1,015	1,015	1,015	1,015	1,015
R^2 (within)	0.446	0.422	0.362	0.390	0.436

Note: Difference-in-differences estimates with entropy-balanced weights comparing high-investment counties to all control counties. All specifications include county and time fixed effects. Columns (1)–(4) use alternative definitions of CAPEX investment intensity: median, 75th percentile, 90th percentile, and mean CAPEX thresholds. Column (5) reports the "All Projects" specification from the main results for comparison, it is equivalent to column 3 in Table 3. All columns include the lag of Senate Democratic vote share in 2018. Standard errors clustered at the county level in parentheses; $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table A8: Treatment Effects on Gubernatorial Democratic Vote Share by Project Implementation Stage

	Treatment Group Definition			
	Announced Only (1)	Construction Only (2)	Operating Only (3)	All Projects (4)
Post × Treated	0.459	−0.688	0.044	0.488
	(1.165)	(1.714)	(0.793)	(0.568)
Pretreatment Elections	2016–2020	2016–2020	2016–2020	2016–2020
Observations	1,344	1,290	1,347	1,698
Treated Counties	27	9	28	145
Control Counties	421	421	421	421
R^2 (within)	0.340	0.612	0.294	0.276

Note: Difference-in-differences estimates with entropy-balanced weights of IRA-linked private investment effects on Democratic *Governor* vote share. All specifications include county and time fixed effects. Columns (1)-(3) restrict treatment to counties with projects at a single implementation stage as of Dec 31 2024. Column (4) reports the "All Projects" specification from the main results for comparison, it is equivalent to column 4 in Table 3. All columns include outcome lags for Governor Democratic vote share in 2016 and 2020. Standard errors clustered at the county level in parentheses; $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table A9: Treatment Effects on Gubernatorial Democratic Vote Share by Investment Intensity

	Median Investment (1)	75th Percentile (2)	90th Percentile (3)	Mean Investment (4)	All Projects (5)
Post × Treated	−0.140	−0.377	0.289	0.103	0.488
	(0.631)	(0.862)	(1.230)	(0.996)	(0.568)
Pretreatment Elections	2016–2020	2016–2020	2016–2020	2016–2020	2016–2020
Observations	1,497	1,407	1,329	1,365	1,698
Treated Counties	78	48	22	34	145
Control Counties	421	421	421	421	421
R^2 (within)	0.449	0.496	0.471	0.506	0.276

Note: Difference-in-differences estimates with entropy-balanced weights comparing high-investment counties to all control counties. All specifications include county and time fixed effects. Columns (1)–(4) use alternative definitions of CAPEX investment intensity: median, 75th percentile, 90th percentile, and mean CAPEX thresholds. Column (5) reports the "All Projects" specification from the main results for comparison, it is equivalent to column 4 in Table 3. All columns include outcome lags for Governor Democratic vote share in 2016 and 2020. Standard errors clustered at the county level in parentheses; $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

A5.2 Analysis of incumbents

Here we present results for incumbent Senators and Governors. We do not present incumbent House results because House elections are recorded at the county level in our data, but counties overlap multiple congressional districts, so we cannot reliably assign a single incumbent representative at the county level.

Table A10 shows the effect IRA-related investment across five election types. Column (1) covers Senate Class 1 counties and compares 2024 to 2018 (same as our main specification). Column (2) covers Senate Class 3 counties[12] and compares 2022 to 2016. Column (3) reports gubernatorial incumbent vote share for states that voted in 2016, 2020, and 2024, with 2024 as post-treatment and 2016 and 2020 as pre-treatment periods. Column (4) reports gubernatorial incumbent vote share for states that voted in 2019 and 2023, with 2023 as post-treatment and 2019 as pre-treatment. Column (5) reports gubernatorial incumbent vote share for states that voted in 2018 and 2022, with 2022 as post-treatment and 2018 as pre-treatment.

Again, similar to our main results, we do not find evidence that incumbents were rewarded or punished for IRA-induced investments.

Table A10: Treatment Effects on Incumbent Vote Share for Senators and Governors

	Senate (Class 1)	Senate (Class 3)	Governor (I)	Governor (II)	Governor (III)
	(1)	(2)	(3)	(4)	(5)
Post × Treated	0.357	2.090	−0.507	−1.289	2.325
	(1.158)	(1.454)	(1.060)	(2.710)	(1.428)
Pretreatment Elections	2018	2016	2016–2020	2019	2018
Observations	3,556	3,184	1,132	474	3,341
Treated Counties	716	228	145	32	305
Control Counties	1,062	1,364	421	205	1,321
R^2 (within)	0.013	0.067	0.011	0.574	0.439

Note: Difference-in-differences estimates with entropy-balanced weights of IRA-linked private investment on incumbent vote shares. Column (1) reports Senate Class 1 results comparing 2024 to 2018 and includes Senate incumbent outcome lag in 2018; column (2) reports Senate Class 3 results comparing 2022 to 2016 and includes Senate incumbent outcome lag in 2016. Columns (3) and (4) report gubernatorial incumbent vote share results for counties voting in 2024–2020–2016 and 2023–2019, and includes gubernatorial incumbent outcome lags in 2016 and 2020, and 2019 respectively. Column (5) reports gubernatorial incumbent vote share results for counties voting in 2022–2018, and includes gubernatorial incumbent outcome lag in 2018. Observations are county × election-year. All specifications include county and time fixed effects. Standard errors are clustered at the county level and shown in parentheses; $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

[12]Class 3 Senate races consist of the 34 Senate seats that were running for election in 2022, which took place in the following states: Alabama, Alaska, Arizona, Arkansas, California, Colorado, Connecticut, Florida, Georgia, Hawaii, Idaho, Illinois, Indiana, Iowa, Kansas, Kentucky, Louisiana, Maryland, Missouri, Nevada, New Hampshire, New York, North Carolina, North Dakota, Ohio, Oklahoma, Oregon, Pennsylvania, South Carolina, South Dakota, Utah, Vermont, Washington, and Wisconsin.

A5.3 Analyzing 2022 as a post-treatment election

In this section, we present results for Democratic vote share in the House, Senate, and gubernatorial elections using 2022 as a post-treatment election, which are shown in table A11.

In the case of House elections, this introduces a case of staggered adoption or treatment over time, as all counties vote in a House election every two years. Therefore, there will be an early-treated group of counties that received treatment after the passage of the IRA in late August and before the midterm election in early November 2022. The remaining treated counties are then not-yet treated counties during this brief post-treatment period. However, staggered treatment adoption does not work well with our setup of using time-invariant entropy-balanced weights, because it means assigning time-invariant weights to not-yet treated counties. This may be problematic because if not-yet treated counties' are controls in 2022 and treated in 2024, they should not be assigned a time-invariant weight. Instead, we estimate the effects of early-treated counties by comparing early-treated counties with never-treated counties only, and drop not-yet treated counties in the estimation.

The analysis is more straightforward for Senate and gubernatorial elections. Given the staggered nature of these elections, different counties are participating in the 2022 and 2024 elections. We can therefore focus on counties that participated in the 2022 election. We compare early-treated counties (those treated before the 2022 election) with never-treated counties (those that remain control throughout the analysis period).

The coefficients are positive and larger than our main results, though again not statistically significant.

A5.4 Analysis of potential NIMBYism

We analyze projects that may be more likely to elicit NIMBY-related backlash – wind and solar power projects (Jarvis, 2025; O'Neil, 2021; Stokes, 2016; Wolsink, 2007) . Table A12 reproduces our main results, but includes only counties that have solar and wind projects at any stage of development. We observe positive, but statistically insignificant, coefficients.

A5.5 Swing states and counties

Last, we analyze whether IRA-related investments had greater effects in electorally competitive swing counties and states. We define swing counties as counties whose Democratic vote share in the previous general election in 2020 was within 5 percentage points of 50 percent. Swing states for the 2024 election are Arizona, Georgia, Michigan, Nevada, North Carolina, Pennsylvania, and Wisconsin. Tables A13 and A14 present the results

Table A11: Treatment Effects on Democratic Vote Share using 2022 as a Post-Treatment Election

	House (1)	Senate (2)	Governor (3)
Post × Treated	1.117	1.574	1.812
	(0.681)	(1.134)	(1.025)
Pretreatment Elections	2016–2020	2016	2018
Observations	11,814	3,144	3,252
Treated Counties	374	223	305
Control Counties	1,999	1,349	1,321
R^2 (within)	0.184	0.153	0.080

Note: Difference-in-differences estimates with entropy-balanced weights comparing early-treated counties (treated before the 2022 midterm and gubernatorial elections) to never-treated counties only; not-yet-treated counties are excluded. The post treatment period is defined as 2022 and later. Observations are county × election-year. Column 1 includes House democratic outcome lag in 2016 and 2020; column 2 includes Senate democratic outcome lag in 2016 and column 3 includes gubernatorial democratic outcome lag in 2018. Entropy balancing's convergence tolerance level here is set to 0.04 for House results in column 1 to avoid convergence failure. Default convergence tolerance level is 0.015. All specifications include county and time fixed effects. Standard errors are clustered at the county level and shown in parentheses; $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

for swing states and swing counties, respectively.

In the two tables, coefficients are generally negative for presidential outcomes, mixed for House election outcomes, and positive for Senate and gubernatorial election outcomes. No results are statistically significant.

Table A12: Treatment Effects Wind and Solar Projects on Democratic Vote Share

	President (1)	House (2)	Senate (3)	Governor (4)
Post × Treated	0.294	0.386	0.641	0.517
	(0.316)	(0.425)	(0.380)	(0.680)
Pretreatment Elections	2000–2020	2016–2022	2018	2016–2020
Observations	20,531	14,601	3,314	1,611
Treated Counties	934	933	647	116
Control Counties	1,999	1,999	1,010	421
R^2 (within)	0.359	0.122	0.478	0.283

Note: Difference-in-differences estimates with entropy-balanced weights for the effect of IRA-induced wind and solar projects on Democratic vote share. Column (1) - (4) reports results for presidential, House, Senate, and gubernatorial elections, respectively. All columns include democratic outcome lags in 2016 and 2020, except for column (3) which includes Senate outcome lag in 2018. Observations are county × election-year. Entropy balancing's convergence tolerance level here is set to 0.5 for Senate results in column 3 to avoid convergence failure. Default convergence tolerance level is 0.015. All specifications include county and time fixed effects. Standard errors are clustered at the county level and shown in parentheses; $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table A13: Treatment Effects on Democratic Vote Share in Swing States

	President (1)	House (2)	Senate (3)	Governor (4)
Post × Treated	−0.044	−1.174	0.255	1.340
	(0.532)	(0.997)	(1.055)	(0.833)
Pretreatment Elections	2000–2020	2016–2022	2018	2016–2020
Observations	15,288	10,884	2,216	1,371
Treated Counties	185	185	102	36
Control Counties	1,999	1,999	1,006	421
R^2 (within)	0.359	0.043	0.649	0.171

Note: Difference-in-differences estimates with entropy-balanced weights of private IRA investment on Democratic vote shares in swing states. Column (1) - (4) reports results for presidential, House, Senate, and gubernatorial elections, respectively. All columns include democratic outcome lags in 2016 and 2020, except for column (3) which includes Senate outcome lag in 2018. Observations are county × election-year. Entropy balancing's convergence tolerance levels here are set to 1.9 for Senate results in column 3 and 4.4 for Governor results in column 4 to avoid convergence failure. Default convergence tolerance level is 0.015. All specifications include county and time fixed effects. Standard errors are clustered at the county level and shown in parentheses; $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table A14: Treatment Effects on Democratic Vote Share in Swing Counties

	President (1)	House (2)	Senate (3)	Governor (4)
Post × Treated	−0.128	1.432	0.379	0.947
	(0.781)	(0.889)	(1.032)	(1.396)
Pretreatment Elections	2000–2020	2016–2022	2020	2016–2020
Observations	15,120	10,757	2,264	1,164
Treated Counties	161	161	117	21
Control Counties	1,999	1,999	1,015	367
R^2 (within)	0.230	0.109	0.378	0.108

Note: Difference-in-differences estimates with entropy-balanced weights of private IRA investment on Democratic vote shares in swing counties. Column (1) - (4) reports results for presidential, House, Senate, and gubernatorial elections, respectively. All columns include democratic outcome lags in 2016 and 2020, except for column (3) which includes Senate outcome lag in 2018. Observations are county × election-year. Entropy balancing's convergence tolerance level here is set to 0.7 for Governor results in column 4 to avoid convergence failure. Default convergence tolerance level is 0.015. All specifications include county and time fixed effects. Standard errors are clustered at the county level and shown in parentheses; $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.