Paying at the Pump and the Ballot Box: Electoral Penalties of Motor Fuels Taxes*

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June 25, 2026

Abstract

The greatest challenge to addressing climate change comprehensively through government policy has been limited political feasibility. We investigate whether politicians are punished by voters for increasing motor fuel taxes using comprehensive data on state legislative election outcomes and gasoline taxes. Leveraging a difference-in-discontinuities research design, we estimate the effect of legislated gasoline tax changes on incumbent state legislators' subsequent electoral outcomes. For very close elections, we show how the incumbency advantage attenuates when gasoline tax increases have been legislated in the intervening legislative session. Specifically, we find a moderate, but economically and statistically meaningful decrease in the incumbency advantage of 1.3 to 2.0 percentage points for Republican and Democratic incumbents, respectively. This penalty represents 15–22% of the overall electoral advantage of incumbents in our sample, which highlights the relative importance of environmental and energy taxes in voter priorities.

Keywords: gasoline tax, transportation, climate change, political economy, public finance
JEL Codes: H23, H71, D72, Q58

*Corresponding author: awaxman@utexas.edu. Outstanding research assistance was provided by Ross Buchannan, Carlos Parra, and Shreya Ashokkrishnan. We are grateful to Joshua Linn and Mary Evans for their comments. This work was supported, in part, by the University of Texas Energy Institute. Declarations of interest: none.

1 Introduction

Pigouvian taxes on negative environmental externalities are widely viewed by many economists and policymakers as politically difficult despite their economic efficiency. Gas taxes, the focus of this paper, are a specific form of taxation with environmental externality implications that have their own long history of backlash (Knittel, 2014). Past work has studied the political acceptability of environmental taxes using surveys (Dechezleprêtre et al., 2025), experiments (Carattini et al., 2025), and referenda (Anderson et al., 2023; Burkhardt and Chan, 2017), but environmental policies are rarely adopted through direct democracy. Instead, most environmental taxes in representative democracies are enacted via legislation drafted and voted into law by elected representatives (OECD, 2025).If voters punish politicians for raising energy prices, this creates a political barrier to efficient environmental policy. But do voters actually punish politicians who enact environmental taxes? Despite prevailing opinions among economists, a narrow but growing plurality of Americans support increasing taxes on fossil fuels to address climate change (Agrawal and Nixon, 2019).

Understanding the basis for electoral support or opposition to these policies therefore requires a clearer understanding of the push and pull forces of policy formation, the legislative process, and electoral accountability. While there is considerable research on the political acceptability of environmental policies and taxes, and some work on how political forces shape the formation of environmental legislation (Landry, 2021)there is little empirical evidence of the penalty to elected representatives from enacting environmental taxes. The political economy literature on electoral accountability, starting with Ferejohn (1986) and Besley and Case (1992), which studies whether voters discipline politicians for policy choices, has rarely examined environmental taxation, despite its importance for climate policy. In order to unlock this question about the incentives faced by elected officials for enacting environmental policies, we explore the electoral consequences of gasoline tax increases for state legislators. While not direct carbon taxes themselves, gasoline taxes are the closest long-running real-world analogue to carbon taxes; analysis of their electoral consequences provides revealed-preference evidence on the political cost of environmental pricing policies. If policymakers in the U.S. intend to meet decarbonization goals while avoiding fossil fuel taxes for political reasons, these debates ought to be informed about the extent to which voters punish elected officials when taxes are increased.

Gasoline taxes in U.S. states are an ideal empirical setting for studying the political repercussions of raising environmental taxes. Gas taxes were introduced and continue to be used to finance transportation infrastructure, mainly roads and highways. They have also long been recognized by environmental economists as a second-best Pigouvian instrument that can partially internalize the external costs of fossil fuel consumption and driving, such as greenhouse gas emissions, local air pollution, congestion, and accidents (Parry et al., 2007). Even if not intended as climate policy, gas taxes are also the largest existing price on carbon in most countries and U.S. states. Moreover, whether they approve of it or not, many voters recognize that gas taxes can serve this purpose as well (Agrawal and Nixon, 2019). Legislated changes to gas taxes are economically significant and also highly salient to consumers as purchases occur frequently with prices displayed publicly. Opposition to gas tax increases is consistent with evidence that 70–80% of the tax is passed through to consumers as higher after-tax gas prices. Gasoline consumption has also been shown to be more responsive to changes in taxes than pre-tax prices (Li et al., 2014). Lastly, gas taxes are directly attributable to state politicians, since there are very limited local gas taxes, the federal gas tax remains unchanged since 1993[1] Therefore, gas tax increases generate observable, attributable price shocks, which are necessary for us to observe retrospective voting.

In this paper, we measure the existence and extent of the electoral punishment of politicians by voters when gasoline taxes are increased. Acknowledging that legislated gas tax increases are not random, we measure voter responses to discrete policy "shocks" that occur from legislated increases in gas taxes. The goal in our empirical exercise is to understand the role of electoral accountability to gas tax increase shocks.[2] This approach is consistent with retrospective voting models (Besley and Case, 1995; Ferraz and Finan, 2008; Healy and Malhotra, 2013), which study how voters respond to observable policy decisions. In our setting, this corresponds to legislative changes in gas taxes that voters can attribute to incumbent politicians, rather than market-driven price changes such as supply disruptions.

To answer this question, we compile a comprehensive dataset on state legislative election outcomes and gasoline taxes over 1982–2016. Measuring gas tax penalties at the state level is ideal from an analytical perspective because there is substantial variation in the trend of gas tax changes over the past several decades as illustrated in Panel (c) of Figure 1. We focus on the impact of gas taxes to political parties of incumbents in office during gas tax increases. If voters are hoping to hold politicians accountable for costly policies, they are likely to do so to the politicians who are in office when the policy is enacted. Indeed, the public choice literature has long stressed the fact that voters are more likely to evaluate the policy stances of politicians based on what they do in office versus what they say while running (Ferejohn, 1986).

A challenge with applying a standard quasi-experimental design like difference-in-differences (DiD) or more sophisticated staggered treatment estimators (De Chaisemartin and d'Haultfoeuille, 2024) is that both the outcome and the variable of interest (vote shares and gas tax increases) are the result of potentially related political processes. These political processes may themselves be affected by time-variant unobservables, particularly state-level economic conditions (e.g., unemployment, cost of living). Instead, we leverage quasi-random variation in incumbency status from close elections, a long-standing empirical approach in electoral studies (Lee, 2008) that has been validated for state legislative elections specifically (Hainmueller et al., 2015). Combining this with state-by-calendar year variation in gas tax increases results in a "Difference-in-Discontinuities" (Diff-in-Disc) research design that has itself been widely used in the empirical political economy literature (Grembi et al., 2016).

A Diff-in-Disc research design allows us to utilize both RDD and DiD sources of variation to compare effects between winners and losers from close elections. Doing so is useful for two reasons. First, winners from close elections are specifically the politicians who are likely to be the most vulnerable to electoral punishment from (potentially necessary but) unpopular policies like a gas tax increase. Second, a standard DiD estimator compares treated and untreated winners, a comparison which is vulnerable to statewide political (or economic) shocks. In contrast, our

[1] Hawaii, Florida, Nevada, Oregon, New York and Illinois have local taxes, but comprehensive historical data is difficult to obtain.

[2] This is distinct from a goal of recovering the average treatment effect of a marginal change in gas taxes.

Diff-in-Disc approach limits bias from broader state-level political trends by combining the quasi-random assignment of incumbency status with a comparison across states over time. The close-election design differences out state-level shocks that affect winners and losers similarly. This is consistent with recent econometric work on Diff-in-Disc estimators, which shows that in contexts like ours, time invariant factors cancel out around the discontinuity, and Diff-in-Disc achieves lower bias and better coverage than cross-sectional RDD when the data-generating processes are approximately time-stable (Tramontin Shinoki et al., 2024).Our pre-trend null results are broadly consistent with this requirement.

Potential threats to identification come from trends that differentially affect close winners and losers, which we test for via a local parallel trends. We show there is no evidence for such a pattern in pre-trends or placebo tests. We also show coincident changes in other state-level taxes, policies, or economic outcomes do not affect our results. An added advantage of this research design is that variation across districts in election year timing allows us to include state-by-election year fixed effects which do not partial out state-by-calendar year variation in gas taxes as we explain in Section 2. Moreover, we provide substantial evidence from a number of perspectives that, while gas tax increases are not random, there is reason to believe that increases in taxes are not correlated with unobserved factors that determine state legislative elections, as has been noted elsewhere in the state tax policy literature (Robinson and Tazhitdinova, 2022).

Our headline results show that an increase in gasoline taxes imposes a 1.3–1.4 percentage point penalty on Republican incumbents and a 1.6–2.0 percentage point penalty on Democratic incumbents. Given that we estimate an overall 8.8–9.3 percent advantage for Republican incumbents who won from a previously close election and a 8.8–9.0 percent advantage for Democrats, these effects are substantial: approximately 15–22 percent reductions in the incumbency advantage.[3] Our results illustrate that these effects are robust to the inclusion of other state tax increases. We also show that states with and without gas tax increases do not have differential patterns of pre-determined outcomes for close elections (like vote share in pretreatment periods) or for placebo cut-offs away from a majority victory (50% of vote share). We show that the magnitude of gas tax increases may likely matter, but it does not meaningfully change our central estimates.

We also show that there is no evidence for reverse causation: that is whether close elections preceding tax increases could explain our main result. In principle, this is unlikely to occur because the number of close elections preceding a gas tax vote within any state is small relative to the number of legislators voting on gas taxes. However, we conduct a simulation exercise to see if marginal election outcomes could flip roll call votes on a subset of tax bills and find that this is unlikely to affect passage of gas tax increases. This supports the assertion that the marginal legislator is a "policy-taker."

Through a series of extensions, our results indicate that there is no evidence that these penalties occur at other representative levels (e.g., gubernatorial elections), and that they cannot be attributed to down-ballot effects (e.g.,

[3]Following Lee (2008), our main estimates are at the party and not candidate level for identification reasons and so these penalties should be thought as apply to incumbent parties although we provide suggestive evidence that some of this may apply to individual candidates as well.

whether the presidential candidate is from the same party as the incumbent). Lastly, we decompose the correlates of recovered incumbency penalties of gas tax increases into party alignment, ideology and the economic cost of the gas tax increase. We find evidence that greater ideological intensity is consistent with lower penalties for Democrats and higher penalties for Republicans. Similarly, there is evidence for stronger penalties for Republicans when the per capita cost of the gas tax increase is higher, but not for Democrats.

This paper contributes to a large and broad literature has established that gas taxes can be an economically efficient second-best instrument for correcting driving externalities. Prior work has pointed to the fact that optimal U.S. fuel taxes are significantly larger than their current levels (Parry and Small, 2005; Langer et al., 2017). Nevertheless, increasing gasoline taxes closer to optimal levels has faced significant political difficultly over time (Knittel, 2014). Part of the blame for this has been attributed to distributional concerns common to many forms of environmental taxation (Sallee, 2019).Whether environmental taxes, and specifically gas taxes, are indeed regressive has been debated, since the income elasticity of fuel intensity is likely to be positive (Metcalf, 2023). As noted earlier, a large literature on the political economy of environmental taxation has used surveys, experiments and referenda, but does not really address electoral accountability. Muehlegger and Epstein (2023) considers votes on a referendum to repeal gasoline tax increases in California, but this is more of a direct measure of voter preferences, not electoral accountability.

Despite a spate of research on gasoline taxes more broadly, no research has leveraged state-by-state variation in gas taxes and tied them to political outcomes. At a federal level, the failure of national climate legislation took center stage through the defeat of the American Clean Energy and Security Act of 2009 (Landry, 2021). However, a broader range of legislation is tied to the political momentum related to climate change. For instance, Gagliarducci et al. (2024) study how quasi-random exposure to hurricane damage affects voter support for environmental legislation in the U.S. Congress. Boomhower (2021) considers the extent to which voters punish politicians responsible for natural gas permitting for fracking-related earthquakes. Yet it remains an as of yet unanswered empirical question: how badly are politicians punished for raising gasoline taxes? Our paper aims to fill this cap.

A second stream of literature in political economy explores whether voters discipline politicians through retrospective voting. Early theoretical models formalize how electoral incentives shape policymaker behavior. If voters are hoping to hold politicians accountable for costly policies, they are likely to do so to the politicians who are in office when the policy is enacted. Voters are more likely to evaluate the policy stances of politicians based on what they do in office versus what they say while running (Ferejohn, 1986). Besley and Case (1992, 1995); Besley and Coate (2003) also provide a rationale for the extent to which voters punish incumbents for policies they do not like. A rich literature in political science establishes the tendency for voters to penalize ideological extremism (Hall, 2015). Some studies have found this penalty is declining in the U.S. (Bonica and Cox, 2025).Work in political science has considered the extent of political accountability in state legislatures. Rogers (2017) evaluates roll call votes of state legislators in 11 states and finds no evidence that voters punish their representatives for voting for bills that are "ideologically distant" from their districts preferences. A broader literature in political science (Ashworth, 2012) suggests that while elections may struggle to discipline candidates' broad ideological positioning, voters can and do hold incumbents accountable for salient, specific policy actions. This finding is confirmed by Fouirnaies and Hall (2022) based on detailed bill and roll call data for state legislatures. Lowry et al. (1998) find differential penalties for tax and spend policies between policies: Republicans are penalized, Democrats are unaffected and potentially rewarded in vote shares. We show that electoral penalties are positively correlated with partisanship for Republicans, and negatively for Democrats consistent with the fiscal preferences of more extreme voters in these parties. Our approach is the first, to our knowledge, that uses close elections to understand the effect of environmental taxes on the accountability of incumbents.

Third, we contribute to the literature on close-election research designs and policy accountability. Close elections have been shown to be likely to have their outcome at least partially influenced by factors orthogonal to the actual political process (e.g., weather on election day as in Fujiwara et al. 2016). Following Lee (2008), this as-good-as-random variation has been leveraged in regression discuontinuity designs to recover the electoral advantage of incumbents using the margin of victory as the running variable. Initial work raised concerns that close winners and close losers may not be observationally equivalent, but subsequent studies have shown that this assumption holds in almost all electoral cases, including state legislative elections (Eggers et al., 2015; Hainmueller et al., 2015).

A subset of this literature combines regression discontinuity design for close elections with policy outcomes, such as taxation and expenditures (Grembi et al., 2016; Ferreira and Gyourko, 2014; Cellini et al., 2010; Pettersson-Lidbom, 2008; Asher and Novosad, 2017), where the difference-in-discontinuities approach we use has been applied.[4] The approach is also useful for comparing effects of close elections on different types of politicians: for example, Ferreira and Gyourko (2014) compare outcomes for male and female mayoral candidates; in this paper, we compare the incumbency advantage between political parties. Gas tax increases in a Republican district may result in a larger penalty because constituents in that district may respond more negatively to the same tax increase, consistent with theoretical insights from Loeper and Dziuda (2024).

2 Data & stylized facts

2.1 Data

Almost all variation in gas taxes in the last three decades has come from legislation at the state level. We take advantage of this variation using a comprehensive dataset on state legislative electoral outcomes combined with state gasoline tax changes for all state legislative districts (SLDs) over 1982–2016, excluding Nebraska, Louisiana, Puerto Rico and the District of Columbia.[5] The unit of observation is an SLD in an election year, t, where the calendar years between election years t and $t+1$ can vary by district as explained in detail below. We complement this dataset with

[4] Our study is unique in that we are interested in the election outcome itself as it is affected by fiscal legislation, whereas prior studies have typically used elections to explain fiscal outcomes.

[5] Nebraska is excluded because its elections are nonpartisan, and Louisiana is excluded because its nonpartisan primary/runoff system does not yield party-level general elections comparable to other states.

state transportation, demographic, and electoral data. More information about dataset construction can be found in Appendix A.

State legislative elections. We construct a panel of election outcomes at the district and election-year level using data on general elections from the State Legislative Returns database from Klarner (2018). This panel includes the share of votes of incumbents and challengers, and party and legislative session information (e.g., number of candidates and incumbents running, tenure in office, experience). We also use these data to compile the lower and upper house partisan balance.

SLDs have different boundaries than U.S. Congressional districts as shown in Appendix Figure A1: while upper houses (i.e., state Senates) typically have fewer members (one-third to one-half) than lower houses, they still have predefined districts within the state. All state legislatures in our sample are bicameral. We focus our attention on single office-holder districts (i.e., single-member districts) which constitute approximately 90% of the districts in our sample.[6] We also account for redistricting in the sample and describe further details about this in Appendix A. On average, a state has approximately 84 districts that elect one lower house representative and 38 districts that elect one upper house senator.[7] State House elections occur more frequently than state Senate elections, with an average interval of two years and three years, respectively, although for most elections, terms are either 2 or 4 years.[8]

State gasoline tax, pre-tax price, and indexing. We use state gasoline tax rates by year obtained from the Highway Statistics Series at the Federal Highway Administration. These data indicate the year that a gasoline tax change was enacted.[9] We complement this dataset with pre-tax gasoline prices, which come from the Energy Information Administration, and on gasoline tax indexing, which we build based on a variety of journal articles and national and state technical reports. More information about the gasoline tax indexing, data collection and construction can be found in Appendix A.

Additional state transportation, demographic, economic, and electoral data. We gather state road mileage, licensed drivers, and vehicle miles traveled from Li et al. (2014) and the Bureau of Transportation Statistics. We collect state personal income from the Bureau of Economic Analysis, and state population and unemployment rates from FRED. We also include data from MEDSL (2017) on the U.S. president's party and presidential election years. Lastly, for the correlated policy confounders considered in Section 4.2.2, we use data on other state-level taxes from Robinson and Tazhitdinova (2022), budget deficits from the Census of Governments, changes in the number of interstate toll facilities from the Federal Highway Administration, and state-level inflation from Hazell et al. (2022).

[6] Although multi-member districts have existed in 17 states during our sample period, their number has been slightly declining over time. For example, from 2013 to 2016, only 10 states had at least one multi-member district.

[7] We will refer to upper houses as "Senates" and lower houses as "Houses" for parsimony, although in practice these bodies have various names (e.g., some states call the lower house the General Assembly).

[8] See Appendix Table A1, Panel (b), and the NCSL table for further details.

[9] We cannot identify the passage year for all legislated gas tax increases in our sample. Among 16 bills between 2013–2019, the passage-to-enactment gap averaged 114 days, ranging from 3 to 418 days, with most bills passed in March, April, or May. Because elections in our data are at least two years apart, most enacted increases were likely passed within the same interval between elections t and $t + 1$.

2.2 Stylized facts

Most single-member elections involve a Democratic–Republican binary choice. Panel (a) of Figure 1 shows the share of elections by the number of candidates from each party. Throughout the period, 32% of the elections are uncontested, and 59% have two candidates running. The major parties, Democratic and Republican, field one candidate in 84% and 78% of the elections, respectively. In contrast, only 14% of the elections have one or more candidates outside one of the major parties. Panel (b) of Figure 1 shows how the share of incumbents changes over time. The total incumbent share in single-member districts has slightly increased and currently represents around 45% of all candidates.[10] However, the Democratic incumbent share in single-member SLDs has been declining since the end of the 1980s, with a corresponding increase in the Republican incumbent share. Over the sample period, the share of incumbents from other parties is close to zero. In the last decade, the share of incumbents for Democrats and Republicans has been around 20% and 24%, respectively.

State gasoline taxes grew slowly on average, but with sizable variation across states. Figure 1, Panel (c) illustrates that while the average state gasoline tax grew by only 15.1 cents over the sample period, there is substantial heterogeneity in gasoline tax increases across states. Figure 1, Panel (d), shows that some states, such as Pennsylvania, Washington, and New Jersey, experienced gasoline tax increases during our sample exceeding 29 cents per gallon. In contrast, Virginia, South Carolina, and Alaska underwent increases smaller than 6 cents per gallon. Examining the yearly variation in our sample, 28% of state-years see a change in gasoline tax and the mean change is small, with an average increase of half a cent. Throughout our sample, we observe 367 gas tax increases and 60 gas tax decreases.[11]

Election cycles experience differential within-state exposure to gas tax increases. A unique feature of our data is that we observe state legislative district elections with varying within-state timing as shown in an illustrative example from Washington state in Panel (a) of Figure 2. The electoral penalty estimation that we describe in Section 3 compares electoral outcomes between consecutive SLD election years, which we denote as t and $t+1$. Because of the variation in election timing, districts in the same state that share an election in year t may not have their next elections in the same $t+1$ year. As a result, we have the ability to compare sets of elections in the *same state* that are differentially exposed to gas tax increases after the same initial election in period t.

As the figure illustrates, district 38024 has an election in period $t+1$ (1990) that occurs after a 1989 gas tax increase, while the other two districts have their $t+1$ elections before the 1989 tax increase. This means that 38024 is exposed to the tax increase, while the other two districts are not exposed until their subsequent election cycle in $t+2$. This differential exposure means that a gas tax increase affects districts heterogeneously across state-by-election-years, which will be important for interpreting the fixed effects in our econometric model.

[10] This share is less than 50% because some elections are open-seat races with no incumbent running, and because some elections have more than two candidates.

[11] For more details about the distribution of gas tax changes, see Appendix Figure A2.

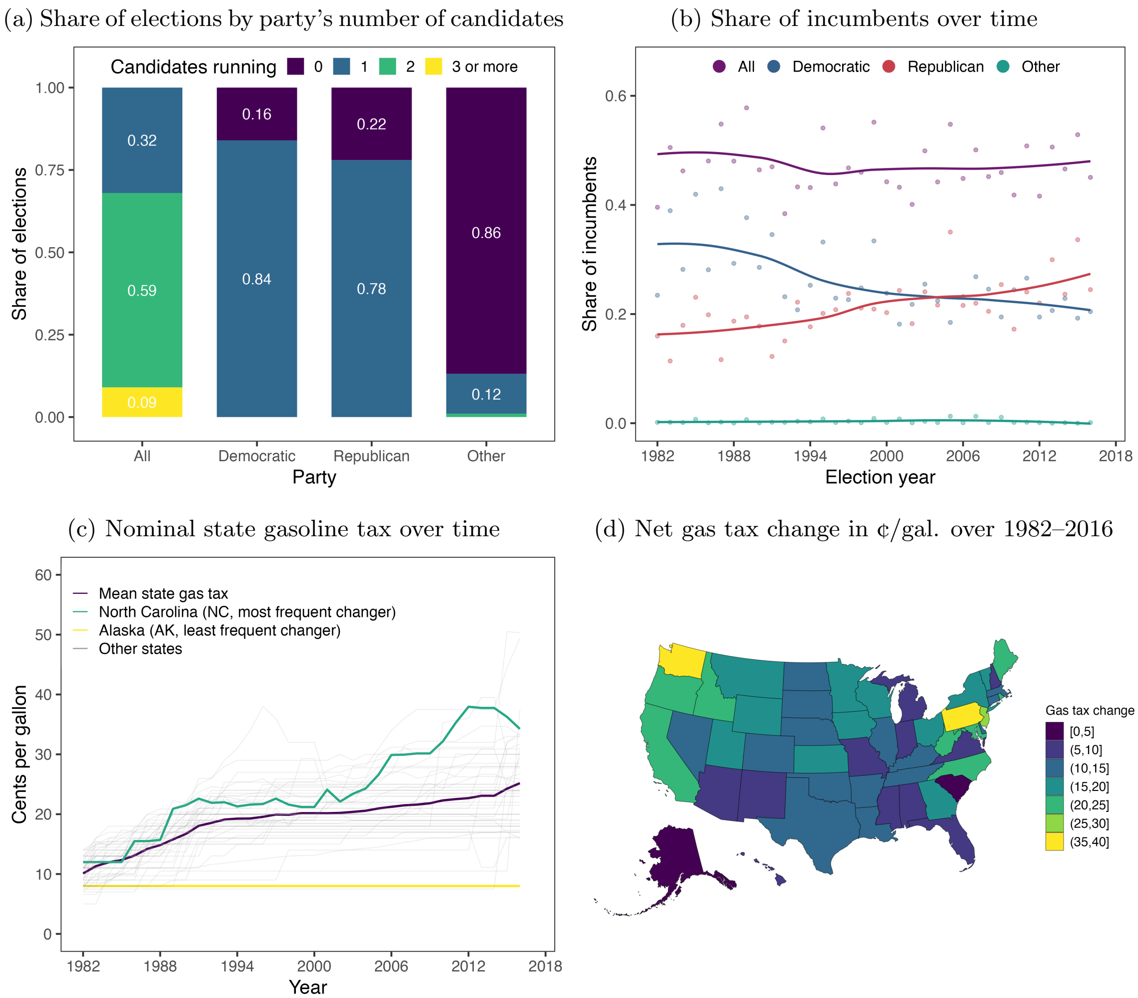


Figure 1: Sample summary statistics

Notes: Panel (a) shows the share of elections with 0, 1, 2, or 3 or more candidates, separately for all candidates and for Democratic, Republican, and other candidates. Panel (b) shows the share of candidates in SLD elections who are incumbents over 1982–2016, with separate series for all incumbents and for Democratic, Republican, and other incumbents. Panel (c) plots state gas tax rates from 1982 to 2016 for all states, along with the annual cross-state mean, and highlights the most and least frequent changers. Panel (d) shows the net change in state gas tax rates between 1982 and 2016. Panels (b)–(d) include gasoline tax changes that are indexed by legislation to inflation or other economic variables, which we control for in our regression estimates.

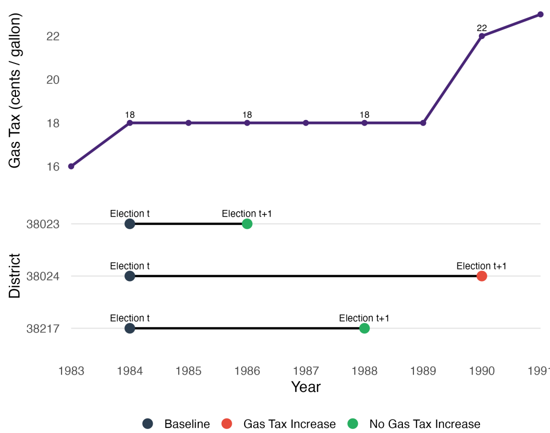
Panel (b) of Figure 2 provides a broader overview of the overall distribution of election timing across our sample.[12] As the bottom half of the panel shows, typically elections are spaced every 2 years for lower houses and 2 or 4 years for upper houses (Senate). However, we observe changes within states during our sample in the timing of state elections. Because of these changes, we have elections occurring in differently spaced calendar years for the same state. This differential timing is a second mechanism for heterogeneity in exposure to state-by-calendar-year gas tax increase shocks. We explore this issue in more detail in Section 3.3.

Gas tax increases uncorrelated with observable political and economic conditions. Given the design of our study, it is helpful to ask why state legislatures might change gas taxes as these changes may be correlated with unobserved determinants of election outcomes. Robinson and Tazhitdinova (2022) examine the determinants of various tax changes across the U.S. and find that it is generally difficult to identify a consistent set of drivers of tax changes across states. However, they do provide evidence that tax competition between states helps to predict some of the variation in gasoline taxes. Sciara et al. (2024) use focus groups and nationally representative surveys of state legislators working on transportation funding to examine priorities for state legislators in transportation committees. They find that gas tax increases often reflect longer-term, decade-long efforts to address structural funding issues for transportation infrastructure.

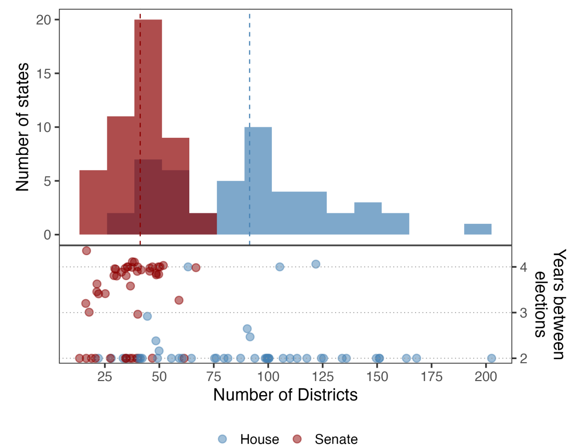
To gauge potential determinants of gas tax increases, we estimate linear probability models in which the outcome is an indicator for whether the state gas tax increases. Panel (c) of Figure 2 reports bivariate regressions on a range of political and economic variables, including state and year fixed effects. Multivariate regressions are reported in Appendix Table A3. Among the variables we examine, gas tax changes are only correlated with pre-tax gas prices and gas tax indexing once state and year fixed effects are included. In Section 3.3, we discuss the implications of this result for the identifying assumptions in our econometric model. Because electoral accountability also requires voters to notice these tax changes, Section 5.1 provides suggestive evidence using Google Trends data that gas tax increases are salient to state residents relative to other policy issues in the year in which they occur. In the following section, we explain how these facts inform our empirical strategy.

[12] Additional summary statistics for our election data can be found in Appendix Table A1. Most states are characterized by a large number of districts, particularly in the lower body (House).

(a) Example of election spacing



(b) Distribution of election spacing



(c) Bivariate regressions of gas tax increases on potential determinants

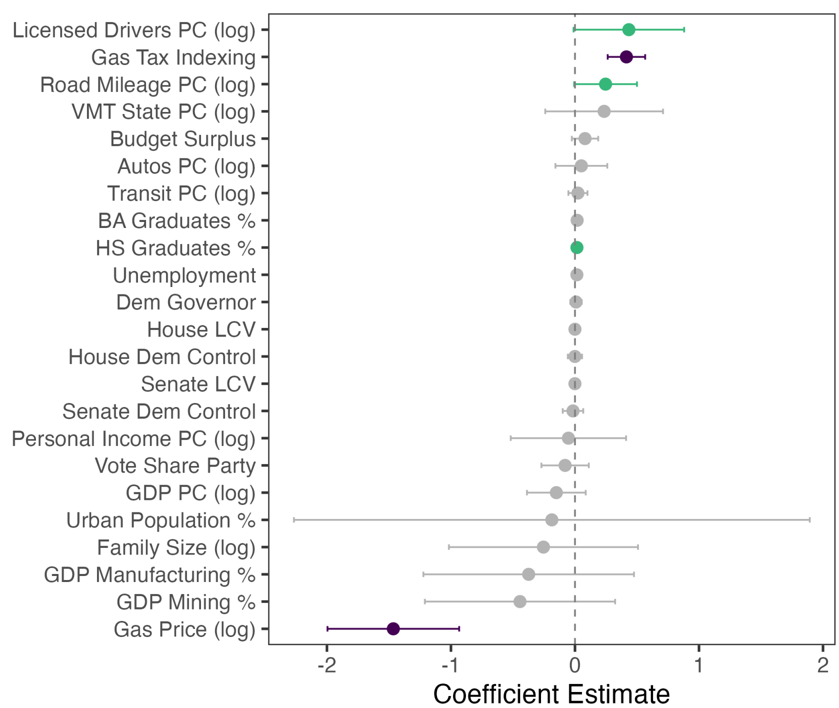


Figure 2: Election summary statistics

Notes: Panel (a) illustrates the timing of gas tax changes and elections for three districts in Washington between 1983–1991. The top line shows gasoline tax levels, and the remaining lines show district-specific election timing. Because election spacing varies across districts, the 1989 gas tax increase occurs before the $t+1$ election in district 38024 but not in the other two districts. Panel (b) shows the distribution of average legislative districts per state across redistricting regimes and their relationship with average election spacing, separately for House and Senate elections. Each dot is a state, and election spacing is the mean gap between consecutive elections within district, averaged to the state level. Panel (c) reports bivariate linear probability models of gas tax increases on the variables shown on the vertical axis, including state and year fixed effects. "Gas Tax Indexing" equals one in state-years when the gas tax rate varied automatically with an external variable, such as fuel prices, CPI, or highway construction costs. "GDP" is state gross domestic product, and "LCV" is the League of Conservation Voters' annual congressional scorecard. Standard errors are clustered at the state level, and intervals indicate 95% confidence intervals.

3 Empirical methodology

Figure 3 illustrates the identifying variation for our Differences-in-Discontinuities (Diff-in-Disc) estimation approach. The running variable on the horizontal axis, M_{it}, is the margin of victory for the political party in question in SLD i and election t. M_{it} measures the party's vote share minus the vote share of its main opponent, where $M_{it} < 0$ means that the party lost that election. The vertical axis depicts V_{it+1}, the party's share of votes in the subsequent election, $t+1$. Note that the time index, t, counts elections, so that the number of calendar years between election t and $t+1$ may vary within states depending upon the spacing between elections as discussed in Section 2.

The empirical estimate of interest, the difference-in-discontinuity, is the difference in the conditional expectation of the $t+1$ vote share between close winners and close losers for districts in states with gas tax increases between t and $t+1$ relative to the same difference in states without these increases. We denote this estimate as τ_{DD}, and it can be decomposed into two terms $\tau_{DD} = \tau_{WE} - \tau_{LE}$. Here, τ_{WE} is the cross-group gap in $t+1$ vote share between close winners exposed to a gas tax increase and close winners not exposed to a gas tax increase. τ_{LE} is the corresponding cross-group gap among close losers. For close winners, this gap reflects both the effect of the gas tax shock and pre-existing differences between gas-tax and non-gas-tax states. For close losers who did not control the seat after election t, the gap captures these pre-existing differences. Subtracting τ_{LE} from τ_{WE} nets out these pre-existing differences, yielding the Diff-in-Disc estimate of the electoral penalty of the gas tax increase on incumbents.

By focusing on the effect of a tax increase on the vote share for incumbents in the next election after the increase, we are assessing the direct effect that the bill may have on voter behavior. As we will argue below, close elections prior to the gas tax increase provide quasi-random variation in which party holds the seat when the increase occurs. Under particular identifying assumptions, this allows us to isolate subsequent vote share variation that is plausibly attributable to the gas tax increase rather than to unobserved economic and political factors. Of course, the gas tax increases themselves are not randomly assigned, but random treatment is not necessary to credibly recover the average effect of a gas tax increase on the incumbency advantage among districts near the close-election threshold which is, in and of itself, the relevant statistic for understanding retrospective voter penalization of incumbent politicians.

We measure the gas tax increase, G_{it}, using an indicator variable for whether there was any increase in gas taxes between elections t and $t+1$. This discrete treatment follows from the structure of our research design, which compares electoral outcomes based on whether districts were exposed to a tax increase before the next election.[13] In Section 4.3, we explore whether our results vary with the size and direction of gas tax changes. The discontinuity around $M_{it} = 0$ then corresponds to the incumbency advantage within each exposure group. This advantage is represented in Figure 3 by $\tau_{RD_{G=0}}$ for districts without a gas tax increase ($G_{it} = 0$) in red and $\tau_{RD_{G=1}}$ for districts with a gas tax increase ($G_{it} = 1$) in green.

Following the approach in Lee (2008), we estimate the electoral penalty separately for Democratic and Republican

[13] Because the calendar interval between elections varies (most notably between two-year lower-house terms and four-year upper-house terms), districts with longer intervals have a mechanically higher probability of undergoing a gas tax increase between cycles. To ensure that our estimates are not an artifact of pooling chambers with different term lengths and baseline incumbency advantages, we demonstrate in Appendix Table A16 that our estimated penalties remain highly stable when restricting the sample exclusively to House elections.

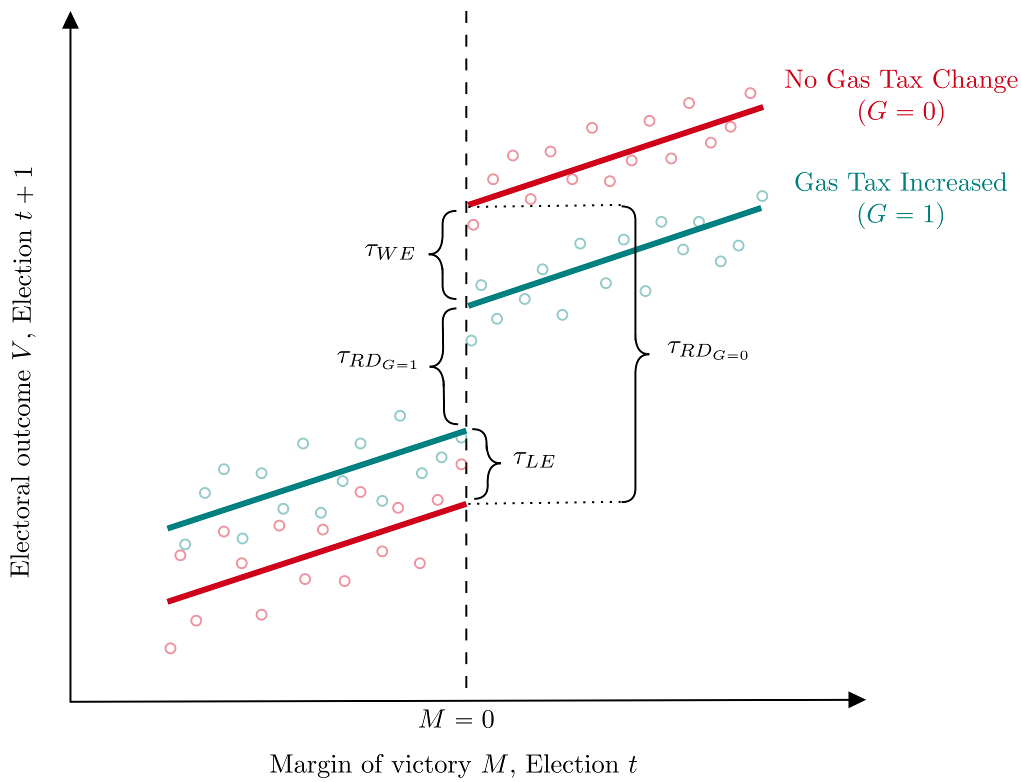


Figure 3: Graphical representation of Difference-in-Discontinuities design for electoral incumbent penalty
Notes: The figure illustrates the Difference-in-Discontinuities (Diff-in-Disc) identification strategy. The horizontal axis shows the margin of victory M in election t, and the vertical axis shows the electoral outcome V in election $t+1$. The dashed vertical line at $M = 0$ separates districts whose party marginally lost (left) from those whose party marginally won (right) in election t. Red markers correspond to districts in states with no gas tax increase ($G = 0$) and green markers to districts in states with a gas tax increase ($G = 1$). The discontinuity in each regression line at $M = 0$ identifies the party's incumbency advantage for each group, $\tau_{RD_{G=0}}$ and $\tau_{RD_{G=1}}$ respectively. The gap τ_{LE} to the left of the threshold captures residual vote share differences among losing districts across the two groups, which would bias a naive comparison of the two discontinuities. The Diff-in-Disc estimator nets out τ_{LE} to recover the causal electoral penalty of the gas tax on incumbents.

incumbents, pooling across upper and lower legislative houses, rather than tracking individual candidates over time. Doing this mitigates selection bias that would arise from candidate attrition when candidates change districts, drop out, or enter the panel. In addition, the party-level approach is consistent with the idea that voters may attribute the policy legacy of state government to the incumbent party rather than to a particular legislator.[14]

In our baseline model, we allow voters to penalize incumbent Democrats and Republicans differently. This specification is based upon theories of differential party perceptions and accountability patterns from the political economy literature discussed in Section 1, but we test this difference empirically. Lastly, electoral penalties may also appear in higher state office elections, namely gubernatorial elections, or through down-ballot effects during presidential or gubernatorial election years. We show that there is no evidence for penalties to governors and that down-ballot effects do not appear to drive our results.

[14] We include elections with Independent or third-party candidates and winners, but do not estimate a separate electoral penalty for these groups given how few observations they represent.

3.1 Potential outcomes framework

Our Diff-in-Disc estimator recovers a difference-in-differences-style effect comparing districts in states with and without gas tax increases, on either side of the margin of victory discontinuity in Figure 3. We first formalize this estimand in a potential outcomes framework and then state the assumptions required for identification.

Estimate of interest. We now formalize the Diff-in-Disc estimand, τ_{DD}, in a potential outcomes framework. Appendix B provides the full derivation for our setting. Related formal results for a more general Diff-in-Disc estimator are provided by Tramontin Shinoki et al. (2024).

Define $V(m, g)$ as the party's potential vote share in election $t + 1$ as a function of incumbency status $m \in \{0, 1\}$ and gas tax status $g \in \{0, 1\}$. Let $\mathbb{E}^{RD}[\cdot] = \mathbb{E}[\cdot \mid M \approx 0]$ denote expectations local to the close-election threshold. We then define $\tau(g) = V(1, g) - V(0, g)$ as the party's potential incumbency advantage under gas tax status g.

With this in mind, we can define two terms:

$$\begin{aligned} &\textbf{Target Estimate:} \quad && \underbrace{\mathbb{E}^{RD}[\tau(1) \mid G = 1]}_{\text{observed}} - \underbrace{\mathbb{E}^{RD}[\tau(0) \mid G = 1]}_{\text{counterfactual}} \\ &\textbf{Actual Estimate:} \quad && \underbrace{\mathbb{E}^{RD}[\tau(1) \mid G = 1]}_{\text{observed}} - \underbrace{\mathbb{E}^{RD}[\tau(0) \mid G = 0]}_{\text{observed}}. \end{aligned} \tag{1}$$

The first line in Equation (1) presents the *Target Estimate*: the effect of a gas tax increase on the party's incumbency advantage in closely contested districts among states that actually increased the gas tax. The first term is observed, while the second term is counterfactual because it represents the incumbency advantage that treated states would have experienced in the absence of a gas tax increase. The second line presents the *Actual Estimate*, which replaces this counterfactual term with the observed incumbency advantage in closely contested districts among states that did not implement a gas tax increase.

Given this framework, the key identifying restriction is that the counterfactual untreated incumbency advantage for treated states is equal to the observed untreated incumbency advantage for untreated states:

$$\underbrace{\mathbb{E}^{RD}[\tau(0) \mid G = 1]}_{\text{counterfactual}} = \underbrace{\mathbb{E}^{RD}[\tau(0) \mid G = 0]}_{\text{observed}}.$$

That is, the second term in the *Target Estimate* must equal the second term in the *Actual Estimate*. Equivalently, the untreated incumbency advantage $\tau(0)$ must be conditionally independent of gas tax status G at the discontinuity threshold, that is $\tau(0) \perp G \mid M \approx 0$. We now state the assumptions under which this equality holds.

A1. Continuity. The conditional expectations of the potential outcomes must be continuous in the running variable, M_{it}, at the discontinuity threshold $M_{it} = 0$ (Imbens and Lemieux, 2008). This assumption would be violated if parties or candidates could precisely manipulate close election outcomes, for example through electoral fraud, causing units just above and below the threshold to differ systematically (Lee, 2008). We assess this concern using standard manipulation tests for the density of the running variable described by McCrary (2008). We provide evidence of no manipulation around the margin of victory threshold in Section 4.2.4.

A2. Local parallel trends. As with a standard DiD estimate, it must be the case that treated and untreated districts are on parallel trends in the absence of a gasoline tax increase. However, this only needs to hold for the estimates *in the neighborhood of the margin of victory threshold.* This is a restriction on the "gap" at the discontinuity, not on the level or slope of vote share curves to the left of the threshold. Grembi et al. (2016) provide a formal exposition of this requirement and Tramontin Shinoki et al. (2024) describe this as a "time-stability" condition under which they demonstrate conditions under which the bias from a Diff-in-Disc estimator approaches zero.[15]

In our empirical context, this assumption means that the untreated incumbency advantage, $\tau(0)$, does not differ systematically between districts in states that will raise the gas tax and those that will not, in the absence of the tax increase. Local parallel trends concerns the incumbency advantage (the discontinuity at $M_{it} = 0$) and not the level or slope of the curves to the left of the threshold. The latter reflects the baseline differences in vote share levels among losers of close elections across gas-tax and non-gas-tax states at the threshold, which is instead addressed by Assumption A3. An appropriate test for the local parallel trends assumption is to estimate the Diff-in-Disc coefficient (τ_{DD}) in pre-treatment periods, comparing the close-election discontinuity between districts in states that will experience a gas tax increase in the future and districts in states that will not. We provide evidence of no differential trends in Section 4.2.1 and further support in Section 4.3.

A3. Accounting for τ_{LE}, the baseline difference among close losers. Our final assumption is that the cross-group difference between gas-tax and non-gas-tax states among parties that narrowly lost the previous election is accounted for in our regression estimates from Equation (2). This left-side gap, τ_{LE}, absorbs both pre-existing selection differences and any potential effect of the gas tax on the non-incumbent party. This assumption would be violated if there were left-side differences that we were unable to credibly control for. In practice, our Diff-in-Disc specification nets this term out directly, and we find that this combined baseline gap (τ_{LE}) is statistically indistinguishable from zero.

3.2 Estimation

Our Diff-in-Disc regression is estimated separately for Republican and Democratic incumbents:

$$V_{it+1} = \beta_0 + \beta_1 M_{it} + \beta_2 G_{it} + \beta_3 \mathbf{1}\{M_{it} \geqslant 0\} + \beta_4 G_{it} \cdot \mathbf{1}\{M_{it} \geqslant 0\} + \beta_5 M_{it} \cdot \mathbf{1}\{M_{it} \geqslant 0\} + \boldsymbol{X\gamma} + \alpha_{s(i),t} + \varepsilon_{it+1} \quad (2)$$

In Equation (2), M_{it} is the margin of victory for the political party in question in SLD i and election t and G_{it} is equal to one when there is a gas tax increase in district i's state between election t and $t+1$, and zero otherwise.

[15] An analogous assumption has been developed in the border discontinuity literature: Butts (2023) shows (Assumption 2 and Theorem 2) that as long as no other policies turn on between periods creating a new discontinuity, a difference-in-discontinuity estimate cancels out any time-invariant state-level discontinuity as M_{it} approaches 0. In practice, gas tax increases are not absent potential contamination from coincident policies, namely other state-level tax increases, but we find that coincident policies that are likely to be salient to voters and attributable to state legislators do not affect our baseline results. While border discontinuity designs have other features that differ from ours, this finding is equivalent.

To make our main results easier to interpret, we exclude the small number of observations for which the gas tax *decreases*, but consider the effect of including these changes in Section 4.3.

$\boldsymbol{X}$ is a matrix of state- and district-level controls. This includes covariates determined one year before election year $t+1$: the state unemployment rate, per capita road mileage, licensed drivers per capita, state vehicle miles traveled per capita, real personal income per capita, the state's average pre-tax real gas price, indicators for the party that controls the state house and senate, and their interaction with an indicator for the current president's party. We also include an indicator for whether the gas tax is indexed (e.g., to inflation, population, etc.), and an indicator for whether election year $t+1$ is a presidential election year. Covariates determined in election year t include the number of candidates running, the number of incumbents running, the party's tenure in office, and the normal party vote share. Importantly, our baseline specification includes state-by-election year (t) fixed effects. We discuss what these fixed effects recover next in Section 3.3.

β_4 is our coefficient of interest and represents the difference in the close-election discontinuity between districts exposed to a gas tax increase and districts not exposed to an increase.[16] This coefficient therefore corresponds to the electoral penalty on an incumbent party for a gas tax increase. It recovers the difference-in-discontinuities estimate, $\hat{\tau}_{DD} = \tau_{WE} - \tau_{LE}$ in Figure 3. In Appendix Figure A3, we provide a full schematic of which coefficients recover each effect.[17]

We estimate Equation (2) using local linear regression functions fitted on both sides of the cutoff at $M_{it} = 0$ within MSE-optimal bandwidths, selected following Calonico et al. (2019).[18] We present results with both uniform and triangular kernels, with our preferred specification utilizing the latter.[19]

We now turn to the identifying variation underlying Equation (2) and the empirical tests we use to assess the plausibility of our identifying assumptions.

3.3 Identification

The source of identifying variation in Equation (2) comes from differences in gas tax changes across states over time, interacted with as-good-as-random variation in party incumbency status around the discontinuity at $M_{it} = 0$. If state-level trends in political or economic conditions are correlated with both gas tax changes and voting patterns, they could confound our estimates of β_4 from Equation (2). This concern hinges on whether Assumption A2 (Local

[16] We do not directly recover $\tau_{RD_{G=1}}$ in Equation (2), but it can be constructed as $\hat{\beta}_3 + \hat{\beta}_4$. We also do not include interactions of G_{it} with M_{it} or the triple interaction of G_{it}, M_{it}, and $\mathbf{1}\{M_{it} \geqslant 0\}$. These terms would allow the fitted lines to differ by gas tax status on each side of the cutoff. Our baseline instead estimates an average slope to the left of $M_{it} = 0$, and an average change in slope to the right. Allowing these additional slope interactions yields individually insignificant slope terms in both parties, a more imprecisely estimated main effect, a negligible improvement in within-R^2 (less than 0.0001 in both panels), and worse model fit by AIC and BIC, providing no evidence that the baseline specification is misspecified by imposing equal slopes across gas tax status.

[17] In practice, $\tau_{LE} = \beta_2$ is always negative and statistically insignificant. This would place the red line slightly above the green line to the left of the cutoff, but we abstract from this small difference in Figure 3 to make the graphical intuition easier to see.

[18] To evaluate how the bandwidth choice affects the sample, we present summary statistics for different bandwidth sizes in Appendix Table A4.

[19] The triangular kernel is known for its point estimation mean squared error (MSE) optimality when paired with an MSE-optimal bandwidth selection.In contrast, the uniform kernel minimizes the asymptotic variance of the local polynomial estimator, exhibiting inference optimality.

Parallel Trends) from Section 3.1 holds, which, in turn, can be understood as requiring two related conditions:

1. No selection into gas tax increases based on potential incumbency advantage, and
2. No common causes affecting both the likelihood of gas tax increases and counterfactual incumbency advantage.

The first condition (no selection into treatment) we defend through four empirical tests. First, we compare pre-trends in incumbency advantage prior to gas tax decisions to confirm parallel trajectories across states that would later make different tax choices. A second test is based on the principle of "dilution through aggregation." Gas tax decisions are made at the state-legislature level, while our identifying variation comes from a small set of close-election districts. If these districts represent only a small share of the voting body, it is unlikely that states select into gas tax increases based on the counterfactual incumbency advantage of parties near the threshold. We assess this by calculating the share of legislative seats represented by close-election districts and showing that they constitute a small fraction of the chamber. As a third test, in Section 4.2.3 we reconstruct state legislative voting records on gas tax increases and show that gas tax decisions would have been the same even if all close-election districts had voted the opposite way, confirming these districts were not pivotal. Fourth, we demonstrate that gas tax decisions are predicted by state-level factors but not by the number or characteristics of close-election districts.

The second condition (no common causes) we defend through three empirical tests. First, the pre-trends test described above also addresses this condition, since diverging pre-trends could arise from common causes as much as from selection. Second, we construct placebo tests examining whether gas tax decisions predict outcomes that should be unaffected by taxes but would be influenced by potential common causes, such as incumbency effects in elections preceding tax decisions or in unrelated political offices, and show no evidence of such effects. Third, we include state-by-election year fixed effects to absorb unobserved factors common to districts within the same state and election year.

The inclusion of state-by-election year fixed effects is made possible by an additional advantage of our empirical design over past work: we observe multiple electoral outcomes occurring in the same state for a given election year, as described in Section 2. Because election spacing varies across houses within states and changes within states over time, districts in the same state and election year can differ in whether a gas tax increase occurred between elections t and $t+1$ (see Panel (a) of Figure 2 and Table A1). As a result, G_{it} varies within state-by-election year cells, and $G_{it} \cdot \mathbf{1}\{M_{it} \geqslant 0\}$ varies at the district level within those cells. This allows us to absorb state-level shocks common to the base election-year cycle without partialling out the identifying variation in β_4. These fixed effects do not account for unobserved state-level shocks that occur in the differing calendar years of election $t+1$. However, such subsequent shocks are differenced out by our design unless they differentially affect narrow winners relative to narrow losers—a threat we rule out through our local parallel trends tests and correlated policy checks. The robustness of our results to the inclusion of these fixed effects suggests that state-level unobservables tied to the base election cycle are unlikely to drive our estimates.

These empirical validity checks are also consistent with the institutional context of state transportation finance. As we show in the bivariate regressions described in Section 2, gas tax increases are not strongly correlated with contemporaneous economic and political variables. This pattern has also been documented in the empirical public finance literature (Li et al., 2014).One reason is that transportation funding and finance has traditionally been less partisan.A second reason is the substantial time lag between deliberations over tax changes and their enactment. In our regressions, the dependent variable reflects vote share in the election *after* a potential gas tax increase, usually several years after the tax change. This makes it less likely that the error term is correlated with the political and economic unobservables that determine gas tax changes. Gas taxes are often increased in response to longer term structural infrastructure financing needs, rather than contemporaneous fluctuations in economic and political conditions, as we establish in separate qualitative work (Sciara et al., 2024).Nevertheless, to be conservative, we control for a host of economic and political variables in our main regressions. We also perform robustness checks that include interactions with other salient state-level taxes, policies, and economic outcomes, and find that their inclusion does not meaningfully change the gas tax penalty we recover.

4 Results

We begin by presenting visual evidence of the impact of gasoline tax changes on electoral outcomes across single-member SLDs in the U.S. and then quantify this effect in our regression results for each political party. We then proceed to demonstrate validity of our empirical approach, specifically that gasoline tax increases are salient to potential voters and that our estimates do not violate the local parallel trends assumption.

4.1 Estimates of the incumbency penalty of gas tax increases

Visual comparison. Figure 4, shows the incumbency advantage and the electoral penalty of gas tax increases using binned scatterplots based on raw data. The figure plots incumbent-party vote share in election $t+1$ on the y-axis and the party's margin of victory in election t on the x-axis.Panels (a) and (b) show the incumbency advantage itself corresponding to β_3 in Equation (2). Green and red markers represent local sample means of the vote share of district elections in states with and without gas tax increases between elections t and $t+1$, respectively. The incumbency effect appears larger, on average, for Republicans (Panel (a)) than for Democrats (Panel (b)). The difference in election $t+1$ vote share between green and red markers is also less substantial to the left of the discontinuity (losers in election t) than to the right (winners in election t). This suggests that the cross-group difference among close losers, which is captured by τ_{LE} in Equation (2), is relatively small, which is relevant for our third identifying assumption. Lastly, there is greater variance in vote shares for Democratic winners ($M_{it} \geqslant 0$) than for Republican winners in election t.

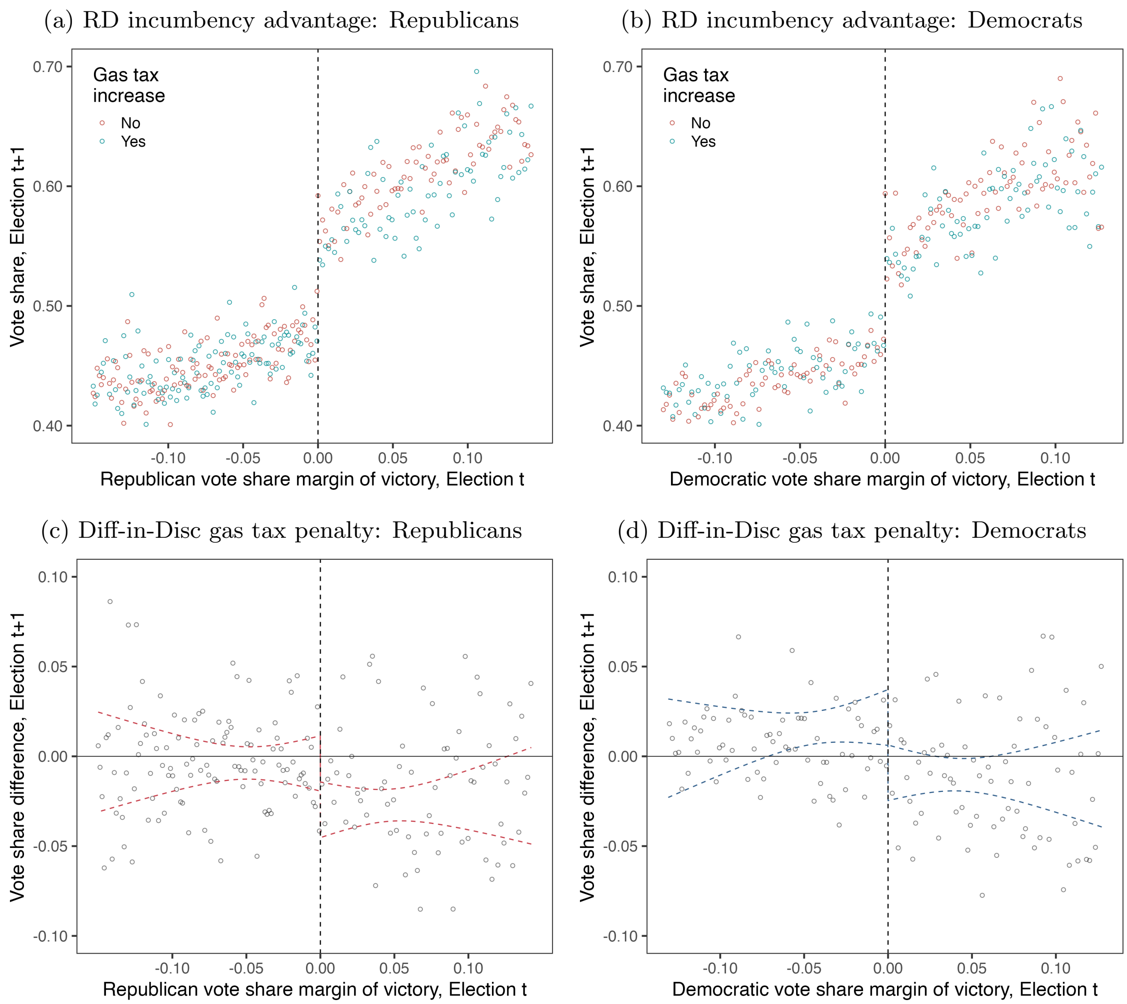


Figure 4: Regression Discontinuity and Difference-in-Discontinuities unconditional plots for vote share

Notes: Panels (a) and (b) show local sample means of the vote share in a given election year $t+1$ over quantile-spaced bins of the margin of victory of the political party in the previous election year t. Green dots represent vote share in election year $t+1$ for districts where the gas tax increased relative to the gas tax level at year t. Panels (c) and (d) show local sample means over quantile-spaced bins, represented by dots, of the vote share difference between elections in districts with and without a gas tax increase. Panels (c) and (d) also show the 95% confidence interval represented by dashed lines from standard errors clustered by state recovered from local linear regression using a triangular kernel weighting and estimated on a granular binned version of the data over intervals of 0.005 percentage points. The optimal number of bins and the bandwidth are selected using a data-driven procedure (with quantile-spaced bins and mimicking variance), following Calonico et al. (2014).

In Panels (c) and (d), the data are collapsed into differences in vote share between districts in states with and without gas tax increases. To the left of the discontinuity, these differences correspond to close elections that were lost in election t. For Republicans, the 95% confidence interval around the binned scattermarks includes zero over the entire domain of negative margins of victory, indicating no statistically significant difference associated with gas tax increases among close losers. For Democrats, there is some statistically significant difference closer to the discontinuity. We examine these left-side differences directly by estimating β_2 in Equation (2) in the regressions below. To the right of the discontinuity, the plotted differences correspond to close elections that were won in election t. The discontinuous jump between the left-side difference and the right-side difference at the cutoff provides the visual analogue of β_4 in Equation (2) and τ_{DD} in Figure 3. Here, more markers fall below zero, suggesting a reduction in the incumbency advantage for districts in states where the gas tax was increased between elections t and $t + 1$.

Regression estimates. To formalize this suggestive evidence, we report the primary Diff-in-Disc regression estimates from Equation (2) for the Republican and Democratic parties in Table 1. The first row reports the incumbency advantage in districts without a gas tax increase, corresponding to the coefficient (β_3) on the indicator variable $\mathbf{1}\{M_{it} \geqslant 0\}$. The second row reports the gas tax effect among losers, corresponding to the coefficient (β_2) on G_{it}. Finally, the coefficient of interest is reported in the last row: the electoral penalty of the gas tax for incumbents, corresponding to the coefficient (β_4) on $\mathbf{1}\{M_{it} \geqslant 0\} \times G_{it}$. In each panel, going from column (1) to (6), our estimates are robust to the inclusion of covariates, state and year fixed effects, state-by-election year fixed effects, and the use of an alternative kernel. Sample sizes differ across columns only due to optimal bandwidth selection, which accounts for the variance of the included covariates. All specifications are estimated on a common sample that excludes observations with missing values in any covariate.

As discussed in Section 2, we observe multiple elections within each state and election year, creating variation in exposure to gas tax increases even after including state-by-election year fixed effects. The fact that moving from columns (3) and (4) to columns (5) and (6) does not meaningfully affect our estimates suggests that the gas tax incumbency penalty is robust to unobserved state-by-election year shocks.

Table 1: Diff-in-Disc estimates of electoral incumbent penalty: Vote share in election t+1

Panel (a). Republican party	(1)	(2)	(3)	(4)	(5)	(6)
Win Election ($\mathbf{1}\{M_{it} \geqslant 0\}$)	0.091***	0.091***	0.088***	0.092***	0.089***	0.093***
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Gas Tax (G_{it})	-0.002	-0.003	-0.003	-0.004	0.007	-0.004
	(0.005)	(0.005)	(0.005)	(0.006)	(0.014)	(0.012)
Win Election × Gas Tax	-0.015**	-0.014*	-0.013*	-0.013*	-0.013*	-0.014**
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.006)
State FEs			X	X		
Year FEs			X	X		
State-Year FEs					X	X
Covariates			X	X	X	X
Right Bandwidth	0.123	0.165	0.128	0.176	0.128	0.176
Left Bandwidth	0.125	0.174	0.112	0.149	0.112	0.149
Kernel	Uniform	Triangular	Uniform	Triangular	Uniform	Triangular
Observations	10,053	13,502	9,768	13,182	9,768	13,182
Panel (b). Democratic party	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Win Election ($\mathbf{1}\{M_{it} \geqslant 0\}$)	0.088***	0.086***	0.090***	0.089***	0.088***	0.089***
	(0.009)	(0.008)	(0.009)	(0.008)	(0.008)	(0.008)
Gas Tax (G_{it})	0.008	0.007	0.009	0.008	0.004	0.003
	(0.005)	(0.005)	(0.006)	(0.005)	(0.017)	(0.017)
Win Election × Gas Tax	-0.021***	-0.018***	-0.020**	-0.019***	-0.017*	-0.016**
	(0.008)	(0.007)	(0.008)	(0.007)	(0.008)	(0.008)
State FEs			X	X		
Year FEs			X	X		
State-Year FEs					X	X
Covariates			X	X	X	X
Right Bandwidth	0.126	0.151	0.118	0.137	0.118	0.137
Left Bandwidth	0.109	0.144	0.103	0.142	0.103	0.142
Kernel	Uniform	Triangular	Uniform	Triangular	Uniform	Triangular
Observations	9,500	11,851	8,964	11,234	8,964	11,234

Notes: The table reports 12 estimates of Equation (2), separately for Republican and Democratic vote share in election year $t+1$, V_{it+1}. The running variable is the margin of victory in election year t, M_{it}; "Win Election" is $\mathbf{1}\{M_{it} \geqslant 0\}$; and Gas Tax, G_{it}, equals one if the gas tax increased between elections t and $t+1$. Observations with gas tax decreases are excluded. Columns (1), (3), and (5) use a uniform kernel; columns (2), (4), and (6) use a triangular kernel. Columns (3)–(4) include state and year fixed effects, while columns (5)–(6) include state-by-election year fixed effects. Columns (3)–(6) include covariates measured before election year $t+1$: unemployment, road mileage per capita, licensed drivers per capita, vehicle miles traveled per capita, real personal income per capita, pre-tax real gas prices, legislative chamber control, gas tax indexing, and presidential election year. They also include election-year-t controls for the number of candidates, number of incumbents, party tenure, and normal party vote share. Bandwidths are selected using the MSE-optimal procedure of Calonico et al. (2014). Sample sizes differ only because optimal bandwidths vary with the included covariates. All specifications use a common sample excluding observations with missing covariates. State-clustered standard errors are in parentheses.

These estimates provide compelling evidence that incumbent politicians are penalized for gas tax increases: raising gasoline taxes since the last election reduces the vote share of the incumbent party in a statistically and economically significant manner. Focusing on our preferred results in column (6), we find that the Republican party's incumbency advantage decreases by 1.4 percentage points, which is statistically significant at the 95% level. The Democratic party's incumbency advantage drops by 1.6 percentage points, also statistically significant at the 95% level. This result is consistent with the interpretation that voters tend to punish sitting representatives for passing a gasoline tax increase. Although the point estimate for Democrats is somewhat larger in magnitude and more consistently statistically significant across specifications, the difference between parties is not statistically significant.

Without a gas tax increase, the incumbency advantage hovers between 8–9 percentage points. Based on our preferred specification in column (6), gas tax increases therefore reduce the incumbency advantage by 15.1% for Republicans and 18.0% for Democrats. The mean gas tax increase in our data is 2.32 cents, implying an estimated penalty of 0.61 percentage points per one-cent increase for Republicans and 0.69 percentage points per one-cent increase for Democrats, evaluated at the mean and assuming a constant relationship. We discuss the policy implications of this result in more detail in Section 6.

Probability of winning as dependent variable. We also consider alternative dependent variables in Equation (2) to provide additional context for our central results. In Appendix Table A5, we replace the dependent variable with an indicator for whether the party wins in election $t+1$: $\mathbf{1}\{M_{it+1} \geqslant 0\}$, an extensive margin measure of incumbency advantage. Our findings indicate that increasing the gas tax reduces the probability of victory by roughly 3–4 percentage points, although this effect is statistically significant only for the Democratic party.

Gas tax decrease "treatment." We also examine the impact of a gas tax *decrease* on vote share in the next election in Appendix Table A6. For Republicans, we find no evidence that gas tax decreases affect the incumbency advantage. For Democrats, we find a statistically significant negative effect, but only when indexed decreases are included. Importantly, as mentioned in Section 2, there are far fewer gas tax decreases in our sample, and nearly three out of four of them are indexed. In contrast, a large majority of gas tax increases (two out of three) are not indexed. Since indexed decreases occur mechanically under pre-existing adjustment rules, they are unlikely to capture a salient legislative tax cut. The fact that the negative effect for Democrats disappears once indexed decreases are excluded suggests that this result is more likely to reflect the composition or timing of indexed adjustments than a direct electoral response to gas tax decreases.

Pooled estimates. Last, as discussed at the beginning of Section 3, voters may apply electoral penalties to incumbents irrespective of their political party. In Appendix Table A7, we estimate an alternative Diff-in-Disc model that stacks election observations from *both* parties and includes an indicator for whether the outcome corresponds to the Democratic party. We interact this indicator with the gas tax increase indicator and the indicator for whether the party won election t, which allows us to test directly whether the incumbency penalty differs across parties. We include but do not report, the coefficients for the Democratic party indicator and its lower-order interactions with the gas tax and win indicators, which are statistically insignificant. The pooled estimates are consistent with our main results in Table 1: we find no statistically significant difference in the incumbency penalty between Republicans and Democrats. This may reflect the basis of a meaningful partisan difference, or limited statistical power to detect one.

4.2 Identification and validity checks

4.2.1 Local parallel trends

We now test the key identifying assumption from Section 3.1: local parallel trends. A standard staggered-treatment difference-in-differences design would test for parallel trends using control groups that are either never treated or never change treatment status (De Chaisemartin and d'Haultfoeuille, 2024). This approach is not feasible in our case—as we document in Appendix B—given the structure of our data: 93.8% of states raise the gas tax more than once, and only 2 states never raise it. It is also not desirable, since such an approach would require imposing *global* parallel trends, which Figure 5 suggests may be unlikely to hold over broader event-time horizons. In lieu of "never-treated" and "never-changer" control groups, we use "not-yet-treated" observations as controls.

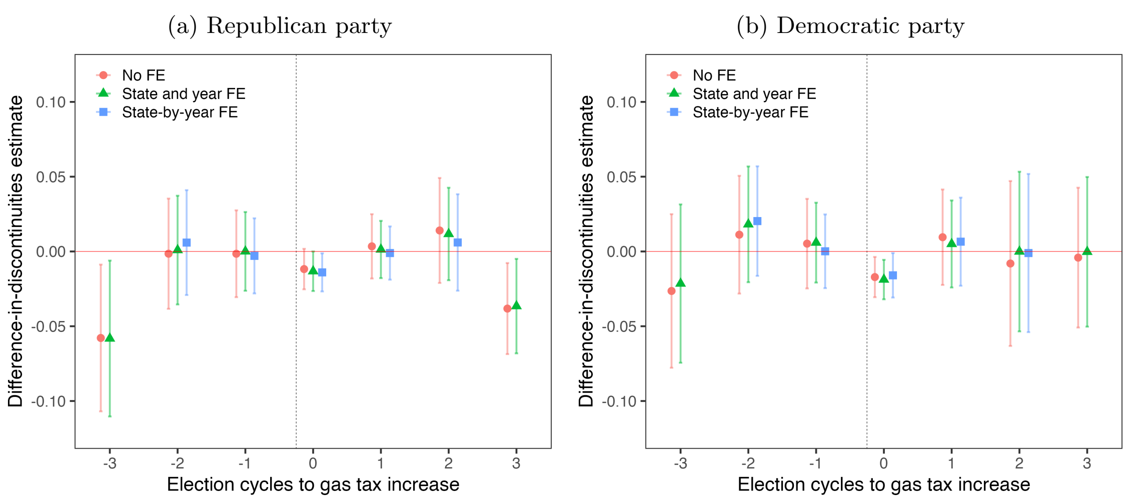


Figure 5: Diff-in-Disc election cycle estimates for testing local pre- & post-trends

Notes: This figure tests local parallel trends by estimating Difference-in-Discontinuities coefficients up to three election cycles before and after a gas tax increase. Coefficients are interactions between G_{it} and $\mathbf{1}\{M_{it} \geqslant 0\}$ at each event time. For negative event times, treated observations are districts in states that will experience a gas tax increase in the indicated number of future election cycles; for positive event times, they are districts in states that experienced one in the indicated number of previous cycles. In both cases, states with intervening gas tax increases or decreases are excluded, and control observations are defined analogously over the relevant event-time window. Estimates come from local linear regressions using the preferred specification in Table 1, with covariates and a triangular kernel. The figure reports results with three fixed-effect structures: none, state and election year, and state-by-election year. The bandwidth is the $k = 0$ MSE-optimal bandwidth from Calonico et al. (2014), held fixed across event times so all coefficients use the same local neighborhood. Error bars indicate 95% confidence intervals based on state-clustered standard errors.

In Panels (a) and (b) of Figure 5, we test for pre-trends by estimating whether the incumbency advantage differs between districts in states that will later raise the gas tax and districts in states that do not. We also estimate dynamic effects for up to three elections after the tax increase. For each event time k, we estimate a separate regression in which the coefficient of interest is the interaction between G_{it} and $\mathbf{1}\{M_{it+k} \geqslant 0\}$.[20] For pre-treatment coefficients, treated observations are districts in states that experience a gas tax increase $|k|$ election cycles later; for post-treatment coefficients, they are districts in states that experienced a gas tax increase k election cycles earlier. In both cases, we exclude observations with intervening gas tax increases, and the control group is defined analogously as observations with no gas tax increase over the relevant event-time window. Pre-treatment treated groups are mutually exclusive by construction, so pre-trend coefficients are estimated on disjoint samples with no mechanical correlation across event times. Appendix B provides a concrete example.

These estimates should be interpreted with some caution. As discussed in Section 2, redistricting creates substantial attrition as we move several election cycles into the past or future. This affects the local parallel-trends test, but not our main estimates from Equation (2), which compare outcomes only between elections t and $t+1$. Statistical power declines at longer horizons both because fewer districts can be followed across redistricting cycles and because the event-time window requires no intervening gas tax changes. For example, only 13.3–13.8% of observations have three valid forward periods, and sample sizes fall from about 3,800–4,200 district observations at $k = -1$ to about 500–620 at $k = -3$ across 8–9 states. The pre-trend evidence is therefore most informative at $k = -1$ and $k = -2$.

Pre-treatment estimates capture differences in incumbency advantage between districts in states that will later raise the gas tax and districts in states that do not, while post-treatment estimates indicate whether effects persist or emerge with delay. The Diff-in-Disc coefficient at $k = 0$ corresponds to the main estimate β_4 reported in Figure 3 and Table 1. For Republicans, we find no statistically significant effect in the two election cycles before treatment, although the estimate is negative and significant at $k = -3$. Given that this estimate is based on only 535 Republican district observations across 8 states, compared with roughly 3,800 at $k = -1$, we view it as plausibly reflecting the high variance of longer-horizon estimates rather than a violation of local parallel trends. Moreover, the pattern is not one of monotonically diverging pre-trends: the negative difference at $k = -3$ disappears in the next two pre-treatment periods. For Democrats, no coefficient other than the main effect at $k = 0$ is statistically significant, providing no evidence of systematic pre-trends.

4.2.2 Robustness to correlated state policies

While our state-by-election year fixed effects and rich set of covariates likely absorb a large degree of unobserved heterogeneity, we now test whether the incumbency penalty effect is sensitive to controlling for other policy variables. One could make a case for extending this analysis to a broad set of policy and economic variables. However, the case for collecting data on and adding controls for these policies should depend upon their relative salience to voters, voters' ability to attribute the policy to state legislative action, and comparability over the legislative-political cycle. Transportation expenditures and road quality are clearly important to voters,[21] but difficult to quantify in a manner comparable at the electoral district level and attributable to legislators.

[20] Unlike a traditional DiD event-study plot, these estimates come from separate regressions and therefore have no omitted event-time reference period.

[21] A further challenge is that these variables are generally not dichotomous, so constructing comparable indicators would require arbitrary thresholds with unclear interpretation.

To gauge how correlated our results are with other policies, we first report regressions of gas tax increases on increases of other state-level taxes and state-by-year fixed effects in Appendix Table A8. In Panel (a), there is weak but statistically significant correlation with sale, income, cigarette, and diesel taxes. There is no meaningful correlation with corporate or spirits taxes.[22]

Diesel taxes, typically increased in transportation bills, are strongly correlated with gas tax increases. Given this strong correlation, we first ask whether gas tax increases induce an incumbency penalty in the *absence* of diesel tax increases. In Appendix Table A9, we estimate our baseline model after dropping observations in which the diesel tax also increased. The results remain qualitatively similar to our main estimates, except that the incumbency penalty for Republicans is no longer statistically significant. The estimate retains the same magnitude, but the smaller sample, and especially the reduction in treated observations, reduces precision.

Given this finding, one may want to know whether diesel tax increases have an electoral penalty that is detectable separately from gasoline tax increases. To examine this question, we regress the indicator for a diesel tax increase on an indicator for a gas tax increase and state-by-election year fixed effects. We then interact the residuals from this regression with an indicator for whether the incumbent party won the last election and add this interaction to our baseline model, as shown in Appendix Table A10. Because gas and diesel tax increases are often enacted together, this exercise cannot distinguish whether voters respond specifically to the gasoline tax itself or to the broader transportation-tax package in cases where both taxes increase. Instead, it tests whether variation in diesel tax increases that is orthogonal to gas tax increases is associated with an additional electoral penalty. We find no evidence of such an independent diesel-tax penalty. One possible interpretation is that diesel taxes are less likely to be salient to voters, who are primarily affected by them indirectly if they are passed through into the price of goods. Overall, the results suggest that the estimated electoral penalty is not driven by diesel-tax variation that can be separately detected from gasoline tax increases.

For this reason, we turn to a broader set of potential confounding policies that voters may find salient and penalize politicians for. We begin with policies that are likely to be salient and correlated with gas tax increases. Table 2 includes interactions between the indicator for winning a close election and indicators for increases in state sales, income, corporate, cigarette and spirits taxes. These taxes are predominantly enacted through state legislation and are therefore attributable to state legislators. We also include an interaction with an indicator for whether the state ran a budget deficit, measured as total expenditures less total revenues divided total revenues. Columns (1) and (3) show our preferred specifications from Table 1 for comparison. We find that including these potential confounding policies has next to no effect on our estimates of interest. There is a small reduction in the magnitude of the electoral penalty for Republicans, but this difference is not statistically significant. There is no change in the penalty for Democrats. Moreover, the only statistically significant penalties are for budget deficits and spirits taxes for Republican incumbents.

[22] Sales, corporate, and income taxes are more comparable to gas taxes since they are major revenue generators, are sometimes substitutes for gas taxes, and spirits and cigarette taxes are taxes on products with health externalities and internalities (i.e., "sin taxes").

In Appendix Table A11, we include two additional variables which speak to broader policy correlations. Transportation legislation sometimes raises revenues from road users through tolls, so we include an indicator for whether a state has increased the number of toll facilities on its interstates since the last election. In addition, to the extent that gas taxes pass through to consumers and other products (Kaufmann, 2019), voters may attribute broader state inflation to this legislation. Both variables have more limited state and time coverage, so in columns (2) and (5) we first show how restricting the sample affects our estimates. Compared with our baseline results in columns (1) and (4), the restricted sample shows even stronger electoral penalties. In columns (3) and (6), we include the two variables jointly and find that neither changes the estimated electoral penalties of gas tax increases. Only inflation appears to have an effect on Democratic vote shares. In sum, while one could hypothetically attempt to collect data on a still broader set of economic and policy confounders, we have demonstrated that the most comparable and salient policies have no discernible effect on our estimates of interest.

4.2.3 Effect of close elections on gas tax increases

As discussed in Section 3.3, the first identifying assumption of our Difference-in-Discontinuities design is that there is no selection into gas tax increase based on potential incumbency advantage. Specifically, this means that, among closely contested districts, the electoral advantage incumbents would have enjoyed in the absence of a gas tax increase is the same, on average, across states that raised gas taxes and states that did not. A threat to this assumption arises if gas tax decisions are driven by marginally elected legislators, since those are precisely the legislators whose electoral outcomes help identify the incumbency advantage. If marginally elected legislators were pivotal in passing the gas tax increase, their vote could reflect private information about, or anticipation of, electoral conditions in their own districts, making the gas tax decision endogenous to the very districts that identify our estimates.

To address this concern, we exploit the fact that gas tax decisions are made collectively at the state level: in a legislature with many districts, marginally elected legislators constitute a small minority and are unlikely to be decisive. We provide empirical support for this argument through three exercises, all relying on roll call data from 13 gas tax votes in our sample for which individual legislator votes are available. First, we regress an indicator for voting in favor of the gas tax increase on a legislator's victory margin in their most recent election and an indicator for whether they were marginally elected, controlling for chamber, party affiliation, and tenure, with state and year fixed effects. Results are reported in Appendix Table A13. We find that legislators from closely contested districts are no more likely, and if anything less likely, to vote in favor of a gas tax increase. Second and third, we assess how fragile the passage of each gas tax bill would have been had marginally elected legislators voted differently. Panel (a) of Appendix Figure A5 reports the simulated probability that each bill would have failed to pass if marginally elected politicians changed their vote from 'Yes' to 'No.' We do this by randomly selecting legislators with varying probability, based on 1,000 Monte Carlo simulations per bill. Panel (b) aggregates these results into the share of bills that would have failed under deterministic counterfactuals. The results show that the vast majority of bills in our sample could never have failed regardless of how marginally elected legislators voted, and that even under the most

Table 2: Diff-in-Disc estimates of electoral incumbent penalty: Robustness to potentially correlated policies

Party	Republican		Democratic	
	(1)	(2)	(3)	(4)
Win Election ($\mathbf{1}\{M_{it} \geqslant 0\}$)	0.093***	0.097***	0.089***	0.091***
	(0.006)	(0.006)	(0.008)	(0.008)
Gas Tax (G_{it})	-0.004	-0.004	0.003	0.003
	(0.012)	(0.012)	(0.017)	(0.017)
Win Election × Gas Tax	-0.014**	-0.012*	-0.016**	-0.016**
	(0.006)	(0.007)	(0.008)	(0.008)
Win Election × Sales Tax		-0.003		-0.006
		(0.008)		(0.011)
Win Election × Income Tax		0.014		0.000
		(0.012)		(0.012)
Win Election × Corporate Tax		-0.005		0.002
		(0.012)		(0.013)
Win Election × Cigarette Tax		-0.006		0.000
		(0.007)		(0.008)
Win Election × Spirits Tax		-0.020**		0.006
		(0.010)		(0.014)
Win Election × Budget Deficit		-0.013*		-0.011
		(0.008)		(0.007)
State-Year FEs	X	X	X	X
Covariates	X	X	X	X
Right Bandwidth	0.176	0.170	0.137	0.136
Left Bandwidth	0.149	0.150	0.142	0.144
Kernel	Triangular	Triangular	Triangular	Triangular
Observations	13,182	12,965	11,234	11,295

Notes: The table reports estimates of Equation (2) for party vote share in election year $t+1$, V_{it+1}. Columns (1)–(2) report results for Republicans, and columns (3)–(4) report results for Democrats. The running variable is the margin of victory in election year t, M_{it}; 'Win Election" is $\mathbf{1}\{M_{it} \geqslant 0\}$; and Gas Tax, G_{it}, equals one if the gas tax increased between elections t and $t+1$. Observations with gas tax decreases are excluded. Columns (1) and (3) reproduce the preferred specification in Table 1. Columns (2) and (4) additionally interact 'Win Election" with indicators for increases in other taxes, sales, personal income, corporate, cigarette, and spirits, between elections t and $t+1$, and with an indicator for a state budget deficit in election year $t+1$. For each other tax, the indicator equals one if the tax increased over the electoral cycle and zero otherwise; observations with decreases in that tax are excluded. All columns use a triangular kernel, state-by-election year fixed effects, and covariates. Bandwidths are selected using the MSE-optimal procedure of Calonico et al. (2014). State-clustered standard errors are in parentheses.

conservative definition of marginally elected, all marginal legislators who voted 'Yes' would need to flip their vote to 'No' for even a small share of bills to fail. Taken together, these findings suggest that gas tax decisions in our sample were not driven by marginally elected legislators, supporting the validity of our first identifying assumption.

4.2.4 Continuity, placebo cutoffs, predetermined variables

In Panels (a) and (b) of Appendix Figure A6, we plot the density of the running variable in the vicinity of the cutoff using the test proposed by McCrary (2008). We estimate two local linear regressions, one on either side of $M_{it} = 0$, separately for districts in states with and without gas tax increases between elections t and $t + 1$. Graphically, we do not observe evidence of manipulation as confidence intervals substantially overlap. A test of the log difference between the density functions in Appendix Table A14 also shows that we cannot reject the null hypothesis of no manipulation of the density at the margin of victory threshold.[23] Panels (c) and (d) of Appendix Figure A6 provide an additional check by examining the density of the difference in the running variable between districts in states with and without gas tax increases. The difference in the density of bin midpoints appears largely continuous across the cutoff.

Cattaneo et al. (2019) suggest two additional validity checks of continuity assumption: examining predetermined outcomes and placebo tests. In Panel (a) of Appendix Figure A7, we test for whether there is a discontinuity in predetermined district variables in election t (β_3 in Equation (2)).[24] The estimates suggest that there is no discontinuity for these predetermined outcomes. In Panel (b), we examine whether the regression functions for districts with and without a gas tax increase exhibit discontinuities at points other than the true margin of victory threshold. We find no significant electoral penalty at placebo cutoffs for either Republicans or Democrats, suggesting that our main estimates are not driven by discontinuities at other points in the running variable.

4.3 Additional robustness and heterogeneity

Lastly, we explore the robustness of our results to various modifications, as shown in Figure 6. Overall, we find limited heterogeneity of effects.

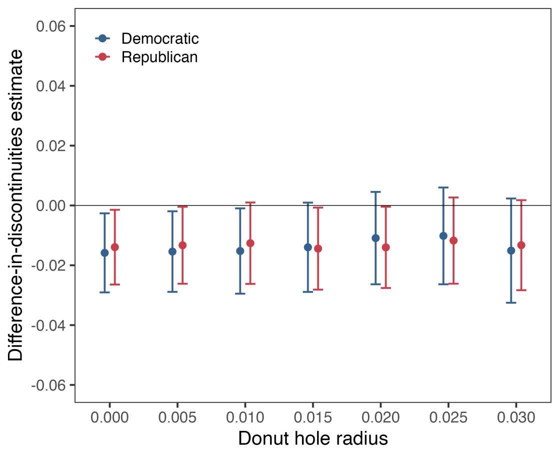
Removing area around discontinuity, bandwidth sensitivity. In Panel (a), we examine sensitivity to observations very close to the margin of victory threshold by removing a "donut" of observations with margins between -0.03 and 0.03 (Barreca et al., 2011). Estimates are comparable to our main results.

In Panel (b), we test the sensitivity of our estimates to alternative bandwidth sizes. As the figure illustrates, our estimates remain highly stable across wider bandwidths, which marginally increases statistical power without meaningfully changing the results. Conversely, validating our results at narrower bandwidths is critical for statistical

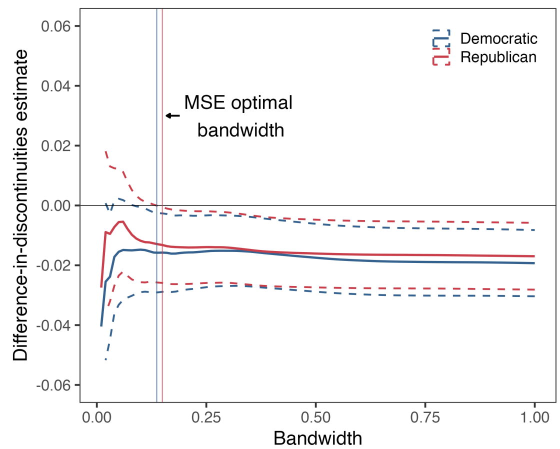
[23] While there is some visual variation in the estimated intercepts for Democratic candidates, formal density tests confirm there is no statistically significant discontinuity in the running variable.

[24] Predetermined variables used are vote share in election $t-1$, an indicator for the party's victory in $t-1$, candidate tenure, the number of incumbents, and the number of candidates as of election t. We also report full regression table results in Appendix Table A12.

(a) Sensitivity to observations near the cutoff



(b) Sensitivity to bandwidth choice



(c) Sample restrictions

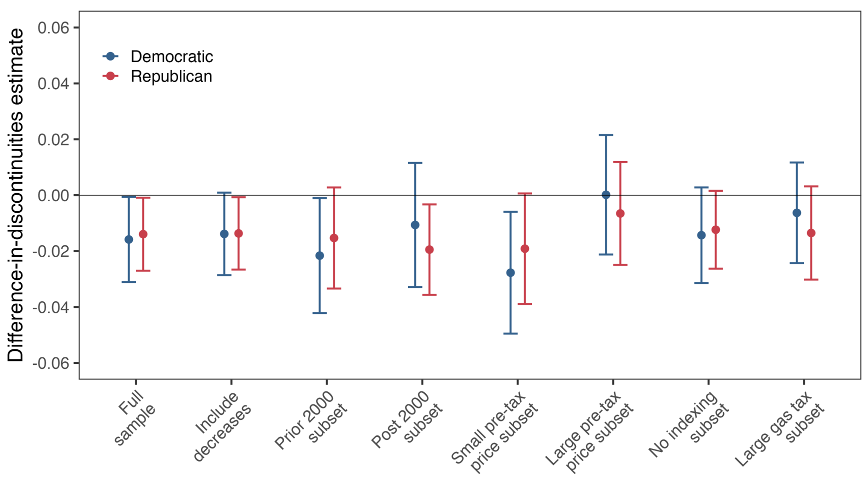


Figure 6: Robustness checks

Notes: Panel (a) reports estimates of β_4 from Equation (2) after excluding observations within donut holes of different radii around the margin-of-victory threshold. Panel (b) reports estimates using alternative bandwidths. Panel (c) reports estimates using alternative samples. 'Full sample" corresponds to the preferred specification in Table 1. 'Include decreases" codes gas tax decreases as $G_{it} = 0$. 'Prior 2000" and 'Post 2000" split the sample around 2000. 'Small pre-tax price" and 'Large pre-tax price" split the sample by the median pre-tax gas price. 'No indexing" restricts the sample to non-indexed gas tax changes. 'Large gas tax" codes $G_{it} = 1$ only for increases above the within-state median among positive changes and drops smaller increases; Appendix Figure A8 varies this cutoff. All regressions include covariates, state-by-election year fixed effects, and a triangular kernel. Bandwidths are selected using two-sided MSE following Calonico et al. (2014). Standard errors are clustered at the state level, and error bars or dashed lines indicate 95% confidence intervals.

inference. Because our Diff-in-Disc specification is estimated manually, our baseline models report conventional clustered standard errors at the MSE-optimal bandwidth. Calonico et al. (2014) note that conventional standard errors at MSE-optimal bandwidths can be subject to smoothing bias, which may cause confidence intervals to undercover the true parameter. To ensure our statistical significance is secure against this bias, we formally re-estimate the specifications in Table 1 using the Coverage-Error-Rate (CER) optimal bandwidths (Calonico et al., 2020). The CER-optimal bandwidth is specifically designed to be smaller than the MSE bandwidth, driving asymptotic smoothing bias to a negligible level so that conventional confidence intervals become valid. As shown in Appendix Table A15, our estimates remain robust and statistically significant under these narrower CER-optimal bandwidths, confirming that our baseline inference is reliable.

Including gas tax decreases, dynamics, gas prices, indexed changes, magnitude of gas tax increase. In Panel (c), we modify our central estimates to account for a variety of factors. First, we include gas tax decreases in our main estimation sample, but code them as if they were "untreated" so that $G_{it} = 0$, and find that the effect for Democrats is slightly weaker. Second, we observe a partisan divergence over time: the estimated penalty for Democrats is larger in magnitude before 2000, whereas the penalty for Republicans becomes larger after 2000. This temporal split anticipates our later findings in Section 5.3 regarding ideology and polarization, suggesting that as parties polarized post-2000, conservative constituencies punished taxes more heavily while liberal constituencies became more accepting of environmental taxation. Third, we split the sample by whether the pre-tax gas price is above or below the median and find that the incumbency penalty is larger in states with lower pre-tax gas prices. This difference may reflect voters being more sensitive to tax increases when gas prices have not been as high. Fourth, restricting the sample to non-indexed gas tax increases (e.g., not tied to inflation) does not affect the magnitude of the estimates but slightly reduces the precision. Fifth, we examine the role of the size of the gas tax increase by recoding the treatment, $G_{it} = 1$, only when the gas tax increases are larger than the median in our sample in percentage terms. The effect is similar but slightly attenuated. In Appendix Figure A8, we re-estimate our preferred specification using alternative size thresholds for defining positive gas tax increases, and find similar results. We report full regression tables of these results in Appendix Table A16.

5 Extensions

5.1 Salience of gas tax increases

For voters to hold politicians accountable for policies they do not like, retrospective voting models require these policies to be salient to voters (Healy and Malhotra, 2013). We now provide suggestive evidence for this awareness from state-level Google Trends search results. Google Trends provides a large sample of real-world Google search requests going back to 2004, enabling the analysis of historic search trends.

In Appendix Figure A4, we show that Google searches jump for the keyword "Gas tax" in the year a gas tax increase was implemented.[25] There is no effect on Google searches for keywords associated with other taxes, politics, transportation, or other general policies. This suggests that gas tax increases are likely to be something that the general population is aware of and may respond to. We describe estimation details in Appendix C.

5.2 To who do voters assign electoral penalties?

We have shown evidence that when state legislatures increase gasoline taxes, voters in those states reduce their support for incumbents. An open question, however, is how voters attribute responsibility for gas tax increases. We now explore heterogeneity in the implied electoral penalties by estimating variants of our main specification for particular subsets of state legislators and electoral contexts in which voters may assign responsibility differently. These results are summarized in Figure 7 and full regression coefficients are reported in Appendix Table A17. The estimates reported in the figure correspond to the sum of the baseline incumbency penality, $\hat{\beta}_4$, and the relevant triple interaction with the variable of interest.

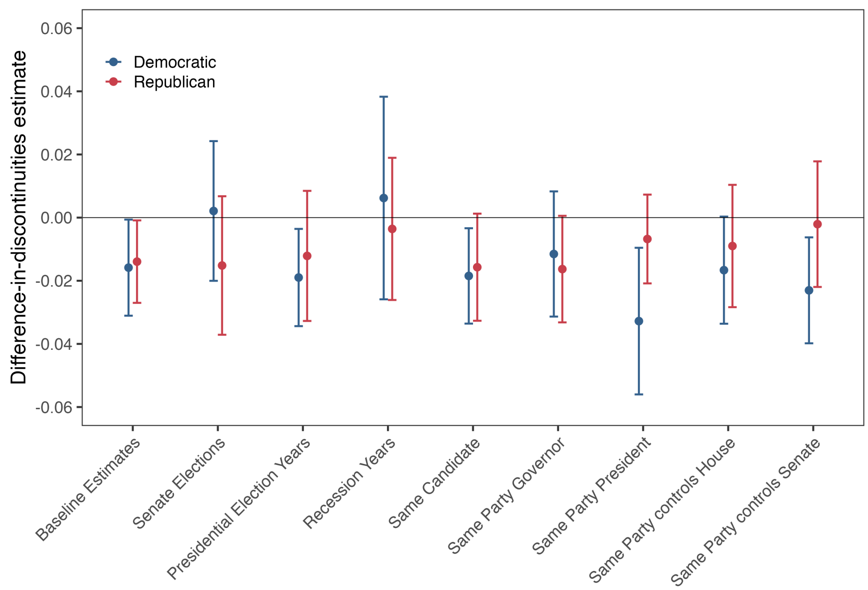


Figure 7: Gas tax penalty attribution

Notes: The figure shows the sum of $\hat{\beta}_4$ from Equation (2) and the coefficient on the triple interaction between $\mathbf{1}\{M_{it} \geqslant 0\} \cdot G_{it}$ and the subgroup indicator listed on the horizontal axis. This sum captures the effect for the corresponding subgroup. All regressions include covariates, state-by-election year fixed effects, and a triangular kernel. 'Baseline Estimates" correspond to our preferred specification in Table 1. 'Same Candidate" equals one when the same identifiable candidate appears in election t and the subsequent election; observations without a match in the subsequent election are coded as zero. Other subgroup indicators are defined by the corresponding figure labels. Standard errors are clustered at the state level, and the standard errors for summed coefficients are computed using the delta method. Error bars indicate 95% confidence intervals.

First, we consider whether the estimates differ for elections to the upper house of the legislature. Although gas tax legislation is generally enacted through both legislative chambers, voters may have more familiarity with, or attribute responsibility disproportionally to, one chamber versus the other. The estimated penalties for upper-house elections are weaker for Democrats. Second, triple interactions with whether it was a presidential election year does

[25]Estimation and data are described in more detail in Appendix C.

not have a meaningful impact on our results, suggesting that down-ballot voting in presidential election years is unlikely to explain our estimates. Third, we test whether voters react to gas taxes differently during worsening economic conditions. When we examine elections that take place during NBER-identified recessions (NBER, 2024), the estimated penalty is attenuated and becomes indistinguishable from zero. Conversely, the penalty remains strong and statistically significant during non-recession years (Appendix Table A16).

Following concerns about selection from Lee (2008), our main specification uses political parties to identify incumbents for a district seat (i.e., whether someone from that party, Democrat or Republican, was elected in a close election prior to the gas tax increase). To consider whether voters attribute responsibility to parties or to individual politicians, we include a triple interaction of the gas tax and winning an election with an indicator for whether the same candidate appears in election t and $t + 1$ before and after the gas tax increase. This allows us to isolate the specific penalty applied when candidate continuity is observed and show that the effects are comparable to our baseline estimates.[26]

Finally, we consider whether electoral penalties differ when the incumbent party is aligned with other political actors. We find that penalties are not meaningfully different when the incumbent party is the same party as the governor. When the incumbent party is the same party as the President, the estimated penalty is larger for Democrats but does not meaningfully change for Republicans. Electoral penalties are also weaker when Republicans control the upper or lower house of the legislature, but potentially larger when Democrats control the upper house. Overall, electoral penalties vary across some political contexts, but they do not point to a single, consistent channel through which voters assign responsibility for gas tax increases.

5.3 Where do penalties come from?

A more challenging prospect underlying models of political accountability is whether it is possible to attribute changes in voter support for elected politicians to particular policies enacted (Ferejohn, 1986). We now present suggestive evidence for the extent to which electoral penalties applied to incumbents in our sample may be explained by particular factors. Specifically, we explore whether three factors: partisan balance, ideological intensity, and the implied financial cost to a representative resident of a gas tax increase, influence the magnitude of the penalties we recover. Partisan balance and ideology are correlated, but not perfectly, since states where voters identify more strongly with one party may vary in the ideological intensity of those voters.

In Table 3, we report non-causal estimates of this regression by party and pooled across both parties. We provide further details on sample and variable construction for this analysis in Appendix D. It is worth noting first that the dependent variable is a constructed measure of electoral penalty exposure, rather than the district-level vote share outcome used in our main specification. As a result, the magnitudes in this table should not be interpreted as directly comparable to the main estimates in Table 1. In models with state and year fixed effects, gas tax increases

[26] When examining the full estimates in Appendix Table A17, the indicator for winning a close election corresponds only to the component associated with the party, while the indicator for the same candidate recovers the remaining incumbency advantage. Their sum is comparable to the incumbency advantage in our baseline specification.

Table 3: Determinants of Gas Tax Penalties to Incumbents

	Electoral Penalty (pp)					
Sample	Democratic		Republican		Pooled	
	(1)	(2)	(3)	(4)	(5)	(6)
Gas Tax	-3.255***	-3.053***	-1.816***	-1.797***	-2.630***	-2.521***
	(0.148)	(0.123)	(0.137)	(0.095)	(0.104)	(0.074)
Gas Tax × Gas Tax Cost Change	0.171	0.077	-0.393***	-0.223*	-0.048	-0.026
	(0.105)	(0.075)	(0.145)	(0.114)	(0.056)	(0.041)
Gas Tax × Ideological Intensity	0.340**	0.241**	-0.509**	-0.442**	-0.363***	-0.490***
	(0.130)	(0.096)	(0.210)	(0.177)	(0.113)	(0.075)
Gas Tax × Partisan Balance	0.300***	0.156**	0.142	0.303**	0.128	0.090*
	(0.105)	(0.075)	(0.147)	(0.116)	(0.088)	(0.048)
Year FEs		X		X		X
State FEs		X		X		X
Dependent variable mean	-1.142	-1.142	-0.786	-0.786	-1.032	-1.032
Observations	391	391	393	393	391	391
Within R^2		0.635		0.337		0.727

Notes: This table reports OLS estimates of the determinants of electoral penalties for gas tax increases. The dependent variable is the estimated electoral penalty, expressed in percentage points, constructed from first-step difference-in-discontinuities estimates at the election-year-by-party level and assigned to each state that raised its gasoline tax in that year. Observations correspond to state-year-party cells. We restrict to election years with at least 50 legislative races within the bandwidth, which corresponds to a natural gap in the data between thin off-cycle years and regular even-year elections. Results are robust to alternative thresholds. "Gas Tax" is an indicator equal to one if the state increased its gasoline tax in election year $t + 1$, and zero otherwise. Continuous covariates are standardized to have mean zero and unit standard deviation. "Gas Tax Cost Change" is measured as the per capita change in gasoline expenditures induced by the tax increase, computed as gallons consumed per capita multiplied by the change in the tax rate; it enters the specification only through its interaction with the Gas Tax indicator. "Ideological Intensity" is based on party-specific ideology scores derived from Pew Research surveys (Republican conservatism for Republican incumbents, Democratic liberalism for Democratic incumbents, and their average for the pooled sample), and is standardized within party. "Partisan Balance" captures the partisan orientation of voters in a state, with higher values indicating a more evenly balanced electorate regardless of which party it leans toward. Clustered standard errors at the state level are in parentheses.

are associated with a 1.8–3.0 percentage point incumbency penalty, as indicated in the first row. Interestingly, the per-capita tax burden of the gas tax does seem to be correlated with greater incumbency penalties for Republicans, but not for Democrats, which also washes out in the pooled results. Ideological intensity is associated with greater penalties for Republicans, but lower penalties for Democrats, consistent with a story of preference for lower taxes in states with greater ideological conservatism and public good provision preference (and associated higher taxes) for more liberal states. Lastly, partisan balance reduces the incumbency penalty for both parties, but more strongly for Republicans at least in the specification with state and year fixed effects. One interpretation is that, separate from ideological preferences, incumbents in states that lean more strongly toward their party may be more insulated from the electoral penalties of policies that voters do not tend to like. On the whole, these results point to differing potential explanations for electoral penalties for Republicans versus Democrats that reflect the conflicting forces of ideology, partisanship, and willingness to pay for public goods across U.S. States.

6 Discussion & conclusions

In this paper, we have demonstrated the persistence of electoral penalties from gas tax increases. We focus on penalties imposed on incumbents because, unlike policy positions more generally, it is incumbents who would be held accountable for legislation while in office. Both Republican and Democratic incumbents face similar electoral penalties, and we show, as reflected by Google Trends searches, that these tax increases are salient to the public in each state around the timing of their enactment in a manner that other important political issues (income taxes, abortion, crime) do not seem to be. Moreover, our evidence on penalties that are attributable to the gas tax increase shocks speaks to retrospective voting models predict punishment for salient policy decisions affecting voter welfare. Gasoline tax increases are visible and attributable increases in household costs, and their electoral penalties complement existing work on policy formation in legislative settings. Our results help explain why gasoline taxes remain below socially optimal levels. Electoral penalties create incentives for politicians to avoid tax increases so that these political costs can contribute to persistent underpricing of transportation externalities.

Given that the electoral penalties in our estimates are identified from the subset of incumbents who won close elections, how generalizable are our headline results? Following List (2020), a general–perhaps obvious–point is that every result is valid in some setting and none are valid in all settings. A broad literature has established the extent of incumbency advantage: that incumbents tend to fare better in election outcomes than an equally matched challenger. If environmental policies or those financing costly infrastructure are politically unpopular for a variety of reasons including their potential regressivity, then passage of these necessary but painful bills relies on the leadership of incumbents with sizable electoral advantages. Our work suggests that despite a considerable electoral advantage (9 percentage points), gas tax increases have the ability to cut that advantage down considerably (18% for Democrats).

A major factor inducing states to raise gasoline taxes is a widening gap between road user revenues (gas taxes, tolls and registration fees) and transportation expenditures. This reinforces the policy relevance of studying electoral responses to these shocks. A comparison of 2018 to 2022 state highway user revenues relative to expenditures from FHWA indicates that 24 states show declines and 10 states collected less than 50% of their revenues this way in 2022 (Tax Foundation, 2025). Intergovernmental transfers, specifically from the Federal Highway Trust Fund, provide some cushion against this pressure. However, a more fundamental challenge is the shift in the vehicle fleet towards electric, hybrid, and high fuel economy vehicles. As a result, despite growing vehicle traffic on roads (and therefore deterioration), revenues from gas taxes fall. A transition towards vehicle miles traveled charges will help to counteract this trend, but relatively few states are in a position yet to transition to this financing model.

Our results have important implications for the spatial heterogeneity of optimal taxes. If localized gas taxes, such as county level taxes, were a feasible policy option, this could substantially affect political feasibility. Second-best optimal gas taxes should vary in space, since the health effects from pollution externalities and the congestion and accident effects from distance-based externalities are strongly correlated with population density. Nehiba (2022) calculates county level driving externalities for 380 U.S. counties and demonstrates that for many rural counties, moving toward optimality would actually mean gas tax *decreases*. While state legislative districts (unlike, say, U.S.

Senate districts) are population-apportioned, they are not perfectly apportioned so the effect of rural county gas tax decreases could meaningfully offset voter backlash to gas tax increases despite population differences. In Appendix Figure A9, we show the distribution of gas tax changes for the set of 380 U.S. counties including in that analysis.[27] A large number of counties with lower population levels have significantly lower levels of negative externalities from driving and so could potentially see gas tax *decreases* under a system of optimal county-level gas taxes. For this set of counties, the average change in tax by state is lower in 32 states. More urbanized counties would face higher gas taxes, but these locations may be more accepting of higher taxes due to their effects on negative externalities, particularly congestion. Moreover these locations may be more accepting of higher gas taxes than more rural counties.[28] While it is beyond the scope of our present analysis to calculate the implied out-of-sample penalty associated with these changes, the results suggest that political constraints on fuel pricing are real but not insurmountable. In particular, they may be mitigated if geographical heterogeneity in costs and political support can be exploited in policy design.

[27] Details of this analysis are provided in Appendix E.

[28] The differential effect also matters because rural electoral districts are often still subject to population-based apportionment even in state senates (National Conference of State Legislatures, 2025).

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Appendix
“Paying at the Pump and the Ballot Box: Electoral Penalties of Motor Fuels Taxes”

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June 25, 2026

Appendix A. Data appendix

State election data

We use Klarner (2018) dataset on state legislative elections covering 1967–2016. The raw dataset comprises 378,345 observations at the candidate level. We restrict our analysis to general elections, excluding special elections, primaries, and non-partisan special elections. Additionally, we omit write-in candidates due to inconsistent collection methods across states, resulting in a refined dataset of 303,772 observations. Because seats are elected at the district level, we then aggregate these candidate returns to construct race-level totals for each district election. State legislative districts change their configuration approximately every decade in our sample due to redistricting legislation. We account for these changes by treating districts before redistricting as different from those afterwards, regardless of whether boundaries were changed. This approach ensures that all aggregate election outcomes correspond to the same geographic unit. Also, if the winner's vote count is missing in a district-level election, we exclude that election from our sample. These refinements result in a dataset of 273,356 observations. In summary, for each general election in each district, we have the candidates, their respective vote counts, and characteristics, such as outcome, incumbency status, tenure, and party. At the election level, we calculate the total number of votes and compute the vote share for each candidate.

Using these data, we create the Diff-in-Disc datasets for the Democratic and Republican parties. Following Lee (2008), the analysis is at the party level to avoid selective “drop-out” at the individual candidate level. For example, in cases when the incumbent does not run in the next election, we identify their party and assign incumbency status to the candidate of the same party with the largest vote share in the next election. We also exclude multi-member districts because the methodology is designed for scenarios where candidates compete for a single seat. Hence, for the party under analysis, we have its vote share and the party's strongest opponent's vote share in each election. If a party runs uncontested, the opposing party is given a vote share of 0. Using these variables, we calculate the party's margin of victory. The party under analysis wins the election when its margin of victory is positive, and loses the election otherwise. We follow the same process for both the current (t) and the next election ($t + 1$).

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Gas tax indexing

To the best of our knowledge, there is no consolidated source of gasoline tax indexing across states over time. We build our own gasoline tax indexing dataset based on institutional knowledge and a variety of journal articles, national, or state technical reports. We start by identifying periods of time in different states where the gasoline tax did not change. For example, Alabama (between 1992 and 2018), Alaska (between 1982 and 2019), Arizona (between 1990 and 2019), Colorado (between 1991 and 2019), Delaware (between 1995 and 2019), Kansas (between 2004 and 2019), or Texas (between 1991 and 2019) did not change their gasoline tax and, thus, they are unlikely to be indexed. Consequently, we create a dummy variable that indicates gasoline tax indexing, and these states and years are coded as zeros.

In this process, we also identify states where gasoline taxes change often. For example, California (between 2010 and 2019), Florida (between 1990 and 2019), or North Carolina (between 1986 and 2019) experienced changes in gasoline tax almost every year. This variation is unlikely to come from statutory changes only and, thus, they need further investigation. This first check allows us to focus our attention on the periods of time in different states where the gasoline tax was likely indexed, representing 702 out of 1938 observations (approximately 36%). The second step consists of focusing on periods of time and states for which we suspect the gasoline tax could have been indexed.

For the period from 1982 to 1983, we follow Bowman and Mikesell (1983), who identified seven states with a variable (indexed) gas tax rate in those years (Indiana, Kentucky, Massachusetts, New Mexico, Ohio, Rhode Island, and Washington). Importantly, we realized that there were some states that have statutes allowing for variable rates, but the effective per-gallon rate has remained constant. For this reason, we created a new dummy variable to account for this behavior.

For the period from 1984 to 1999, we follow Ang-Olson et al. (1999), who described the experience of states that experimented with gasoline tax automatic adjustments indexed to changes in gasoline price, consumer price index, or highway construction and maintenance costs. Towards the end of this period, Florida, Nebraska, North Carolina, and Wisconsin had gas taxes that varied automatically, and other states such as Indiana, Maryland, Michigan, New Mexico, Virginia, and Washington had a gas tax indexed, which was repealed. This finding is confirmed by Li et al. (2014), who affirm that in the late 1980s many variable gasoline tax rate states reverted to a fixed-tax rate.

For the period from 2000 to 2010, we build our gas tax indexing dataset based on specific state reports. These reports are often used to compare historical fuel tax policies with other states and propose alternatives for transportation and highway financing.

For the period from 2011 to 2019, we use data from the Institute of Taxation and Economic Policy, which gathers information on gasoline tax indexing in reports in 2011, 2014, 2015, 2016, 2017, and 2019. We complete the missing years (2012, 2013, and 2018) through conjectures in which we check, for each state, whether or not the gasoline tax indexing in the last report available is still in place in the immediate next report available. If so, we assume the indexing is not modified during the missing years. If not, we search for alternative sources such as the information on variable gas taxes by the National Conference of State Legislatures (`https://www.ncsl.org/`

transportation/variable-rate-gas-taxes) or the reports from the Transportation Investment Advocacy Center (https://www.artba.org/wp-content/uploads/2015/02/Variable-Rate-State-Gas-Tax-Report1.pdf).

Additional Economic & Political Variables

In Appendix Table A3, we regress an indicator variable for gas tax changes in our sample on a range of additional economic and political variables. These include data from the U.S. Bureau of Economic Analysis on Gross State Product (GSP) per capita, the share of state population living within counties within a Metropolitan Statistical Area, the unemployment rate, manufacturing share and mining share of GSP. Average family size, the fraction of adults graduating high school and the fraction with a bachelor's degree are drawn from Li et al. (2014), who compiled them from the Current Population Survey, BEA, and Census data. For the period 2009–2016, we collect those variables from the American Community Survey 1-year estimates. From the Census of Governments, we calculate the share of the state budget surplus relative to revenues. From the League of Conservation Voters Scorecard, we include average scores of environmental voting records for legislators from each house. From the Bureau of Transportation Statistics, we include the number of public transit ridership, personal vehicles, and licensed drivers per capita. Lastly, the population share in an MSA with rail comes from Li et al. (2014) and is originally from the U.S. Statistical Abstract of the U.S.

Appendix B. Difference-in-discontinuities treatment effect

Taking the potential outcomes framework, if we denote $V_{it+1}(m, g)$ as the potential outcome in vote share for election $t+1$, where $m \in \{0,1\}$ indexes whether the party won the election in period t, with $m = 1$ corresponding to $\mathbf{1}\{M_{it} \geqslant 0\} = 1$, and $G_{it} \equiv g \in \{0,1\}$ denotes an indicator for increase of the gas tax between period t and $t+1$, then

$$\begin{aligned} V_{it+1} =& \mathbf{1}\{M_{it} \geqslant 0\} G_{it} V_{it+1}(1,1) + \mathbf{1}\{M_{it} \geqslant 0\}(1 - G_{it}) V_{it+1}(1,0) + \big(1 - \mathbf{1}\{M_{it} \geqslant 0\}\big) G_{it} V_{it+1}(0,1) \\ & + \big(1 - \mathbf{1}\{M_{it} \geqslant 0\}\big)(1 - G_{it}) V_{it+1}(0,0). \end{aligned} \tag{A1}$$

In an ideal scenario, we would like to identify the outcome of interest $V_{it+1}(1,1) - V_{it+1}(1,0)$, which is the effect of a gas tax increase on incumbents' vote share. However, this is unfeasible because we do not observe both states of the world for the same district i and period $t+1$. Using the notation developed by Hahn et al. (2001), for any outcome Z occurring in the election *after* the election determining incumbency, let us define the untreated limits as

$$Z^- \equiv \lim_{r \to 0^-} \boldsymbol{E}\Big[Z_{it+1} \mid M_{it} = r,\, G_{it} = 0\Big], \tag{A2}$$

$$Z^+ \equiv \lim_{r \to 0^+} \boldsymbol{E}\Big[Z_{it+1} \mid M_{it} = r,\, G_{it} = 0\Big]. \tag{A3}$$

Similarly, define the treated limits as

$$\tilde{Z}^{-} \equiv \lim_{r \to 0^{-}} \boldsymbol{E}\Big[Z_{it+1} \mid M_{it} = r,\, G_{it} = 1\Big], \tag{A4}$$

$$\tilde{Z}^{+} \equiv \lim_{r \to 0^{+}} \boldsymbol{E}\Big[Z_{it+1} \mid M_{it} = r,\, G_{it} = 1\Big]. \tag{A5}$$

The left-side limit corresponds to parties that barely lost election t, while the right-side limit corresponds to parties that barely won election t. Note the distinction between the potential outcome indexed by g, which could occur for either group of districts, and the treatment group $G_{it} = 1$, which is the set of districts in our sample for which we observe a gas tax increase.

From this definition, a standard RDD estimator among treated observations recovers

$$\begin{aligned}
\hat{\tau}^{G=1}_{RDD} &= \tilde{V}^{+} - \tilde{V}^{-} \qquad\qquad (\text{A6})\\
&= \boldsymbol{E}\Big[V_{it+1}(1,1) - V_{it+1}(0,1) \mid M_{it} = 0, G_{it} = 1\Big]\\
&= \underbrace{\boldsymbol{E}\Big[V_{it+1}(1,1) - V_{it+1}(1,0) \mid M_{it} = 0, G_{it} = 1\Big]}_{\text{Causal effect of interest}} + \underbrace{\boldsymbol{E}\Big[V_{it+1}(1,0) - V_{it+1}(0,0) \mid M_{it} = 0, G_{it} = 1\Big]}_{\text{Untreated incumbency advantage in treated states}}\\
&\quad - \underbrace{\boldsymbol{E}\Big[V_{it+1}(0,1) - V_{it+1}(0,0) \mid M_{it} = 0, G_{it} = 1\Big]}_{\text{Effect of gas tax on party that lost last election}}.
\end{aligned}$$

Clearly, the presence of the second and third terms means that a standard RDD estimator among treated observations will not generally provide the causal estimate of interest. Hence, to recover the causal effect of interest, we must difference out the untreated incumbency advantage and account for any effect of the gas tax on parties that lost the previous election.

Now, define the Difference-in-Discontinuities (Diff-in-Disc) estimator as

$$\hat{\tau}_{DD} = \left(\tilde{V}^{+} - \tilde{V}^{-}\right) - \left(V^{+} - V^{-}\right). \tag{A7}$$

The $\hat{\tau}_{DD}$ estimator identifies the average treatment effect on the treated (ATT) of a gas tax increase on incumbents' vote share under three assumptions. The first assumption is a standard assumption for the RDD context that allows us to equate outcome values in the limit. The second assumption requires that, as with DiD designs, there are no pre-trends, but crucially this assumption only needs to hold within the neighborhood of the discontinuity. The third assumption requires us to have sufficiently controlled for differences in vote share between districts in states with and without gas tax treatment on the left side of the limit at $M_{it} = 0$.

Assumption 1 (Continuity): All potential outcomes are continuous in M_{it} at 0.

Assumption 2 (Local Parallel Trends): In the absence of a gas tax increase, the local incumbency advantage would be the same in states with and without gas tax increases:

$$\boldsymbol{E}\Big[V_{it+1}(1,0) - V_{it+1}(0,0) \mid M_{it} = 0, G_{it} = 1\Big] = \boldsymbol{E}\Big[V_{it+1}(1,0) - V_{it+1}(0,0) \mid M_{it} = 0, G_{it} = 0\Big]. \tag{A8}$$

<u>**Assumption 3 (Accounting for τ_{LE}, the baseline difference among close losers):**</u> Differences in the vote share between districts in states with and without gas tax increases which had losers in period t, that is where $M_{it} < 0$, capture both pre-existing selection differences and any potential effect of the gas tax on non-incumbents. We assume this combined baseline difference can be fully controlled for and netted out in our regression, or that the groups are observationally equivalent. In practice, this implies that

$$\boldsymbol{E}\Big[V_{it+1}(0,1) - V_{it+1}(0,0) \mid M_{it} = 0, G_{it} = 1\Big] = 0. \tag{A9}$$

To show that the Difference-in-Discontinuities estimator, $\hat{\tau}_{DD}$, recovers the desired ATT of the gasoline tax,

$$\boldsymbol{E}\Big[V_{it+1}(1,1) - V_{it+1}(1,0) \mid M_{it} = 0, G_{it} = 1\Big], \tag{A10}$$

note that

$$\begin{aligned}
\hat{\tau}_{DD} &\equiv \left(\tilde{V}^{+} - \tilde{V}^{-}\right) - \left(V^{+} - V^{-}\right) \qquad\qquad (\text{A11})\\
&= \underbrace{\boldsymbol{E}\Big[V_{it+1}(1,1) - V_{it+1}(0,1) \mid M_{it} = 0, G_{it} = 1\Big]}_{\text{Discontinuity in treated states}} - \underbrace{\boldsymbol{E}\Big[V_{it+1}(1,0) - V_{it+1}(0,0) \mid M_{it} = 0, G_{it} = 0\Big]}_{\text{Discontinuity in untreated states}}\\
&= \underbrace{\boldsymbol{E}\Big[V_{it+1}(1,1) - V_{it+1}(1,0) \mid M_{it} = 0, G_{it} = 1\Big]}_{\text{Causal effect of interest}} + \underbrace{\boldsymbol{E}\Big[V_{it+1}(1,0) - V_{it+1}(0,0) \mid M_{it} = 0, G_{it} = 1\Big]}_{\text{Untreated incumbency advantage in treated states}}\\
&\quad - \underbrace{\boldsymbol{E}\Big[V_{it+1}(0,1) - V_{it+1}(0,0) \mid M_{it} = 0, G_{it} = 1\Big]}_{\text{Effect of gas tax on parties that lost last election}} - \underbrace{\boldsymbol{E}\Big[V_{it+1}(1,0) - V_{it+1}(0,0) \mid M_{it} = 0, G_{it} = 0\Big]}_{\text{Untreated incumbency advantage in untreated states}}\\
&= \boldsymbol{E}\Big[V_{it+1}(1,1) - V_{it+1}(1,0) \mid M_{it} = 0, G_{it} = 1\Big],
\end{aligned}$$

where the second equality follows from the definition of the discontinuities and Assumption 1. The third equality decomposes the treated-state discontinuity by adding and subtracting $V_{it+1}(1,0)$ and $V_{it+1}(0,0)$ within the same conditioning set, $M_{it} = 0, G_{it} = 1$. The final equality follows from Assumption 2, which equates the untreated incumbency advantage in treated and untreated states, and Assumption 3, which sets the effect of gas tax increases on barely losing parties equal to zero.

Diff-in-Disc coefficients

In Appendix Figure A3, we reproduce Figure 3, but with the corresponding coefficients from Equation (2) shown. Here we rewrite that equation to clarify in detail what each set of coefficients recovers.

$$\begin{aligned} V_{it+1} = & \underbrace{\beta_0}_{\text{"untreated losers"}} + \underbrace{\beta_1 M_{it}}_{\text{"slope losers"}} + \underbrace{\beta_2 G_{it}}_{+\beta_0 = \text{"treated losers"}} + \underbrace{\beta_3 \mathbf{1}\{M_{it} \geqslant 0\}}_{+\beta_0 = \text{"untreated winners"}} \\ & + \underbrace{\beta_4 G_{it} \cdot \mathbf{1}\{M_{it} \geqslant 0\}}_{+\beta_0 + \beta_2 + \beta_3 = \text{"treated winners"}} + \underbrace{\beta_5 M_{it} \cdot \mathbf{1}\{M_{it} \geqslant 0\}}_{\text{"slope winners"}} + \boldsymbol{X\gamma} + \varepsilon_{it+1}. \end{aligned} \tag{A12}$$

Each of the terms in underbraces corresponds to a particular outcome (or a component that is added to others to yield the outcome) that has a graphical counterpart in Figure A3, which we now itemize.

- Untreated$\rightarrow$ $G_{it} = 0$
  - Losers $\rightarrow$ $\mathbf{1}\{M_{it} \geqslant 0\} = 0 \Rightarrow \lim_{r \to 0^-} \mathbb{E}\big[V_{it+1} | M_{it} = r, \boldsymbol{X}, G_{it} = 0\big] = \hat{\beta}_0$
  - Winners $\rightarrow$ $\mathbf{1}\{M_{it} \geqslant 0\} = 1 \Rightarrow \lim_{r \to 0^+} \mathbb{E}\big[V_{it+1} | M_{it} = r, \boldsymbol{X}, G_{it} = 0\big] = \hat{\beta}_0 + \hat{\beta}_3$
  - Difference: $\lim_{r \to 0^+} \mathbb{E}\big[V_{it+1} | M_{it} = r, \boldsymbol{X}, G_{it} = 0\big] - \lim_{r \to 0^-} \mathbb{E}\big[V_{it+1} | M_{it} = r, \boldsymbol{X}, G_{it} = 0\big]$
    $= \left(\hat{\beta}_0 + \hat{\beta}_3\right) - \left(\hat{\beta}_0\right) = \hat{\beta}_3$
- Treated$\rightarrow$ $G_{it} = 1$
  - Losers $\rightarrow$ $\mathbf{1}\{M_{it} \geqslant 0\} = 0 \Rightarrow \lim_{r \to 0^-} \mathbb{E}\big[V_{it+1} | M_{it} = r, \boldsymbol{X}, G_{it} = 1\big] = \hat{\beta}_0 + \hat{\beta}_2$
  - Winners $\rightarrow$ $\mathbf{1}\{M_{it} \geqslant 0\} = 1 \Rightarrow \lim_{r \to 0^+} \mathbb{E}\big[V_{it+1} | M_{it} = r, \boldsymbol{X}, G_{it} = 1\big] = \hat{\beta}_0 + \hat{\beta}_2 + \hat{\beta}_3 + \hat{\beta}_4$
  - Difference: $\lim_{r \to 0^+} \mathbb{E}\big[V_{it+1} | M_{it} = r, \boldsymbol{X}, G_{it} = 1\big] - \lim_{r \to 0^-} \mathbb{E}\big[V_{it+1} | M_{it} = r, \boldsymbol{X}, G_{it} = 1\big]$
    $= \left(\hat{\beta}_0 + \hat{\beta}_2 + \hat{\beta}_3 + \hat{\beta}_4\right) - \left(\hat{\beta}_0 + \hat{\beta}_2\right)$
    $= \hat{\beta}_3 + \hat{\beta}_4$
- Difference Treated - Difference Untreated =
  $\lim_{r \to 0^+} \mathbb{E}\big[V_{it+1} | M_{it} = r, \boldsymbol{X}, G_{it} = 1\big] - \lim_{r \to 0^-} \mathbb{E}\big[V_{it+1} | M_{it} = r, \boldsymbol{X}, G_{it} = 1\big]$
  $- \lim_{r \to 0^+} \mathbb{E}\big[V_{it+1} | M_{it} = r, \boldsymbol{X}, G_{it} = 0\big] + \lim_{r \to 0^-} \mathbb{E}\big[V_{it+1} | M_{it} = r, \boldsymbol{X}, G_{it} = 0\big]$
  $= \left(\hat{\beta}_3 + \hat{\beta}_4\right) - \left(\hat{\beta}_3\right)$
  $= \hat{\beta}_4$

Local parallel trends test & panel construction

Here we provide more details about the construction of our data and the interpretation of the local parallel trends test described in Section 4.2.1 and shown in Figure 5.

To test for local parallel trends, we estimate the Diff-in-Disc coefficient $\hat{\beta}_4^{(k)}$ at each event time

$$k \in \{-3, -2, -1, 0, +1, +2, +3\}$$

relative to a gas tax increase. At each horizon we define a treated and control group from the full district-level panel and estimate a separate Diff-in-Disc regression comparing the two. Because the same state can raise its gas tax multiple times over the sample period, a single cohort assignment per district is not possible. Instead, groups are defined event-time by event-time, and any district-year observation can contribute to different groups at different event times.

Pre-treatment event times ($k = -j,\ j \geqslant 1$). At event time $k = -j$, the treated group consists of district-year observations satisfying three conditions: no gas tax change in the current period or the next $j-1$ periods, a gas tax increase exactly j periods ahead, and no decrease anywhere in that window. The control group requires the same absence of decreases but no increase anywhere in the next j periods. Both groups are untreated when the outcome is measured; what distinguishes them is whether an increase lies exactly j periods ahead.

The control group becomes progressively more restrictive as j increases, since a longer window imposes more no-increase conditions: the set of valid control observations at $k = -3$ is a strict subset of those at $k = -2$, which is in turn a strict subset of those at $k = -1$. We do not restrict what happens beyond j periods: a district with increases at both $j = 2$ and $j = 3$ periods ahead can appear in the $k = -2$ treated group. Imposing stricter cohort isolation would eliminate most observations at $k = -2$ and nearly all at $k = -3$. We note that this property holds for the local parallel trends test, but does not for our main estimates from Equation (2), since this only compares election t with $t + 1$ outcomes.

Post-treatment event times ($k = +j,\ j \geqslant 1$). The logic is symmetric. The treated group consists of district-year observations with a gas tax increase exactly j periods ago, no change in the intervening periods or the current period, and no decrease anywhere in that backward window. The control group requires no increase over the same backward window.

Contemporary event time ($k = 0$). At $k = 0$, the treated group consists of all districts for which a gas tax increase occurs between the current and next election, and the control group consists of all remaining districts with no change in either direction. This uses the full district sample without restriction and corresponds directly to the main Diff-in-Disc estimation in Equation (2).

Gas tax decreases. Observations with a gas tax decrease between elections t and $t + 1$ are excluded from both groups at every event time. Leads and lags of the decrease indicator are constructed over the complete panel before any filtering, so the temporal sequence within each district is preserved. This ensures that a decrease observed between two elections is not silently omitted when constructing the window of a neighboring election cycle.

Regression specification. At each event time k, we pool the treated and control districts for that horizon and estimate the following regression within the MSE-optimal bandwidth, applying a triangular kernel weight based on the electoral margin M_{it}:

$$V_{it+1} = \beta_0 + \beta_1 M_{it} + \beta_2 G_{it}^{(k)} + \beta_3 \mathbf{1}\{M_{it} \geqslant 0\} + \beta_4^{(k)} G_{it}^{(k)} \cdot \mathbf{1}\{M_{it} \geqslant 0\} \tag{A13}$$
$$+ \beta_5 M_{it} \cdot \mathbf{1}\{M_{it} \geqslant 0\} + \boldsymbol{X}_{it}\boldsymbol{\gamma} + \alpha_s + \alpha_t + \varepsilon_{it+1},$$

where $G_{it}^{(k)}$ is the event-time-specific treatment indicator defined above. The coefficient of interest is $\hat{\beta}_4^{(k)}$, which recovers the Diff-in-Disc estimate of the differential incumbency advantage between close winners and close losers in states with versus without a gas tax change at horizon k. Standard errors are clustered at the state level throughout. The bandwidth is estimated once using the MSE-optimal procedure of Calonico et al. (2014) on the $k = 0$ sample and applied uniformly across all event times, so that every coefficient in Figure 5 refers to the same local neighborhood of the margin-of-victory threshold.

Properties of our sample

Sample overlap. We estimate a separate regression at each event time and reports point-wise confidence intervals. Overlap is present: a district whose state raises taxes 2 periods after the current election qualifies as treated at $k = -2$ in the current year and as a control unit at $k = -1$ in the following election year. This overlap arises from the same staggered election cycle design that generates the within-state identifying variation. Our standard errors do not account for this cross-event-time covariance. The individual confidence intervals are valid, but a formal joint test of pre-trend coefficients would require additional adjustment. In practice, we rely on visual evidence from Figure 5. In comparison, staggered DiD estimators either assign each unit to exactly one treatment cohort (Callaway and Sant'Anna, 2021) or groups treated units into cohorts based on when they first change their treatment, and estimates effects separately for each cohort before aggregating (De Chaisemartin and d'Haultfoeuille, 2024). For the first approach, there is no mechanical overlap: a unit in cohort g contributes to $ATT(g, t)$ cells only. For the latter, overlap between cohorts is avoided by construction: treated units are grouped into cohorts based on when they first change their treatment, and estimates effects separately for each cohort before aggregating. The control group—units that never change their treatment—is the same across all cohorts. Because each treated unit belongs to exactly one cohort, the same observation cannot appear as treated in two different cohort-level comparisons.

Sample sizes by event time. Table A19 reports, for each event time k, the number of district-year observations and contributing states in the treated group before applying the bandwidth filter. At $k = -j$, treated observations are districts with a gas tax increase exactly j election cycles ahead; at $k = +j$, they experienced an increase exactly j cycles ago. The rows for $k = -4$ and $k = +4$ confirm that extending the test to $K = 4$ yields no treated observations at those horizons, establishing $K = 3$ as the feasible pre- and post-trend horizon.

Panel depth limits the pre-trend horizon. Table A20 reports the share of district-year observations with at least j valid forward election periods. An observation requires j valid forward periods to potentially contribute to the $k = -j$ treated group. Only 0.3% of observations have four valid forward periods, and even among these none has a gas tax increase exactly four cycles ahead with no intervening increase or decrease, leaving the $k = -4$ treated group empty. The sharp decline in coverage from $j = 1$ to $j = 3$—from 60.8% to 13.3% in the Republican sample—reflects redistricting-induced attrition: district boundaries change approximately every decade, truncating the forward sequences available for observations near a redistricting boundary. This attrition is the primary structural reason why pre-trend evidence is most informative at $k = -1$ and $k = -2$, and why the $k = -3$ estimates carry wide confidence intervals relative to those at shorter horizons.

Never-changers are too scarce to serve as controls. Table A21 reports the number of gas tax increases per state over the sample period. In the data, 93.8% of states increase the gas tax more than once, and only two states never increase it. This rules out the two standard control group constructions from the modern DiD literature. The Callaway and Sant'Anna (2021) estimator prohibits recurring treatments by construction and is therefore infeasible. The De Chaisemartin and d'Haultfoeuille (2024) estimator accommodates recurring treatments but requires never-changers as the control group—a pool of only two states in our sample, far too small to serve as a credible counterfactual for 46 treated states. The not-yet-treated control group with event-time-specific windows, described in Section 4.2.1, is therefore the only feasible option.

Overlap between event times. Table A22 reports the pairwise overlap between treated groups at different event times for the Republican sample; a similar pattern holds for Democrats. The diagonal entries are the sizes of each treated event-time group, while the off-diagonal entries show how often observations are reused across treated event-time groups. Off-diagonal counts arise when the same observation can be assigned to more than one treated event-time group, that is observations classified as treated at a given event time, not the full regression sample.

800 district observations—21% of the $k = -1$ treated group—also appear in the $k = +1$ treated group, and 163 observations appear in both $k = -1$ and $k = +2$. This overlap arises from the staggered election cycle design: the same district can face a gas tax change that qualifies it as treated at one event time and as a not-yet-treated control at a different event time. It is a consequence of the within-state variation that identifies the main effect.

Critically, the overlap is exclusively between pre- and post-treatment event times. There is no overlap among the pre-treatment event times themselves: $k = -1$, $k = -2$, and $k = -3$ are estimated on fully disjoint samples, so there is no mechanical correlation among the pre-trend coefficients. The pre-trend test is therefore not affected by this problem. The concern would arise only if one attempted a joint Wald test combining coefficients from both sides of treatment, which we do not pursue; the pre-trend evidence instead relies on visual inspection of the point-wise confidence intervals in Figure 5.

Appendix C. Google trends data & LP-DiD estimation details

Google trends data

Google Trends normalizes search data by dividing each data point by the total searches in that time and location, scaling this relative popularity measure from 0 to 100. This normalization allows for comparison between different terms and regions with varying overall search volumes. Google Trends filters out low volume searches, repeated searches by individuals, and queries with special characters, while only including popular search terms in the data sample[1]. The Google Trends data span from January 2004 to March 2022 and are available at the state and month level. Since our gas tax dataset is at the state and year level, we calculate the yearly mean Google search relative popularity index to aggregate the Google Trends data for each state.

LP-DiD estimator

The LP-DiD estimator is a versatile regression-based framework developed by Dube et al. (2023) for estimating average treatment effects on the treated (ATT) in a setting with multiple treatment cohorts. The LP-DiD combines the *local projections* approach (Jordà, 2005) to estimate heterogeneous and dynamic responses with the *clean control* condition (Cengiz et al., 2019) to limit the set of permissible comparisons and avoid bias. LP-DiD is flexible and can accommodate the classic binary absorbing treatment without covariates but can also be generalized to include control variables or deal with non-absorbing treatments or continuous treatments.

In our context, states undergo gasoline tax increases more than once with arguably not permanent effects on Google searches. That is, in our setting, the treatment is non-absorbing. The LP-DiD estimator addresses non-absorbing treatment by incorporating, on top of the no anticipation and parallel trends assumptions, the additional assumption that dynamic effects stabilize after a finite number of periods, denoted as L. Leveraging this assumption, the estimator calculates a convex weighted average treatment effects on the treated (ATT) on the dependent variable $\Delta_h y_{it} = y_{t+h} - y_{t-1}$ by excluding observations experiencing a change in treatment status between $t - L$ and $t - 1$ or between $t + 1$ and $t + h$.

To estimate the effect of a gas tax increase on the number of Google searches, we estimate the following LP-DiD specification,

$$\Delta_h y_{it} = \beta_h \Delta D_{it} + \sum_{p=1} \gamma_p^h \Delta y_{i,t-p} + \delta_t^h + e_{it}^h, \tag{A14}$$

where the dependent variable $\Delta_h y_{it}$ is the long difference in Google searches between $t + h$ and $t - 1$. The independent variable ΔD_{it} is an indicator for a gas tax increase, and the specification also includes outcome lags Δy_{it-p} and time fixed effects δ_t^h.

[1] see: https://medium.com/google-news-lab/what-is-google-trends-data-and-what-does-it-mean-b48f07342ee8

Appendix D. Construction of penalty determinants data

Dependent Variable. The outcome is a constructed state-year-party measure of electoral penalty exposure. In a first step, we recover year-party-level penalty estimates $\hat{\tau}^{p}_{DD,t+1}$ by augmenting the baseline Diff-in-Disc specification in Equation (2) with interactions between the incumbent-winner indicator and election-year dummies, estimated separately for each party using a 0.15 bandwidth. Year-party cells with fewer than 50 legislative races within the bandwidth are excluded. The resulting estimates, plotted in Appendix Figure A10, represent the average incumbency penalty (in vote-share units) attributable to a gas tax increase in a given election year, normalized to zero in 1984.

In the second step, we assign $\hat{\tau}^{p}_{DD,t+1}$ to all states that raised the gas tax in that year-party cell, and zero to states that did not. The zero assignment is structural: the incumbency penalty requires both winning and raising the gas tax simultaneously, so non-raising states have zero penalty exposure by construction. The constructed state-level outcome is therefore $\tilde{\hat{\tau}}^{p}_{DD,s,t+1} = 100 \times \hat{\tau}^{p}_{DD,t+1} \cdot G_{s,t+1}$, where $G_{s,t+1}$ is an indicator for a gas tax increase. Observations in which the gas tax decreased are excluded. The sample is restricted to 1994–2016, the period for which Cook PVI data are available (allowing up to three years of extrapolation from the 1997 baseline).

Covariates. Three state-level covariates are matched to each penalty year via interpolation and standardized to have mean zero and unit standard deviation before entry into the regression.

Partisan balance ($PartBal_{s,t+1}$) measures how electorally competitive the state is. At the district level, each congressional district's Cook PVI score is converted to a signed measure: Republican-leaning districts receive a positive value and Democratic-leaning districts a negative value, so that a district rated R+5 is coded as +5 and one rated D+5 as −5. These are averaged across all congressional districts in the state to yield a state-level partisan lean, $\text{lean}_{s,t+1}$. Partisan balance is then defined as:

$$PartBal_{s,t+1} = -|\text{lean}_{s,t+1}|,$$

so that values closer to zero correspond to more politically balanced states. The variable is z-scored globally across all state-cycle observations.

Ideological intensity ($Ideol^{p}_{s,t+1}$) measures how ideologically extreme the party's identifiers and leaners are in a given state and year. It is constructed from two Pew Research survey series: Trends in Political Values and Core Attitudes (1987–2003) and the Political Typology Survey (2004, 2011, 2014, 2017), with linear interpolation over three-year intervals between waves.

For each survey respondent, we score nine issue questions as +1 (conservative response), −1 (liberal response), or 0 (other, don't know, or missing), and sum across questions to yield an individual ideology score $\iota_i \in [-9, 9]$. We then compute state-year-party means separately for Republican and Democratic identifiers and leaners, yielding

$\bar{I}^R_{s,t+1}$ and $\bar{I}^D_{s,t+1}$. Ideological intensity is then defined as:

$$Ideol^p_{s,t+1} = \begin{cases} \bar{I}^R_{s,t+1} & \text{if } p = \text{Republican}, \\ -\bar{I}^D_{s,t+1} & \text{if } p = \text{Democratic}, \\ \dfrac{\bar{I}^R_{s,t+1} - \bar{I}^D_{s,t+1}}{2} & \text{if } p = \text{Pooled}, \end{cases}$$

so that higher values consistently indicate greater ideological intensity for the relevant party: conservatism among Republicans, liberalism among Democrats, and ideological distance between the parties in the pooled sample. The variable is z-scored within each party sample, so that Republican, Democratic, and pooled observations are each compared to others from the same sample.

Per-capita tax cost ($\Delta COSTpc_{s,t+1}$) measures the direct fiscal burden of the gas tax increase on a representative resident. It is defined as:

$$\Delta COSTpc_{s,t+1} = \text{gallons}_{s,t+1} \times \Delta\text{gastax}_{s,t+1},$$

where $\Delta\text{gastax}_{s,t+1}$ is the change in the state gas tax rate (cents per gallon) and $\text{gallons}_{s,t+1}$ is per-capita gasoline consumption, constructed as:

$$\text{gallons}_{s,t+1} = \frac{\text{VMT per capita}_{s,t+1}}{\text{fleet fuel economy}_{s,t+1}}.$$

VMT per capita comes from the Bureau of Transportation Statistics; fleet fuel economy comes from the EPA Automotive Trends Report (Table 3.1). The variable is standardized globally across all state-cycle observations. For non-raising states, $\Delta\text{gastax}_{s,t+1} = 0$ and therefore $\Delta COSTpc_{s,t+1} = 0$, which after global standardization takes the constant value $c = -\mu_{\Delta COSTpc}/\sigma_{\Delta COSTpc}$. This implies an exact linear relationship between the main effect of $\Delta COSTpc_{s,t+1}$, the interaction $G_{s,t+1} \times \Delta COSTpc_{s,t+1}$, and the treatment indicator $G_{s,t+1}$, so the main effect is not separately identified. Only the interaction is included in the regression; it captures heterogeneity in the per-capita tax burden across raising states.

Regression. We estimate the following specification by OLS separately for Democratic, Republican, and pooled samples:

$$\begin{aligned} \tilde{\hat{\tau}}^p_{DD,s,t+1} &= \beta_1 G_{s,t+1} + \beta_2 Ideol^p_{s,t+1} + \beta_3 PartBal_{s,t+1} + \beta_4 G_{s,t+1} \times Ideol^p_{s,t+1} \\ &\quad + \beta_5 G_{s,t+1} \times PartBal_{s,t+1} + \beta_6 G_{s,t+1} \times \Delta COSTpc_{s,t+1} + \theta_s + \lambda_{t+1} + \varepsilon_{s,t+1}, \end{aligned} \tag{A15}$$

where $G_{s,t+1}$ is a binary variable equal to one if the state raised the gas tax in the electoral cycle leading up to election $t+1$. The main effects β_2 and β_3 partial out baseline differences in the covariates for non-raising states, where $\tilde{\hat{\tau}}^p_{DD,s,t+1} = 0$ by construction; the interaction coefficients β_4–β_6 are the objects of interest. Columns without fixed effects identify from both cross-sectional and temporal variation; columns with state and year fixed effects (θ_s, λ_{t+1}) absorb persistent state differences and common year shocks. Because $\tilde{\hat{\tau}}^p_{DD,t+1}$ varies only at the year-party level, the within-R^2 in fixed-effects specifications is largely driven by year fixed effect absorption. Standard errors are clustered at the state level throughout.

Appendix E. Localized gasoline taxes

Nehiba (2022) calculates county-level gasoline taxes that reflect the local marginal external cost of driving, which varies substantially within states. The paper derives county-level taxes for 380 counties across the U.S. based on availability of gas price data. The author solves for optimal county level vehicle miles traveled (VMT) in 2019 that would maximize consumer surplus net of external costs and calculates county-specific fuel taxes as the difference between those at optimum VMT and current fuel prices including current taxes. He also imposes constraints on how high fuel taxes could be set reflecting political constraints about extreme adjustments. The optimization problem that recovers these tax levels is:

$$\max_{VMT} \sum_{i=1}^{380} \int_0^{VMT_i} P_i(\nu_i) d\nu_i - C_i\left(VMT_i\right)$$
$$\text{subject to } \sum_{i=1}^{380} t_i \cdot VMT_i = \sum_{i=1}^{380} t_s \cdot V\bar{M}T_i,$$

where the first term in the integral is the inverse demand function, the second are private and social costs of driving in county i. The constraint ensures revenue neutrality t_s is current state-level tax. There is also a non-negativity constraint on taxes. Lastly, the author imposes a variety of constraints on how high the gas tax can be set, which are intended to reflect political constraints to allowing gas tax to be too large. Here we impose the constraint from the baseline of those results and constrain increases to be no larger than \$1.50. Based on the optimal vector of VMT, optimal county taxes can be derived from the difference between fuel prices at the optimum VMT determined by the model (the first term in the integral with optimum VMT substituted and pretax average fuel costs in a county. Looking at the set of tax changes relative to state taxes in Appendix Figure A9, 73.4% are *decreases*, and the largest tax increases are in urban areas where externalities are concentrated.

Appendix figures & tables

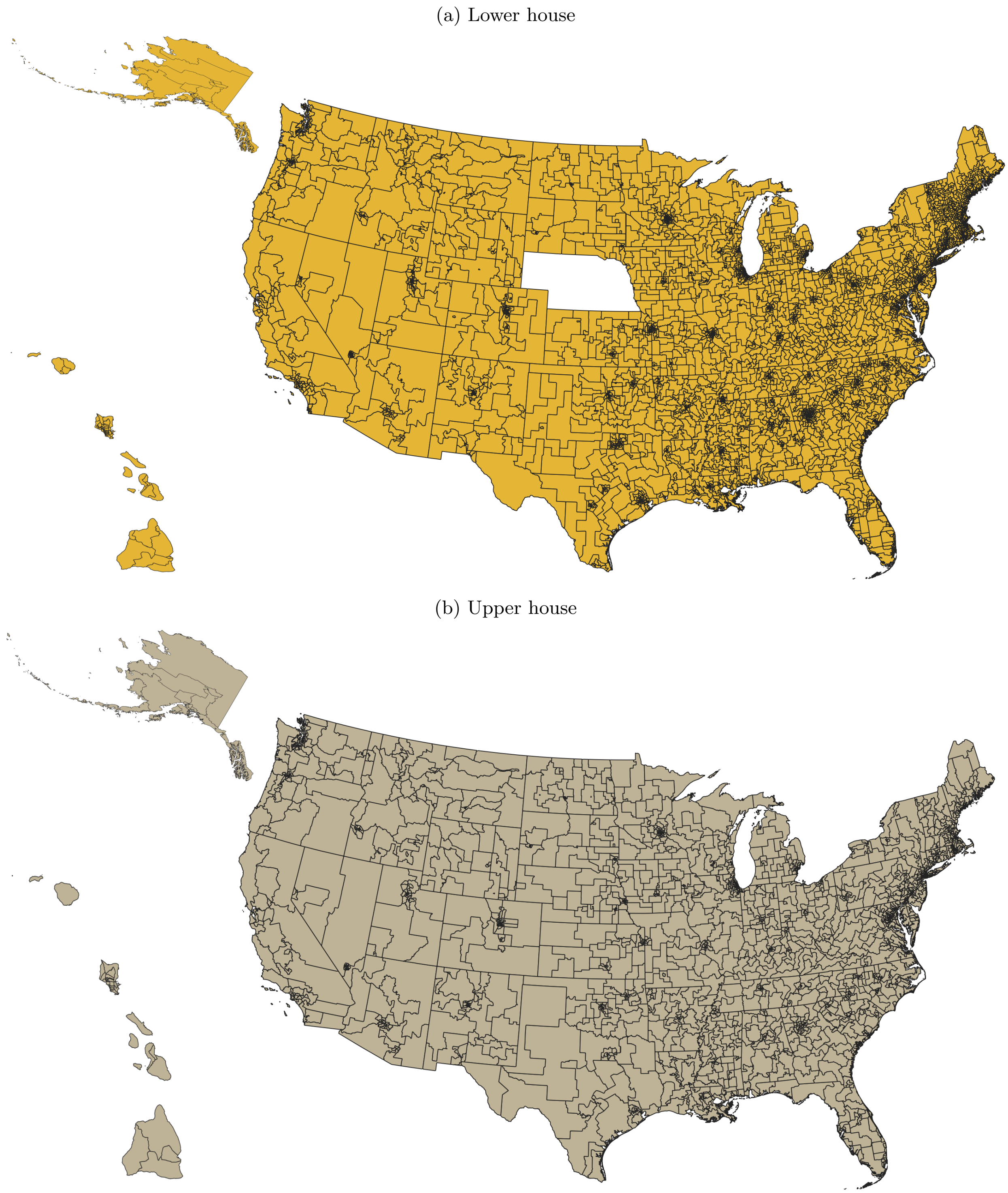


Figure A1: State legislative boundaries 2010

Notes: Nebraska is missing from lower house boundaries because it has a unicameral state legislature (i.e., only one legislative body) corresponding to the boundaries in Panel (b). Boundaries in the maps are not to scale as Alaska and Hawaii's sizes have been adjusted to make their state legislative district boundaries clearer.

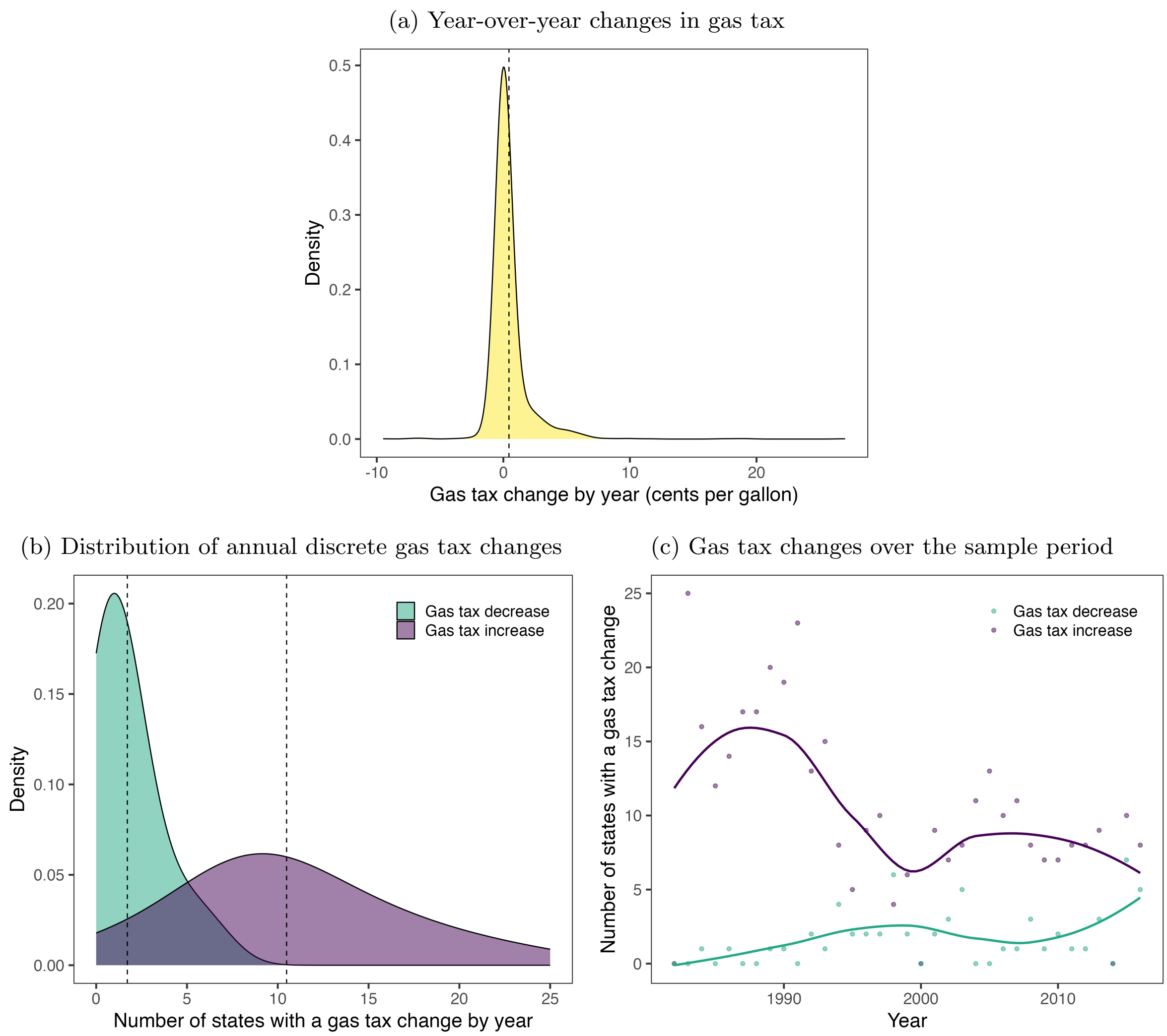


Figure A2: Increases and decreases of the gas tax

Notes: Panel (a) shows the distribution of year-over-year changes in state gas tax rates over 1982–2016, pooling observations across states and years. Panel (b) shows the distribution of the number of states with a gas tax increase or decrease in a given year. The vertical dashed line denotes the mean of each distribution. Panel (c) shows the number of U.S. states that increased (in purple) or decreased (in green) their gasoline tax in each year from 1982 to 2016. All panels include gasoline tax changes that are indexed by legislation to inflation or other economic variables, which we control for in our regression estimates.

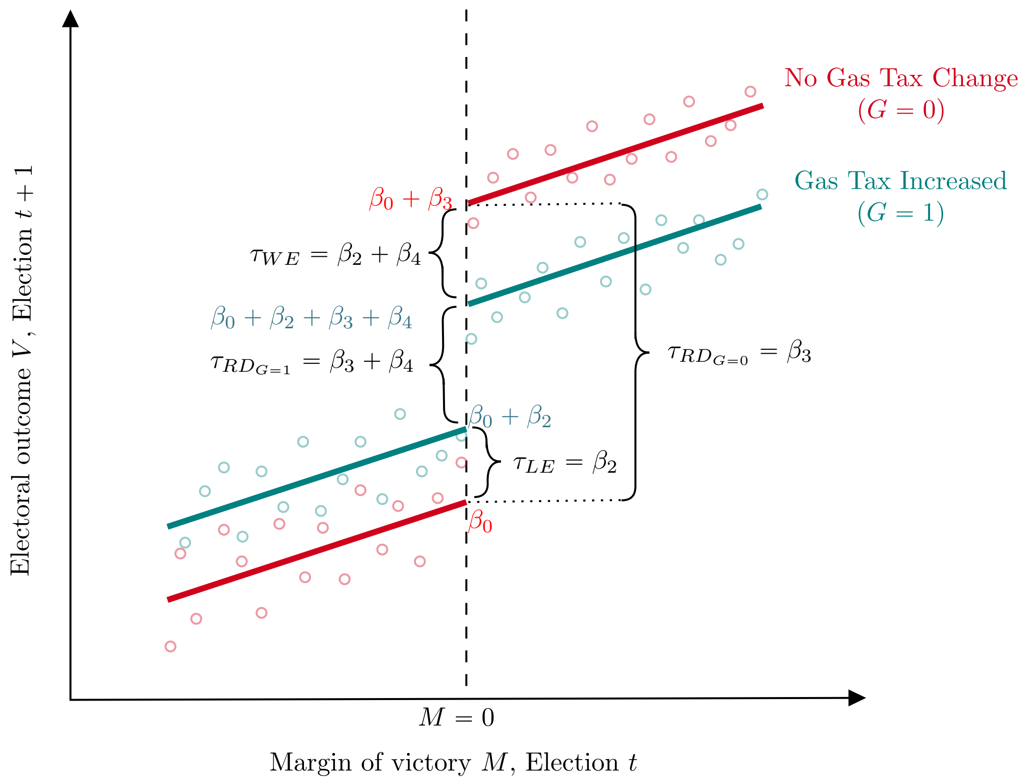


Figure A3: Graphical representation of Difference-in-Discontinuities design for electoral incumbent penality

Notes: This figure is the same as Figure 5, but provides the underlying geometry behind our "Diff-in-Disc" estimates obtained from Equation (2). Our difference-in-discontinuities estimator recovers the average treatment effect on the treated: τ_{DD}. This is the vertical difference between the regression discontinuity estimate for districts where there is no gas tax increase, $\tau_{RD_{G=0}}$, and the regression discontinuity estimate for districts where there is a gas tax increase, $\tau_{RD_{G=1}}$. τ_{DD} is identified because we control for the effect on losers, represented by τ_{LE}. The vertical axis shows the electoral outcome V in election $t + 1$, which is the vote share of the incumbent party in our main regressions.

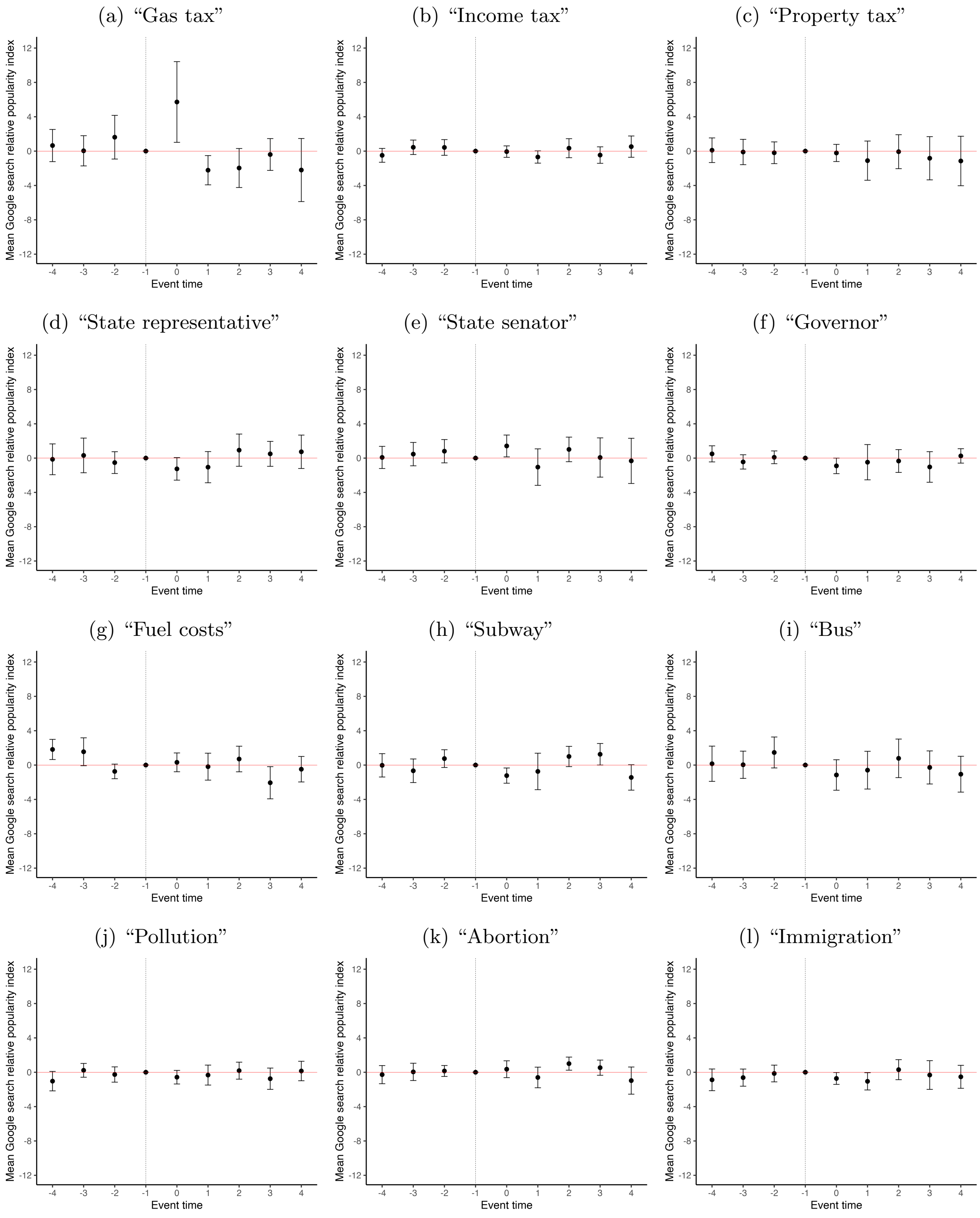
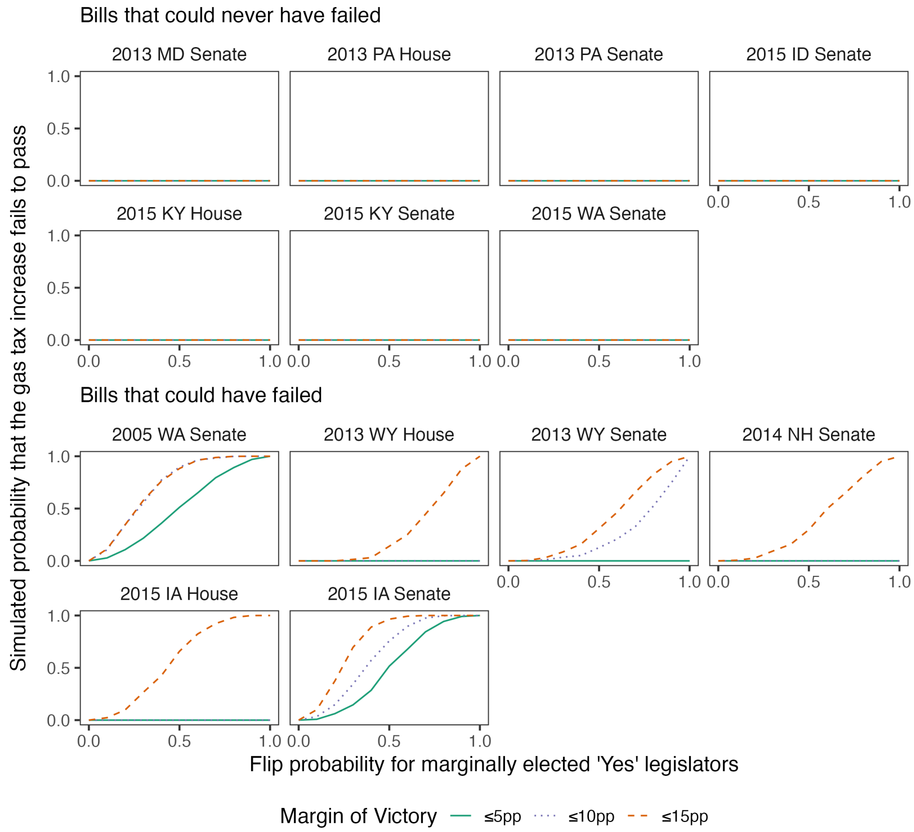


Figure A4: Gas tax increases and Google searches

Notes: Panels (a)–(l) show the effect of a gas tax increase on the number of Google searches for different keywords in the (available) period 2006–2019. Point estimates and their 95% confidence intervals are displayed in black dots and error bars, respectively. The event study model uses local projections with a non-absorbing treatment to estimate $\Delta_h y_{it} = \beta_h \Delta D_{it} + \sum_{p=1} \gamma_p^h \Delta y_{i,t-p} + \delta_t^h + e_{it}^h$, as described in Dube et al. (2023). The dependent variable $\Delta_h y_{it}$ is the long difference in Google searches between $t+h$ and $t-1$. The independent variable ΔD_{it} is an indicator for a gas tax increase, and the specification also includes outcome lags Δy_{it-p} and time fixed effects δ_t^h. We use pre- and post-event windows $h = -4, -3, ..., 3, 4$. The baseline (omitted) period is one year prior to the gas tax increase, indicated by the dashed vertical line. 95% confidence intervals are based on standard errors clustered by state.

(a) Simulated probability that gas tax increases fail if marginal winners switched their "Yes" vote



(b) Share of gas tax increases that would fail if marginal winners switched their "Yes" vote

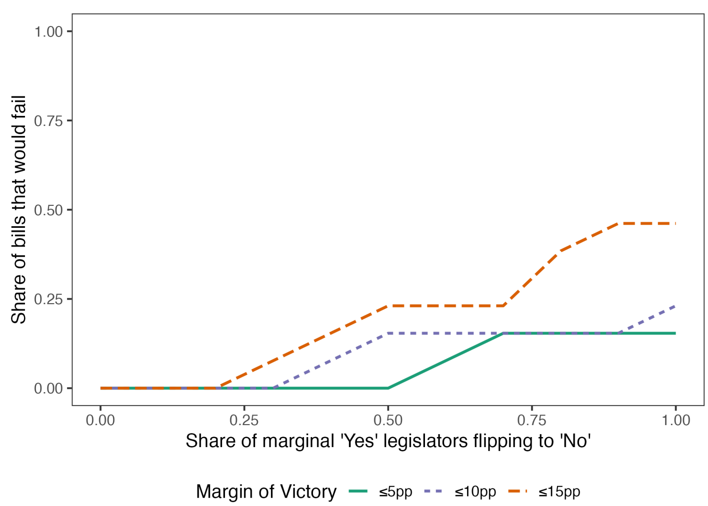


Figure A5: Fragility of gas tax passage to turnover of marginal winners

Notes: This figure presents counterfactual calculations assessing the fragility of gas tax passage to turnover among marginally elected legislators. The sample is restricted to single-member districts across eight states (IA, ID, KY, MD, NH, PA, WA, WY), and pools roll-call votes from 13 gas tax bills. Each legislator–vote observation is matched to the most recent preceding election for that legislator. Marginal legislators are defined using the margin of victory thresholds shown in the legend. Panel (a) reports the simulated probability that a bill fails to pass when marginally elected "Yes" legislators are randomly switched to "No" with the probability shown on the x-axis. Each point is based on 1,000 Monte Carlo simulations. Panel (b) reports the share of bills that would fail under deterministic counterfactuals in which a given fraction of marginal "Yes" legislators switch their vote.

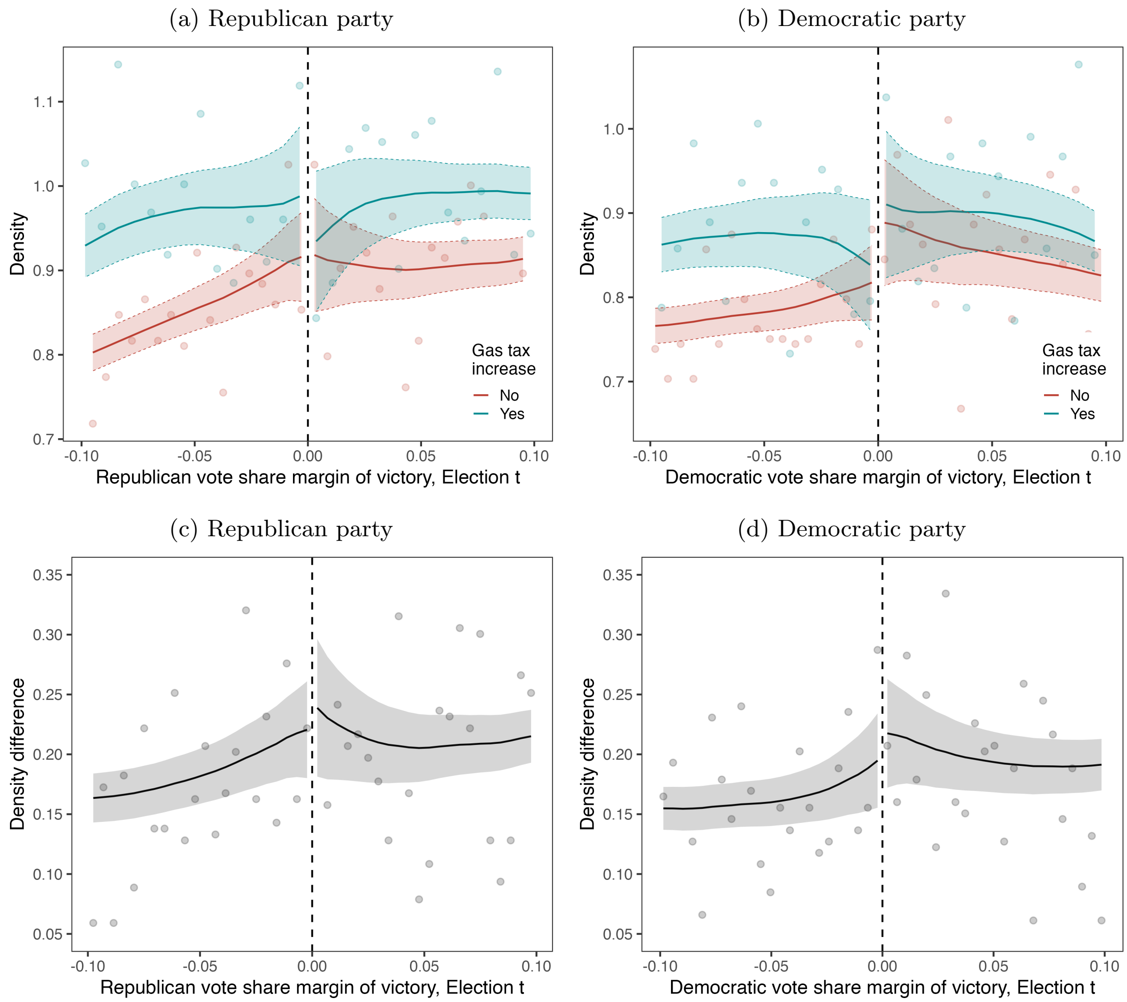
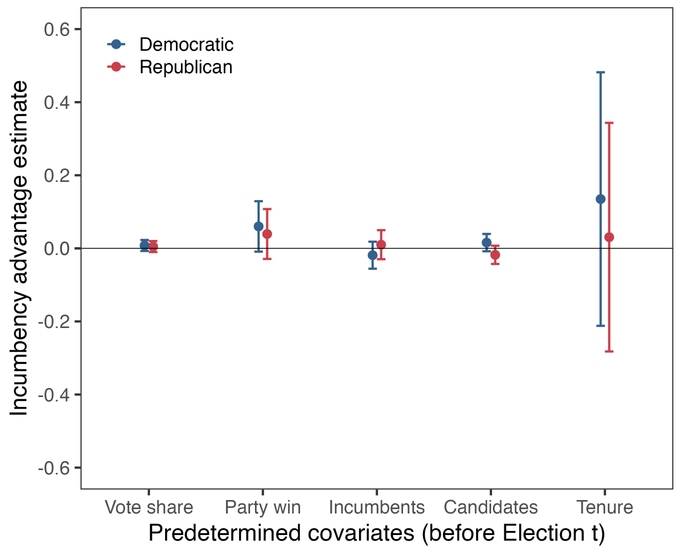


Figure A6: Tests for continuity & manipulation around the discontinuity

Notes: Panels (a) and (b) assess the continuity assumption by plotting the density of the margin of victory in election t separately for districts in states with and without a gas tax increase between elections t and $t+1$. Panels (c) and (d) assess continuity in the difference in these densities around the cutoff. All panels display binned densities (or density differences) and local linear fits on either side of the zero threshold, using a triangular kernel and a common bandwidth. Shaded areas indicate 95% confidence intervals.

(a) Predetermined covariates



(b) Placebo cutoffs

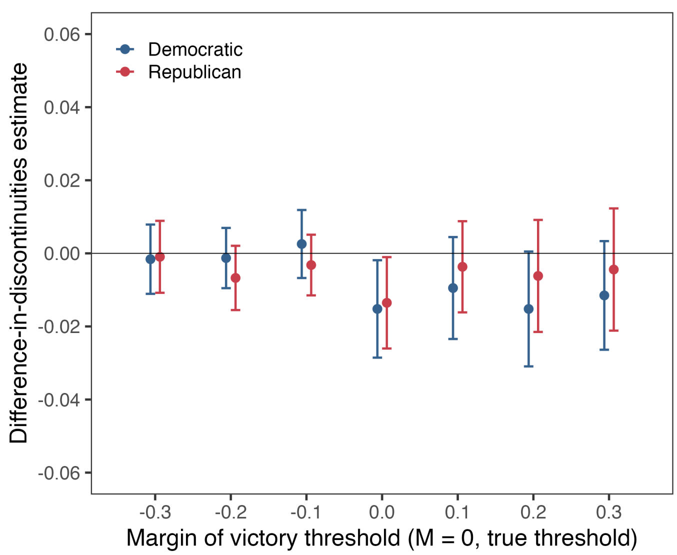


Figure A7: Additional tests for continuity & manipulation around the discontinuity

Notes: Panel (a) shows coefficient estimates from local linear regression discontinuity models in which the dependent variables are predetermined relative to the electoral outcome in election year t. Each specification regresses the outcome on the margin of victory, an indicator for being above the zero threshold, and their interaction, estimated locally around the cutoff using a triangular kernel. The reported coefficients correspond to the discontinuity at the margin-of-victory threshold. "Vote share" is the proportion of votes the party obtained in election year $t-1$. "Party win" is an indicator for whether the party won the race in election year $t-1$. "Incumbents" is the number of sitting members running for re-election in election year t. "Candidates" is the number of candidates running in election year t. "Tenure" is the number of years the party had held the seat prior to election year t. Bandwidths are selected using outcome-specific MSE-optimal two-sided procedures (Calonico et al., 2014). Standard errors are clustered at the state level, and error bars indicate 95% confidence intervals. Panel (b) shows placebo-cutoff estimates from local linear regressions in which we artificially vary the margin-of-victory threshold. For each cutoff, we redefine the indicator for being above the threshold and re-estimate the preferred specification from Table 1. The reported coefficients correspond to β_4 in Equation (2). All regressions include covariates, state-by-election year fixed effects, and a triangular kernel. Bandwidths are selected separately for each placebo cutoff using the MSE-optimal two-sided procedure. Error bars indicate 95% confidence intervals based on standard errors clustered at the state level.

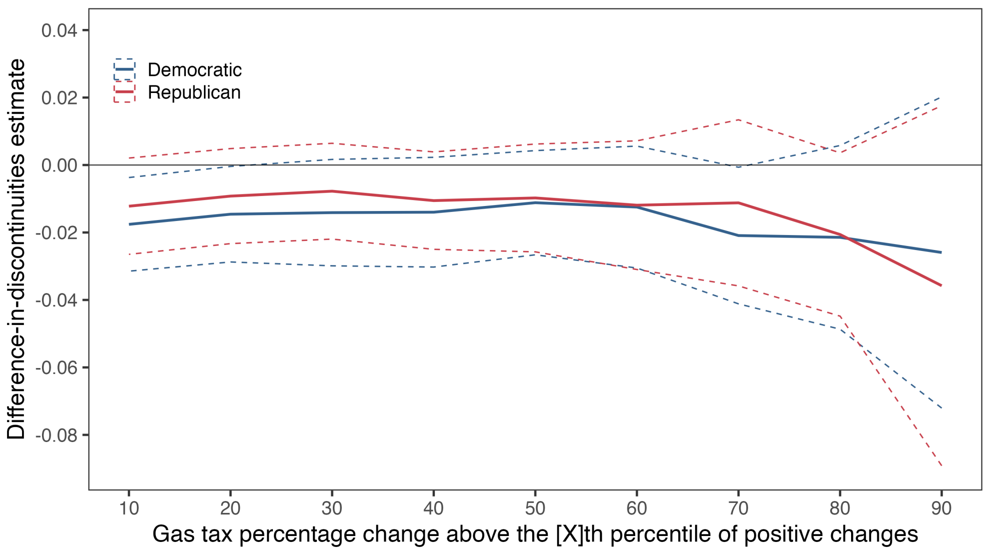


Figure A8: Effect of the size of gas tax increases, categorized by percentile

Notes: The figure presents coefficient estimates of β_4 from Equation (2) using alternative election samples that exclude positive gas tax increases at or below a given within-state percentile of gas tax percentage changes. The estimates are obtained from nine regressions, with thresholds ranging from the 10th to the 90th percentile of positive gas tax percentage changes within each state. In all cases, the sample excludes elections followed by gas tax decreases. All regressions are estimated using our preferred specification from Table 1, including covariates, state-by-election year fixed effects, and a triangular kernel. Shaded areas indicate 95% confidence intervals based on standard errors clustered at the state level.

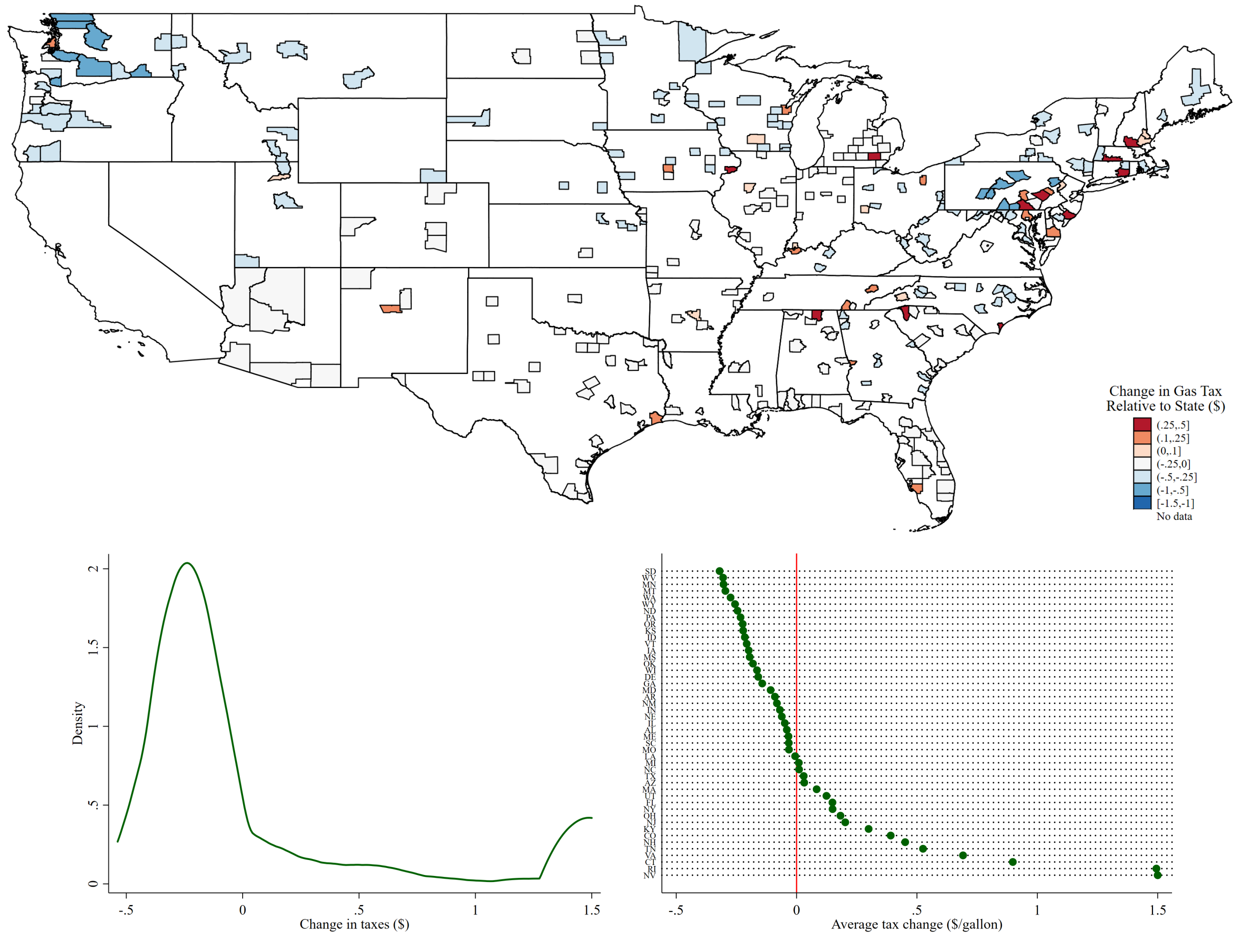


Figure A9: Distribution of optimal tax changes across 380 counties constraining increases $\leqslant$ \$1.50

Notes: Panel (a) plots a map and the distribution of the implied gas tax changes from converting state-level gas taxes in 2019 to their second-best optimal county-level following Nehiba (2022). Panel (b) shows the distribution those tax changes and Panel (c) shows average state tax changes. The procedure for calculating optimal taxes is described in Appendix E.

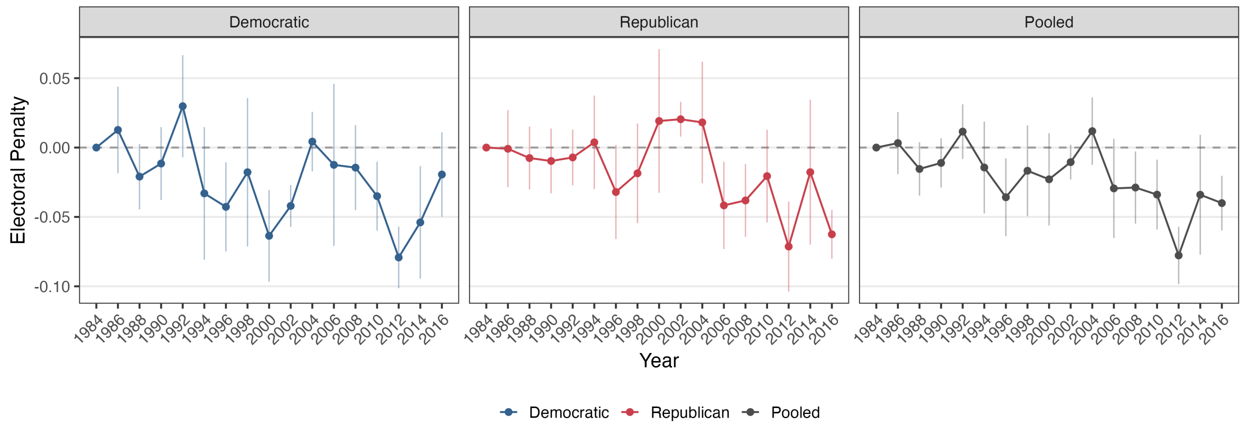


Figure A10: Yearly Penalty Estimates

Notes: This figure shows yearly penalties estimated over the sample as described in Appendix D and which are used to decompose components of electoral penalty.

Table A1: Election summary statistics

Panel (a). Election composition.				
	Percentage of elections			
	No candidates	1 candidate	2 candidates	⩾ 3 candidates
All	0.00	0.32	0.59	0.09
Democrats	0.16	0.84	0.00	0.00
Republicans	0.22	0.78	0.00	0.00
Other party	0.86	0.12	0.01	0.00
Panel (b). Districts and spacing of election years at the state level.				
	Mean	Std. Deviation	Min	Max
Districts				
House	90.23	41.77	21	203
Senate	36.18	12.57	13	67
Elections spacing				
House	2.18	0.51	2	4
Senate	3.31	0.85	2	4

Notes: This table shows summary statistics for state legislative elections over 1982–2016. Panel (a) shows the share of elections in single-member districts with 0, 1, 2, or 3 or more candidates, separately for all candidates and for Democratic, Republican, and other candidates. Panel (b) shows summary statistics for the number of districts and the spacing between elections for State House and State Senate chambers. The unit of observation is the state. Mean, standard deviation, minimum, and maximum are computed across states, where each state's value is averaged across redistricting regimes. Election spacing is measured as the number of years between consecutive elections within a district, averaged within each state and chamber.

Table A2: Gas tax summary statistics in cents

Panel (a). Raw sample	Mean	Std. Deviation	Min	Max	Obs.
Levels	19.02	6.31	5.000	50.50	1785
All changes	0.44	1.64	-9.500	27.00	427
Decreases	-1.39	1.99	-9.500	-0.05	60
Increases	2.32	2.69	0.025	27.00	367
Panel (b). Republican estimation sample	**Mean**	**Std. Deviation**	**Min**	**Max**	**Obs.**
Levels	18.83	6.38	5.00	39.50	47138
All changes	1.06	2.58	-9.50	19.10	18854
Decreases	-1.73	2.26	-9.50	-0.10	2428
Increases	3.31	3.20	0.05	19.10	16426
Panel (c). Democrat estimation sample	**Mean**	**Std. Deviation**	**Min**	**Max**	**Obs.**
Levels	18.64	6.52	5.00	39.50	51047
All changes	1.06	2.46	-9.50	19.10	20782
Decreases	-1.67	2.22	-9.50	-0.10	2569
Increases	3.21	2.94	0.05	19.10	18213

Notes: This table shows summary statistics for state gasoline taxes (in cents per gallon) over 1982–2016. The first row in each panel reports statistics on tax levels, while the remaining rows describe the distribution of tax changes: all nonzero changes, decreases only, and increases only. Panel (a) uses the raw state-year panel and defines tax changes on an annual (year-over-year) basis. Panels (b) and (c) use the estimation samples for Republican and Democratic legislators, respectively, used to estimate Equation (2), and define tax changes over electoral cycles, i.e., between elections t and $t + 1$.

Table A3: Factors that explain gas tax increases

	(1)	(2)	(3)	(4)	(5)	(6)
Log road mileage per capita	0.053	0.118	-0.005	-0.010	0.055	0.154
	(0.033)	(0.142)	(0.036)	(0.165)	(0.175)	(0.192)
Log licensed drivers per capita	0.081	0.014	0.155	-0.021	0.195	-0.257
	(0.187)	(0.247)	(0.203)	(0.253)	(0.358)	(0.307)
Log autos per capita	0.171**	0.049	0.099	0.034	0.034	0.081
	(0.069)	(0.092)	(0.070)	(0.084)	(0.121)	(0.094)
Log vehicle miles traveled per capita	-0.158	0.102	-0.212	0.028	0.109	-0.017
	(0.115)	(0.249)	(0.130)	(0.333)	(0.375)	(0.398)
Log real personal income per capita	0.405**	0.229	0.099	0.024	0.032	-0.004
	(0.199)	(0.500)	(0.225)	(0.659)	(0.700)	(0.840)
Log tax-exclusive gas price	0.032	0.056	-0.782***	-1.492***	-1.309***	-1.394***
	(0.043)	(0.050)	(0.202)	(0.216)	(0.281)	(0.319)
Gas tax indexing	0.392***	0.438***	0.382***	0.428***	0.444***	0.395***
	(0.068)	(0.077)	(0.061)	(0.070)	(0.102)	(0.071)
Log average family size	-0.271	-0.361	-0.350	-0.315	-0.256	-0.102
	(0.286)	(0.381)	(0.305)	(0.395)	(0.427)	(0.427)
Fraction of adults graduating high school	0.008**	0.013	0.012***	0.010	0.011	0.028*
	(0.004)	(0.012)	(0.004)	(0.013)	(0.014)	(0.017)
Fraction of adults with BA	-0.002	0.012	-0.004	0.004	0.007	0.013
	(0.005)	(0.016)	(0.005)	(0.019)	(0.026)	(0.024)
Urban population share	0.145	-0.829	-0.114	-1.117	-1.107	0.151
	(0.129)	(1.498)	(0.150)	(1.376)	(1.653)	(2.109)
Log GDP per capita	-0.365***	-0.600**	0.021	-0.152	-0.323	-0.172
	(0.112)	(0.271)	(0.175)	(0.464)	(0.507)	(0.684)
Manufacturing share of GDP	0.199	0.295	-0.196	-0.082	-0.320	0.742
	(0.273)	(0.395)	(0.305)	(0.657)	(0.769)	(1.007)
Mining share of GDP	-0.034	0.736	-0.486	-0.095	-0.037	-0.269
	(0.207)	(0.599)	(0.338)	(0.940)	(1.119)	(1.081)
Percent state budget surplus	0.049	0.034	0.060	0.057	0.073	0.021
	(0.044)	(0.042)	(0.058)	(0.050)	(0.186)	(0.045)
Unemployment	0.012	0.009	0.015	0.016	0.019	0.019
	(0.010)	(0.011)	(0.014)	(0.019)	(0.020)	(0.020)
Democratic vote share	-0.055	-0.005	-0.099	-0.059	-0.119	0.023
	(0.078)	(0.076)	(0.086)	(0.086)	(0.119)	(0.085)
Democrat control of state house	0.023	0.033	0.011	0.014	0.004	0.019
	(0.041)	(0.032)	(0.039)	(0.031)	(0.037)	(0.034)
Democrat control of state senate	0.021	-0.017	0.034	-0.025	-0.030	-0.012
	(0.041)	(0.046)	(0.040)	(0.045)	(0.044)	(0.047)
Average house LCV rating	0.000	0.000	0.000	-0.001	0.000	0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Average senate LCV rating	-0.001	0.000	-0.001	0.000	-0.001	0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Democrat governor	0.010	-0.001	-0.006	-0.002	-0.007	0.002
	(0.019)	(0.023)	(0.018)	(0.023)	(0.027)	(0.025)
Population share in MSA with rail					0.554	
					(0.645)	
Log transit ridership per capita						0.032
						(0.036)
Year FEs			X	X	X	X
State FEs		X		X	X	X
Sample period	1983–2016	1983–2016	1983–2016	1983–2016	1983–2008	1991–2016
Observations	1,600	1,600	1,600	1,600	1,208	1,237

Notes: All specifications are linear probability models. The dependent variable is an indicator equal to one if the state gasoline tax increases relative to the previous year, and zero otherwise. The set of fixed effects varies by column (none, state, year, or state and year) and is reported in the table. "Gas Tax Indexing" is a binary indicator that equals 1 in state-years in which the gasoline tax rate effectively varied due to automatic adjustment to an external variable (e.g., wholesale fuel prices, CPI, or highway construction costs), rather than requiring legislative action to change. "GDP" is the state-level gross domestic product. "LCV rating" is the League of Conservation Voters' annual score card of environmental voting records for members of the U.S. Congress. Columns (5) and (6) add measures of transit access and use (rail MSA coverage and transit ridership per capita), which are available for shorter subperiods as indicated. Standard errors are clustered at the state level and reported in parentheses. Significance levels are denoted as: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A4: Coverage and representativeness of various bandwidths

	Bandwidth size						
Panel (a). Republican party	.5 pp	1 pp	2 pp	5 pp	10 pp	20 pp	100 pp
<u>Election level</u>							
Percent of total elections	0.9	1.8	3.7	9.1	18.2	35.6	100.0
Northeast	0.2	0.4	0.9	2.3	4.7	9.1	26.7
Midwest	0.3	0.6	1.1	2.8	5.6	11.2	30.4
South	0.2	0.4	0.8	2.0	4.1	7.9	23.6
West	0.2	0.4	0.8	2.0	3.8	7.4	19.3
Mean winner margin of victory	0.3	0.5	1.0	2.5	5.0	10.1	47.6
Mean tenure	1.8	1.7	1.7	1.7	1.7	2.0	3.2
Mean number of candidates	2.1	2.1	2.1	2.1	2.1	2.1	1.9
Mean number of incumbents	0.6	0.6	0.6	0.6	0.6	0.7	0.8
<u>State level</u>							
Mean road mileage per capita	24.7	24.0	23.4	23.7	23.6	23.4	21.1
Mean driver license per capita	692.3	694.8	695.7	695.0	694.9	693.3	686.8
Mean VMT per capita	9.8	9.7	9.7	9.7	9.7	9.7	9.6
Mean real personal income per capita	38.9	38.9	39.1	38.9	39.0	39.1	39.5
Percent with gas tax increase ($t+1$)	32.1	35.6	36.6	37.1	37.7	37.0	34.8

	Bandwidth size						
Panel (b). Democratic party	.5 pp	1 pp	2 pp	5 pp	10 pp	20 pp	100 pp
<u>Election level</u>							
Percent of total elections	0.8	1.7	3.4	8.4	16.8	32.7	100.0
Northeast	0.2	0.4	0.9	2.2	4.4	8.6	29.8
Midwest	0.3	0.5	1.0	2.6	5.2	10.2	28.0
South	0.2	0.4	0.8	1.9	3.8	7.1	25.1
West	0.2	0.4	0.7	1.8	3.5	6.8	17.1
Mean winner margin of victory	0.3	0.5	1.0	2.5	5.0	9.7	55.0
Mean tenure	2.2	2.1	2.0	2.2	2.3	2.4	4.5
Mean number of candidates	2.1	2.1	2.1	2.1	2.1	2.1	1.9
Mean number of incumbents	0.6	0.6	0.6	0.6	0.7	0.7	0.8
<u>State level</u>							
Mean road mileage per capita	24.6	24.0	23.4	23.6	23.5	23.3	19.8
Mean driver license per capita	692.7	694.9	695.3	695.1	695.0	693.4	686.2
Mean VMT per capita	9.7	9.7	9.7	9.6	9.7	9.6	9.4
Mean real personal income per capita	38.9	38.9	39.0	39.0	39.1	39.1	39.4
Percent with gas tax increase ($t+1$)	33.2	36.3	36.8	37.2	37.9	37.2	35.7

Notes: This table shows changes in the composition of the sample in terms of key outcomes and attributes (rows) as the bandwidth for margin of victory is widened around 0. Panel (a) is constructed using elections with Republican incumbents and Panel (b) using elections with Democratic incumbents. Columns correspond to the percentage point (pp) symmetric bandwidth, where the rightmost column (100 pp) is the entire sample of Republican or Democratic incumbents, respectively.

Table A5: Diff-in-Disc estimates of electoral incumbent penalty: Probability of victory in election t+1

Panel (a). Republican party	(1)	(2)	(3)	(4)	(5)	(6)
Win Election ($\mathbf{1}\{M_{it} \geqslant 0\}$)	0.337***	0.344***	0.347***	0.348***	0.354***	0.355***
	(0.026)	(0.025)	(0.025)	(0.025)	(0.024)	(0.025)
Gas Tax (G_{it})	-0.003	-0.001	-0.006	-0.005	0.021	0.044
	(0.027)	(0.026)	(0.024)	(0.023)	(0.037)	(0.041)
Win Election × Gas Tax	-0.037*	-0.036*	-0.034*	-0.032	-0.033*	-0.030
	(0.019)	(0.019)	(0.019)	(0.019)	(0.019)	(0.021)
State FEs			X	X		
Year FEs			X	X		
State-Year FEs					X	X
Covariates			X	X	X	X
Right Bandwidth	0.101	0.130	0.100	0.130	0.100	0.130
Left Bandwidth	0.108	0.145	0.101	0.130	0.101	0.130
Kernel	Uniform	Triangular	Uniform	Triangular	Uniform	Triangular
Observations	8,445	11,082	8,184	10,542	8,184	10,542

Panel (b). Democratic party	(1)	(2)	(3)	(4)	(5)	(6)
Win Election ($\mathbf{1}\{M_{it} \geqslant 0\}$)	0.344***	0.342***	0.361***	0.352***	0.359***	0.358***
	(0.027)	(0.028)	(0.028)	(0.027)	(0.026)	(0.026)
Gas Tax (G_{it})	0.034	0.033	0.039*	0.040*	0.000	0.008
	(0.021)	(0.022)	(0.020)	(0.020)	(0.053)	(0.053)
Win Election × Gas Tax	-0.049***	-0.046**	-0.049**	-0.044**	-0.045**	-0.042**
	(0.018)	(0.019)	(0.018)	(0.019)	(0.018)	(0.020)
State FEs			X	X		
Year FEs			X	X		
State-Year FEs					X	X
Covariates			X	X	X	X
Right Bandwidth	0.094	0.128	0.087	0.118	0.087	0.118
Left Bandwidth	0.101	0.128	0.097	0.125	0.097	0.125
Kernel	Uniform	Triangular	Uniform	Triangular	Uniform	Triangular
Observations	7,897	10,323	7,522	9,783	7,522	9,783

Notes: The table presents estimates from 12 regressions, shown separately for the Republican and Democratic parties. The dependent variable is an indicator for whether the indicated party wins the election in year $t+1$. The running variable is the margin of victory in election year t, M_{it}. "Win Election" is an indicator equal to one for a positive margin of victory in election t, $\mathbf{1}M_{it} \geqslant 0$. Gas Tax ($G_{it}$) is an indicator equal to one if the gas tax increased between elections t and $t+1$. Observations with gas tax decreases are excluded. Columns (1), (3), and (5) use a uniform kernel, while columns (2), (4), and (6) use a triangular kernel. Columns (3) and (4) include state and year fixed effects. Columns (5) and (6) include state-by-election year fixed effects. Columns (3)–(6) include covariates. Covariates determined one year before election year $t+1$ include the state unemployment rate, per capita road mileage, licensed drivers per capita, state vehicle miles traveled per capita, real personal income per capita, the state's average pre-tax real gas price, indicators for the party controlling the state house and senate, and their interaction with an indicator for the current president's party. Additional covariates include an indicator for whether the gas tax is indexed (to e.g., wholesale fuel prices, CPI, or highway construction costs) and an indicator for whether election year $t+1$ is a presidential election year. Covariates determined in election year t include the number of candidates running, number of incumbents running, party tenure in office, and normal party vote share. Bandwidths are selected using the MSE-optimal procedure of Calonico et al. (2014). Sample sizes differ across columns only due to optimal bandwidth selection, which depends on the included covariates. All specifications are estimated on a common sample that excludes observations with missing values in any covariate. Clustered standard errors at the state level are in parentheses.

Table A6: Diff-in-Disc estimates of electoral incumbent penalty: vote share in election t+1 when gas tax decreases

Panel (a). Republican party	(1)	(2)	(3)	(4)
Win Election ($\mathbf{1}\{M_{it} \geqslant 0\}$)	0.089***	0.089***	0.090***	0.089***
	(0.007)	(0.007)	(0.006)	(0.006)
Gas Tax Decrease (GD_{it})	0.002	-0.005	0.052***	0.032
	(0.007)	(0.011)	(0.019)	(0.023)
Win Election × Gas Tax Decrease	0.000	0.002	0.001	0.012
	(0.015)	(0.031)	(0.013)	(0.029)
State-Year FEs			X	X
Covariates			X	X
Decreases include indexed changes	X		X	
Right Bandwidth	0.194	0.194	0.179	0.179
Left Bandwidth	0.171	0.171	0.160	0.160
Kernel	Triangular	Triangular	Triangular	Triangular
Observations	9,682	9,682	8,969	8,969
Panel (b). Democratic party	(1)	(2)	(3)	(4)
Win Election ($\mathbf{1}\{M_{it} \geqslant 0\}$)	0.084***	0.082***	0.087***	0.086***
	(0.008)	(0.008)	(0.008)	(0.008)
Gas Tax Decrease (GD_{it})	0.006	0.010	0.008	0.030
	(0.009)	(0.020)	(0.033)	(0.019)
Win Election × Gas Tax Decrease	-0.033***	-0.020	-0.027***	-0.006
	(0.010)	(0.019)	(0.010)	(0.015)
State-Year FEs			X	X
Covariates			X	X
Decreases include indexed changes	X		X	
Right Bandwidth	0.145	0.145	0.135	0.135
Left Bandwidth	0.142	0.142	0.130	0.130
Kernel	Triangular	Triangular	Triangular	Triangular
Observations	7,566	7,566	7,003	7,003

Notes: The table presents estimates from 8 regressions, shown separately for the Republican and Democratic parties. The dependent variable is vote share for the indicated party in election year $t+1$, V_{t+1}, as in Equation (2). The running variable is the margin of victory in election year t, M_{it}. "Win Election" is an indicator equal to one for a positive margin of victory in election t, $\mathbf{1}\{M_{it} \geqslant 0\}$. Gas Tax Decrease ($GD_{it}$) is an indicator equal to one if the gas tax decreased between elections t and $t+1$. Observations with gas tax increases are excluded. Columns (1) and (2) use a triangular kernel without covariates, while columns (3) and (4) add state-by-election year fixed effects and covariates. Columns (1) and (3) include indexed gas tax decreases in GD_{it}, while columns (2) and (4) set GD_{it} to zero for states with an indexed gas tax. Covariates determined one year before election year $t+1$ include the state unemployment rate, per capita road mileage, licensed drivers per capita, state vehicle miles traveled per capita, real personal income per capita, the state's average pre-tax real gas price, indicators for the party controlling the state house and senate, and their interaction with an indicator for the current president's party. Additional covariates include an indicator for whether the gas tax is indexed (to e.g., wholesale fuel prices, CPI, or highway construction costs) and an indicator for whether election year $t+1$ is a presidential election year. Covariates determined in election year t include the number of candidates running, number of incumbents running, party tenure in office, and normal party vote share. Bandwidths are selected using the MSE-optimal procedure of Calonico et al. (2014). All specifications are estimated on a common sample that excludes observations with missing values in any covariate. Clustered standard errors at the state level are in parentheses.

Table A7: Diff-in-Disc estimates of electoral incumbent penalty: vote share in election t+1 with pooled sample

Pooled sample	(1)	(2)	(3)	(4)	(5)	(6)
Win Election ($\mathbf{1}\{M_{it} \geqslant 0\}$)	0.087***	0.090***	0.090***	0.090***	0.090***	0.091***
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Gas Tax (G_{it})	-0.004	-0.003	-0.001	0.000	0.000	-0.002
	(0.005)	(0.005)	(0.005)	(0.005)	(0.013)	(0.014)
Win Election × Gas Tax	-0.012*	-0.013*	-0.013**	-0.013*	-0.015**	-0.014**
	(0.007)	(0.007)	(0.007)	(0.007)	(0.006)	(0.006)
Win Election × Gas Tax × Democratic	-0.008	-0.005	-0.006	-0.006	0.000	-0.003
	(0.007)	(0.007)	(0.007)	(0.007)	(0.008)	(0.007)
State-Party FEs			X	X		
Year-Party FEs			X	X		
State-Year-Party FEs					X	X
Covariates			X	X	X	X
Right Bandwidth	0.111	0.142	0.103	0.131	0.103	0.131
Left Bandwidth	0.114	0.149	0.113	0.144	0.113	0.144
Kernel	Uniform	Triangular	Uniform	Triangular	Uniform	Triangular
Observations	18,232	23,363	17,419	22,164	17,419	22,164

Notes: The table presents estimates from 6 regressions on the pooled Republican and Democratic sample. The dependent variable is the party's vote share in election year $t+1$, V_{t+1}. The running variable is the margin of victory in election year t, M_{it}. "Win Election" is an indicator equal to one for a positive margin of victory in election t, $\mathbf{1}\{M_{it} \geqslant 0\}$. Gas Tax ($G_{it}$) is an indicator equal to one if the gas tax increased between elections t and $t+1$. Observations with gas tax decreases are excluded. "Democratic" is an indicator equal to one for Democratic observations. Columns (1), (3), and (5) use a uniform kernel, while columns (2), (4), and (6) use a triangular kernel. Columns (3) and (4) include state-party and year-party fixed effects. Columns (5) and (6) include state-year-party fixed effects. Columns (3)–(6) include covariates. All specifications are estimated on a common sample that excludes observations with missing values in any covariate and include the full set of interactions of the 'Democratic" indicator with each term in the base specification in Equation (2). The 'Democratic" indicator itself is not reported: it is absorbed by the party-specific fixed effects in columns (3)–(6), and omitted from columns (1)–(2) as it is not a coefficient of interest. Covariates determined one year before election year $t+1$ include the state unemployment rate, per capita road mileage, licensed drivers per capita, state vehicle miles traveled per capita, real personal income per capita, the state's average pre-tax real gas price, indicators for the party controlling the state house and senate, and their interaction with an indicator for the current president's party. Additional covariates include an indicator for whether the gas tax is indexed (to e.g., wholesale fuel prices, CPI, or highway construction costs) and an indicator for whether election year $t+1$ is a presidential election year. Covariates determined in election year t include the number of candidates running, number of incumbents running, party tenure in office, and normal party vote share. Bandwidths are selected on the pooled sample using the MSE-optimal procedure of Calonico et al. (2014). Sample sizes differ across columns only due to optimal bandwidth selection, which depends on the included covariates. Clustered standard errors at the state level are in parentheses.

Table A8: Correlation between gas tax increases and other tax increases within bandwidth

Panel (a). Raw sample	Sales Tax (1)	Income Tax (2)	Corporate Tax (3)	Cigarette Tax (4)	Spirits Tax (5)	Diesel Tax (6)
Gas Tax (G_{it})	0.039**	0.038**	0.018	0.062**	0.000	0.771***
	(0.019)	(0.016)	(0.018)	(0.030)	(0.011)	(0.031)
State FEs	X	X	X	X	X	X
Year FEs	X	X	X	X	X	X
Outcome Mean	0.059	0.039	0.036	0.153	0.021	0.215
Within R^2	0.004	0.006	0.001	0.005	0.000	0.570
Observations	1,655	1,655	1,655	1,655	1,655	1,655
Panel (b). Republican estimation sample	**Sales Tax (1)**	**Income Tax (2)**	**Corporate Tax (3)**	**Cigarette Tax (4)**	**Spirits Tax (5)**	**Diesel Tax (6)**
Gas Tax (G_{it})	0.151**	0.027	0.079**	0.276***	0.018	0.879***
	(0.058)	(0.019)	(0.032)	(0.071)	(0.011)	(0.051)
State-Year FEs	X	X	X	X	X	X
Right Bandwidth	0.176	0.176	0.176	0.176	0.176	0.176
Left Bandwidth	0.149	0.149	0.149	0.149	0.149	0.149
Outcome Mean	0.118	0.075	0.085	0.333	0.042	0.381
Within R^2	0.049	0.002	0.014	0.050	0.003	0.620
Observations	13,180	13,180	13,180	13,180	13,180	13,180
Panel (c). Democrat estimation sample	**Sales Tax (1)**	**Income Tax (2)**	**Corporate Tax (3)**	**Cigarette Tax (4)**	**Spirits Tax (5)**	**Diesel Tax (6)**
Gas Tax (G_{it})	0.137**	0.021	0.063**	0.258***	0.001	0.847***
	(0.062)	(0.015)	(0.029)	(0.069)	(0.001)	(0.060)
State-Year FEs	X	X	X	X	X	X
Right Bandwidth	0.137	0.137	0.137	0.137	0.137	0.137
Left Bandwidth	0.142	0.142	0.142	0.142	0.142	0.142
Outcome Mean	0.117	0.073	0.084	0.335	0.043	0.385
Within R^2	0.039	0.001	0.008	0.043	0.000	0.583
Observations	11,232	11,232	11,232	11,232	11,232	11,232

Notes: The table presents estimates from 15 regressions across three panels. Each regression tests whether a gas tax increase predicts a contemporaneous increase in another tax. Panel (a) uses state-year level tax data and defines tax increases on an annual basis. Panels (b) and (c) use the estimation samples for Republican and Democratic legislators, respectively, corresponding to the difference-in-discontinuities specifications in Table 1, and define tax increases over electoral cycles, i.e., between elections t and $t+1$. The dependent variable is an indicator equal to one if the indicated tax increased over the relevant period. Gas Tax (G_{it}) is an indicator equal to one if the gas tax increased over the same period. In Panels (b) and (c), observations with gas tax decreases are excluded. Panel (a) includes state and year fixed effects. Panels (b) and (c) include state-by-election year fixed effects. The bandwidth and triangular kernel in Panels (b) and (c) correspond to those of the preferred specification in column (6) of Table 1. Clustered standard errors at the state level are in parentheses.

Table A9: Diff-in-Disc estimates of electoral incumbent penalty: Robustness to dropping diesel tax increases

Panel (a). Republican party	(1)	(2)	(3)	(4)	(5)	(6)
Win Election ($\mathbf{1}\{M_{it} \geqslant 0\}$)	0.092***	0.091***	0.095***	0.094***	0.093***	0.093***
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Gas Tax (G_{it})	-0.016	-0.015	-0.012	-0.008	0.034	0.023
	(0.010)	(0.010)	(0.012)	(0.013)	(0.041)	(0.042)
Win Election × Gas Tax	-0.012	-0.009	-0.015	-0.013	-0.016	-0.014
	(0.016)	(0.013)	(0.014)	(0.012)	(0.014)	(0.012)
State FEs			X	X		
Year FEs			X	X		
State-Year FEs					X	X
Covariates			X	X	X	X
Right Bandwidth	0.162	0.171	0.145	0.174	0.145	0.174
Left Bandwidth	0.125	0.165	0.120	0.159	0.120	0.159
Kernel	Uniform	Triangular	Uniform	Triangular	Uniform	Triangular
Observations	7,254	8,336	6,676	8,301	6,676	8,301
Panel (b). Democratic party	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Win Election ($\mathbf{1}\{M_{it} \geqslant 0\}$)	0.086***	0.085***	0.090***	0.089***	0.090***	0.089***
	(0.009)	(0.008)	(0.009)	(0.008)	(0.009)	(0.008)
Gas Tax (G_{it})	0.015*	0.011	0.018*	0.011	0.042	0.023
	(0.008)	(0.008)	(0.009)	(0.008)	(0.051)	(0.041)
Win Election × Gas Tax	-0.021*	-0.018	-0.020*	-0.018*	-0.019*	-0.017*
	(0.011)	(0.011)	(0.010)	(0.010)	(0.010)	(0.010)
State FEs			X	X		
Year FEs			X	X		
State-Year FEs					X	X
Covariates			X	X	X	X
Right Bandwidth	0.120	0.141	0.114	0.139	0.114	0.139
Left Bandwidth	0.100	0.145	0.096	0.133	0.096	0.133
Kernel	Uniform	Triangular	Uniform	Triangular	Uniform	Triangular
Observations	5,505	7,085	5,242	6,753	5,242	6,753

Notes: The table presents estimates from 12 regressions, shown separately for the Republican and Democratic parties. The dependent variable is vote share for the indicated party in election year $t+1$, V_{t+1}, as in Equation (2). The running variable is the margin of victory in election year t, M_{it}. "Win Election" is an indicator equal to one for a positive margin of victory in election t, $\mathbf{1}\{M_{it} \geqslant 0\}$. Gas Tax ($G_{it}$) is an indicator equal to one if the gas tax increased between elections t and $t+1$. Observations with gas tax decreases are excluded. In addition, observations with diesel tax increases between elections t and $t+1$ are excluded. Columns (1), (3), and (5) use a uniform kernel, while columns (2), (4), and (6) use a triangular kernel. Columns (3) and (4) include state and year fixed effects. Columns (5) and (6) include state-by-election year fixed effects. Columns (3)–(6) include covariates. Covariates determined one year before election year $t+1$ include the state unemployment rate, per capita road mileage, licensed drivers per capita, state vehicle miles traveled per capita, real personal income per capita, the state's average pre-tax real gas price, indicators for the party controlling the state house and senate, and their interaction with an indicator for the current president's party. Additional covariates include an indicator for whether the gas tax is indexed (to e.g., wholesale fuel prices, CPI, or highway construction costs) and an indicator for whether election year $t+1$ is a presidential election year. Covariates determined in election year t include the number of candidates running, number of incumbents running, party tenure in office, and normal party vote share. Bandwidths are selected using the MSE-optimal procedure of Calonico et al. (2014). Sample sizes differ across columns only due to optimal bandwidth selection, which depends on the included covariates. All specifications are estimated on a common sample that excludes observations with missing values in any covariate. Clustered standard errors at the state level are in parentheses.

Table A10: Diff-in-Disc estimates of electoral incumbent penalty: Robustness to inclusion of diesel tax residuals

Party	Republican		Democratic	
	(1)	(2)	(3)	(4)
Win Election ($\mathbf{1}\{M_{it} \geqslant 0\}$)	0.093***	0.094***	0.089***	0.089***
	(0.006)	(0.006)	(0.008)	(0.008)
Gas Tax (G_{it})	-0.004	-0.003	0.003	0.003
	(0.012)	(0.012)	(0.017)	(0.018)
Win Election × Gas Tax	-0.014**	-0.015**	-0.016**	-0.016**
	(0.006)	(0.007)	(0.008)	(0.008)
Residuals of Diesel Tax		0.008		-0.012
		(0.024)		(0.015)
Residuals of Win Election × Diesel Tax		-0.012		-0.005
		(0.012)		(0.012)
State-Year FEs	X	X	X	X
Covariates	X	X	X	X
Right Bandwidth	0.176	0.180	0.137	0.136
Left Bandwidth	0.149	0.151	0.142	0.142
Kernel	Triangular	Triangular	Triangular	Triangular
Observations	13,182	13,388	11,234	11,174

Notes: The table presents estimates from four regressions of party vote share in election year $t+1$, V_{t+1}, as in Equation (2). Columns (1)–(2) report results for the Republican Party, and columns (3)–(4) report results for the Democratic Party. The running variable is the margin of victory in election year t, M_{it}. "Win Election" is an indicator equal to one for a positive margin of victory in election t, $\mathbf{1}\{M_{it} \geqslant 0\}$. Gas Tax ($G_{it}$) is an indicator equal to one if the gas tax increased between elections t and $t+1$. Observations with gas tax decreases are excluded. Columns (1) and (3) replicate the preferred specification in Table 1. Columns (2) and (4) augment the preferred specification with two residual controls to address the potential confounding role of diesel tax increases. Specifically, let D_{it} be an indicator equal to one if the diesel tax increased between elections t and $t+1$. The first residual, "Residuals of Diesel Tax," is obtained by regressing D_{it} on G_{it} with state-by-election year fixed effects and saving the residuals. The second residual, "Residuals of Win Election × Diesel Tax," is obtained analogously by regressing the interaction $\mathbf{1}\{M_{it} \geqslant 0\} \times D_{it}$ on $\mathbf{1}\{M_{it} \geqslant 0\} \times G_{it}$ with state-by-election year fixed effects. By construction, these residuals are orthogonal to G_{it} and $\mathbf{1}\{M_{it} \geqslant 0\} \times G_{it}$, respectively, so that their inclusion does not affect the variation identifying the gas tax interaction while controlling for the component of diesel tax variation uncorrelated with gas tax changes. All columns use a triangular kernel, state-by-election year fixed effects, and covariates. Covariates determined one year before election year $t+1$ include the state unemployment rate, per capita road mileage, licensed drivers per capita, state vehicle miles traveled per capita, real personal income per capita, the state's average pre-tax real gas price, indicators for the party controlling the state house and senate, and their interaction with an indicator for the current president's party. Additional covariates include an indicator for whether the gas tax is indexed (e.g., to inflation or population) and an indicator for whether election year $t+1$ is a presidential election year. Covariates determined in election year t include the number of candidates running, the number of incumbents running, party tenure in office, and normal party vote share. Bandwidths are selected using the MSE-optimal procedure of Calonico et al. (2014). Clustered standard errors at the state level are in parentheses.

Table A11: Diff-in-Disc estimates of electoral incumbent penalty: Robustness to potentially correlated policies with partial coverage

Party	Republican			Democratic		
	(1)	(2)	(3)	(4)	(5)	(6)
Win Election ($\mathbf{1}\{M_{it} \geqslant 0\}$)	0.093***	0.095***	0.101***	0.089***	0.091***	0.083***
	(0.006)	(0.007)	(0.009)	(0.008)	(0.009)	(0.010)
Gas Tax (G_{it})	-0.004	0.004	0.002	0.003	-0.013	-0.024
	(0.012)	(0.033)	(0.035)	(0.017)	(0.046)	(0.038)
Win Election × Gas Tax	-0.014**	-0.024**	-0.022**	-0.016**	-0.020*	-0.020*
	(0.006)	(0.010)	(0.011)	(0.008)	(0.010)	(0.010)
Win Election × Sales Tax			-0.005			0.003
			(0.014)			(0.019)
Win Election × Income Tax			-0.008			-0.022
			(0.017)			(0.015)
Win Election × Corporate Tax			0.020			0.003
			(0.019)			(0.019)
Win Election × Cigarette Tax			-0.007			0.003
			(0.011)			(0.012)
Win Election × Spirits Tax			-0.024			0.014
			(0.024)			(0.022)
Win Election × Budget Deficit			-0.011			0.003
			(0.008)			(0.009)
Win Election × Toll Facility			-0.006			-0.011
			(0.012)			(0.028)
Win Election × State Inflation			-0.004			0.014*
			(0.007)			(0.008)
State-Year FEs	X	X	X	X	X	X
Covariates	X	X	X	X	X	X
Right Bandwidth	0.176	0.160	0.156	0.137	0.170	0.170
Left Bandwidth	0.149	0.158	0.156	0.142	0.117	0.120
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular
Sample coverage	Full	Partial	Partial	Full	Partial	Partial
Observations	13,182	6,234	6,094	11,234	5,485	5,544

Notes: The table presents estimates from six regressions of party vote share in election year $t+1$, V_{t+1}, as in Equation (2). Columns (1)–(3) report results for the Republican Party, and columns (4)–(6) report results for the Democratic Party. The running variable is the margin of victory in election year t, M_{it}. "Win Election" is an indicator equal to one for a positive margin of victory in election t, $\mathbf{1}\{M_{it} \geqslant 0\}$. Gas Tax ($G_{it}$) is an indicator equal to one if the gas tax increased between elections t and $t+1$. Observations with gas tax decreases are excluded. Columns (1) and (4) replicate the preferred specification in Table 1 using the full sample. Columns (2) and (5) replicate the same specification but restrict the sample to observations with non-missing values for all confounder variables, providing a benchmark to isolate the effect of the sample restriction from the effect of including confounder interactions. Restricting to observations with non-missing confounders reduces the sample by 45.5% for Republicans and 48.7% for Democrats, before optimal bandwidth selection. Columns (3) and (6) augment the preferred specification with interactions between "Win Election" and indicators for increases in other taxes (sales, personal income, corporate, cigarette, and spirits) between elections t and $t+1$, as well as an interaction with an indicator equal to one if the state ran a budget deficit in election year $t+1$, an indicator for toll facility increases, and an indicator for state CPI increases; these columns use the same restricted sample as columns (2) and (5). Each tax indicator equals one if the corresponding tax increased over the electoral cycle and zero otherwise; observations with decreases in those taxes are excluded. All columns use a triangular kernel, state-by-election year fixed effects, and covariates. Covariates determined one year before election year $t+1$ include the state unemployment rate, per capita road mileage, licensed drivers per capita, state vehicle miles traveled per capita, real personal income per capita, the state's average pre-tax real gas price, indicators for the party controlling the state house and senate, and their interaction with an indicator for the current president's party. Additional covariates include an indicator for whether the gas tax is indexed (e.g., to inflation or population) and an indicator for whether election year $t+1$ is a presidential election year. Covariates determined in election year t include the number of candidates running, the number of incumbents running, party tenure in office, and normal party vote share. Columns (3) and (6) additionally include the confounder variables as covariates. Bandwidths are selected using the MSE-optimal procedure of Calonico et al. (2014). Clustered standard errors at the state level are in parentheses.

Table A12: Regression discontinuity falsification test on predetermined variables

Panel (a). Republican party	Vote share (1)	Party win (2)	Cand. Tenure (3)	Incumbents (4)	Candidates (5)
Win Election ($\mathbf{1}\{M_{it} \geqslant 0\}$)	0.005	0.039	0.031	0.010	-0.018
	(0.007)	(0.034)	(0.155)	(0.020)	(0.012)
Right Bandwidth	0.139	0.141	0.151	0.160	0.223
Left Bandwidth	0.204	0.137	0.151	0.234	0.261
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular
Observations	7,856	6,431	12,235	15,365	18,950
Panel (b). Democratic party	**Vote share (1)**	**Party win (2)**	**Cand. Tenure (3)**	**Incumbents (4)**	**Candidates (5)**
Win Election ($\mathbf{1}\{M_{it} \geqslant 0\}$)	0.008	0.060*	0.135	-0.019	0.016
	(0.007)	(0.034)	(0.172)	(0.018)	(0.012)
Right Bandwidth	0.212	0.144	0.140	0.234	0.301
Left Bandwidth	0.202	0.154	0.154	0.178	0.219
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular
Observations	9,868	6,866	11,880	16,368	20,361

Notes: The table reports estimates from local linear regression discontinuity models in which the dependent variables are predetermined relative to the electoral outcome in election year t. Panel (a) shows results for Republicans and Panel (b) for Democrats. Each specification regresses the outcome on the margin of victory, an indicator for being above the zero threshold, and their interaction, estimated locally around the cutoff using a triangular kernel. The reported coefficients correspond to the discontinuity at the margin-of-victory threshold. "Vote share" is the proportion of votes the party obtained in election year $t-1$. "Party win" is an indicator for whether the party won the race in election year $t-1$. "Incumbents" is the number of sitting members running for re-election in election year t. "Candidates" is the number of candidates running in election year t. "Tenure" is the number of years the party had held the seat prior to election year t. Bandwidths are selected using outcome-specific MSE-optimal two-sided procedures (Calonico et al., 2014). Standard errors clustered at the state level are reported in parentheses.

Table A13: Effect of marginally elected legislators on gas tax increase

	(1)	(2)	(3)	(4)
Margin of Victory	0.078	0.059	0.044	0.039
	(0.050)	(0.038)	(0.037)	(0.025)
Marginally Elected ($\leqslant$ 5%)	-0.088			
	(0.086)			
Marginally Elected ($\leqslant$ 10%)		-0.088		
		(0.051)		
Marginally Elected ($\leqslant$ 15%)			-0.081	
			(0.048)	
Marginally Elected ($\leqslant$ 20%)				-0.067
				(0.051)
Year FEs	X	X	X	X
State FEs	X	X	X	X
Covariates	X	X	X	X
Observations	822	822	822	822
Within R^2	0.055	0.057	0.057	0.056

Notes: The table presents estimates from 4 regressions of the relationship between a legislator's electoral margin and their probability of voting in favor of a gas tax increase, as part of a set of exercises assessing whether marginally elected legislators were pivotal in passing gas tax legislation. The dependent variable is an indicator equal to one if the legislator voted in favor of the bill. "Marginally Elected" is a binary indicator equal to one if the legislator's margin of victory in the most recent preceding election was at or below the threshold shown in each column. "Margin of Victory" is the continuous vote share difference between the winner and runner-up. The sample is restricted to single-member districts across eight states (IA, ID, KY, MD, NH, PA, WA, WY), and pools roll-call votes from 13 gas tax bills. Each legislator–vote observation is matched to the most recent preceding election for that legislator. Covariates include indicators for chamber (Senate), party affiliation (Democrat), and tenure. All specifications include state and year fixed effects. Clustered standard errors at the state level are in parentheses.

Table A14: Test for manipulation around the discontinuity

Party	Gas tax increase	$\log(\hat{f}^{+}) - \log(\hat{f}^{-})$	p-value
Republican	No	0.000	1.000
Republican	Yes	-0.069	0.945
Democratic	No	0.082	0.938
Democratic	Yes	0.093	0.929

Notes: This table shows the results of the McCrary (2008) test for sorting of legislators around the margin-of-victory threshold. We report in column (2) the difference in the log of the density of the running variable to the right, $\log(\hat{f}^{+})$, and to the left, $\log(\hat{f}^{-})$, of the discontinuity at $M = 0$. Column (3) reports the p-value for whether this difference is statistically different from zero. Results are reported separately for Republican and Democratic legislators, and within each party for elections followed by a gas tax increase and those that are not. Observations with gas tax decreases between elections t and $t+1$ are excluded from the sample.

Table A15: Diff-in-Disc estimates of electoral incumbent penalty with CER optimal bandwidth: Vote share in election t+1

Panel (a). Republican party	(1)	(2)	(3)	(4)	(5)	(6)
Win Election ($\mathbf{1}\{M_{it} \geqslant 0\}$)	0.090***	0.089***	0.090***	0.090***	0.091***	0.091***
	(0.007)	(0.007)	(0.007)	(0.006)	(0.006)	(0.006)
Gas Tax (G_{it})	-0.003	-0.003	-0.004	-0.004	-0.010	-0.012
	(0.005)	(0.005)	(0.006)	(0.006)	(0.015)	(0.015)
Win Election × Gas Tax	-0.015**	-0.013*	-0.014**	-0.013*	-0.014**	-0.013*
	(0.006)	(0.007)	(0.007)	(0.007)	(0.006)	(0.006)
State FEs			X	X		
Year FEs			X	X		
State-Year FEs					X	X
Covariates			X	X	X	X
Right Bandwidth	0.098	0.131	0.102	0.140	0.102	0.140
Left Bandwidth	0.099	0.138	0.089	0.119	0.089	0.119
Kernel	Uniform	Triangular	Uniform	Triangular	Uniform	Triangular
Observations	8,031	10,877	7,809	10,548	7,809	10,548
Panel (b). Democratic party	(1)	(2)	(3)	(4)	(5)	(6)
Win Election ($\mathbf{1}\{M_{it} \geqslant 0\}$)	0.082***	0.082***	0.089***	0.087***	0.089***	0.088***
	(0.008)	(0.008)	(0.009)	(0.008)	(0.008)	(0.008)
Gas Tax (G_{it})	0.006	0.007	0.006	0.007	0.003	-0.001
	(0.005)	(0.005)	(0.006)	(0.006)	(0.018)	(0.019)
Win Election × Gas Tax	-0.015**	-0.018***	-0.017**	-0.018***	-0.013*	-0.015**
	(0.007)	(0.006)	(0.007)	(0.007)	(0.008)	(0.007)
State FEs			X	X		
Year FEs			X	X		
State-Year FEs					X	X
Covariates			X	X	X	X
Right Bandwidth	0.100	0.120	0.094	0.109	0.094	0.109
Left Bandwidth	0.086	0.114	0.082	0.113	0.082	0.113
Kernel	Uniform	Triangular	Uniform	Triangular	Uniform	Triangular
Observations	7,633	9,495	7,229	8,994	7,229	8,994

Notes: The table presents estimates from 12 regressions, shown separately for Republican and Democratic parties. The dependent variable is vote share for the indicated party in election year $t+1$, V_{t+1}, as in Equation (2). The running variable is the margin of victory in election year t, M_{it}. "Win Election" is an indicator equal to one for a positive margin of victory in election t, $\mathbf{1}\{M_{it} \geqslant 0\}$. Gas Tax ($G_{it}$) is an indicator equal to one if the gas tax increased between elections t and $t+1$. Observations with gas tax decreases are excluded. Columns (1), (3), and (5) use a uniform kernel, while columns (2), (4), and (6) use a triangular kernel. Columns (3) and (4) include state and year fixed effects. Columns (5) and (6) include state-by-election year fixed effects. Columns (3)–(6) include covariates. Covariates determined one year before election year $t+1$ include the state unemployment rate, per capita road mileage, licensed drivers per capita, state vehicle miles traveled per capita, real personal income per capita, the state's average pre-tax real gas price, indicators for the party controlling the state house and senate, and their interaction with an indicator for the current president's party. Additional covariates include an indicator for whether the gas tax is indexed (to e.g., wholesale fuel prices, CPI, or highway construction costs) and an indicator for whether election year $t+1$ is a presidential election year. Covariates determined in election year t include the number of candidates running, number of incumbents running, party tenure in office, and normal party vote share. Bandwidths are selected using the Coverage-Error-Rate (CER) optimal procedure following Calonico et al. (2020) to ensure valid baseline inference. Sample sizes differ across columns only due to optimal bandwidth selection, which depends on the included covariates. All specifications are estimated on a common sample that excludes observations with missing values in any covariate. Clustered standard errors at the state level are in parentheses.

Table A16: Diff-in-Disc estimates of electoral incumbent penalty: Vote share in election t+1 for robustness check sample restrictions

Panel (a). Republican party	(1) Full sample	(2) Include decreases	(3) House	(4) Prior 2000	(5) Post 2000	(6) Presi-dential	(7) House control	(8) Senate control	(9) No recession	(10) Small pre-tax price	(11) Large pre-tax price	(12) No in-dexing	(13) Large gas tax
Win Election ($\mathbf{1}\{M_{it} \geqslant 0\}$)	0.093***	0.092***	0.096***	0.106***	0.075***	0.083***	0.078***	0.082***	0.097***	0.109***	0.077***	0.091***	0.093***
	(0.006)	(0.006)	(0.006)	(0.008)	(0.007)	(0.008)	(0.007)	(0.008)	(0.006)	(0.009)	(0.007)	(0.006)	(0.006)
Gas Tax (G_{it})	-0.004	-0.004	-0.013	0.000	-0.073*	0.052	-0.010	-0.019	0.001	0.015	-0.037	-0.001	-0.008
	(0.012)	(0.012)	(0.016)	(0.015)	(0.042)	(0.083)	(0.023)	(0.024)	(0.017)	(0.023)	(0.028)	(0.016)	(0.021)
Win Election × Gas Tax	-0.014**	-0.014**	-0.014*	-0.015*	-0.019**	-0.013	-0.012	-0.004	-0.015**	-0.019*	-0.007	-0.012*	-0.014
	(0.006)	(0.006)	(0.007)	(0.009)	(0.008)	(0.010)	(0.010)	(0.010)	(0.007)	(0.010)	(0.009)	(0.007)	(0.008)
Right Bandwidth	0.176	0.175	0.182	0.163	0.150	0.154	0.182	0.163	0.188	0.146	0.170	0.167	0.169
Left Bandwidth	0.149	0.146	0.145	0.181	0.133	0.201	0.190	0.222	0.161	0.183	0.136	0.141	0.183
Observations	13,182	13,708	10,339	7,775	4,958	6,596	6,026	6,717	12,002	6,457	6,323	10,814	10,563

Panel (b). Democratic party	(1) Full sample	(2) Include decreases	(3) House	(4) Prior 2000	(5) Post 2000	(6) Presi-dential	(7) House control	(8) Senate control	(9) No recession	(10) Small pre-tax price	(11) Large pre-tax price	(12) No in-dexing	(13) Large gas tax
Win Election ($\mathbf{1}\{M_{it} \geqslant 0\}$)	0.089***	0.087***	0.093***	0.105***	0.066***	0.093***	0.104***	0.098***	0.090***	0.106***	0.069***	0.089***	0.088***
	(0.008)	(0.007)	(0.009)	(0.010)	(0.009)	(0.009)	(0.011)	(0.012)	(0.008)	(0.009)	(0.008)	(0.008)	(0.008)
Gas Tax (G_{it})	0.003	0.000	0.014	0.019	0.056***	0.054	0.033	0.010	0.008	0.008	0.022	0.000	0.014
	(0.017)	(0.015)	(0.021)	(0.021)	(0.019)	(0.059)	(0.022)	(0.026)	(0.021)	(0.024)	(0.018)	(0.021)	(0.023)
Win Election × Gas Tax	-0.016**	-0.014*	-0.021**	-0.022**	-0.011	-0.018**	-0.017*	-0.018**	-0.020**	-0.028**	0.000	-0.014*	-0.006
	(0.008)	(0.007)	(0.009)	(0.010)	(0.011)	(0.008)	(0.009)	(0.008)	(0.008)	(0.011)	(0.011)	(0.009)	(0.009)
Right Bandwidth	0.137	0.138	0.135	0.179	0.128	0.143	0.165	0.176	0.151	0.164	0.126	0.127	0.132
Left Bandwidth	0.142	0.139	0.145	0.151	0.157	0.143	0.159	0.184	0.153	0.159	0.154	0.147	0.127
Observations	11,234	11,740	8,775	7,556	4,945	5,593	7,396	7,352	10,358	6,335	5,740	9,521	7,893

Notes: The table shows estimates plotted in Panel (c) of Figure 6 of β_4 from Equation (2) across 13 sample restrictions, shown separately for Republicans in Panel (a) and Democrats in Panel (b). The dependent variable is the party's vote share in election year $t+1$. Each column runs the same specification as the main results —a triangular kernel with state-by-election year fixed effects and covariates— on a different subsample. Column (1) is the baseline sample excluding gas tax decreases. Column (2) retains gas tax decreases. Column (3) restricts to House races. Columns (4) and (5) split the sample at year 2000. Column (6) restricts to presidential election years. Columns (7) and (8) restrict to years in which the indicated party controls the state house and senate, respectively. Column (9) excludes recession years. Columns (10) and (11) split the sample at the median pre-tax real gas price. Column (12) excludes states with an indexed gas tax. Column (13) restricts gas tax increases to those above the state-specific median increase, as a share of the existing gas tax level. Bandwidths are selected using the MSE-optimal two-sided procedure of Calonico et al. (2014) and are re-estimated for each subsample. Clustered standard errors at the state level are in parentheses.

Table A17: Diff-in-Disc estimates of electoral incumbent penalty: Gas tax penalty attribution

Panel (a). Republican party	(1) Baseline estimates	(2) Senate elections	(3) Presidential year	(4) Recession year	(5) Same candidate	(6) Same party governor	(7) Same party president	(8) Same party house	(9) Same party senate
Win Election ($\mathbf{1}\{M_{it} \geqslant 0\}$)	0.093***	0.097***	0.100***	0.094***	0.043***	0.097***	0.094***	0.107***	0.105***
	(0.006)	(0.006)	(0.008)	(0.006)	(0.007)	(0.007)	(0.007)	(0.007)	(0.010)
Gas Tax (G_{it})	-0.004	-0.005	-0.010	-0.001	-0.001	0.004	0.009	0.005	0.022
	(0.012)	(0.013)	(0.014)	(0.014)	(0.012)	(0.020)	(0.020)	(0.014)	(0.016)
Win Election × Gas Tax	-0.014**	-0.013*	-0.016*	-0.015**	-0.017*	-0.013	-0.024**	-0.023***	-0.026***
	(0.006)	(0.007)	(0.009)	(0.007)	(0.009)	(0.010)	(0.009)	(0.008)	(0.008)
Win Election × Gas Tax + Win Election × Gas Tax × Variable		-0.015	-0.012	-0.004	-0.016*	-0.016*	-0.007	-0.009	-0.002
		(0.011)	(0.010)	(0.011)	(0.008)	(0.008)	(0.007)	(0.010)	(0.010)
Right Bandwidth	0.176	0.176	0.176	0.176	0.176	0.176	0.176	0.176	0.176
Left Bandwidth	0.149	0.149	0.149	0.149	0.149	0.149	0.149	0.149	0.149
Observations	13,182	13,182	13,182	13,182	13,182	13,182	13,182	13,182	13,182

Panel (b). Democratic party	(1) Baseline estimates	(2) Senate elections	(3) Presidential year	(4) Recession year	(5) Same candidate	(6) Same party governor	(7) Same party president	(8) Same party house	(9) Same party senate
Win Election ($\mathbf{1}\{M_{it} \geqslant 0\}$)	0.089***	0.094***	0.083***	0.090***	0.028***	0.090***	0.095***	0.082***	0.083***
	(0.008)	(0.009)	(0.009)	(0.008)	(0.009)	(0.009)	(0.010)	(0.009)	(0.008)
Gas Tax (G_{it})	0.003	0.012	-0.001	0.003	0.007	0.010	0.002	0.014	0.003
	(0.017)	(0.017)	(0.017)	(0.017)	(0.017)	(0.018)	(0.017)	(0.023)	(0.021)
Win Election × Gas Tax	-0.016**	-0.020**	-0.012	-0.020**	-0.003	-0.021**	-0.011	-0.021	-0.008
	(0.008)	(0.009)	(0.011)	(0.008)	(0.009)	(0.010)	(0.009)	(0.014)	(0.014)
Win Election × Gas Tax + Win Election × Gas Tax × Variable		0.002	-0.019**	0.006	-0.018**	-0.012	-0.033***	-0.017**	-0.023***
		(0.011)	(0.008)	(0.016)	(0.008)	(0.010)	(0.012)	(0.008)	(0.008)
Right Bandwidth	0.137	0.137	0.137	0.137	0.137	0.137	0.137	0.137	0.137
Left Bandwidth	0.142	0.142	0.142	0.142	0.142	0.142	0.142	0.142	0.142
Observations	11,234	11,234	11,234	11,234	11,248	11,234	11,234	11,234	11,234

Notes: Notes: The table presents estimates from 9 specifications, shown separately for Republican and Democratic legislators. The dependent variable is vote share for the indicated party in election year $t+1$, V_{t+1}, as in Equation (2). The running variable is the margin of victory in election year t, M_{it}. "Win Election" ($\mathbf{1}\{M_{it} \geqslant 0\}$) is an indicator equal to one for a positive margin of victory in election t. Gas Tax (G_{it}) is an indicator equal to one if the gas tax increased between elections t and $t+1$. Observations with gas tax decreases are excluded. Each specification estimates a triple-interaction model in which the treatment $\mathbf{1}\{M_{it} \geqslant 0\} \cdot G_{it}$ is interacted with an indicator for the subgroup described in the column header. The row "Win × Gas Tax" reports $\hat{\beta}_4$ from Equation (2), capturing the effect for the baseline group (indicator equal to zero). The row "Win × Gas Tax + Win × Gas Tax × Variable" reports the sum of $\hat{\beta}_4$ and the triple-interaction coefficient, capturing the effect for the subgroup defined by the column indicator. The standard errors reported for this sum are computed using the delta method. Column (1) corresponds to our main specification in Table 1 and does not include a triple interaction. "Senate Elections" interacts the treatment with an indicator for whether the election is for a state senate seat. "Presidential Election Years" interacts the treatment with an indicator for whether the election takes place during a presidential election year. "Recession Years" interacts the treatment with an indicator for whether the year is classified as a recession by the NBER. "Same Candidate" interacts the treatment with an indicator equal to one if the same identifiable candidate appears in election t and the subsequent election; observations without a match are coded as zero. "Same Party Governor" and "Same Party President" interact the treatment with indicators for whether the candidate belongs to the same party as the Governor or the President, respectively. "Same Party controls House" and "Same Party controls Senate" interact the treatment with indicators for whether the candidate's party holds a majority of seats in the corresponding legislative chamber following election year t. All regressions include covariates and state-by-election year fixed effects. Bandwidths are selected optimally using two-sided mean squared error following Calonico et al. (2014). Standard errors are clustered at the state level..

Table A18: Diff-in-Disc estimates of electoral incumbent penalty: Governor's vote share in election t+1

Panel (a). Republican party	(1)	(2)	(3)	(4)
Win Election ($\mathbf{1}\{M_{it} \geqslant 0\}$)	0.085***	0.091***	0.078**	0.082**
	(0.026)	(0.026)	(0.033)	(0.032)
Gas Tax (G_{it})	0.005	0.011	0.027	0.025
	(0.017)	(0.019)	(0.027)	(0.026)
Win Election × Gas Tax	-0.010	-0.024	-0.010	-0.007
	(0.027)	(0.030)	(0.030)	(0.029)
Year FEs			X	X
State FEs			X	X
Covariates			X	X
Right Bandwidth	0.095	0.122	0.089	0.120
Left Bandwidth	0.125	0.121	0.114	0.126
Kernel	Uniform	Triangular	Uniform	Triangular
Observations	202	222	189	216
Panel (b). Democratic party	**(1)**	**(2)**	**(3)**	**(4)**
Win Election ($\mathbf{1}\{M_{it} \geqslant 0\}$)	0.131***	0.158***	0.103***	0.106***
	(0.030)	(0.037)	(0.037)	(0.036)
Gas Tax (G_{it})	0.000	-0.011	-0.069*	-0.064*
	(0.021)	(0.026)	(0.036)	(0.038)
Win Election × Gas Tax	-0.007	0.004	0.045	0.041
	(0.031)	(0.037)	(0.040)	(0.042)
Year FEs			X	X
State FEs			X	X
Covariates			X	X
Right Bandwidth	0.133	0.127	0.106	0.137
Left Bandwidth	0.108	0.112	0.091	0.108
Kernel	Uniform	Triangular	Uniform	Triangular
Observations	214	214	185	210

Notes: The table presents estimates from 8 regressions, shown separately for the Republican and Democratic parties. The dependent variable is vote share for the indicated party in election year $t+1$, V_{t+1}, as in Equation (2). The running variable is the margin of victory in election year t, M_{it}. "Win Election" is an indicator equal to one for a positive margin of victory in election t, $\mathbf{1}\{M_{it} \geqslant 0\}$. Gas Tax ($G_{it}$) is an indicator equal to one if the gas tax increased between elections t and $t+1$. Observations with gas tax decreases are excluded. Covariates determined one year before election year $t+1$ include per capita road mileage, licensed drivers per capita, state vehicle miles travelled per capita, real personal income per capita, and the state's average pre-tax real gas price. Bandwidths are selected optimally using two-sided MSE, following Calonico et al. (2014). Clustered standard errors at the state level are in parentheses.

Table A19: District observations and contributing states by event time

	Republican		Democratic	
Event time (k)	N districts	N states	N districts	N states
$k = -4$	0	0	0	0
$k = -3$	535	8	565	8
$k = -2$	1,540	20	1,619	20
$k = -1$	3,830	38	4,185	38
$k = \ \ 0$	16,426	46	18,213	46
$k = +1$	3,920	34	4,406	34
$k = +2$	1,133	17	1,208	17
$k = +3$	499	9	620	9
$k = +4$	0	0	0	0

Notes: The table reports the number of district-year observations and contributing states in the treated group at each event time k, before applying the bandwidth filter. At $k = -j$, treated observations are districts whose state will raise the gas tax exactly j election cycles ahead, with no increase or decrease in the intervening periods. At $k = +j$, treated observations experienced a gas tax increase exactly j election cycles ago, with no change in the intervening periods. The $k = -4$ and $k = +4$ rows confirm that extending the test to $K = 4$ yields no treated observations at those horizons, establishing the feasible pre- and post-trend horizon as $K = 3$.

Table A20: Observations with valid forward observations by horizon

	Republican		Democratic	
Horizon	N obs	% of sample	N obs	% of sample
$j = 1$	28,662	60.8%	31,302	61.3%
$j = 2$	14,708	31.2%	16,229	31.8%
$j = 3$	6,282	13.3%	7,021	13.8%
$j = 4$	149	0.3%	164	0.3%

Notes: The table reports the number and share of district-year observations that have at least j valid forward election periods. An observation requires j valid forward periods to potentially contribute to the $k = -j$ treated group. The near-zero share at $j = 4$ explains why the $k = -4$ treated group in Table A19 is empty: even among the 149–164 observations with four valid forward periods, none has a gas tax increase exactly four cycles ahead with no intervening increase or decrease. The sharp decline from $j = 1$ to $j = 3$ reflects redistricting-induced attrition: district boundaries change approximately every decade, and observations near a redistricting boundary have truncated forward sequences.

Table A21: Distribution of gas tax increases per state

Number of increases	N states (Republican sample)	N states (Democratic sample)
1	1	1
2	3	3
3	3	3
4	10	10
5	7	4
6	3	6
7	4	5
8	3	3
9	5	6
10	3	1
11	2	1
12	1	2
14	1	1
Never increase	2	2

Notes: The table reports the number of gas tax increases per state over the sample period 1982–2016, separately for the Republican and Democratic estimation samples. Small differences between samples reflect variation in district-level election timing and coverage. Across both samples, 93.8% of states raise the gas tax more than once. Only two states never raise the gas tax during the sample period. This distribution renders two standard control group constructions infeasible: a never-treated control group (as in Callaway and Sant'Anna 2021) is infeasible because treatment recurs in the vast majority of states; and a global never-changer control group (the baseline approach in De Chaisemartin and d'Haultfoeuille 2024) is infeasible because only two states never raise the gas tax, an insufficient pool to serve as a credible counterfactual for 46 treated states. The not-yet-treated control group with event-time-specific windows is therefore the only feasible option.

Table A22: Pairwise overlap of treated district observations between event times (Republican sample)

	k=−4	k=−3	k=−2	k=−1	k=0	k=+1	k=+2	k=+3	k=+4
$k = -4$	0	—	—	—	—	—	—	—	—
$k = -3$	—	535	—	—	—	—	—	—	—
$k = -2$	—	—	1,540	—	—	163	—	—	—
$k = -1$	—	—	—	3,830	—	800	163	—	—
$k = 0$	—	—	—	—	16,426	—	—	—	—
$k = +1$	—	—	163	800	—	3,920	—	—	—
$k = +2$	—	—	—	163	—	—	1,133	—	—
$k = +3$	—	—	—	—	—	—	—	499	—
$k = +4$	—	—	—	—	—	—	—	—	0

Notes: The table reports pairwise overlap in treated district observations between event times for the Republican sample; the Democratic sample exhibits a similar pattern. Diagonal entries report the number of treated districts at that event time (identical to Table A19). Off-diagonal entries report the number of district observations appearing in the treated group at both event times shown. Dashes indicate pairs for which overlap is zero by construction. Two features of the overlap structure are noteworthy. First, pre-treatment event times ($k = -1$, $k = -2$, $k = -3$) have zero pairwise overlap among themselves: the treated groups are mutually exclusive by construction, so pre-trend coefficients are estimated on fully disjoint samples and there is no mechanical correlation among them. Second, overlap exists exclusively between pre- and post-treatment horizons (e.g., 800 observations appear in both $k = -1$ and $k = +1$), arising from the staggered election cycle design in which the same district can face a gas tax change qualifying it as treated at one event time and as a not-yet-treated control at another. This overlap does not affect the validity of the pre-trend test, which relies only on the pre-treatment coefficients.

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