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

Matías Navarro[1], Gian-Claudia Sciara[2], and Andrew R. Waxman[3]

[1] *Charles H. Dyson School of Applied Economics & Management, Cornell University, Ithaca, NY, USA*
[2] *Community & Regional Planning Program, School of Architecture, University of Texas at Austin, Austin, TX, USA*
[3] *Lyndon Baines Johnson School of Public Affairs, University of Texas at Austin, Austin, TX, USA*

April 7, 2024

Abstract

Perhaps the greatest challenge to addressing climate change comprehensively through government policy in the US has been limited political feasibility. We investigate whether politicians are punished by voters for increasing motor fuel taxes by compiling a comprehensive dataset 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 small, but economically and statistically meaningful decrease in the incumbency advantage of 1.3 to 1.9 percentage points for Republican and Democratic incumbents, respectively. This penalty represents 14–21% 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. This work was supported, in part, by the University of Texas Energy Institute. Declarations of interest: none.

1 Introduction

Perhaps the greatest challenge to addressing climate change comprehensively through government policy in the United States has been limited political feasibility. Even recent federal "wins," such as the Infrastructure Investment and Job Act and Inflation Reduction Act (IRA), have largely relied on generous subsidy programs and technological mandates to achieve environmental objectives instead of pricing externalities. Despite consistent rhetoric about the political infeasibility of pricing carbon, in general, and of taxing driving, in particular, there is limited empirical evidence for this claim. Indeed, polling suggests that a narrow but growing plurality of Americans support increasing taxes on fossil fuels to address climate change (Keller et al., 2012; Fitzpatrick et al., 2018; Agrawal and Nixon, 2019).

Despite the general public support, legislators are often reluctant to raise gasoline taxes: the federal motor fuels tax has not been changed in over three decades, and states vary widely in their patterns of tax change.[1] Parry and Small (2005) found that optimal United States gasoline taxes should be twice what they were.[2] Motor fuels taxation has neither kept pace with transportation expenditures nor inflation nor population growth (Peterson, 2021). This stasis is, in part, a function of the fact that gas taxes are almost exclusively excise taxes and thus are not directly affected by gasoline prices the way a sales tax would be. Only in few states are gas taxes indexed so that they respond to changes in inflation. Given the failure to set gasoline taxes close to their optimal levels for revenue generation and externalities, an important question remains: why is it so politically challenging to raise gasoline taxes? 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 (Li et al., 2014). Hence, consumers may be more responsive to gas tax increases than to comparable pre-tax gasoline price increases. If policymakers in the US 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.

In this paper, we investigate whether politicians are punished by voters for increasing motor fuel taxes by compiling a comprehensive dataset on state legislative election outcomes and gasoline taxes over 1982–2016. Given federal inaction and substantial variation across states in gasoline tax changes over the last decades, states are an ideal empirical setting to study the electoral penalties associated with motor fuel taxation. In the US federalist system, states are in many ways the primary decision-maker for transportation needs. They have a substantial responsibility to raise revenues for and coordinate construction of transportation infrastructure, and this is more so than for other infrastructure sectors.[3] Even the largest source of funding for highways, the Federal Highway Trust Fund, relies on contributions of revenues from state governments, and the authority on how and where these funds are spent often lies with the states. Lastly, the considerable variation in gas tax changes across the 50 states provides a source

[1] The federal gas tax was last raised from 14.1 to 18.4 cents per gallon in 1993.

[2] Available estimates place the external costs of gasoline at about thirty to forty cents per gallon (Parry et al., 2007). Local pollution, congestion, and accident externalities can be approximated as mileage-related costs, and converted to per gallon costs; estimates for these are as great as $2.40 per gallon (Parry et al., 2007; Anderson and Auffhammer, 2014).

[3] A recent article on the narrowing window for states to pass transportation funding to claim funds authorized by the Infrastructure Investment and Jobs Act of 2021 noted "Transportation spending can be a lot like the game plinko—although a lot of money may be poured in at the top, where it actually ends up depends on decisions made at the state and local levels in between federal reauthorizations" (Finn, 2023).

of identifying variation arising from various political, economic, procedural and legal causes that we speak to in discussing our empirical approach.

We focus our analysis on the effect of legislated gasoline tax changes on incumbent state legislator electoral outcomes, specifically the advantage of incumbents in subsequent elections. Incumbency advantage refers to the overall effect on electoral outcome from being the current incumbent or incumbent party. We focus on changes in state gasoline excise taxes and account for states that index taxes to inflation or other variables. A challenge to estimating the electoral penalty associated with gasoline tax changes is that these changes are associated with particular economic, transportation and political features, which may not be observed. Our econometric approach to recover electoral penalties of gas tax increases builds on Lee et al. (2004)'s method to estimate electoral advantage of incumbent politicians. Lee et al. (2004) take advantage of the fact that very close elections are 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). Leveraging this insight, they apply a regression discontinuity design using the margin of victory as the running variable, and compare the vote share in the subsequent election for candidates that just barely lost to those that just barely won.[4]

We apply to this insight a difference-in-discontinuities ("Diff-in-Disc") approach, which compares the incumbent advantage from close elections in states where gasoline taxes were increased to those where it was not. This extension to the methodology has also been widely applied in the public economics literature (Grembi et al., 2016; Cellini et al., 2010; Pettersson-Lidbom, 2012; Ferreira and Gyourko, 2014). We provide analytical results to demonstrate that this estimator, under testable assumptions, recovers the average treatment effect on the treated (ATT). Our approach most closely follows Cellini et al. (2010), in that we are comparing elections for the same electoral unit over time.

Our approach also borrows insights from Ferreira and Gyourko (2014), which compares outcomes for male and female mayoral candidates. In our case we compare the incumbency advantage between political parties. This approach helps to establish whether electoral penalties from gas tax increases affect Democrats more or less than Republicans. On the one hand, Democrats may be more liable to criticism for raising taxes, given that political rhetoric in the US typically labels their party as "tax and spend." As a result, all else equal, one might hypothesize that the same magnitude gas tax increase would have a larger penalty for a Democratic state lawmaker than a Republican one. On the other hand, Democratic and Republican lawmakers represent voters with different preferences for taxation and public spending. So, raising taxes 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).

Our headline results from the difference-in-discontinuities estimation suggest that Republican candidates have a 1.3 percentage point reduced incumbent advantage relative to a 9.2 percent incumbent advantage. Democrats have a 8.9 percent incumbent advantage, which reduces by 1.9 percentage points after a gas tax increase.[5]

[4]Other papers in this literature include DiNardo and Lee (2004); Lemieux and Milligan (2008); Snowberg et al. (2007); Carozzi et al. (2024).

[5]While the effect for Democrats is larger, we do not find the effects to differ between regressions in a statistically meaningful manner.

This paper contributes to three areas of the existing literature. For one, we provide the first empirical estimates of the effect of motor fuels taxation on political outcomes. While a long literature has pointed to the optimality of higher U.S. fuel taxes (Fullerton and West, 2002; West and Williams III, 2005, 2007; Parry and Small, 2005; Langer et al., 2017), the inability to achieve these higher taxes has been attributed to distributional concerns common to many forms of environmental taxation (Sallee, 2019; Fowlie and Perloff, 2013). The general intuition for this impact is that an energy-related costs represent a higher proportion of low income households budgets. However, higher gasoline taxes themselves may actually be progressive given that the income elasticity of fuel intensity is likely to be positive (Metcalf, 2023). As a result, it remains an as of yet unanswered empirical question: how badly are politicians punished for raising gasoline taxes?

We also apply the literature on the determinants of and impacts of close elections to environmental taxes. Close elections can be empirically useful for research design to the extent that differences in observable and unobservable attributes between close winners and close losers are as good as random, as first exploited by Lee et al. (2004). 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). Our study is unique in that we are interested in the election outcome itself as it is affected by fiscal legislation, whereas prior studies use elections to explain fiscal outcomes.

Third, this paper contributes to research on the political economy of environmental policies and taxation. In particular, we provide evidence that speaks to the accountability of politicians to changes in taxes or public goods with particular reference to the environment. Despite a spate of research on gasoline taxes more broadly, no research has leveraged the 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 (otherwise known as the Waxman-Markey bill) (Meng and Rode, 2019; Landry, 2021). However, a broader range of legislation is tied to the political momentum related to climate change. For instance, Gagliarducci et al. (2019) study how quasi-random exposure to hurricane damage affects voter support for environmental legislation in the US Congress. Boomhower (2021) considers the extent to which voters punish politicians responsible for natural gas permitting for fracking-related earthquakes. Lastly, we examine an alternative policy to gasoline taxes: a vehicle miles traveled tax, that would charge drivers based on the distance traveled and the corresponding wear and tear on roads (Parry and Small, 2005; Langer et al., 2017; Glaeser et al., 2022).

2 Data & stylized facts

2.1 Data

To recover the electoral penalty of gas tax increases, we leverage comprehensive state legislative electoral data with state gasoline tax changes for all state legislative districts (SLDs) over 1982–2016, excluding Puerto Rico and the District of Columbia. We complement this dataset with 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). Finally, we use these data to compile the lower and upper house partisan balance.

State gasoline tax, pre-tax price, and indexing. We construct a panel of gasoline tax rates at the state and year level using data from the Highway Statistics Series at the Federal Highway Administration. We complement this dataset with data on before-tax gasoline prices, which comes 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.

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 also collect state personal income from the Bureau of Economic Analysis, and state population and unemployment rate from FRED. We also include data from MIT EDS Lab (2017) on the US President's party and the year of presidential elections.

2.2 Stylized facts

In our analysis, the policy of interest is state gasoline tax changes and their effect on electoral outcomes of SLDs.[6] We focus our attention on single office-holder districts (i.e., single-member districts) which constitute approximately 90% of the districts in our sample.[7] On average, a state has approximately 84 districts that elect one lower house representative and 38 districts that elect one upper house senator.[8] 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.[9]

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

[6] SLDs have slightly different coverage than US 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 except Nebraska are bicameral.

[7] 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.

[8] 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)

[9] See Appendix Table A2 Panel (b) and `https://www.ncsl.org/resources/details/number-of-legislators-and-length-of-terms-in-years` for further details.

[10] This share is less than 50% due to Independents and third-party candidates, as well as a small number of elections where

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.

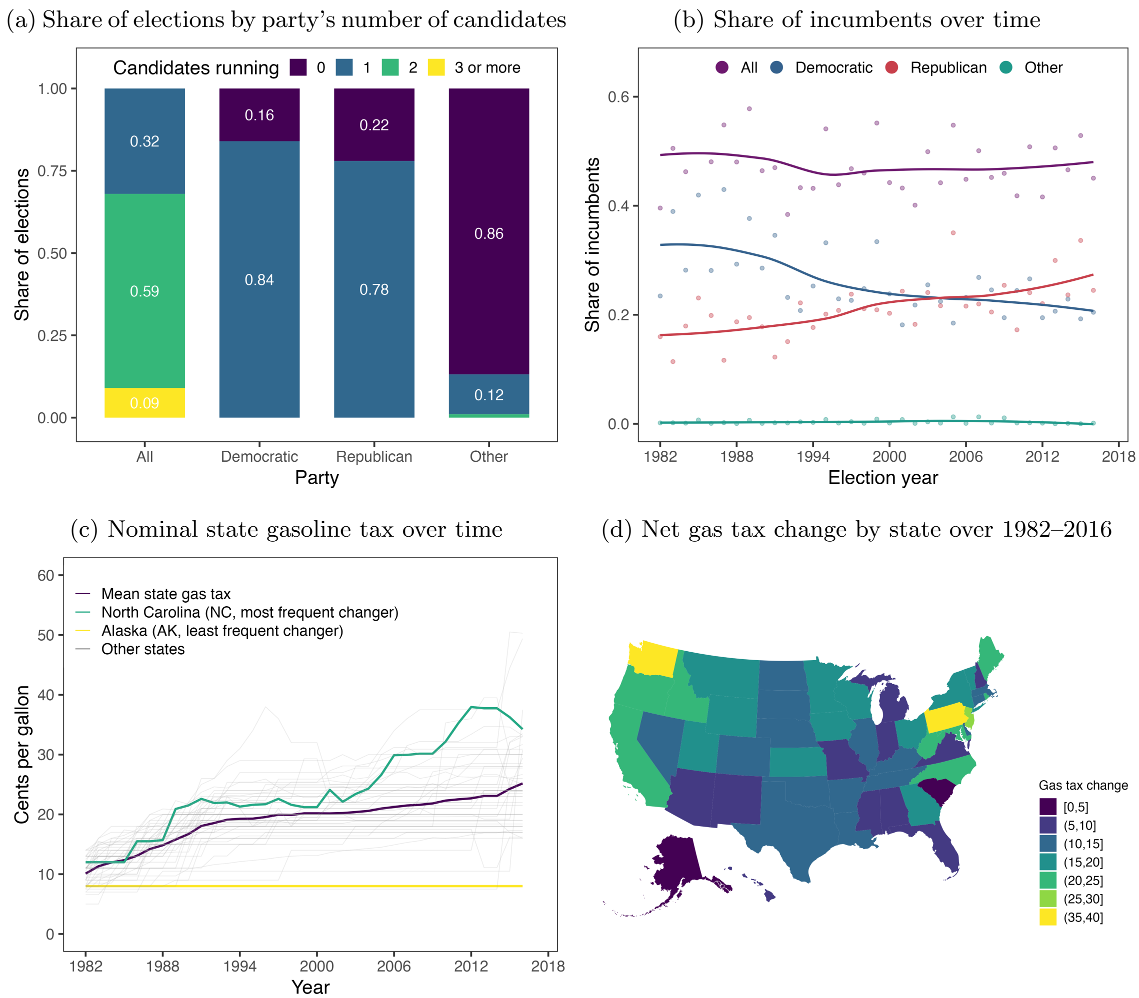


Figure 1: Sample summary statistics

Notes: Panel (a) shows the share of elections, out of a total of 89,494, in which each party fielded 0, 1, 2, or more candidates. Panel (b) shows the share of incumbent parties in SLDs over 1982–2016. Panel (c) shows the evolution of state gas taxes over 1982-2016. Panel (d) shows the cumulative change in the gas tax by state over 1982–2016. 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.

State gasoline taxes grew slowly on average, but with considerable 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 substantial heterogeneity in gasoline tax increases across states. In Figure 1, Panel (d), it is clear that some states, such as Pennsylvania, Washington, and New Jersey, experienced gasoline tax increases during our sample exceeding

there are more than two candidates running.

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 no change in gasoline tax and the mean change is small, with an average increase of half a cent. Throughout our sample, we observed 367 gas tax increases and 60 gas tax decreases.[11]

Gas tax increases only weakly linked to political conditions. Given the design of our study, it is helpful to ask why state legislatures might change gas taxes. Robinson and Tazhitdinova (2022) examine the determinants of various tax changes across the US to show that it is generally difficult to tell a consistent story about the 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 qualitative methods to examine the priorities for state legislators in transportation committees and find that gas tax increases reflect longer term efforts to address structural funding issues for transportation infrastructure. In Table A1, we estimate a linear probability model for there being any change in the gas tax and regress this on the share of registered Democratic voters in the state, the lane-miles of highway per capita, vehicle miles traveled per capital, pre-tax gasoline prices, income per capita and the share of urbanized population. We do this in three specifications with and without year and state fixed effects. The results indicate that more Democratic states are slightly less likely to change taxes. States with more lane miles of highway seem to have higher likelihood of a change, states with higher gas prices are less likely to change the gas tax. In Section 5, we also provide suggestive evidence using Google Trends data that when gas tax increases occur, they are salient to state residents relative to other policy issues in the year that they occur. We explore the implications of these observations for our empirical strategy in the following section.

3 Econometric methodology

Recovering the electoral penalty of state gasoline tax increases can be empirically challenging because state gasoline tax changes are not randomly assigned. Omitted variable bias could arise from unobserved differences in economic and political conditions, driving patterns, infrastructure, and federal policies contributing to changes in motor fuels taxation. Two-way fixed effects (TWFE) estimation accounting for staggered treatment (Roth et al., 2023) could address time-invariant observables, but to the extent that differential selection due to political or economic trends occurs over time, parallel trends may be violated. We rely instead on a more general set of identification assumptions by focusing on a population of elections for which attribution of penalties to the impact of gas tax increases may be clearer: the effect on incumbents who won close elections.

3.1 Difference-in-discontinuities design

Our difference-in-discontinuities (Diff-in-Disc) approach draws on two literatures. First, a long literature has established that incumbents running for office have an electoral advantage, and Lee (2008) shows how this can be

[11] For more details, see Appendix Figure A2.

identified through regression discontinuity design (RDD) by comparing electoral performance in the *next* election between near winners to near losers of relatively close elections. The intuition behind this comparison is that idiosyncratic factors such as weather or unexpected news events may shift the balance in close elections in a manner orthogonal to economic and political dynamics that dictate gas tax increases.[12] Second, the empirical public finance literature has used difference-in-differences approaches to examine the effects of changes in state gasoline taxes on outcomes of interest (Li et al., 2014; Marion and Muehlegger, 2011). In our context, we compare the extent of the incumbency advantage–measured through RDD–between districts in states where gas taxes have been increased to those where it has not. The resulting experimental design is a novel application of the difference-in-discontinuities approach used in a variety of studies in the empirical literature (Grembi et al., 2016; Cellini et al., 2010; Ferreira and Gyourko, 2014; Asher and Novosad, 2017). In particular, the Diff-in-Disc approach offers an advantage over conventional difference-in-differences by allowing us to focus on the penalty for incumbents, for whom the gas tax penalty is salient, since they are in office when gas tax increase legislation is enacted. Moreover, these incumbents are *ex ante* observationally similar to non-incumbents in outcomes and so the approach ought to mitigate the selection bias of focusing on incumbents alone.

Figure 2 illustrates the intuition for our 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 proportion of votes which the Republican or Democratic party won, 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, which translates into different numbers of calendar years depending on the state as discussed in Section 2. 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 is dictated by the nature of our research design, but in Section 4.1, we explore the size and direction of gas tax changes on our results. The discontinuity around $M_{it} = 0$ then corresponds to the incumbency advantage. This advantage is represented in Figure 2 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. τ_{DD} is our parameter of interest and represents a party's electoral penalty associated with a gas tax increase. This estimate measures the *relative decrease* in the size of the incumbency advantage. Specifically, it reflects how much lower the vote share difference is for near winners relative to near losers in districts in states with a gas tax increase relative to those without.

Following the approach in Lee (2008), we estimate the electoral penalty by party (pooled across upper and lower legislative houses) rather than individual candidates. Doing this mitigates selection bias that would come from attrition when candidates change districts or drop out/enter the panel. In addition, voters are likely more aware of the policy legacy of a given party in state government than of a particular legislator.[13]

[12] 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).

[13] We include elections with Independent or third-party candidates and winners do not estimate their party's electoral penalty given how few observations they represent.

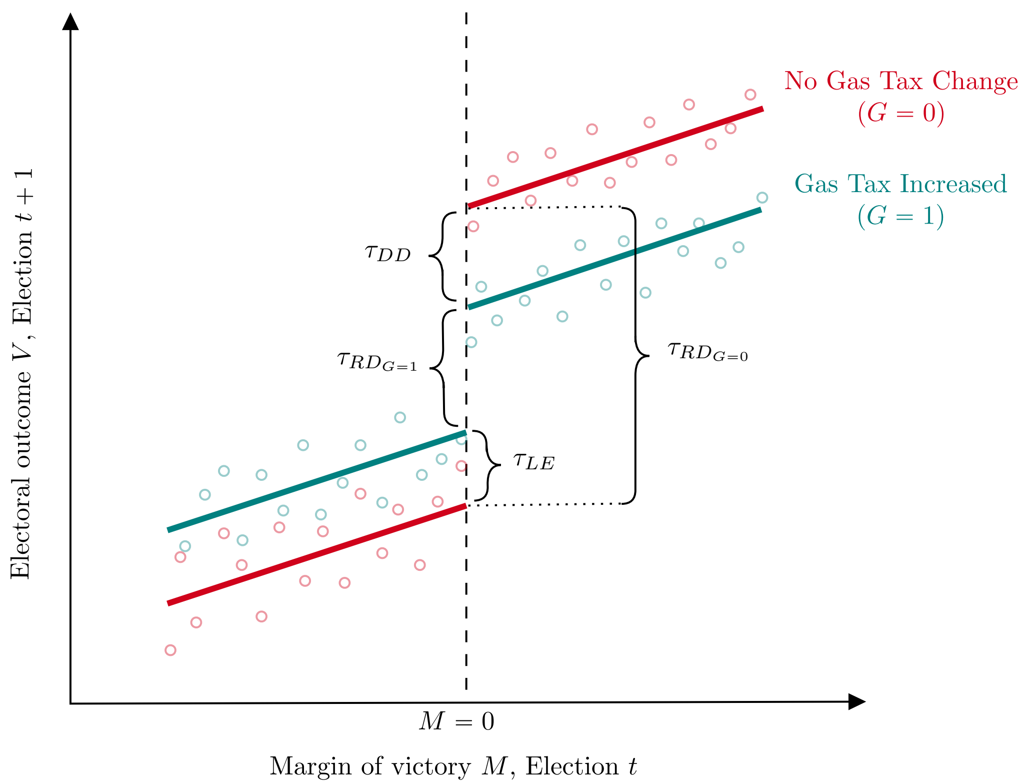


Figure 2: Graphical representation of difference-in-discontinuities design for electoral incumbent penality

Notes: This figure shows how 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.

3.2 Estimation

Our Diff-in-Disc estimator is estimated separately for each party (Republican and Democratic) as:

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

In Equation (1), again 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. 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 4.1. $\boldsymbol{X}$ is a matrix of state and district-level controls discussed in Section 2. We control for economic and transportation variables one year before election year $t+1$ and political variables for election year t as well as an indicator for whether the gas tax increase was indexed to economic variables and state and year fixed effects. The full list of controls are indicated in the notes to regression tables. The parameter β_4 is the estimate of interest and represents the penalty on incumbents' electoral outcomes for districts in states that passed a gasoline tax increase between election t and $t+1$. This estimate recovers the difference-in-discontinuities $\hat{\tau}_{DD}$ in Figure 2.

Intuitively, one may think to recover Diff-in-Disc estimates simply from the difference between regression discontinuity estimates for the sample with and without gas tax increases, corresponding to $\tau_{RD_{G=0}} - \tau_{RD_{G=1}}$ in Figure 2. However, as the figure makes clear, we also need to account for the effect of gas tax increases on losers of close election, reflected by τ_{LE} in the figure and estimated by β_2 in Equation (1).[14] It is noteworthy that we do not include interactions of G_{it} with M_{it} nor the triple interaction of G_{it}, M_{it}, and $\mathbf{1}\{M_{it} \geqslant 0\}$. Doing so would allow us to recover the slopes of the green curves in Figure 2 (the vote share for districts in states with gas tax increases). Instead, β_1 recovers the *average* slope to the left of $M_{it} = 0$ for both lines and β_5 recovers the *average* to the right. As the figure makes clear, including these additional controls for slope differences is not necessary to recover a credible estimate of $\hat{\tau}_{DD}$. Moreover, inclusion of these additional controls would reduce the efficiency of our estimates in Equation (1), since differences in the slopes between each curve would not affect the estimate of interest, β_4.

We estimate Equation (1) by fitting local linear regression functions on both sides of the cutoff at $M_{it} = 0$ within MSE-optimal bandwidths, which are selected following Calonico et al. (2019).[15] We present results with both uniform and triangular kernels, with our preferred specification utilizing the latter.[16]

[14] β_3 corresponds to $\tau_{RD_{G=0}}$ in Figure 2. Accounting for τ_{LE} forms the basis of one of our identifying assumptions detailed in Section 3.3.
We do not recover estimates of $\tau_{RD_{G=1}}$ in our regression, but could from $\hat{\beta}_3 - \hat{\beta}_2 - \hat{\beta}_4$.

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

[16] The triangular kernel is known for its point estimation mean squared error (MSE) optimality when paired with an MSE-optimal bandwidth selection (Cattaneo and Titiunik, 2022). In contrast, the uniform kernel minimizes the asymptotic variance of the local polynomial estimator, exhibiting inference optimality.

3.3 Identification

Gasoline tax increases are not randomly assigned over time. Nevertheless, their likelihood heavily depends on the outcome of the last election. By conditioning the exposure to treatment on incumbency (Democrat or Republican), we account for the source of variation that likely influences the passage of gasoline tax increases. Therefore, our Diff-in-Disc estimator exploits the identifying variation that comes from the random component of close elections in the preceding electoral cycle and the resulting party in power. This allows us to account for the non-random assignment of gasoline tax increases and compare the incumbency advantage between districts with and without a gas tax increase. We formalize this intuition and provide the proof in Appendix C. The consistency of this estimator is based on the following three assumptions.

A1. Continuity. Here we assume the continuity of the conditional mean of the running variable, M_{it}, over the discontinuity at $M_{it} = 0$ (Imbens and Lemieux, 2008). This assumption would be violated if electoral fraud lead to manipulation of the last election outcome and therefore the margin of victory around the threshold (Lee, 2008). We directly test for this assumption through standard approaches described by McCrary (2008). We provide evidence of no manipulation around the margin of victory threshold in Section 4.1.

A2. Local parallel trends. Here we assume that, in the neighborhood of the margin of victory threshold, treated and untreated districts need to be on parallel trends in the absence of a gasoline tax increase (see Grembi et al., 2016 for a formal description). An appropriate test for this assumption is to compare estimates of β_4 for districts in states with gas taxes to those without for close elections in years *prior to the gas tax increase*. We provide evidence of no differential trends in Section 4.1.[17]

A3. Accounting for τ_{LE}, the effect of gas tax increases on losers. Our final assumption is that the electoral penalty of gas tax increases on losers of close elections is controlled for in our regression estimates from Equation (1). This assumption would be violated if this effect were non-zero and we were unable to credibly control for it. In practice, we are and find these effects, reflected by estimates of β_2, are statistically indistinguishable from zero.

4 Empirical results

In this section, we present our empirical estimates of the impact of gasoline tax changes on electoral outcomes across single-member SLDs in the US. We begin our analysis by examining the effect for the incumbent party on its vote share and probability of winning, focusing on the two major parties, Republicans and Democrats. We then examine the robustness of our Diff-in-Disc approach by testing the assumptions required for identification.

[17] Note that local parallel trends does not have any relation to the two curves to the left of the discontinuity in Figure A4, which reflects difference in the slopes for losers of close elections.

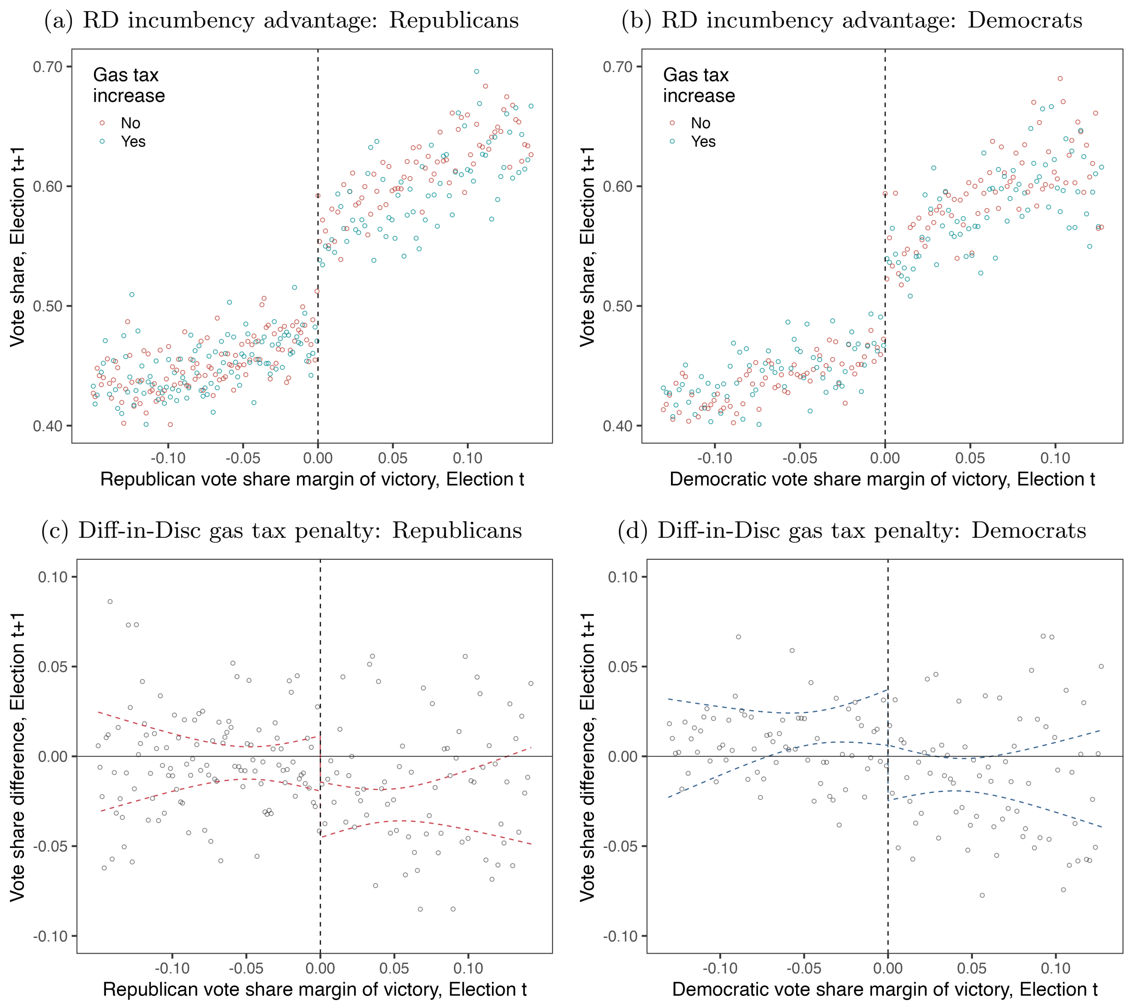


Figure 3: Regression Discontinuity and Difference-in-Discontinuities unconditional plots for vote share

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 Figure 3, we provide visual representations of incumbency advantage and the electoral penalty of gas tax increases from binned scatterplots of the relationship between incumbent party vote share (y-axis) and margin of victory in the previous election (x-axis) using raw data.[18] Panels (a) and (b) show the incumbency advantage itself corresponding to β_3 in Equation (1). 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. First, we notice that the incumbency effect is, on average, larger for Republicans (Panel (a)) than Democrats (Panel (b)). Second, the difference in the election $t+1$ vote share is less substantial between green and red markers to the left of the discontinuity (losers in election t) than to the right (winners in election t). This indicates that while we control for τ_{LE} in Equation (1), it is not large, 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.

In Panels (c) and (d), the data has been collapsed into differences between elections for districts in states with and without gas tax increases. To the left of the discontinuity, we observe the difference in vote shares for close elections that were lost. The 95% confidence interval for the binned scattermarks includes zero for the entire domain of negative margin of victory for Republicans, indicating no statistically significant differences due to gas tax increases on losers. For Democrats there is some statistically significant difference closer to zero. We will compare these differences by estimating β_2 in Equation (1) in our subsequent regressions. To the right of the discontinuity, we observe the difference in vote share for close elections that were won, corresponding to β_4 in Equation (1) and τ_{DD} in Figure A4. Here we see relatively more markers shifting below zero, which suggests a reduction in the size of the incumbency advantage for districts in states where the gas tax was increased between elections t and $t+1$ (represented by β_4 in Equation (1)).

To give this suggestive evidence causal support, we provide the primary Diff-in-Disc regression estimation results of Equation (1) for the Republican and Democratic parties in Table 1. The first estimate is the incumbency advantage effect for the party's vote share in districts without a gas tax increase, which is the coefficient (β_3) on the indicator variable $\mathbf{1}\{M_{it} \geqslant 0\}$. Next, we display the gas tax effect on losers, the coefficient (β_2) on G_{it}. Finally, the coefficient of interest is the last reported, which is the electoral penalty of the gas tax to incumbents, the coefficient (β_4) on $\mathbf{1}\{M_{it} \geqslant 0\} \times G_{it}$. In each panel, going from column (1) to (4), our estimates are robust to the inclusion of covariates, state and year fixed effects, and the use of an alternative kernel. Since we observe multiple elections in each state for a given election year, we can also estimate Equation (1) including state-by-year fixed effects as reported in Table A9. The coefficient estimates of interest in that table remain largely unchanged from the inclusion of these more extensive fixed effects.[19]

[18] We follow the suggestions of Korting et al. (2023), using small bins and avoiding fitted lines. We follow a data-driven approach to select the number of bins, with quantile-spaced and mimicking variance options, following the approach described in Cattaneo et al. (2019). Quantile-spaced bins contain approximately the same number of observations, which eases comparability. Mimicking variance means that the overall variability of the binned means mimic the overall variability in a raw scatter plot of the data.

[19] By controlling for state-by-year variation, estimation using state-by-year fixed effects precludes some of the robustness checks we conduct in Section 4.1, which leverages year-over-year variation across states. Therefore, for consistency of the analysis, we focus our results on models with state and year fixed effects and additional covariates.

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

Panel (a). Republican party	(1)	(2)	(3)	(4)
Win Election ($\mathbf{1}\{M_{it} \geqslant 0\}$)	0.089***	0.090***	0.088***	0.092***
	(0.006)	(0.006)	(0.006)	(0.006)
Gas Tax (G_{it})	-0.002	-0.003	-0.003	-0.004
	(0.005)	(0.005)	(0.005)	(0.006)
Win Election × Gas Tax	-0.015**	-0.013*	-0.013*	-0.013*
	(0.008)	(0.007)	(0.007)	(0.007)
Year FEs			X	X
State FEs			X	X
Covariates			X	X
Right Bandwidth	0.126	0.164	0.128	0.176
Left Bandwidth	0.125	0.174	0.112	0.149
Kernel	Uniform	Triangular	Uniform	Triangular
Observations	10,245	13,560	9,768	13,182
Panel (b). Democratic party	**(1)**	**(2)**	**(3)**	**(4)**
Win Election ($\mathbf{1}\{M_{it} \geqslant 0\}$)	0.086***	0.084***	0.090***	0.089***
	(0.009)	(0.008)	(0.009)	(0.008)
Gas Tax (G_{it})	0.007	0.006	0.009	0.008
	(0.005)	(0.005)	(0.006)	(0.005)
Win Election × Gas Tax	-0.021***	-0.017**	-0.020**	-0.019***
	(0.008)	(0.007)	(0.008)	(0.007)
Year FEs			X	X
State FEs			X	X
Covariates			X	X
Right Bandwidth	0.125	0.151	0.118	0.137
Left Bandwidth	0.109	0.142	0.103	0.142
Kernel	Uniform	Triangular	Uniform	Triangular
Observations	9,561	11,840	8,964	11,234

Notes: The table presents estimates from 8 regressions where the dependent variable is vote share for the indicated party during election year $t+1$, V_{t+1} as indicated from Equation (1). The running variable is the margin of victory in election year t, M_{it}. Win Election is an indicator for a positive margin of victory in election t, $\mathbf{1}\{M_{it} \geqslant 0\}$. Gas Tax ($G_{it}$) is an indicator for observations with a gas tax increase between elections t and $t+1$. We exclude observations for which the gas tax decreased. 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 travelled 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 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, number of incumbents running, party's tenure in office, and the normal party vote share. Bandwidths are selected optimally using two-sided MSE, following Calonico et al. (2014). Clustered standard errors at the state level are in parentheses. Significance levels are denoted as: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

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 both a largely statistically and economically significant manner. The Republican party's incumbency advantage decreases by 1.3 percentage points, but this estimate is only statistically significant to the 90% level. The Democratic party's incumbency advantage drops by 1.9 percentage points, which is statistically significant to the 99% level. This result aligns with the hypothesis that voters tend to punish sitting representatives for passing a gasoline tax increase. Moreover, while differences in the electoral penalty are not statistically different between parties, the difference in magnitude and significance indicates that voters punish Democrats more substantially that Republicans, consistent with the perception that Democrats may be more vulnerable, on average, to voters about this issue. These results speak to a broader literature on the public finance policy record of US political parties and the way in which voters do and do not hold them accountable (Reed, 2006; Clemens and Veuger, 2021; Ferejohn, 1986; Besley and Case, 1992, 1995; Besley and Coate, 2003).

Without a gas tax increase, the incumbency advantage hovers between 8-9 percentage points. As a result, gas tax increases reduce the incumbency advantage by 14.1% for Republicans and 21.3% for Democrats based on our preferred specification in column (4). The mean gas tax increase in our data is 2.32 cents, so the estimated penalty would be 0.6 percentage points per cent increase in the gas tax for Republicans and 0.8 for Democrats at the average and assuming a constant relationship. We discuss the policy implications of this result in more detail in Section 6.

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

We also examine the impact of a gas tax *decrease* on vote share in the next election in Appendix Table A8, and we find a statistically significant effect on the electoral penalty 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.

4.1 Validity & robustness checks

We begin this section by assessing the validity of our estimates by testing Assumptions A.1 and A.2 from Section 3.3. We then consider a broader set of robustness checks.

Continuity. In Panels (a) and (b) of Appendix Figure A3, 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. Also, a test of the log difference between the density functions in Appendix Table A6 shows that it is not possible to reject the null hypothesis of no manipulation of the density at the margin of victory threshold.[20] Panels (c) and (d) of Appendix Figure A3 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 and show that the difference in the density of bin midpoints appears largely continuous across the discontinuity.

Cattaneo et al. (2019) suggest two additional validity checks of continuity assumption: examining pre-determined outcomes and placebo tests. In Panel (a) of Appendix Figure A5, we test for whether there is a discontinuity for predetermined variables of districts in election t (β_3 in Equation (1)).[21] 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 continuity 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 a lack of discontinuity at these other points. In all cases, there is not a statistically significant effect, suggesting our main estimates are not likely attributable to chance.

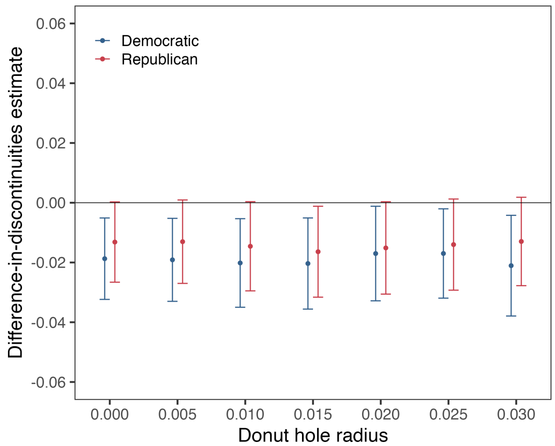
Local parallel trends. In Panels (a) and (b) of Appendix Figure A4, we test for pre-trends in our main estimates by estimating the effect of a gas tax increase between election t and $t+1$ on the effect of winning a close election up to three elections prior. We also examine dynamic effects for three elections *afterwards*. The estimate of interest is the interaction between G_{it} and $\mathbf{1}\{M_{it+k} \geqslant 0\}$, for $k \in \{-3, -2, -1, 0, 1, 2, 3\}$, estimated in separate regressions. The control group for each estimate is all other election outcomes without a gas tax increase. As a result, these estimates from prior elections can be interpreted as the difference in incumbency advantage between districts that eventually observe a gas tax increase and those that do not. In contrast, estimates for future election cycles are indicative of whether treatment effects are delayed. Districts which will receive a gas tax increase in a future election cycle (i.e., "not-yet-treated") are defined as having no gas tax increase in the current election and any addition elections prior to G_{it}.[22] The Diff-in-Disc coefficient reported in election cycle 0 is the actual effect recovered for β_4 in Figure 2 and Table 1. For Republicans (Panel (a)) there is no statistically significant effect in the previous two election cycles, although there is a statistically significant reduction three cycles before and after. For Democrats (Panel (b)), none of effects other than $k = 0$ (corresponding to the estimate of interest in our main results) are statistically significant, suggesting that there is no evidence to reject parallel trends.

[20] There is some variation in the intercept for Democratic candidates, but these are not statistically significant given the extent of confidence interval overlaps.

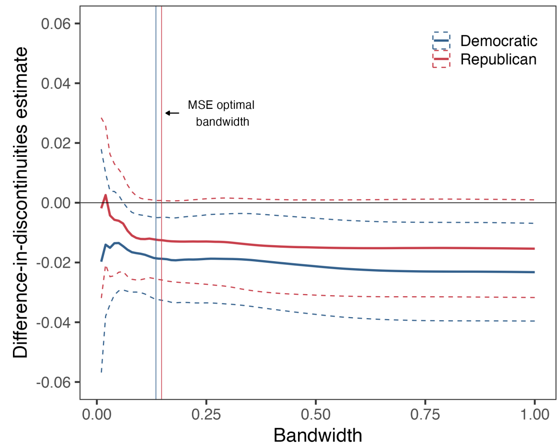
[21] Predetermined variables used are vote share in election $t-1$, an indicator for the party's victory in $t-1$, candidate tenure, the number of incumbents, and the number of candidates as of election t. We also report full regression table results in Appendix Table A7.

[22] For example, for the effect in year $k = -3$, we construct a "treated" group which experiences a gas tax increase between election t and $t+1$, but does not experience an increase between $t-3$ and $t-1$. Similarly for the effect in year $k = 3$, this "treated" group which experiences a gas tax increase between election t and $t+1$, but does not experience an increase between $t+1$ and $t+3$. Our Diff-in-disc estimator then compares the effect of the interaction of the gas tax indicator and a positive margin of victory on vote share for year $t+k$ to all other districts-year elections with no gas tax increase between t and $t+1$.

(a) Sensitivity to observations near the cutoff



(b) Sensitivity to bandwidth choice



(c) Sample restrictions

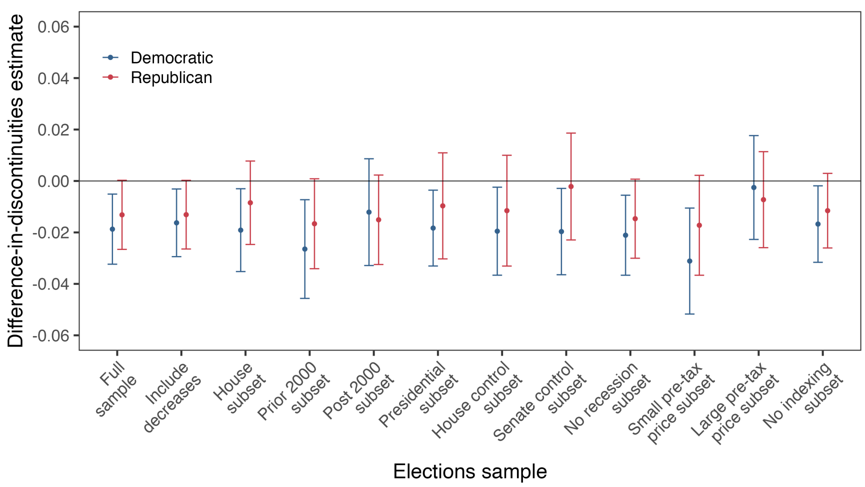


Figure 4: Robustness checks

Notes: Panel (a) shows the coefficient estimates of β_4 from Equation (1) when we exclude units within a "donut hole" with the indicated radius (i.e., + and −) around the margins of victory threshold. Panel (b) shows the coefficient estimates of β_4 where we add units at the end points by choosing varying bandwidth size. Panel (c) shows estimates of β_4 using different election samples. "Full sample" is our main results from Table 1. "Include decreases" includes gas tax decreases and sets them as $G_{it} = 0$. "House" only considers elections for lower houses of state legislatures. "Post/Prior 2000" splits the sample before and after the year 2000. "Presidential" restricts observations to presidential election years. "Senate/House control" restricts to elections when the party in question controls the state upper or lower legislative house. "No recession" excludes observations during NBER-identified recessions. "Large/Small pre-tax-price" indicates whether pre-tax average state annual gas prices are above/below the median pre-tax gas price. "No indexing" restricts the sample to non-indexed gas tax increases. All regressions are estimated using local linear regression corresponding to our preferred specification in Table 1 with covariates, state and year fixed effects and a triangular kernel. Bandwidths are selected optimally using two-sided MSE, following Calonico et al. (2014). Standard errors are clustered at the state level, and error bars or dashed lines indicate confidence intervals at the 95% level.

Robustness checks. Lastly, we explore the robustness of our results to various modifications as shown in Figure 4 and find limited heterogeneity of effects. In Panel (a), we analyze how sensitive the estimates are to the response of potential outliers very close to the margin of victory threshold, by removing a "donut" of observations where the margin of victory is between -0.03 and 0.03 (Bajari et al., 2011; Barreca et al., 2011, 2016). Estimates are comparable to our main results. In Panel (b), we test the sensitivity of our estimates to the bandwidth size. Wider bandwidths do not meaningfully impact the results except by increasing statistical power marginally. In Panel (c), we restrict the sample to account for a variety of factors. First, we include gas tax decreases in our sample (but keep $G_{it} = 0$ for these observations) and find that the effect for Democrats is slightly weaker. Second, estimates for elections in only lower house of the legislature are slightly stronger for Democrats and weaker for Republicans. Third, estimates in our sample before 2000 are of larger magnitude. Fourth, restricting the sample to Presidential election years does not have a meaningful impact on our results, suggesting down-ballot voting is inconsequential for our estimates. Fifth, electoral penalties are less strong when Republicans control the upper house of the legislature, but otherwise party control of the legislature does not effect our results. Seventh, we test for whether voters react to gas taxes differently during worsening economic conditions by restricting the sample to observations outside of NBER-identified recessions (NBER, 2024) and find no meaningful impact on our estimates. Eighth, we examine the effect of higher or lower pre-tax gas prices relative to the median and find that the incumbency penalty is larger for states with smaller pre-tax gas prices. This difference may reflect the fact that voters in these states are more sensitive to tax increases when gas prices have not been as high. Finally, restricting the sample to non-indexed gas tax increases (e.g., not tied to inflation) does not impact the magnitude but slightly reduces the precision of estimates. We report full regression tables of these results in Appendix Table A11.

5 The salience of gas tax increases

Past work has pointed to gasoline consumption responses to gas tax changes. To attribute the electoral penalties to incumbents that we observe, it is instructive to gauge the extent to which the public at large is aware of gas tax changes. We 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.

To gauge the correlation between gas tax increases and Google Trend searches, we apply a Linear Projection-Difference-in-Difference (LP-DiD) estimator, following Dube et al. (2023). LP-DiD allows for the repeated, non-permanent nature of the gas tax change for a given state across time. To the extent that gas tax changes may be anticipated, our estimates may not be causal. We provide more details about the Google Trends data and their estimation in Appendix D.[23] In Figure A6, we show that Google searches jump for the keyword "Gas tax" in the year

[23] Under parallel trends and no-anticipation the LP-DiD estimator without covariates identifies a weighted average of all cohort-specific treatment effects, with weights that are always positive and depend on treatment variance and subsample size. That is, the LP-DiD estimator produces a variance-weighted average treatment-on-the-treated (ATT). The key additional assumption in this framework is that treatment effects stabilize after a specific number of years, we assume this is a single year for our context. Many of the recent DiD estimators can be reproduced as specific sub-cases of their LP-DiD general

a gas tax increase was implemented. There is no effect on Google searches for keywords associated with other taxes, politics, transportation, or other general policies. This suggests that gas tax changes are likely to be something that the general population is aware of and may respond to. Given this suggestive evidence of their salience, we conclude by exploring the electoral implications of moving gas taxes closer to optimal levels.

6 Discussion & Conclusions

Given the presence of electoral penalties on incumbents in our results, an important policy-motivated question is how large these penalties might be if gas taxes were raised closer to the socially optimal level. Raising taxes in this way would address negative externalities from the transportation sector and close the gap in highway infrastructure financing.[24] In our setting, to properly account for the size of these increases, one would ideally recover marginal external damages and infrastructure funding gaps from driving by state. Doing so in a causally identified manner is beyond the scope of this study. Instead, we perform two bounding back-of-the-envelope exercises. First, we examine what setting gas taxes at an optimal *national* level would do, following the analysis in Parry and Small (2005). Second, we consider what county-level gas taxes would look like, following Nehiba (2022). In both cases, we compare effects for a gasoline tax increase and a vehicle miles traveled (VMT) tax. VMT taxes more directly tax the externality associated with driving (i.e., distance-based) and therefore are generally more efficient, being closer to Pigouvian taxes (Metcalf, 2023). The full analysis for these calculations is presented in Appendix E.

Since penalties will likely respond non-linearly to gas tax increases, we begin by converting our central electoral penalty for each part from column (4) of Table 1 into an elasticity using sample gas tax levels and increases reported in Appendix Table A3. Adjusting average gas taxes to their optimal level from Parry and Small (2005) would result in increases between 50.6 and 75.1 cents per gallon and electoral penalties between 1.7 and 16.8 percentage points, with larger penalties for Democrats. It is important to emphasize for the interpretation of these back-of-the-envelope calculations and those below, that the largest implied penalties are reflective of states with lower initial gas tax levels, and so the amount of change to reach optimality would imply very large penalities. Increasing gas taxes to their optimal second-best level may be too extreme as an exercise, so we also consider the effect of raising state taxes enough to cover the current shortfall in *Federal* Highway Trust Fund revenues following Glaeser et al. (2022). This would result in 7.9 and 10.4 percentage point penalties, on average, for Republican and Democratic candidates, respectively.

Because a VMT tax taxes externalities directly, the welfare benefit of higher VMT taxes outweighs the excess burden of taxation by more. Hence, optimal VMT taxes end up being more than twice than their gas tax equivalent levels (\$2.48 per gallon versus \$1.01 per gallon) after accounting for fuel economy and driving responses. Electoral

approach based on either weights assigned to particular treatment events, or the choice of a base period for constructing the local projection. In our case, we measure the effect of increasing the gasoline tax on the Google search relative popularity index in a given year h relative to $t-1$.

[24]Using the gas tax to price externalities is second-best to Pigouvian taxation, and an established literature describes the challenges and limitations of trying to achieve second-best optimality in the transportation sector (Diamond, 1973; Fullerton and West, 2002; Knittel and Sandler, 2018).

penalties to incumbents from optimal VMT taxes therefore are also larger: 2.9 to 48.5 percentage points. In Appendix Table A12, we compare VMT tax-induced electoral penalties for states that have enacted voluntary VMT tax programs or pilots.[25] Comparing penalties to implementation, there is not a clear pattern where states that have adopted VMT taxes have relatively lower penalties. This may not be surprising since these states are not charging VMT taxes at the socially optimal level, but at levels equivalent to gas taxes incidence, so the magnitude of the penalties are much smaller in practice. Many, but not all, of the states have had Democratic-controlled legislatures during our sample, and so this may explain why they are able to enact policies that may still induce some voter backlash, but which may not be enough to vote the party out of control.[26] In addition, as shown in Figure 1, many states with VMT tax programs or pilots also have higher gas tax levels (e.g., Pennsylvania or Washington), and so gas tax levels (or equivalent VMT taxes) are already closer to optimality and so implied electoral penalties from moving to second-best optimality would be smaller.

Second-best optimal gas taxes would also ideally 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 US counties and demonstrates that for many rural counties, moving toward optimality would actually mean gas tax *decreases*. The US electoral system only allocates voting representation proportional to population for lower house state and federal legislatures, with senates, gubernatorial and presidential elections (among others) allocating greater electoral weight to areas with lower population density. As a result, the effect of rural county gas tax decreases could meaningfully offset voter backlash to gas tax increases despite population differences. In Appendix Figure A8, we show the implied tax changes for second-best optimal county gas taxes, and in Appendix Figures A9 and A10, we show implied penalties: roughly a quarter of counties (26.5%), most which are densely populated counties would have potential penalties ranging from 0.01 to 14 percentage points. At the same time, the remainder of less populated counties would have small incumbency advantage increases of 0.77 and 1.02 percentage points, on average, Republicans and Democrats, respectively. While these results are speculative, they point to a potential benefit of disaggregated taxes, not just for efficiency purposes, but also for political feasibility.[27]

In all, this back of the envelope exercise points the relevance of electoral penalties in understanding both the explanations for lower than optimal gas taxes as well as the political cost of moving towards optimality. Nevertheless, it remains to be seen for future work how to bench mark these meaningful and statistically significant effects commensurate with the relative importance of environmental and energy taxes in voter priorities (List and Sturm, 2006). While the magnitudes of the effects we recover in this study could be large enough to tip close races, they are nonetheless unlikely to be large enough to generate broader political opposition on their own. As shown for the

[25] These programs operate by refunding state gas tax revenues back to participants and charging them based on recorded annual vehicle miles traveled. To date, four states have voluntary programs and an additional 10 states have conducted pilot programs. In most cases, VMT fees are set to be be revenue neutral: `https://enotrans.org/article/the-current-status-of-state-vmt-fees/`.

[26] In California, voter backlash to gas taxes induced a referendum in 2018 to recall gas tax increases from recent legislation (Muehlegger and Epstein, 2023).

[27] These counties are close to representative, but not perfect as illustrated in that paper. We note that corresponding care should be taken in extending these calculations to all US counties.

European Union in Douenne and Fabre (2022), combating political opposition may be more about framing and ties to broader political ideology than political backlash to the incidence itself.

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

Matías Navarro

Gian-Claudia Sciara

Andrew R. Waxman*

April 7, 2024

Appendix A. Data appendix

State election data

We use Klarner (2018) dataset on state legislative elections. These data cover 1967-2016. The raw dataset on elections comprises 378,345 observations at the county 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. Since seats are elected at the district level, we aggregate the data for every election (representative or senator) and each candidate. 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 ($t + 1$).

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

*Corresponding author: awaxman@utexas.edu

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).

Appendix B. Review of regression discontinuity approaches to estimating the incumbency advantage

A rich literature in political science over the past decade has shown over many types of elections that incumbents tend to have a meaningful advantage. The regression discontinuity design (RDD) tends to be robust to concerns that candidates that lose very close elections are different than those that win as examined by Hainmueller et al. (2015).

Let v_{ct+1} be the vote share of candidate c in a in election year $t+1$. Then, for any consecutive election years, t and $t+1$:

$$v^{p}_{it+1} = \alpha w_{ct} + \beta v^{p}_{ct} + \gamma d^{p}_{ct+1} + e_{ct+1}, \tag{A1}$$

where w_{ct} is a vector of determinants of election outcomes in year t, d^{p}_{ct+1} is an indicator variable for whether party p was the incumbent in election $t+1$, that is;

$$d^{p}_{ct+1} = \mathbb{1}\Big\{v^{p}_{ct} \geqslant \frac{1}{2}\Big\}. \tag{A2}$$

Three assumptions help identify the incumbency advantage from RDD. First, there is a non-trivial random component to the final vote share, meaning that even if candidates can influence the vote to some extent, there still exists an element of chance that ultimately determines the exact vote share. This could be due to factors such as weather conditions or unforeseen news events that affect the election. Second, the probability density of the vote share conditional on relevant controls $f_{ct}\,(v|w)$ is continuous. This assumption requires that certain kinds of electoral fraud are negligible. Finally, for our analysis to be valid, we must assume that the error term e_{ct} is independent of both w_{ct+1} and v_{ct}. In section 4.1, we perform standard falsification tests to consider the validity of the first assumption, and tests for continuity and manipulation around the discontinuity to test the second assumption, while the third assumption cannot be formally tested.

Appendix C. 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 $\mathbf{1}\{M_{it} \geqslant 0\} \equiv m \in \{0,1\}$ is an indicator of winning the election in period t and $G_{it} \equiv g \in \{0,1\}$ denotes an indicator for increase of the gas tax between period t and $t+1$, then

$$\begin{aligned} V_{it+1} = &\mathbf{1}\{M_{it} \geqslant 0\} G_{it} V_{it+1}(1,1) + \mathbf{1}\{M_{it} \geqslant 0\}\,(1 - G_{it})\, V_{it+1}(1,0) + \big(1 - \mathbf{1}\{M_{it} \geqslant 0\}\big)\, G_{it} V_{it+1}(0,1) \\ &+ \big(1 - \mathbf{1}\{M_{it} \geqslant 0\}\big)\,(1 - G_{it})\, V_{it+1}(0,0). \end{aligned} \tag{A3}$$

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 gas tax 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 the election *after* the election determining incumbency t_0, let us define: $Z^- \equiv \lim_{m\to 0^-} \boldsymbol{E}\Big[Z_{it}|\mathbf{1}\{M_{it} \geqslant 0\} = m,\, G_{it} = 0\Big]$ and $Z^+ \equiv \lim_{m\to 0^+} \boldsymbol{E}\Big[Z_{it}|\mathbf{1}\{M_{it} \geqslant 0\} = m,\, G_{it} = 0\Big]$. Note the distinction here between potential outcome g, which could occur to either group of districts (those that receive a gas tax increase and those that do not). 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, the standard RDD estimator recovers:

$$
\begin{aligned}
\hat{\tau}_{RDD} &= V^- - V^+ \qquad\qquad \text{(A4)}\\
&= V(1,0)^- - V(0,0)^+\\
&= \Big[V(1,0)^- - V(1,1)^-\Big] + \Big[V(1,1)^- - V(0,1)^-\Big] + \Big[V(0,1)^+ - V(0,0)^+\Big]\\
&= -\underbrace{\boldsymbol{E}\Big[V_{it}(1,1) - V_{it}(1,0)|\mathbf{1}\{M_{it} \geqslant 0\} = 0, G_{it} = 1\Big]}_{\text{Causal effect of interest}} + \underbrace{\boldsymbol{E}\Big[V_{it}(1,1) - V_{it}(0,1)|\mathbf{1}\{M_{it} \geqslant 0\} = 0, G_{it} = 1\Big]}_{\text{Incumbent advantage where gas tax increased}}\\
&\qquad + \underbrace{\boldsymbol{E}\Big[V_{it}(0,1) - V_{it}(0,0)|\mathbf{1}\{M_{it} \geqslant 0\} = 0, G_{it} = 1\Big]}_{\text{Effect of gas tax on party that lost last election}}
\end{aligned}
$$

Clearly, the presence of the second and third terms mean that $\hat{\tau}_{RDD}$ will not provide the causal estimates of interest. Hence, to do so, we must control for these last two effects.

For all post gas tax treatment period outcomes, $Z = V, V(1,1), V(1,0), V(0,1), V(0,0)$, let us define the equivalent pre gas tax treatment period as $\tilde{Z}^- \equiv \lim_{m\to 0^-} \boldsymbol{E}\Big[Z_{it}|\mathbf{1}\{M_{it} \geqslant 0\} = m,\, G_{it} = 1\Big]$ and $\tilde{Z}^+ \equiv \lim_{m\to 0^+} \boldsymbol{E}\Big[Z_{it}|\mathbf{1}\{M_{it} \geqslant 0\} = m,\, G_{it} = 1\Big]$

Now, define the Difference-in-Discontinuities (diff-in-disc) estimator as

$$
\hat{\tau}_{DD} = \Big(\tilde{V}^- - \tilde{V}^+\Big) - \big(V^- - V^+\big) \qquad \text{(A5)}
$$

The $\hat{\tau}_{DD}$ estimator identifies the average treatment effect on the treated (ATT) of a gas tax increase on the 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 in the left-side of the limit at $M_{it} = 0$.

Assumption 1 (Continuity): All potential outcomes are continuous in M at 0.

Assumption 2 (Local Parallel Trends): Units i that pass a gasoline tax increase in period $t+1$ do not have a meaningfully different vote share difference for winners and losers during the election in t: $V(1,1) - V(0,1) = \tilde{V}(1,1) - \tilde{V}(0,1)$.

Assumption 3 (Accounting for τ_{LE}, the effect of gas tax increases on losers): Differences in the vote share between districts in states with and without gas tax increases which had losers in period t, that is where $M_{it} \leqslant 0$, can be controlled for in regression or are observationally equivalent. In practice, this implies that $V(0,1) - V(0,0) = 0$. We show in our main estimates that vote shares to the left of the discontinuity are usually statistically insignificant different between districts in states with and without gas tax increases. However, if non-random selection of the gas tax means that there are unobservables that are different between districts in states with and without gas tax changes (i.e., political and economic time invariant observables), this could confound estimation of $\hat{\tau}_{DD}$ we provide evidence through looking at differences between vote shares and other pre-determined variables in period t in Panel (a) of Figure A5 to corroborate this assumption.

To show that the Difference-in-Discontinuities estimator, $\hat{\tau}_{DD}$, recovers the desired ATT of the gasoline tax, $\boldsymbol{E}\Big[V_{it}(1,1) - V_{it}(1,0)|M_{it} = 0\Big]$, note that

$$
\begin{aligned}
\hat{\tau}_{DD} &\equiv \left(\tilde{V}^{-} - \tilde{V}^{+}\right) - \left(V^{-} - V^{+}\right) \\
&= \Big[\tilde{V}(1,1)^{-} - \tilde{V}(0,1)^{+}\Big] - \Big[V(1,0)^{-} - V(0,0)^{+}\Big] \\
&= \Big[\tilde{V}(1,1) - \tilde{V}(0,1)\Big] - \Big[V(1,0) - V(0,0)\Big] \\
&= \Big[V(1,1) - V(0,1)\Big] - \Big[V(1,0) - V(0,0)\Big] \\
&= \Big[V(1,1) - V(1,0)\Big] - \Big[V(0,1) - V(0,0)\Big] \\
&= \Big[V(1,1) - V(1,0)\Big] \\
&= \boldsymbol{E}\Big[V_{it}(1,1) - V_{it}(1,0)|M_{it} = 0\Big],
\end{aligned}
\tag{A6}
$$

where the second line follows from the definition of the discontinuity, the third line comes from applying Assumption 1, the fourth line comes from applying Assumption 2, the fifth line comes from rearranging, the sixth from Assumption 3, and the final one from evaluating the expression in expectation.

Appendix D. 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.

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

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 changes more than once with arguably not permanent effects on electoral outcomes. 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 change on the number of Google searches, we estimate the following LP-DiD specification,

$$\Delta_h y_{it} = \beta_h \Delta D_{it} + \sum_{p=1} \gamma_p^h \Delta y_{i,t-p} + \delta_t^h + e_{it}^h, \tag{A7}$$

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 change, and the specification also includes outcome lags Δy_{it-p} and time fixed effects δ_t^h.

Appendix E. Electoral penalties from gas tax changes

In this section, we detail several back-of-the-envelope calculations discussed in section 6 of the paper.

We now consider the electoral penalties (or benefits) from a set of policy-relevant exercises. We begin by converting our main estimates into an electoral penalty elasticity:

$$\eta^p = \frac{\beta_4^p}{\beta_3^p} \cdot \frac{\bar{g}}{\overline{\Delta g}}, p = Dem, Rep \tag{A8}$$

Here, β_4^p are our main estimates from column 4 of Table 1 (-0.013 for Republicans and -0.016 for Democrats), β_3^p is the incumbency effect from the same column and table (0.090 for Republicans and 0.086 for Democrats). We multiply this by the the average gas tax level in our sample divided by the average gas tax increase in our sample, corresponding to 3.31/18.83 for Republicans and 3.21/18.64 for Democrats. These yield elasticities of electoral penalty of -0.821 for Republicans and -1.08 for Democrats.

Electoral penalty from optimal gas taxes

As suggested by Parry and Small (2005), US gasoline taxes are not set at their optimal second-best level to address externalities associated with driving. In this exercise, we explore the hypothetical electoral penalties that a Republican or Democratic incumbent running for legislative office in the year 2000 might have faced.

Adjusting the average gas taxes in our sample in 2000 of 20.2 cents per gallon to the optimal US tax of $1.01 from Parry and Small (2005), and accounting for federal excise tax if 18.4 cents per gallon, would result in tax increases of between 50.6 and 75.1 cents per gallon with a mean increase of 62.4.

Converting these into percentage terms and multiplying by η_p for each state in 2000 yields penalties ranging from 1.7 to 12.8 percentage points (mean 3.3) for Republicans and from 2.2 to 16.8 (mean 4.3) for Democrats. Considering that the mean incumbency advantage in our data is roughly 9 percentage points, this would substantially reduce the margin of victory for close elections, but not eliminate it. Since we apply this across all 50 states, the difference between parties reflects only the differential effect of penalties by party. Differences between states within parties reflects variation in the size of the gas tax increase required to raise taxes to their optimal level.

Alternatively, the optimal vehicle miles traveled tax from Parry and Small (2005) would be $0.14 per mile, which is equivalent to a fuel tax rate of $2.48 per gallon. The optimal VMT tax is larger because it more directly addresses distance-related externalities, and so results in substantially larger welfare gains than an optimized gasoline tax. This is despite the fact that its incidence is higher. As a consequence the optimal VMT tax would result in even larger penalties ranging from 2.9 to 37.0 percentage points for Republicans and 3.8 to 48.5 for Democrats.

Of potential relevance is comparing this optimized VMT tax for 2000 to the set of states that have implemented voluntary or pilot VMT tax programs. Oregon was the first state to implement a pilot VMT tax in 2001, which subsequently became a voluntary policy in 2015. Voluntary programs allow citizens to opt into a program where they are charged for each mile that their vehicle is driven and, in exchange, receive a rebate of their state gasoline taxes. Pilot programs are similar but are temporary and available to fewer residents.

Examining Table A12, we present the electoral penalties implied for moving from 2000 gas taxes to a level corresponding to the optimal VMT tax ($2.48 minus the federal tax of $0.184). We also show the corresponding quintile of the state in the distribution of penalties for states that have a VMT tax policy or pilot. Almost all of these states hold Democratic majorities in their statehouses during our sample, and Panel A shows that 7 of the 10 states that have instituted a pilot only have penalties for both parties in the bottom (lowest penalty) quintile. Panel B shows a more mixed pattern, with states that have implemented the policy appearing in both top and bottom quintiles of penalties. Since the size of the penalty scales with how much the gas tax would have to be increased, states with very low gas taxes in 2000 may be expected to have larger penalties. States with lower gas taxes may also be less willing to opt to convert to a VMT tax if it is politically challenging.

As a caveat to this analysis, we do not account for the distribution of parties with incumbents up for election, but rather calculate penalties for a hypothetical close election in all 50 states in 2000. We do not account for variation across states in the level of externalities from driving, nor state-level variation in fuel economy or driving, and so this exercise should be seen as a rough back-of-the-envelope exercise.

Electoral penalty from closing Federal Highway Trust Fund gap

Given the challenges of funding highway maintenance and construction using existing gas tax changes, there has long been a call to convert gasoline taxes into vehicle miles traveled (VMT) fees. Parry and Small (2005) calculate the optimal VMT and Glaeser et al. (2023) consider the distributional impacts of a Federal gas tax to VMT fee conversion accounting for potential changes in the vehicle fleet and driving. They find that to cover the shortfall of the Federal Highway trust fund would require an increase in gas taxes of 1.15 cents per mile and would translate into a VMT fee of 0.93 cents per mile. Average passenger vehicle fuel economy according to the Federal Highway Administration is 24.2 miles per gallon, which translate into a gas tax of 27.8 cents per gallon and an equivalent VMT fee of 22.5 cents per gallon. Since the federal gas tax is currently 18.2 cents, this would mean an increase of 4.2 cents. The average gas tax in the last year of our sample is 25.2 cents on average, so a total state and federal gas tax of 43.6 cents. Raising state gas taxes by this amount (9.6%) would result in electoral penalties of 7.9 and 10.4 percentage points for Republican and Democratic candidates, respectively. The 2.5 percentage point difference between these could point to the political expedience of converting to a VMT fee based system even if taxes or fees are not brought fully up to this level.

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 US 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:

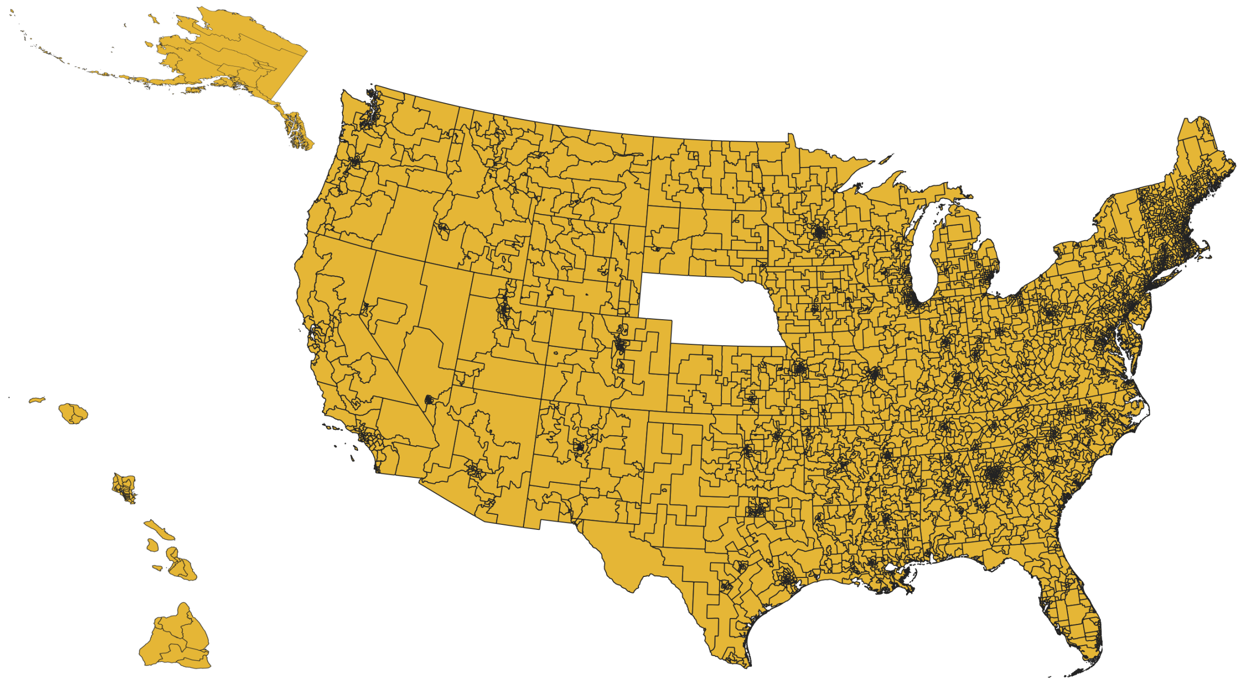
$$\max_{VMT} \sum_{i=1}^{380} \int_0^{VMT_i} P_i(\nu_i)d\nu_i - C_i\left(VMT_i\right)$$
$$\text{subject to } \sum_{i=1}^{380} t_i \cdot VMT_i = \sum_{i=1}^{380} t_s \cdot V\bar{M}T_i,$$

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. Looking at the set of tax changes relative to state taxes in Appendix Figure A8, 73.4% are *decreases*, and the largest tax increases are in urban areas where externalities are concentrated.

Applying these county level gas tax changes to our penalty elasticities from Equation (A8), show the distribution in a map in Appendix Figure A9 and a kernel density plot in Appendix Figure A10. Penalties for Republicans are as low as 14 percentage points, but electoral benefits are as high as 1.09 percentage points, with a mean of a 0.29 percentage point penalty. For Democrats, penalties run between the same ranges, but the average is a penalty of 0.37 percentage points. As the plots make clear, a larger number of counties (73%) have electoral benefits from county gas taxes, and these are less urban. These results illustrate the potential electoral benefits to some locations from disaggregated gas taxes.

Appendix figures & tables

(a) Lower house



(b) Upper house

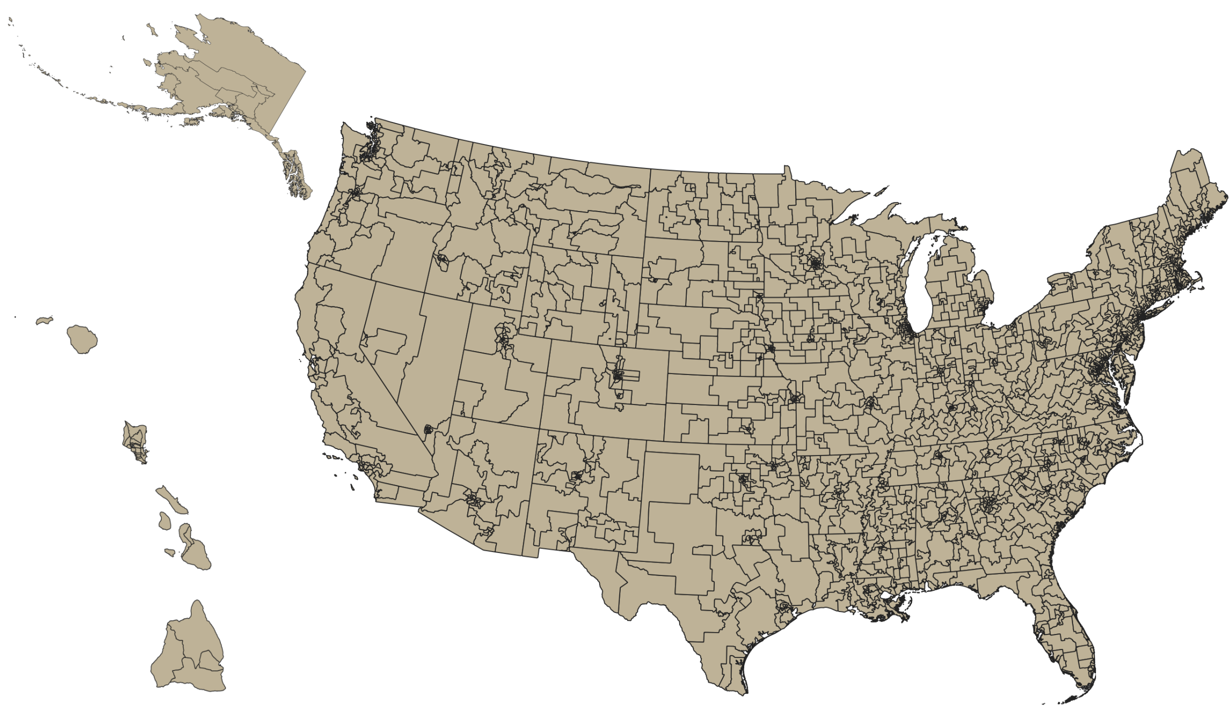


Figure A1: State legislative boundaries 2010

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.

(a) Year-over-year changes in gas tax

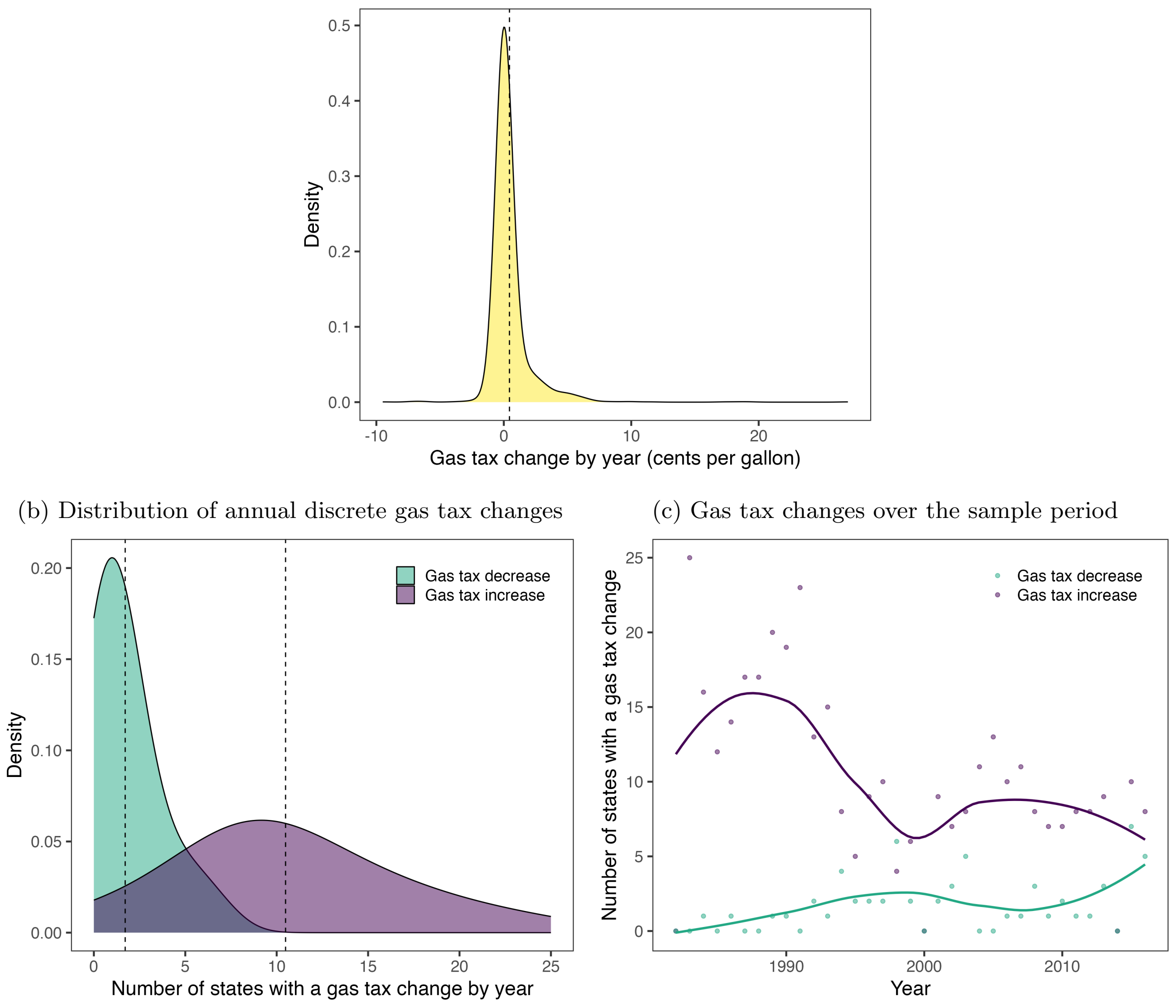


Figure A2: Increases and decreases of the gas tax

Notes: Panel (a) shows year-over-year changes in gas tax over 1982–2016. Panel (b) shows the frequency of gas tax changes over state-years, where the vertical dashed line represents the mean of the distribution. Panel (c) shows the number of US states which 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.

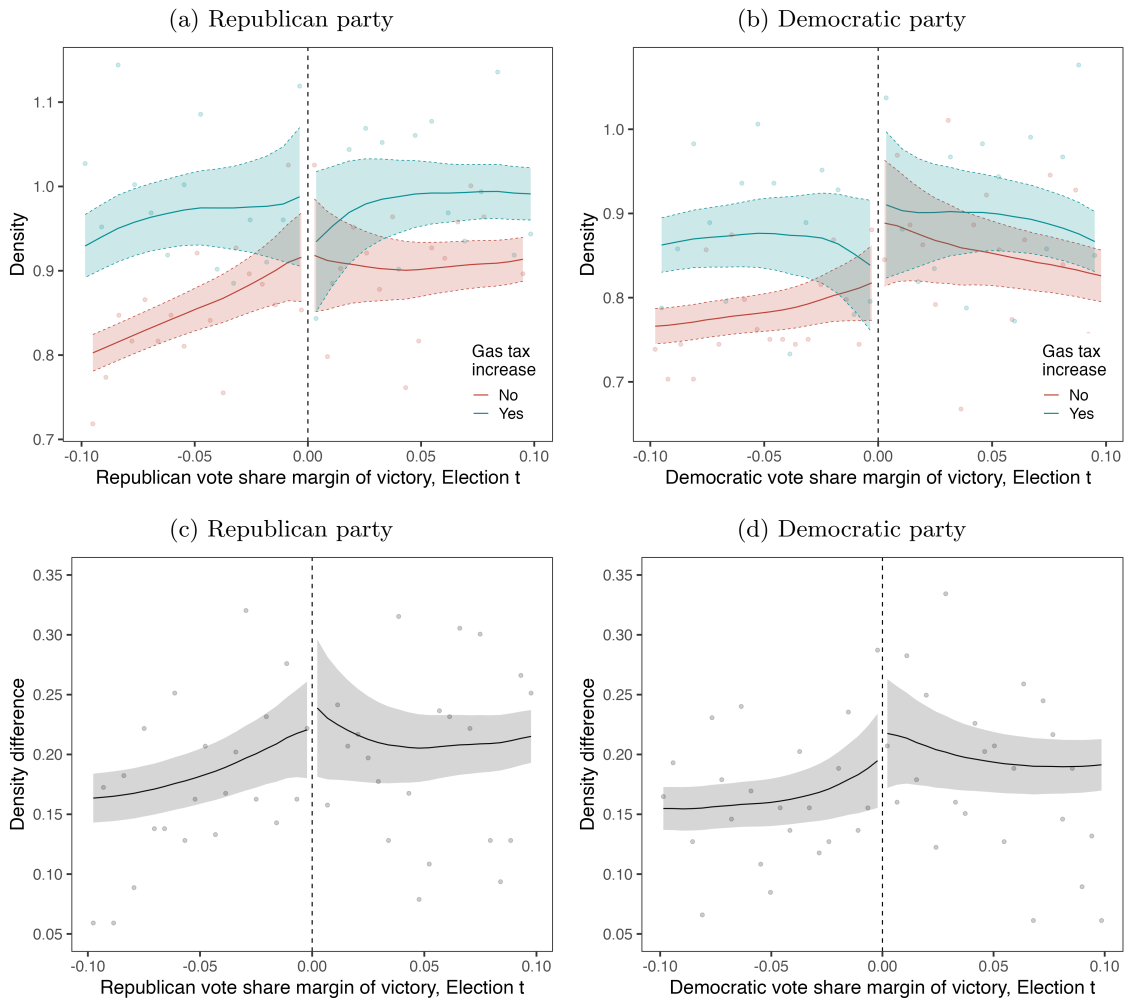
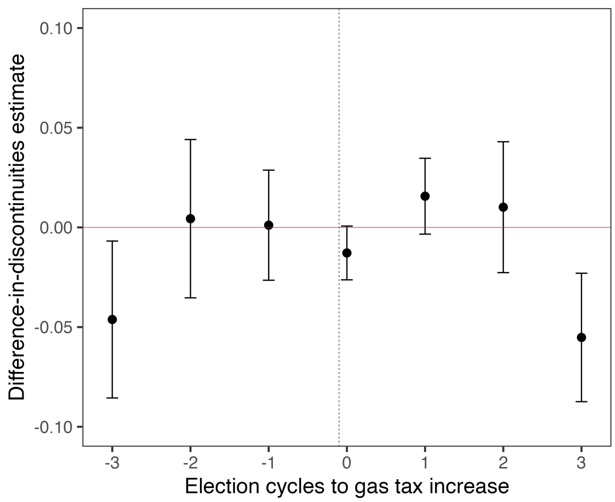


Figure A3: Tests for continuity & manipulation around the discontinuity

Notes: Panels (a) and (b) provide a test of the continuity assumption by plotting the density of the margin of victory in election t for districts in states with and without a gas tax increase between elections t and $t+1$. Panels (c) and (d) show the test of continuity of the difference in density of the margin of victory around the cutoff for districts in states where the gas tax changed relative to where it did not change. Plots show smoothed local linear estimates with a triangular kernel and 95% confidence intervals.

(a) Republican party



(b) Democratic party

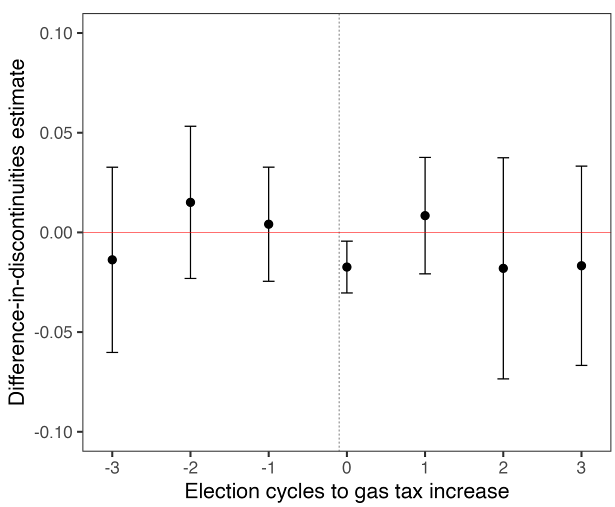
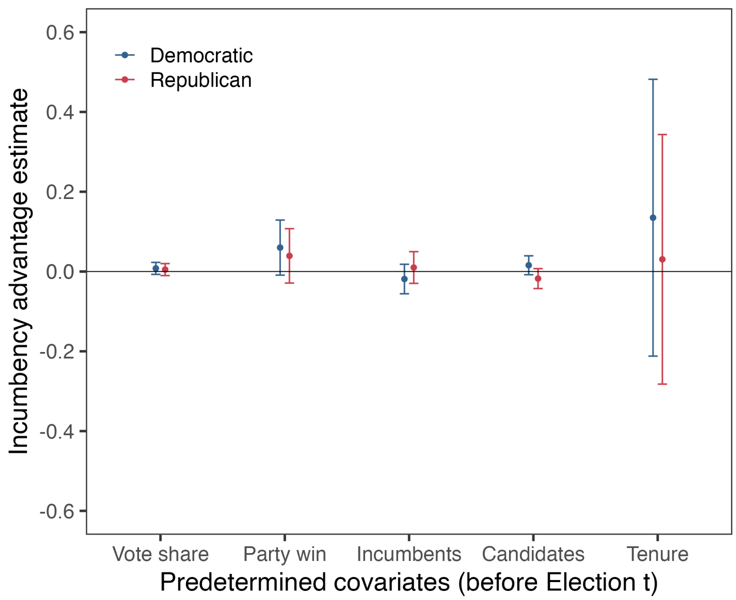


Figure A4: Diff-in-disc election cycle estimates for testing local pre- & post-trends

Notes: This figure shows tests of the local parallel trends assumption by estimating the effect of gas tax increases three elections prior and after. Coefficient estimates are the interaction between an indicator of a gas tax increase and a positive margin of victory for three elections before and after. We define treated observations for the -3 election cycle as those where a district had a gas tax increase exactly three cycles before the current election year and no other changes in between. An election falls into the control group if its district does not see any gas tax increases three election cycles before the current election year nor other changes in between. Coefficients are estimated using local linear regression corresponding to our preferred specification in Table 1 with covariates, state and year fixed effects and a triangular kernel. We choose the optimal bandwidth following Calonico et al. (2014) to select close elections only. We plot the coefficient estimates for the relative decrease in incumbency advantage, and visually examine whether or not treated and untreated districts were following similar trends with respect to when the gas tax increase occurs. For each coefficient estimate, error bars indicate the 95% confidence interval based on standard errors clustered by state.

(a) Predetermined covariates



(b) Placebo cutoffs

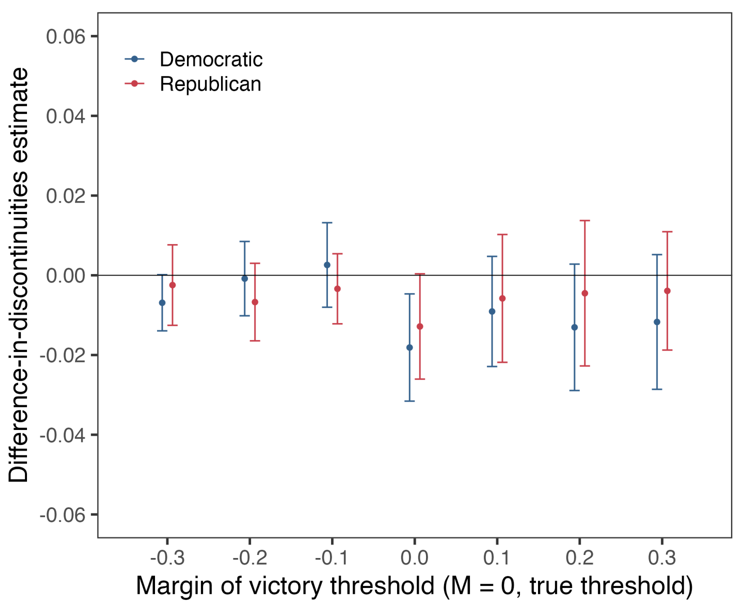


Figure A5: Additional tests for continuity & manipulation around the discontinuity

Notes: Panel (a) shows the coefficient estimates of local linear regressions where the dependent variables are determined before the electoral outcome of election year t is realized. The coefficients are for β_3 in Equation (1). Vote share is the proportion of votes the party obtained in the election year $t-1$. Party win is an indicator for whether the party won the race in the election year $t-1$. Tenure is the number of years the party has been in the seat before election year t. Incumbents is the number of sitting members running for re-election in the election year t. Candidates is the number of candidates running in the election year t. Panel (b) shows the coefficient estimates of local linear regressions where we artificially set different margins of victory thresholds. The coefficients are for β_4 in Equation (1). All regressions are estimated for our preferred specification in Table 1 with covariates, state and year fixed effects and a triangular kernel. Error bars represent 95% confidence intervals based on standard errors clustered by state.

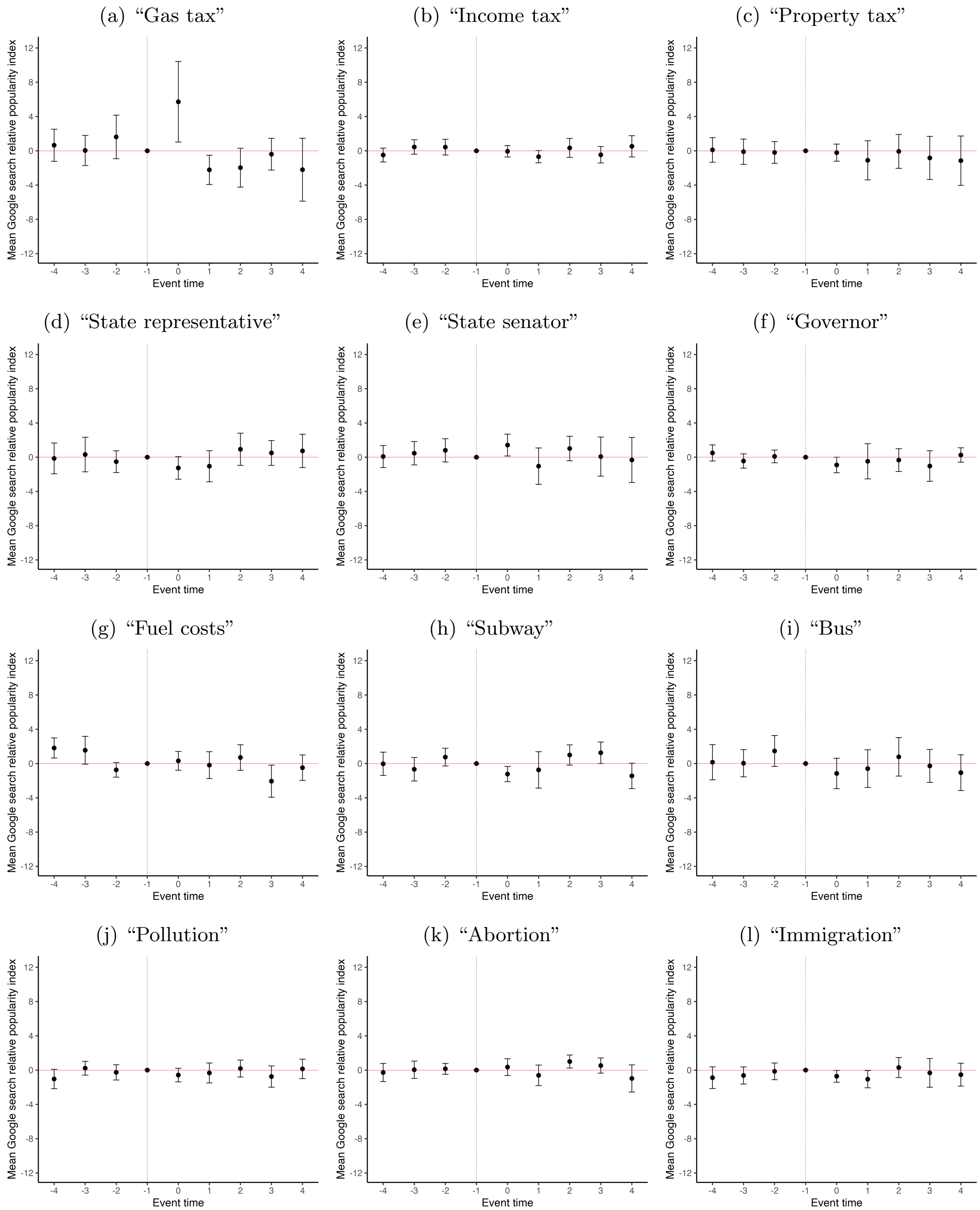
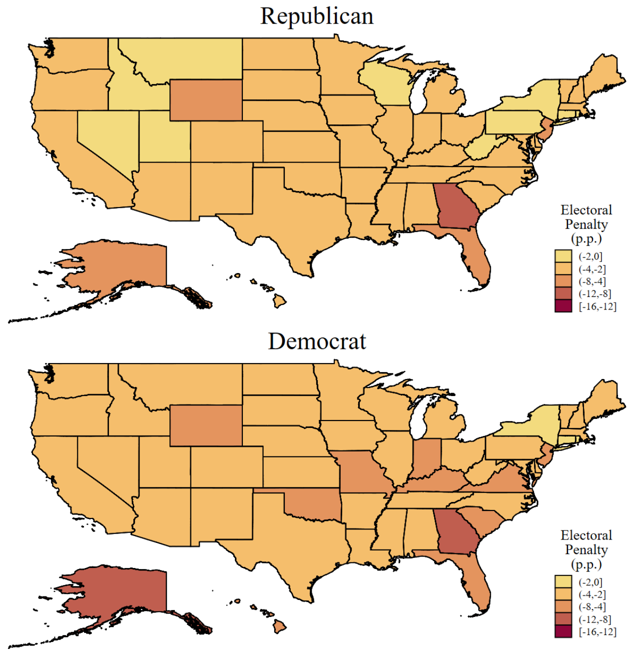


Figure A6: Gas tax changes and Google searches

Notes: Panels (a)–(l) show the effect of a gas tax change 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 change, 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 change, indicated by the dashed vertical line. 95% confidence intervals are based on standard errors clustered by state.

(a) Map of implied electoral penalties



(b) Distribution of implied electoral penalties

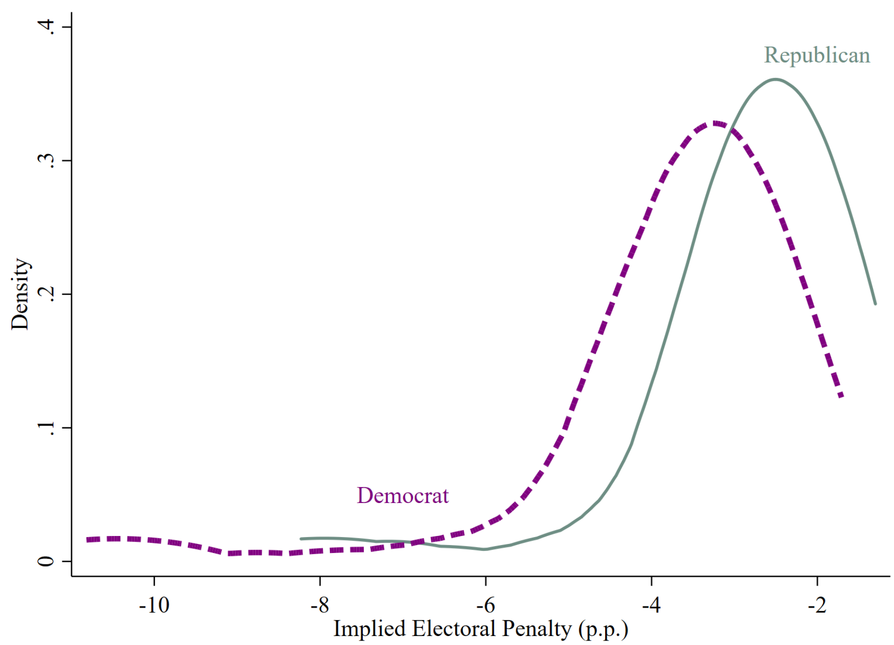


Figure A7: Implied electoral penalties of second-best optimal gas taxes from Parry and Small (2005)

Notes: Figure plots a map and the distribution of the implied electoral penalties from increasing state gas taxes in 2000 to their optimal level following Parry and Small (2005). Negative values are penalties. Calculation uses our main elector penalty results evaluated gas tax sample averages using equation (A8). More detail on calculation provided in Appendix F.

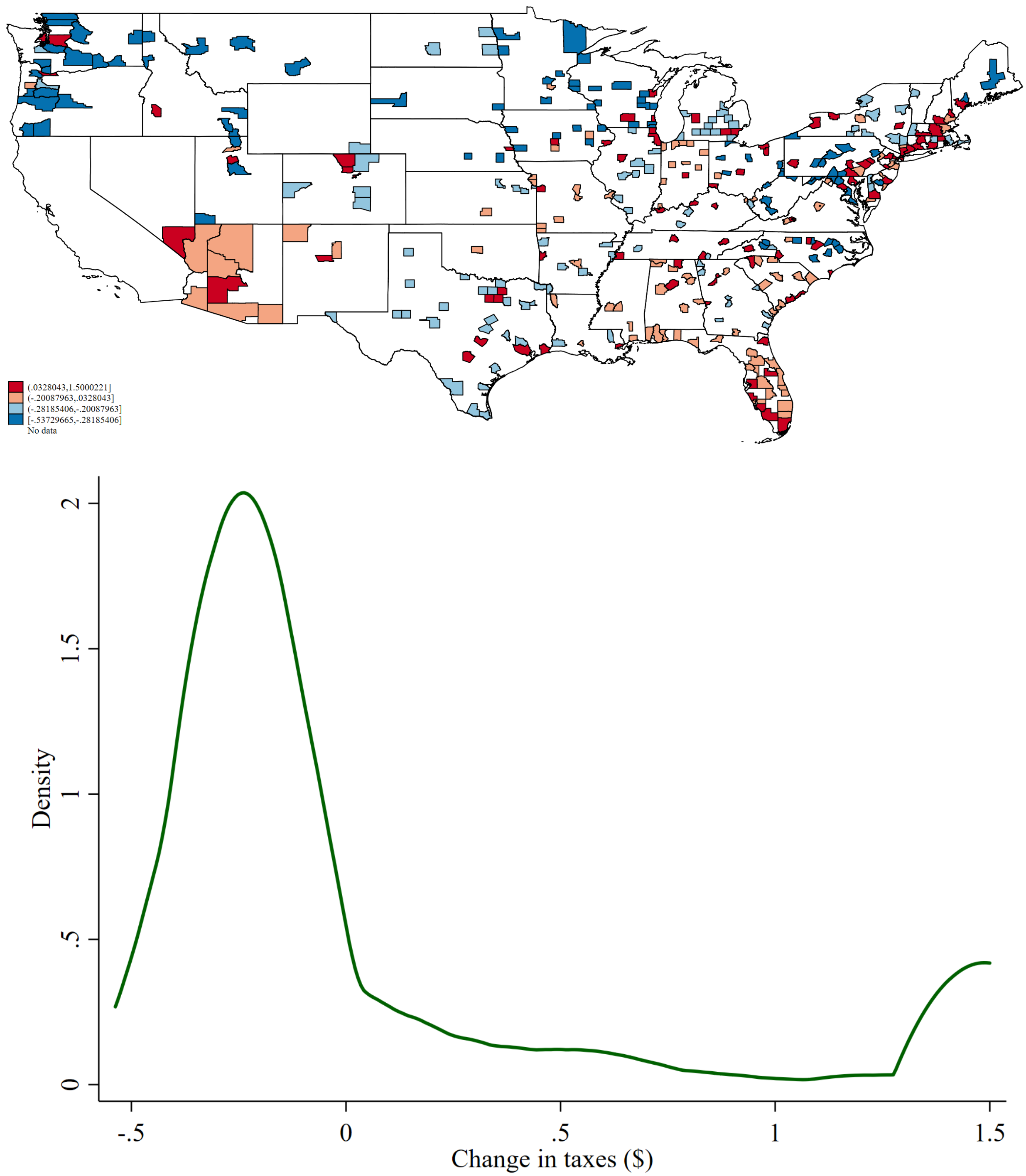


Figure A8: Distribution of optimal tax changes across 380 counties constraining increases $\leqslant$ \$1.50

Notes: Figure 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). The procedure for calculating optimal taxes is described in Appendix F.

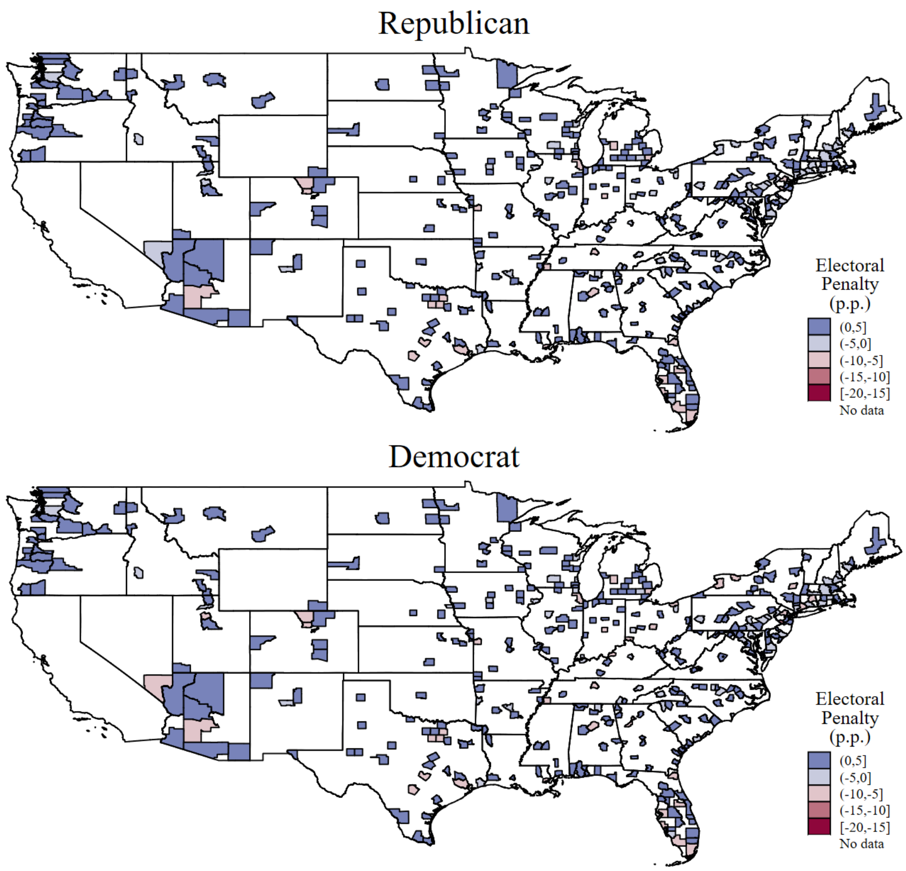


Figure A9: Implied Electoral penalties of county second-best optimal gas taxes

Notes: Figure plots maps of the implied electoral penalties from gas tax changes converting state-level gas taxes in 2019 to their second-best optimal county-level following Nehiba (2022). Negative values are penalties, positive values are electoral benefits. Calculation uses our main elector penalty results evaluated gas tax sample averages using equation (A8). More detail on calculation provided in Appendix F.

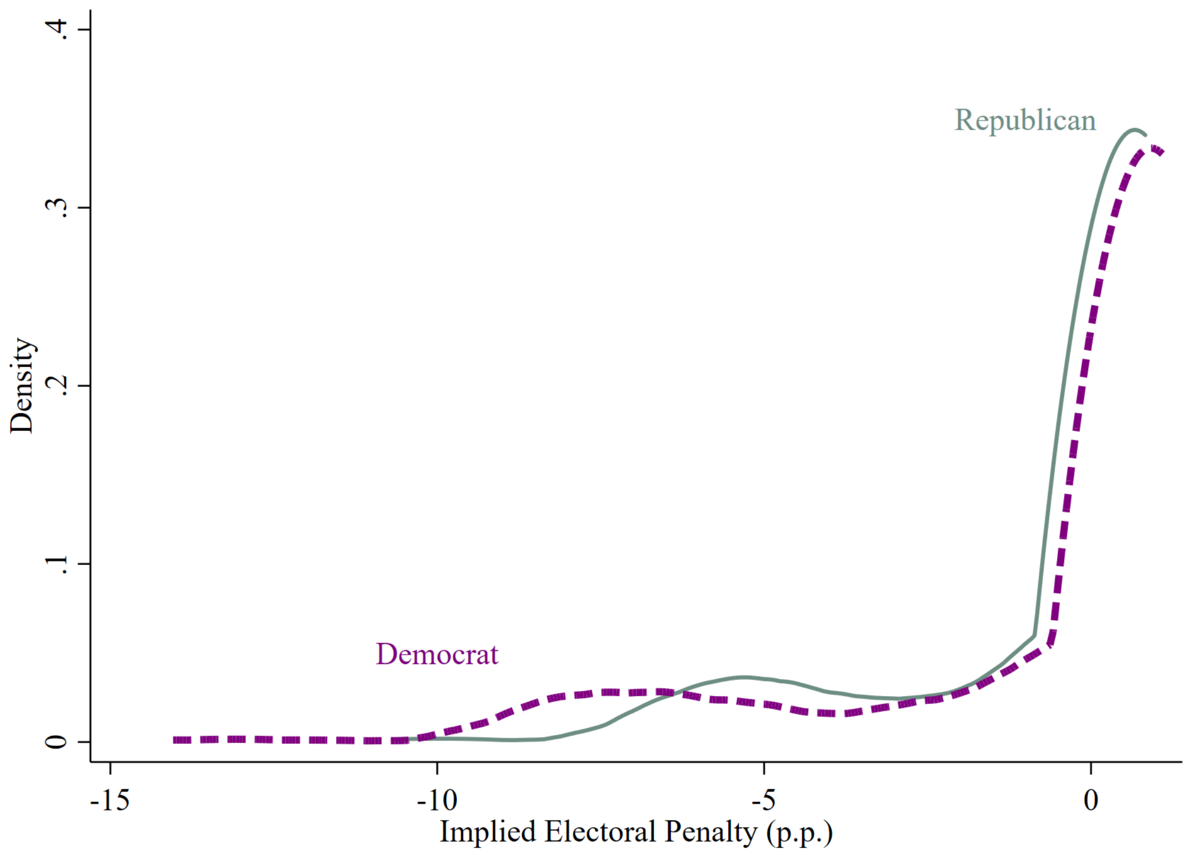


Figure A10: Distribution of implied electoral penalties of county second-best optimal gas taxes

Notes: Figure plots the distribution of the implied electoral penalties from gas tax changes converting state-level gas taxes in 2019 to their second-best optimal county-level following Nehiba (2022). Negative values are penalties, positive values are electoral benefits. Calculation uses our main elector penalty results evaluated gas tax sample averages using equation (A8). More detail on calculation provided in Appendix F.

Table A1: Factors that explain gas tax changes

	(1)	(2)	(3)
Democratic vote share	0.190	0.192	-0.031
	(0.125)	(0.146)	(0.106)
Log road mileage per capita	0.031	0.027	0.120
	(0.052)	(0.046)	(0.155)
Log licensed drivers per capita	0.417	0.533	0.168
	(0.351)	(0.328)	(0.240)
Log vehicle miles traveled per capita	-0.328*	-0.463**	0.064
	(0.166)	(0.183)	(0.281)
Log real personal income per capita	0.001	0.090	0.150
	(0.172)	(0.171)	(0.363)
Unemployment	-0.001	-0.010	0.025
	(0.011)	(0.016)	(0.018)
Log tax-exclusive gas price	-0.060	-1.142***	-1.289***
	(0.041)	(0.334)	(0.243)
Year FEs		X	X
State FEs			X
Observations	1,684	1,684	1,684

Notes: All specifications are linear probability models where the dependent variable is an indicator equal to one if the gasoline tax increased or decreased relative to the previous year, and zero otherwise. Clustered standard errors at the state level are in parentheses. Significance levels are denoted as: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A2: Election summary statistics

Panel (a). Election composition.

	Percentage of elections			
	No candidates	1 candidate	2 candidates	$\geqslant$ 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	83.77	51.15	0	203
Senate	37.62	14.28	1	67
Elections spacing				
House	2.05	0.31	1	4
Senate	3.11	1	1	5

Notes: Panel (a) shows the percentage of elections in which no candidates, one candidate, two candidates, and three or more candidates run for a seat over the sample period 1982–2016. Panel (b) shows the summary statistics for the number of districts and the number of years between elections for the State House of Representatives and Senate over the sample period 1982–2016.

Table A3: 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. Panel (a) shows year-over-year statistics for our 1982-2016 sample, where each observation is a state-year. The first row in each panel corresponds to summary statistics

Table A4: Coverage and representativeness of various bandwidths

	Bandwidth size						
	.5 pp	1 pp	2 pp	5 pp	10 pp	20 pp	100 pp
Panel A: Election level							
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
Panel B: State level							
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						
	.5 pp	1 pp	2 pp	5 pp	10 pp	20 pp	100 pp
Panel A: Election level							
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
Panel B: State level							
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. Panels A and B are constructed using bandwidth for elections with Republican incumbents and panels C and D for 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.

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)
Win Election ($\mathbf{1}\{M_{it} \geqslant 0\}$)	0.337***	0.339***	0.354***	0.348***
	(0.026)	(0.026)	(0.027)	(0.025)
Gas Tax (G_{it})	-0.010	-0.011	-0.013	-0.017
	(0.027)	(0.026)	(0.026)	(0.024)
Win Election × Gas Tax	-0.029	-0.026	-0.030	-0.025
	(0.020)	(0.020)	(0.019)	(0.020)
Year FEs			X	X
State FEs			X	X
Covariates			X	X
Right Bandwidth	0.098	0.126	0.095	0.126
Left Bandwidth	0.104	0.138	0.093	0.123
Kernel	Uniform	Triangular	Uniform	Triangular
Observations	8,239	10,697	7,708	10,137
Panel (b). Democratic party	**(1)**	**(2)**	**(3)**	**(4)**
Win Election ($\mathbf{1}\{M_{it} \geqslant 0\}$)	0.340***	0.344***	0.361***	0.357***
	(0.027)	(0.027)	(0.027)	(0.026)
Gas Tax (G_{it})	0.025	0.024	0.029	0.031
	(0.020)	(0.021)	(0.019)	(0.019)
Win Election × Gas Tax	-0.039**	-0.035*	-0.042**	-0.038*
	(0.018)	(0.019)	(0.018)	(0.019)
Year FEs			X	X
State FEs			X	X
Covariates			X	X
Right Bandwidth	0.095	0.128	0.087	0.117
Left Bandwidth	0.102	0.129	0.098	0.125
Kernel	Uniform	Triangular	Uniform	Triangular
Observations	8,058	10,473	7,500	9,780

Notes: The table presents estimates from 8 regressions where the dependent variable is an indicator function for whether a Republican or Democratic party won the election in year $t+1$. The running variable is the margin of victory in election year t. Win Election is an indicator for a positive margin of victory. Gas Tax is an indicator for observations with a gas tax increase before the $t+1$ election and since the t election. 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 travelled 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 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, number of incumbents running, party's tenure in office, and the normal party vote share. Bandwidths are selected optimally using two-sided MSE, following Calonico et al. (2014). Clustered standard errors at the state level are in parentheses. Significance levels are denoted as: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A6: 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	0.957
Democratic	No	0.082	0.952
Democratic	Yes	0.093	0.945

Notes: This table shows the results of the McCrary (2008) test for manipulation of the treatment status around the discontinuity. We report in column (2) the difference in the log of the density of the outcome to the right, $\log(\hat{f}^{+})$, and the left, $\log(\hat{f}^{+})$, of the discontinuity at $M = 0$. Column (3) reports the p-value for the density test for whether this difference is statistically different from zero.

Table A7: 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 presents estimates from 10 regressions where the dependent variables are determined before the electoral outcome of election year t is realized. Vote Share is the proportion of votes the party obtained in the election year $t-1$. Party win is an indicator for whether the party won the race Tenure is the number of years the party has been in the seat in the election year $t-1$. Incumbents is the number of sitting members running for reelection in the election year $t-1$. Candidates is the number of candidates running in the election year $t-1$. The coefficient estimate displayed is associated with the variable Win Election, which is the indicator for a positive margin of victory. That is, the Win Election coefficient represents the jump at the discontinuity. 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 travelled 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 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, number of incumbents running, party's tenure in office, and the normal party vote share. Bandwidths are selected optimally using two-sided MSE, following Calonico et al. (2014). All regressions are estimated using a triangular kernel. Clustered standard errors at the state level are in parentheses. Significance levels are denoted as: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A8: 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 ($\mathbf{1}\{M_{it} \geqslant 0\}$)	0.088***	0.088***	0.093***	0.089***
	(0.007)	(0.007)	(0.007)	(0.007)
Gas Tax Decrease (GD_{it})	0.002	-0.006	-0.006	-0.020
	(0.007)	(0.011)	(0.009)	(0.018)
Win Election × Gas Tax Decrease	0.000	0.003	0.003	0.003
	(0.015)	(0.031)	(0.014)	(0.030)
Year FEs			X	X
State FEs			X	X
Covariates			X	X
Decreases include indexed changes	X		X	
Right Bandwidth	0.191	0.191	0.179	0.179
Left Bandwidth	0.172	0.172	0.160	0.160
Kernel	Triangular	Triangular	Triangular	Triangular
Observations	9,700	9,700	8,969	8,969
Panel (b). Democratic party	(1)	(2)	(3)	(4)
Win Election ($\mathbf{1}\{M_{it} \geqslant 0\}$)	0.082***	0.080***	0.089***	0.085***
	(0.008)	(0.008)	(0.008)	(0.008)
Gas Tax Decrease (GD_{it})	0.005	0.009	0.017	0.003
	(0.009)	(0.020)	(0.012)	(0.021)
Win Election × Gas Tax Decrease	-0.032***	-0.019	-0.034**	-0.010
	(0.010)	(0.019)	(0.014)	(0.017)
Year FEs			X	X
State FEs			X	X
Covariates			X	X
Decreases include indexed changes	X		X	
Right Bandwidth	0.143	0.143	0.135	0.135
Left Bandwidth	0.138	0.138	0.130	0.130
Kernel	Triangular	Triangular	Triangular	Triangular
Observations	7,497	7,497	7,003	7,003

Notes: The table presents estimates from 8 regressions where the dependent variable is the vote share for the indicated party during election year $t+1$. Gas Tax Decrease is equal to 1 if a state's gas tax decreases between election t and $t+1$ and is zero otherwise. Columns (1) and (3) include indexed gas tax decreases in Gas Tax Decrease, while columns (2) and (4) set this variable to zero for indexed decreases. The running variable is the margin of victory in election year t. Win Election is an indicator for a positive margin of victory. Gas Tax is an indicator for observations with a gas tax increase before the $t+1$ election and since the t election. 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 travelled 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 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, number of incumbents running, party's tenure in office, and the normal party vote share. Bandwidths are selected optimally using two-sided MSE, following Calonico et al. (2014). Clustered standard errors at the state level are in parentheses. Significance levels are denoted as: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A9: Diff-in-Disc estimates of electoral incumbent penalty: vote share in election t+1 (with state-by-year fixed effects.)

Panel (a). Republican party	(1)	(2)	(3)	(4)
Win Election ($\mathbf{1}\{M_{it} \geqslant 0\}$)	0.089***	0.090***	0.089***	0.093***
	(0.006)	(0.006)	(0.006)	(0.006)
Gas Tax (G_{it})	-0.002	-0.003	0.007	-0.004
	(0.005)	(0.005)	(0.014)	(0.013)
Win Election × Gas Tax	-0.015**	-0.013*	-0.013*	-0.014**
	(0.008)	(0.007)	(0.007)	(0.006)
Year FEs			X	X
State FEs			X	X
State-Year FEs			X	X
Covariates			X	X
Right Bandwidth	0.126	0.164	0.128	0.176
Left Bandwidth	0.125	0.174	0.112	0.149
Kernel	Uniform	Triangular	Uniform	Triangular
Observations	10,245	13,560	9,768	13,182

Panel (b). Democratic party	(1)	(2)	(3)	(4)
Win Election ($\mathbf{1}\{M_{it} \geqslant 0\}$)	0.086***	0.084***	0.088***	0.089***
	(0.009)	(0.008)	(0.008)	(0.008)
Gas Tax (G_{it})	0.007	0.006	0.004	0.003
	(0.005)	(0.005)	(0.017)	(0.017)
Win Election × Gas Tax	-0.021***	-0.017**	-0.017*	-0.016**
	(0.008)	(0.007)	(0.009)	(0.008)
Year FEs			X	X
State FEs			X	X
State-Year FEs			X	X
Covariates			X	X
Right Bandwidth	0.125	0.151	0.118	0.137
Left Bandwidth	0.109	0.142	0.103	0.142
Kernel	Uniform	Triangular	Uniform	Triangular
Observations	9,561	11,840	8,964	11,234

Notes: The table presents estimates from 8 regressions where the dependent variable is vote share for the indicated party during election year $t+1$, V_{t+1} as indicated from Equation (1). The running variable is the margin of victory in election year t, M_{it}. Win Election is an indicator for a positive margin of victory in election t, $\mathbf{1}\{M_{it} \geqslant 0\}$. Gas Tax ($G_{it}$) is an indicator for observations with a gas tax increase between elections t and $t+1$. We exclude observations for which the gas tax decreased. 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 travelled 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 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, number of incumbents running, party's tenure in office, and the normal party vote share. Bandwidths are selected optimally using two-sided MSE, following Calonico et al. (2014). Clustered standard errors at the state level are in parentheses. Significance levels are denoted as: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A10: 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 ($\mathbf{1}\{M_{it} \geqslant 0\}$)	0.091***	0.085***	0.082**	0.078**
	(0.026)	(0.026)	(0.032)	(0.033)
Gas Tax (G_{it})	0.011	0.005	0.025	0.027
	(0.019)	(0.017)	(0.026)	(0.027)
Win Election × Gas Tax	-0.024	-0.010	-0.007	-0.010
	(0.030)	(0.027)	(0.029)	(0.030)
Year FEs			X	X
State FEs			X	X
Covariates			X	X
Right Bandwidth	0.122	0.095	0.120	0.089
Left Bandwidth	0.121	0.125	0.126	0.114
Kernel	Triangular	Uniform	Triangular	Uniform
Observations	222	202	216	189
Panel (b). Democratic party	**(1)**	**(2)**	**(3)**	**(4)**
Win Election ($\mathbf{1}\{M_{it} \geqslant 0\}$)	0.158***	0.131***	0.106***	0.103***
	(0.037)	(0.030)	(0.036)	(0.037)
Gas Tax (G_{it})	-0.011	0.000	-0.064*	-0.069*
	(0.026)	(0.021)	(0.038)	(0.036)
Win Election × Gas Tax	0.004	-0.007	0.041	0.045
	(0.037)	(0.031)	(0.042)	(0.040)
Year FEs			X	X
State FEs			X	X
Covariates			X	X
Right Bandwidth	0.127	0.133	0.137	0.106
Left Bandwidth	0.112	0.108	0.108	0.091
Kernel	Triangular	Uniform	Triangular	Uniform
Observations	214	214	210	185

Notes: The table presents estimates from 8 regressions where the dependent variable is the vote share for the indicated party during election year $t+1$. The running variable is the margin of victory in election year t. Win Election is an indicator for a positive margin of victory. Gas Tax is an indicator for observations with a gas tax increase before the $t+1$ election and since the t election. 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. Significance levels are denoted as: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A11: Diff-in-Disc estimates of electoral incumbent penalty: Vote share in election t+1 for different election samples

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
Win Election ($\mathbf{1}\{M_{it} \geqslant 0\}$)	0.092***	0.091***	0.094***	0.108***	0.074***	0.082***	0.078***	0.081***	0.096***	0.107***	0.077***	0.090***
	(0.006)	(0.006)	(0.006)	(0.008)	(0.007)	(0.008)	(0.008)	(0.008)	(0.006)	(0.009)	(0.007)	(0.006)
Gas Tax (G_{it})	-0.004	-0.004	-0.004	0.005	-0.003	-0.009	-0.008	-0.012*	-0.004	0.008	-0.013	-0.007
	(0.006)	(0.005)	(0.006)	(0.007)	(0.008)	(0.008)	(0.008)	(0.006)	(0.006)	(0.006)	(0.009)	(0.007)
Win Election × Gas Tax	-0.013*	-0.013*	-0.008	-0.017*	-0.015*	-0.010	-0.012	-0.002	-0.015*	-0.017*	-0.007	-0.012
	(0.007)	(0.007)	(0.008)	(0.009)	(0.009)	(0.010)	(0.011)	(0.010)	(0.008)	(0.010)	(0.009)	(0.007)
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
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
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
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
Win Election ($\mathbf{1}\{M_{it} \geqslant 0\}$)	0.089***	0.087***	0.092***	0.108***	0.064***	0.089***	0.073***	0.077***	0.090***	0.106***	0.068***	0.088***
	(0.008)	(0.007)	(0.009)	(0.010)	(0.009)	(0.009)	(0.010)	(0.010)	(0.008)	(0.009)	(0.008)	(0.008)
Gas Tax (G_{it})	0.008	0.008	0.005	0.009	0.013*	0.005	0.013	0.013	0.009	0.005	0.006	0.007
	(0.005)	(0.005)	(0.006)	(0.008)	(0.007)	(0.007)	(0.010)	(0.009)	(0.006)	(0.008)	(0.007)	(0.006)
Win Election × Gas Tax	-0.019***	-0.016**	-0.019**	-0.026***	-0.012	-0.018**	-0.025**	-0.011	-0.021***	-0.031***	-0.003	-0.017**
	(0.007)	(0.007)	(0.008)	(0.010)	(0.010)	(0.007)	(0.012)	(0.012)	(0.008)	(0.010)	(0.010)	(0.007)
Right Bandwidth	0.137	0.138	0.135	0.179	0.128	0.143	0.146	0.147	0.151	0.164	0.126	0.127
Left Bandwidth	0.142	0.139	0.145	0.151	0.157	0.143	0.165	0.151	0.153	0.159	0.154	0.147
Observations	11,234	11,740	8,775	7,556	4,945	5,593	5,154	5,436	10,358	6,335	5,740	9,521

Notes: The table presents estimates from 24 regressions of our preferred specification (which uses a Triangular kernel, and includes covariates, state, year, and state-by-year fixed effects) using different election samples. The dependent variable is the vote share for the indicated party during election year $t+1$. The running variable is the margin of victory in election year t. Win Election is an indicator for a positive margin of victory. Gas Tax is an indicator for observations with a gas tax increase before the $t+1$ election and since the t election. 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 travelled 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 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, number of incumbents running, party's tenure in office, and the normal party vote share. Bandwidths are selected optimally using two-sided MSE, following Calonico et al. (2014). Clustered standard errors at the state level are in parentheses. Significance levels are denoted as: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A12: Electoral penalties for states with VMT tax pilots or policies

State	Quintile Penalty Democ.	Penalty Democ. (p.p.)	Quintile Penalty Repub.	Penalty Repub. (p.p.)
NJ	1	-22.5	1	-17.1
CA	2	-12.7	2	-9.7
MN	3	-11.3	3	-8.6
WA	4	-9.7	4	-7.4
DE	4	-9.7	4	-7.4
CO	4	-10.2	4	-7.8
NC	4	-10.6	4	-8.1
CT	5	-6.7	5	-5.1
PA	5	-8.5	5	-6.5
NV	5	-8.9	5	-6.8
State	Quintile Penalty Democ.	Penalty Democ. (p.p.)	Quintile Penalty Repub.	Penalty Repub. (p.p.)
HI	1	-14.4	1	-11.0
VA	2	-13.1	2	-10.0
OR	4	-9.3	4	-7.0
UT	5	-9.0	5	-6.9

Notes: The table shows the implied electoral penalties of optimal VMT taxes applying the penalty elasticity from equation (A8) to data from 2000. Data on VMT-Tax implementation comes from https://enotrans.org/article/the-current-status-of-state-vmt-fees/. Panel A shows the penalties implied for Democratic and Republic candidates. Quintiles for penalties based on penalties for all states for 2000.