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

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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

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Perhaps the greatest challenge to addressing climate change comprehensively through government policy in the United States has been the perception of limited political feasibility. Nevertheless, there is limited empirical evidence for this perception. Indeed, polling suggests that a narrow but growing plurality of Americans support increasing taxes on fossil fuels to address climate change (Fitzpatrick et al., 2018).

The longest-running U.S. tax on fossil fuels, the gas tax, has neither kept pace with transportation expenditures, inflation nor population growth (Peterson, 2021) and the federal motor fuels tax has not been changed in over three decades. While gas taxes are primarily used to pay for roads, not to address climate change, they have long been considered a climate policy tool by economists and policymakers. This is because, in practice, taxing carbon means taxing the consumption of energy products like gasoline, and gasoline demand is relatively elastic in the long run (Fullerton and West, 2002). If policymakers in the U.S. intend to meet decarbonization goals, leveraging existing fiscal instruments will be critical (Rapson and Muehlegger, 2023) and debates about political feasibility ought to be better informed by actual voter behavior.

In this paper, we explore this question using a comprehensive dataset on state legislative election outcomes and gasoline taxes. States are an ideal setting to study the electoral penalties associated with motor fuel taxation. In the U.S. federalist system, states have substantial responsibility to raise revenues for and coordinate construction of transportation infrastructure. The considerable variation in gas tax changes across the 50 states also provides a source of identifying variation arising from various political, economic, procedural and legal causes that we speak to in discussing our empirical approach.

We examine the effect of legislated gasoline tax changes on incumbent state legislator electoral outcomes, specifically the advantage of incumbents in subsequent elections. We apply a difference-in-discontinuities ("Diff-in-Disc") approach, comparing the incumbent advantage from close elections in states where gasoline taxes were increased to those where it was not. This methodology has been widely applied in the public economics literature (Grembi et al., 2016; Cellini et al., 2010; Asher and Novosad, 2017). We demonstrate analytically that this estimator, under testable assumptions, recovers the average treatment effect on the treated (ATT).

We provide a series of empirical tests for whether state-by-year unobserved economic or political

factors associated with gas tax changes are likely to bias our estimates under standard selection-on-observables assumptions. Consistent with prior work (Marion and Muehlegger, 2011; Li et al., 2014), we show that gas tax changes are not correlated with confounding contemporary variation in political or economic factors but rather longer-term structural factors influencing infrastructure financing needs. Our headline results suggest that gas tax increase reduce incumbent advantages by 14.1% for Republicans and 21.3% for Democrats. Since political rhetoric in the U.S. typically labels the Democratic party as "tax and spend," the larger penalty for Democrats is consistent with a preference among voters for policy stability as distinct from responsiveness following Loeper and Dziuda (2024).

We contribute to three areas of the existing literature. 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 (West and Williams III, 2007; Parry and Small, 2005), the inability to achieve these higher taxes has been attributed to distributional concerns common to many forms of environmental taxation (Sallee, 2019). Yet it remains to be determined: how badly are politicians punished for raising gasoline taxes? Two, we contribute to work on close elections and public finance. 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). Third, this paper contributes to a broad literature on the political economy of taxation, whose summary is beyond the scope of this paper, but for a review see Kiser and Karceski (2017). Despite a spate of research on gasoline taxes more broadly, no prior work has tied state-by-state variation in gas taxes to political outcomes.

1 Data & stylized facts

We leverage comprehensive state legislative electoral data with state gasoline tax changes for all state legislative districts (SLDs) over 1982–2016, complemented with state transportation, demographic, and electoral data (details in Appendix A). 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). We combine these data with gasoline tax rates at the state

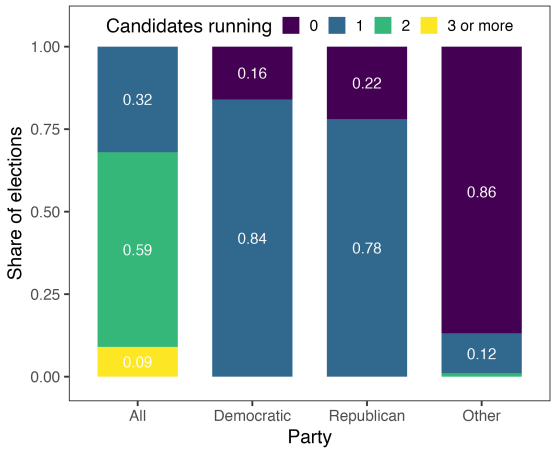
and year level from the Highway Statistics Series at the Federal Highway Administration. Additional covariates include state road mileage, licensed drivers, and vehicle miles traveled from Li et al. (2014) and the Bureau of Transportation Statistics, state personal income from the Bureau of Economic Analysis, state population and unemployment rate from FRED, as well as the US President's party and the year of presidential elections from MIT EDS Lab (2017). We also include as controls before-tax gasoline prices, which come from the Energy Information Administration, and gasoline tax indexing, as described in Appendix A.

Panel (a) of Figure 1 shows the share of elections in SLDs by the number of candidates from each party. We focus on single-member districts which are approximately 90% of all districts. Multi-member districts existed in 17 states during our sample period, with fewer over time. From 2013 to 2016, only 10 states had a multi-member district. Election terms are either 2 or 4 years as shown in Appendix Table A1.

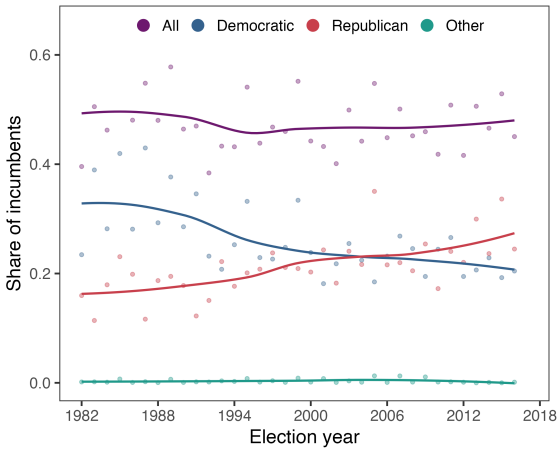
Throughout our sample, 32% of the elections are uncontested, and 59% have two candidates running. Democratic and Republican parties field one candidate in 84% and 78% of the elections, respectively. In contrast, only 14% of the elections have one or more candidates outside one of the major parties. Panel (b) of Figure 1 shows that the total incumbent share in single-member districts has slightly increased and currently represents around 45% of all candidates. This share is less than 50% due to Independents and third-party candidates. The Democratic incumbent share has been declining since the end of the 1980s. The share of incumbents from other parties is close to zero.

Figure 1, Panel (c) illustrates that while the average state gasoline tax grew by only 15.1 cents over the sample period, there is substantial heterogeneity in gasoline tax increases across states. In some states–Pennsylvania, Washington, and New Jersey–gasoline tax increases exceeded 29 cents per gallon as seen in Panel (d) of Figure 1. Virginia, South Carolina, and Alaska underwent increases smaller than 6 cents per gallon. 28% of state-years see no change in gasoline taxes, with an average increase of half a cent. We observe 367 gas tax increases and 60 gas tax decreases in our sample, reflected in Appendix Figure A1.

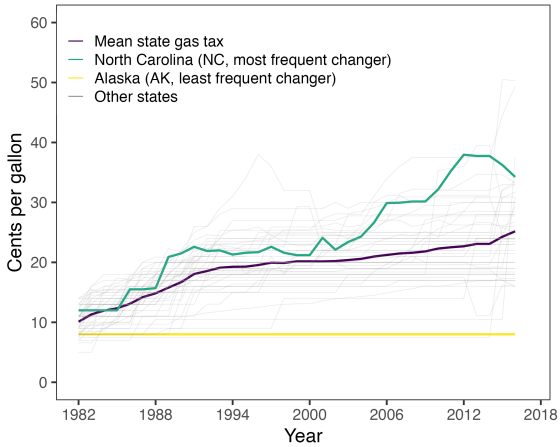
(a) Share of elections by party's number of candidates



(b) Share of incumbents over time



(c) Nominal state gasoline tax over time



(d) Net gas tax change by state over 1982–2016

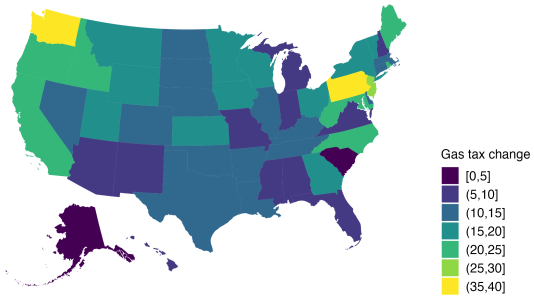


Figure 1: Sample summary statistics

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.

2 Econometric methodology

Our difference-in-discontinuities (Diff-in-Disc) approach builds on past work (Lee, 2008) that shows how the incumbency advantage can be identified through regression discontinuity design (RDD). This is done by comparing electoral performance in the *next* election between near winners to near losers of relatively close elections. In our case, 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. Subsequent studies have shown that this orthogonality holds in almost all electoral cases, including state legislative elections (Eggers et al., 2015).

We combine this insight with a difference-in-differences approach, formalized in Grembi et al. (2016), to compare the extent of the incumbency advantage between districts in states where gas taxes have been increased to those where it has not. The resulting difference-in-discontinuities 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. In contrast, a standard diff-in-diff estimation is less straightforward in this setting, where SLDs may be both treated and control based on multiple tax changes over the course of our sample.

Figure 2 illustrates the intuition for our estimation. 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 1. 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 3.1, 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 and 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.

Since voters are likely more aware of the policy legacy of a given party in state government than of a particular legislator, we follow Lee (2008) and estimate the electoral penalty by party (pooled across upper and lower legislative houses) rather than individual candidates. This mitigates selection bias that would come from attrition when candidates change districts or drop out/enter the panel. We include elections with Independent or third-party candidates and winners, but do not estimate their party's electoral penalty given how few observations they represent.

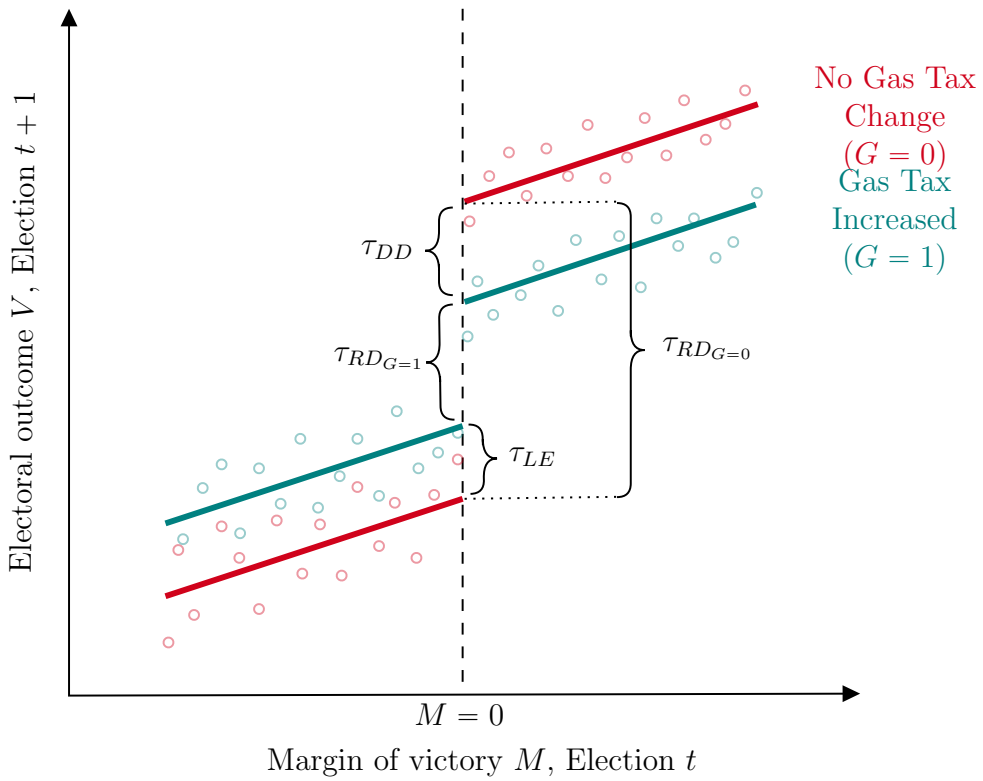


Figure 2: Graphical representation of difference-in-discontinuities design for electoral incumbent penality

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.

2.1 Estimation

Our Diff-in-Disc estimator is estimated separately for each party as:

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

M_{it} is the margin of victory for a given political party in SLD i and election t. G_{it} is an indicator for a gas tax increase in district i's state between election t and $t+1$. For ease of interpretation, we exclude the small number of observations for which the gas tax *decreases*, but re-examine them in Section 3.1. $\boldsymbol{X}$ is a matrix of state and district-level controls discussed in Section 1. 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. In principle, electoral penalties scale with the magnitude of a gas tax increase, but it is less clear that continuous effects are identifiable in this context. Instead, in Section 3.1, we show how effects vary when $G_{it} = 1$ only for larger tax increases.

As Figure 2 makes clear, we need to account for the effect of gas tax increases on losers of close elections, reflected by τ_{LE} and estimated by β_2 in Equation (1). β_3 corresponds to $\tau_{RD_{G=0}}$ in Figure 2. We do not include interactions of G_{it} with M_{it} nor the triple interaction of G_{it}, M_{it}, and $\mathbf{1}\{M_{it} \geqslant 0\}$. 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).

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). To evaluate how the bandwidth choice affects the sample, we present summary statistics for different bandwidth sizes in Appendix Table A3. We present results with both uniform and triangular

kernels, with our preferred specification utilizing the latter following Cattaneo and Titiunik (2022).

2.2 Identification

The exogeneity of gasoline tax increases is a concern if time-variant unobserved variables are correlated with gas tax changes *and* state legislative vote shares from one election to the next. The source of identifying variation from Equation (1) is differences in gas tax changes between states over time interacted with as-good-as-random variation in the incumbent vote share for an election in period t around the discontinuity at $M_{it} = 0$. If there are state-level trends in political or economic conditions that are correlated with the gas tax and voting patterns, this could confound our estimates of β_4 from equation (1). An established literature in empirical public finance has shown that these changes are unlikely to be correlated with *contemporary* state-level political and economic trends (Li et al., 2014; Marion and Muehlegger, 2011). In Appendix Table A4 we regress G_{it}, the indicator variable for a change in the gas tax on a variety of economic and political variables, including Gross State Product per capita, unemployment, urbanization and educational attainment, League of Conservation Voters scores for federal legislative representatives, the governor's political party, indicators for Democratic control of each legislative house, and the state's budget surplus as a fraction of revenues. We find that these contemporaneous economic and political variables explain practically none of the state-by-year variation in gasoline tax rates. The only variables that do influence these are pre-tax gasoline prices and gas tax indexing, which are controls in our main regressions.

We also observe multiple electoral outcomes in the same state for a given election year, so in Section 3.1, we are able to estimate Equation (1) using *state-by-year* fixed effects, which account for time-variant economic or political factors between states. We find no meaningful differences in the results from our main results. In Appendix Table A5, we estimate a modified version of Equation (1), where we recover the average state-level effect of winning a close election on *subsequent* gas tax changes. We find no effect, pointing to the fact that gas tax increases are unlikely to be driven by the source of variation we rely on in our main results: close elections. We believe these three empirical tests demonstrate that state-by-year unobservables associated with gas tax changes are unlikely to undermine the consistency of our estimates. A last question is whether gas tax increases

are correlated with other policies that voters may care about which bias our results. In Appendix Figure A5, we show that gas tax increases correlate with Google Trends data for their search but not searches for other policies. This suggests that other policies may not be a confounding effect on our estimates.

This lack of correlation is to be expected. There is a substantial time lag (in most cases several years) between when deliberations on tax changes occur and their enactment, which is what we actually use in Equation (1). Also, legislative changes in gas taxes typically reflect longer term structural infrastructure financing needs so that contemporaneous fluctuations in economic and political conditions are not highly influential in their passage (Sciara et al., 2024; Waxman and Sciara, 2024).

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 B. The consistency of this estimator is based on the following three assumptions.

First, the conditional mean of the running variable, M_{it}, is continuous 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 test this assumption through standard approaches described by McCrary (2008): there is no evidence of manipulation around the margin of victory threshold as shown in Section 3.1.

Second, in the neighborhood of the margin of victory threshold, treated and untreated districts are on parallel trends in the absence of a gasoline tax increase–see Grembi et al., 2016 for a formal description. A test of 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 3.1. 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.

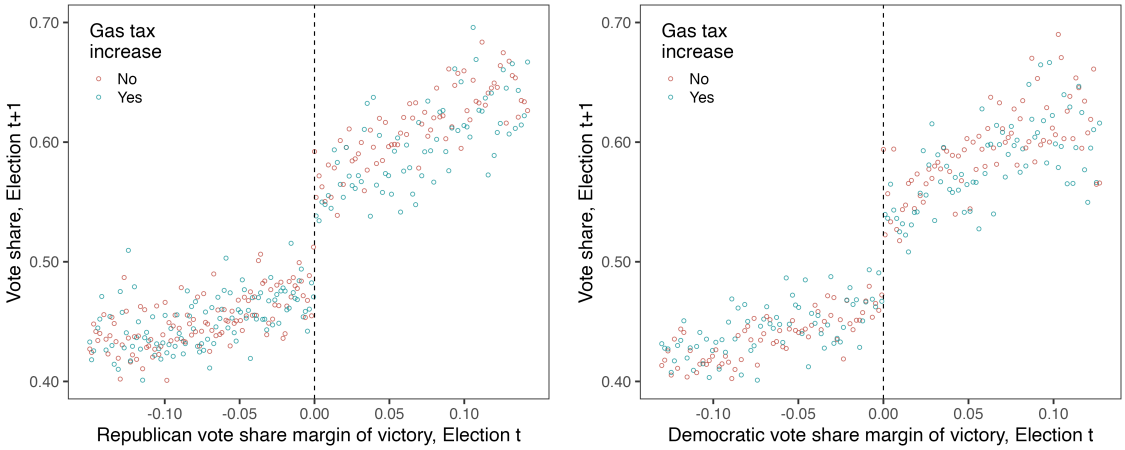
Third, the electoral penalty of gas tax increases on losers of close elections is controlled for in our regression estimates from Equation (1). This assumption would be violated if this effect were non-zero and we were unable to credibly control for it. We are able to control for it and find these effects, from estimates of β_2, are statistically indistinguishable from zero.

3 Results

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, following Korting et al. (2023) and Cattaneo et al. (2019). Panels (a) and (b) show the incumbency advantage 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.

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 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 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)).

(a) RD incumbency advantage: Republicans (b) RD incumbency advantage: Democrats



(c) Diff-in-Disc gas tax penalty: Republicans (d) Diff-in-Disc gas tax penalty: Democrats

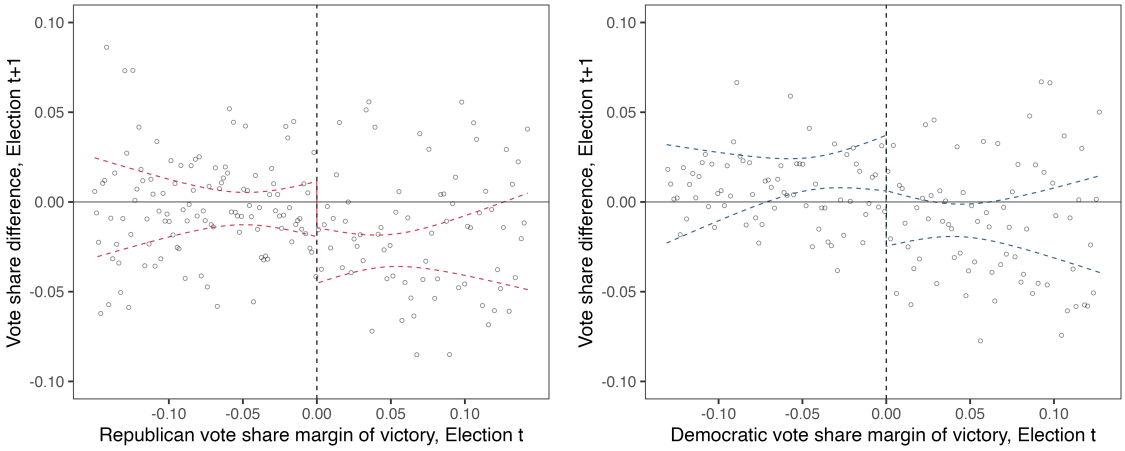


Figure 3: Regression Discontinuity and Difference-in-Discontinuities unconditional plots for vote share

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).

We provide central Diff-in-Disc regression estimation results of Equation (1) for Republican and Democratic parties in Table 1. We note first that Republican and Democrat samples overlap, but partially so that third party candidates or races where candidates run unopposed mean the results for the two parties are not necessarily symmetric, which bears out in the results. 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. This provides a test for whether state-by-year unobservables might bias our results and we find that the coefficient estimates of interest are largely unchanged. We revert to our baseline model for our main analysis, because inclusion of state-by-year fixed effects precludes some of the robustness checks we conduct in Section 3.1, which leverages year-over-year variation across states.

These estimates provide compelling evidence that incumbent politicians are penalized for gas tax increases. 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 suggests that voters tend to punish sitting representatives for passing a gasoline tax increase. While differences in the electoral penalty are not statistically different between parties, our results are consistent with the perception that Democrats may be more vulnerable, on average, to voters for gas tax increases. These results speak to the broader public finance literature on the policy accountability of U.S. political parties (Besley and Case, 1992).

Without a gas tax increase, the incumbency advantage hovers between 8-9 percentage points. 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

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.

in the gas tax for Republicans and 0.8 for Democrats at the average and assuming a constant relationship. A discussion of the policy implications follows in Section 4.

In Appendix Table A6, we examine the impact of gas tax *decreases*. We find a statistically significant effect on the electoral penalty only when indexed decreases are included. As mentioned in Section 1, 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.

3.1 Validity & robustness checks

In Panels (a) and (b) of Appendix Figure A2, we test our first identifying assumption about continuity. 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 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 A7 shows that it is not possible to reject the null hypothesis of no manipulation of the density at the margin of victory threshold. There is some variation in the intercept for Democratic candidates, but these are not statistically significant. Panels (c) and (d) of Appendix Figure A2 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.

In Panel (a) of Appendix Figure A3, we follow Cattaneo et al. (2019) to test for whether there is a discontinuity for predetermined variables of districts in election t (β_3 in Equation (1)). 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.

In Panels (a) and (b) of Appendix Figure A4, we test for pre-trends in our main results 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. 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. Coefficients 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}.[1] 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.

Lastly, we explore the robustness of our results 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 (Barreca et al., 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 the lower house of

[1] 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 $t+1$.

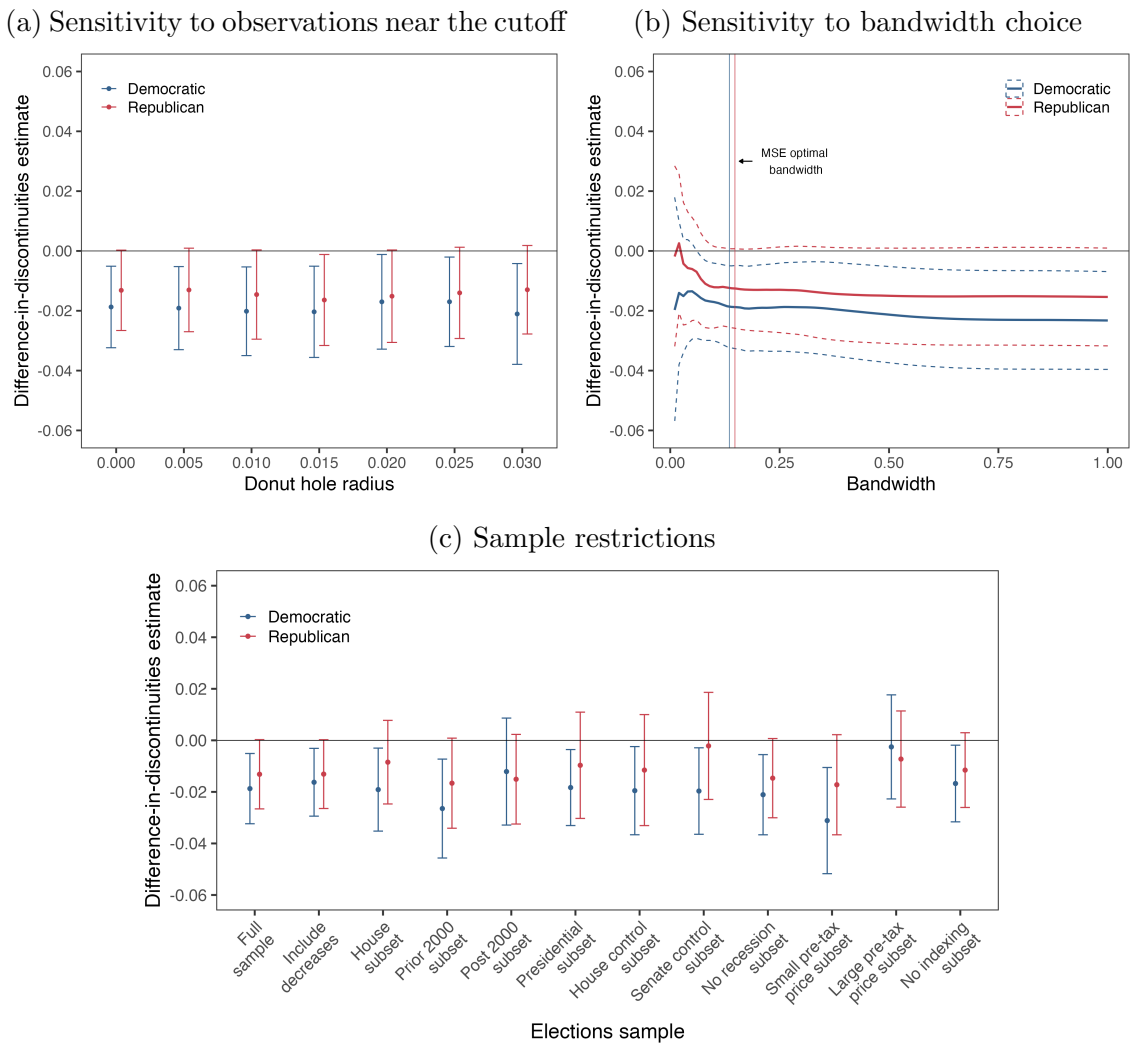


Figure 4: Robustness checks

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.

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 affect 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 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.

4 Discussion

How large might these penalties be if gas taxes were raised closer to the socially optimal level? One would ideally recover marginal external damages and infrastructure funding gaps from driving by state, which is beyond the scope of this study. Instead, we perform two bounding back-of-the-envelope exercises (full details and scope of the analysis in Appendix D).

First, we examine what setting gas taxes at an optimal *national* level would do, following Parry and Small (2005). 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 A2. 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 and the following back-of-the-envelope calculations, 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 penalties.

Second, second-best optimal gas taxes would also ideally vary in space, since negative externalities from driving are strongly correlated with population density. Nehiba (2022) calculates county level driving externalities for 380 U.S. counties and demonstrates that for many rural counties, moving toward optimality would actually mean gas tax *decreases*. The U.S. 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 A6, we show the implied tax changes for second-best optimal county gas taxes, and the 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, for Republicans and Democrats, respectively. These back of the envelope exercises point to 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.

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Online Appendix for Paying at the Pump and the Ballot Box: Electoral Penalties of Gas Taxes

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Appendix A. Data appendix

State election data

We use Klarner (2018) dataset on state legislative elections from 1967-2016 comprising 378,345 observations. 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

We build a gasoline tax indexing dataset based on a variety of journal articles, national, or state technical reports. We start by identifying periods of time in different states where the gasoline tax did not change, for example, Alabama (between 1992 and 2018) andTexas (between 1991 and 2019).

For the period from 1982 to 1983, we follow Bowman and Mikesell (1983), who identified seven states with a variable (indexed) gas tax rate in those years (Indiana, Kentucky, Massachusetts, New Mexico, Ohio, Rhode Island, and Washington). Importantly, we realized that there were some states that have statutes allowing for variable rates, but the effective per-gallon rate has remained constant. For this reason, we created a new dummy variable to account for this behavior. For the period from 1984 to 1999, we follow Ang-Olson et al. (1999), who described the experience of states that experimented with gasoline tax automatic adjustments indexed to changes in gasoline price, consumer price index, or highway construction and maintenance costs. Towards the end of this period, Florida, Nebraska, North Carolina, and Wisconsin had gas taxes that varied automatically, and other states such as Indiana, Maryland, Michigan, New Mexico, Virginia, and Washington had a gas tax indexed, which was repealed. This finding is confirmed by Li et al. (2014), who affirm that in the late 1980s many variable gasoline tax rate states reverted to a fixed-tax rate. For the period from 2000 to 2010, we build our gas tax indexing dataset based on specific state reports. These reports are often used to compare historical fuel tax policies with other states and propose alternatives for transportation and highway financing. For the period from 2011 to 2019, we use data from the Institute of Taxation and Economic Policy, which gathers information on gasoline tax indexing in reports in 2011, 2014, 2015, 2016, 2017, and 2019. We complete the missing years (2012, 2013, and 2018) through conjectures in which we check, for each state, whether or not the gasoline tax indexing in the last report available is still in place in the immediate next report available. If so, we assume the indexing is not modified during the missing years. If not, we search for alternative sources such as the information on variable gas taxes by the National Conference of State

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

Additional Economic & Political Variables

In Appendix Table A4, we regress an indicator variable for gas tax changes in our sample on a range of additional economic and political variables. These include data from the U.S. Bureau of Economic Analysis on Gross State Product (GSP) per capita, the share of state population living within counties within a Metropolitan Statistical Area, the unemployment rate, manufacturing share and mining share of GSP. From the Census American Community Survey 1-year estimates, we include average family size, the fraction of adults graduating high school and the fraction with a bachelor's degree. From the Census of Governments, we calculate the share of the state budget surplus relative to revenues. From the League of Conservation Voters Scorecard, we include average scores of environmental voting records for legislators from each house. From the Bureau of Transportation Statistics, we include the number of public transit ridership, personal vehicles, and licensed drivers per capita. Lastly, the population share in an MSA with rail comes from Li et al. (2014) and is originally from the U.S. Statistical Abstract of the U.S.

Appendix B. Difference-in-discontinuities treatment effect

Taking the potential outcomes framework, if we denote $V_{it+1}(m, g)$ as the potential outcome in vote share for election $t+1$, where $\mathbf{1}\{M_{it} \geqslant 0\} \equiv m \in \{0, 1\}$ 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

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

In an ideal scenario, we would like to identify the outcome of interest $V_{it+1}(1,1) - V_{it+1}(1,0)$, which is the effect of 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:

$$\begin{aligned} \hat{\tau}_{RDD} &= V^- - V^+ \\ &= V(1,0)^- - V(0,0)^+ \\ &= \Big[V(1,0)^- - V(1,1)^-\Big] + \Big[V(1,1)^- - V(0,1)^-\Big] + \Big[V(0,1)^+ - V(0,0)^+\Big] \\ &= -\underbrace{\boldsymbol{E}\Big[V_{it}(1,1) - V_{it}(1,0) | \mathbf{1}\{M_{it} \geqslant 0\} = 0, G_{it} = 1\Big]}_{\text{Causal effect of interest}} + \underbrace{\boldsymbol{E}\Big[V_{it}(1,1) - V_{it}(0,1) | \mathbf{1}\{M_{it} \geqslant 0\} = 0, G_{it} = 1\Big]}_{\text{Incumbent advantage where gas tax increased}} \\ &\quad + \underbrace{\boldsymbol{E}\Big[V_{it}(0,1) - V_{it}(0,0) | \mathbf{1}\{M_{it} \geqslant 0\} = 0, G_{it} = 1\Big]}_{\text{Effect of gas tax on party that lost last election}} \end{aligned} \tag{A2}$$

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

$$\hat{\tau}_{DD} = \left(\tilde{V}^- - \tilde{V}^+\right) - \left(V^- - V^+\right) \tag{A3}$$

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 A3 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

$$\begin{aligned}
\hat{\tau}_{DD} &\equiv \left(\tilde{V}^- - \tilde{V}^+\right) - \left(V^- - V^+\right) \qquad \text{(A4)}\\
&= \Big[\tilde{V}(1,1)^- - \tilde{V}(0,1)^+\Big] - \Big[V(1,0)^- - V(0,0)^+\Big]\\
&= \Big[\tilde{V}(1,1) - \tilde{V}(0,1)\Big] - \Big[V(1,0) - V(0,0)\Big]\\
&= \Big[V(1,1) - V(0,1)\Big] - \Big[V(1,0) - V(0,0)\Big]\\
&= \Big[V(1,1) - V(1,0)\Big] - \Big[V(0,1) - V(0,0)\Big]\\
&= \Big[V(1,1) - V(1,0)\Big]\\
&= \boldsymbol{E}\Big[V_{it}(1,1) - V_{it}(1,0)|M_{it} = 0\Big],
\end{aligned}$$

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 C. 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. 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 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 $t-1$.

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,

$$\Delta_h y_{it} = \beta_h \Delta D_{it} + \sum_{p=1} \gamma_p^h \Delta y_{i,t-p} + \delta_t^h + e_{it}^h, \tag{A5}$$

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.

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

Google trends results

In Figure A5, we show that Google searches jump for the keyword "Gas tax" in the year 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, and less likely to be correlated with other policies.

Appendix D. Electoral penalties from gas tax changes

In this section, we detail several back-of-the-envelope calculations discussed in section 4 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:

$$\eta^p = \frac{\hat{\beta}_4^p}{\hat{\beta}_3^p} \cdot \frac{\bar{g}_p}{\overline{\Delta g}_p}, \quad p = Dem, Rep \tag{A6}$$

Here, $\hat{\beta}_4^p$ are our main estimates from column 4 of Table 1 (-0.013 for Republicans and -0.019 for Democrats), $\hat{\beta}_3^p$ is the incumbency effect from the same column and table (0.092 for Republicans and 0.089 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. From Table A2, this corresponds to 18.83/3.31 for Republicans and 18.64/3.21 for Democrats. These yield elasticities of electoral penalty of -0.803 for Republicans and -1.239 for Democrats. In the following back-of-the-envelope exercises, we quantify the implied magnitude of these penalties for a series of policies studied previously in the transportation literature. These calculations act as a thought exercise to understand our results, understanding that the size of these changes lie out of our sample and that they may also engender additional policies (e.g., transportation sector spending) not modeled directly here.

Electoral penalty from optimal gas taxes

As suggested by Parry and Small (2005), U.S. 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 U.S. 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 1.3 to 8.0 percentage points (mean 2.8) for Republicans and from 2.0 to 12.4 (mean 4.3) for Democrats, as shown in panel (a) of Appendix Figure A6. 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.

Localized gasoline taxes

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

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

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 panel (b) of Appendix Figure A6, 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 (A6), show the distribution in panel (c) of Appendix Figure A6. Penalties for Republicans are as low as 10 percentage points, but electoral benefits are as high as 0.8 percentage points, with a mean of a 0.28 percentage point penalty. For Democrats, penalties run between a penalty of -16 and a benefit of 1.25 percentage points, with an average of a 0.43 penalty. 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.

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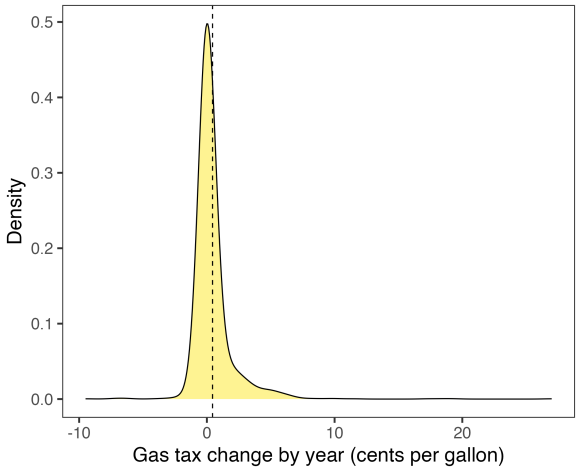
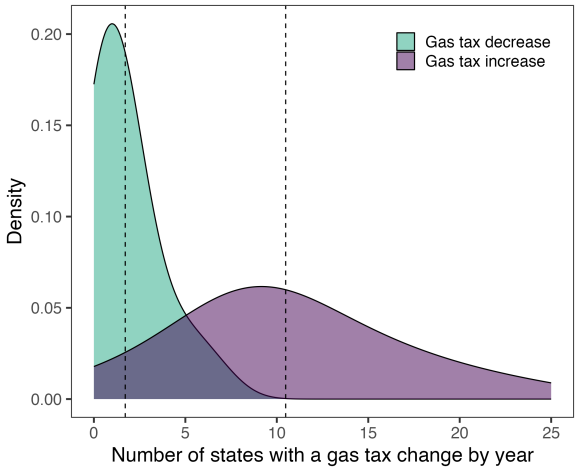
(a) Year-over-year changes in gas tax



(b) Distribution of annual discrete gas tax changes



(c) Gas tax changes over the sample period

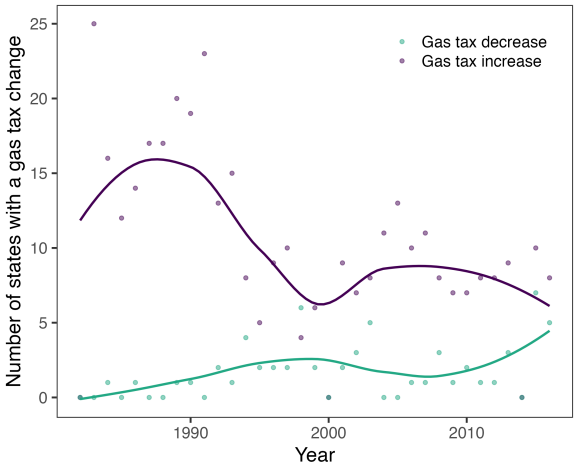


Figure A1: Increases and decreases of the gas tax

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.

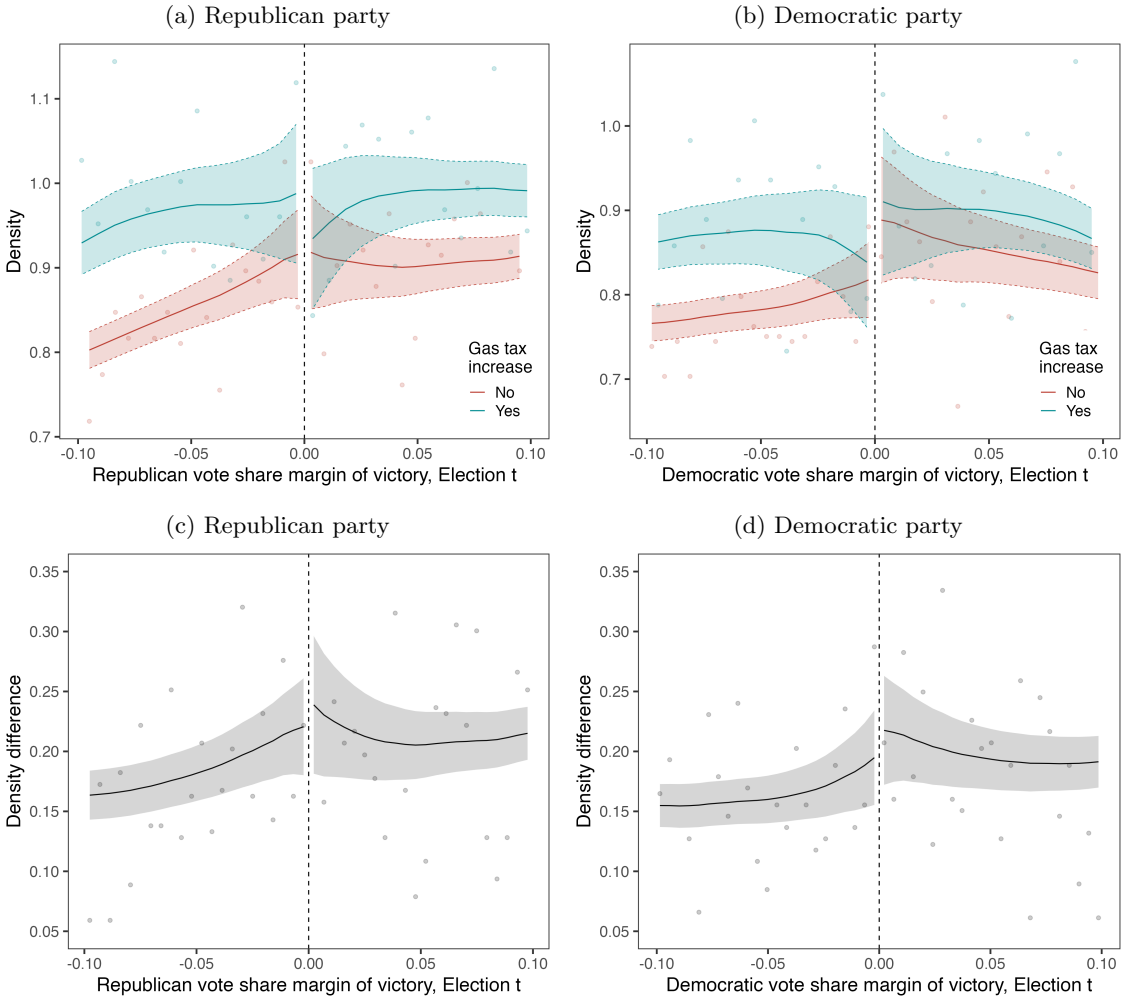
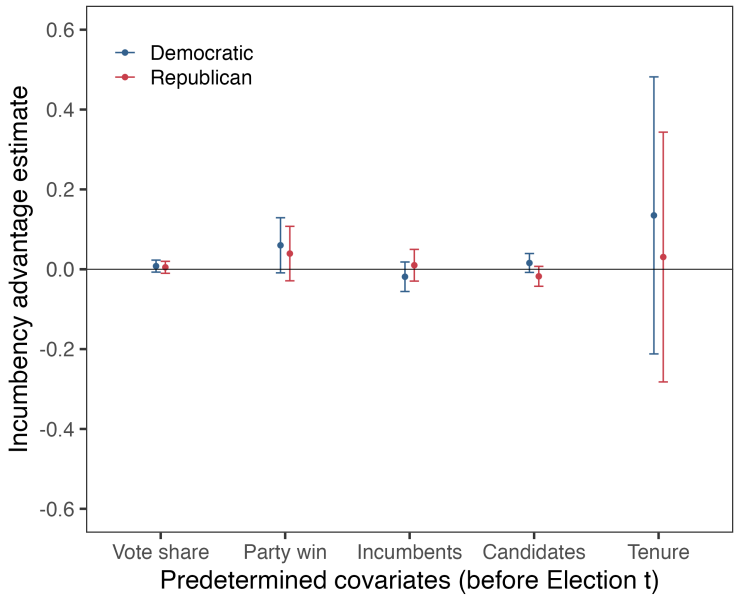


Figure A2: Tests for continuity & manipulation around the discontinuity

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) Predetermined covariates



(b) Placebo cutoffs

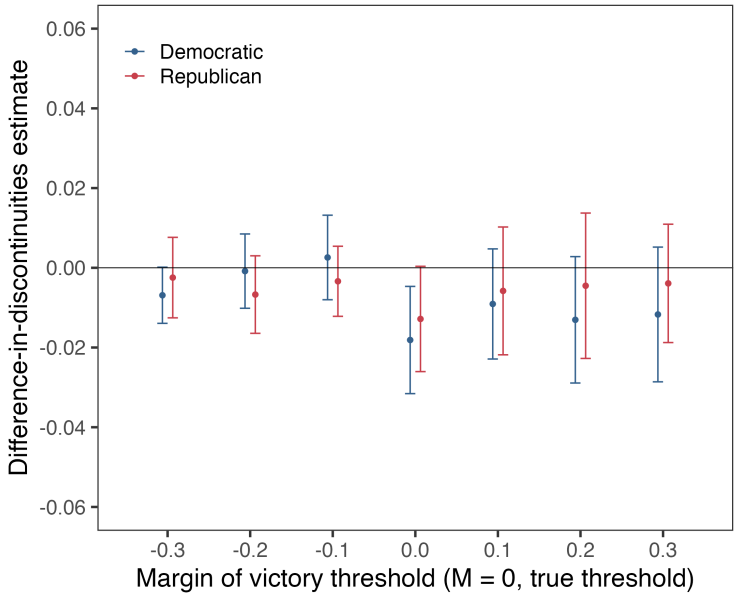


Figure A3: Additional tests for continuity & manipulation around the discontinuity

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.

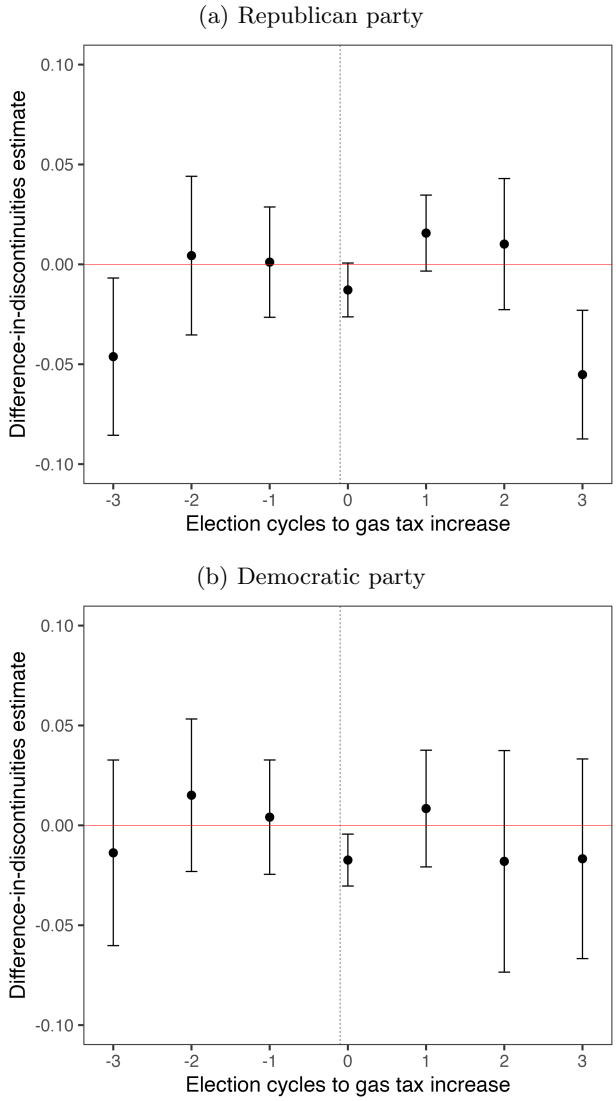


Figure A4: Diff-in-disc election cycle estimates for testing local pre- & post-trends

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.

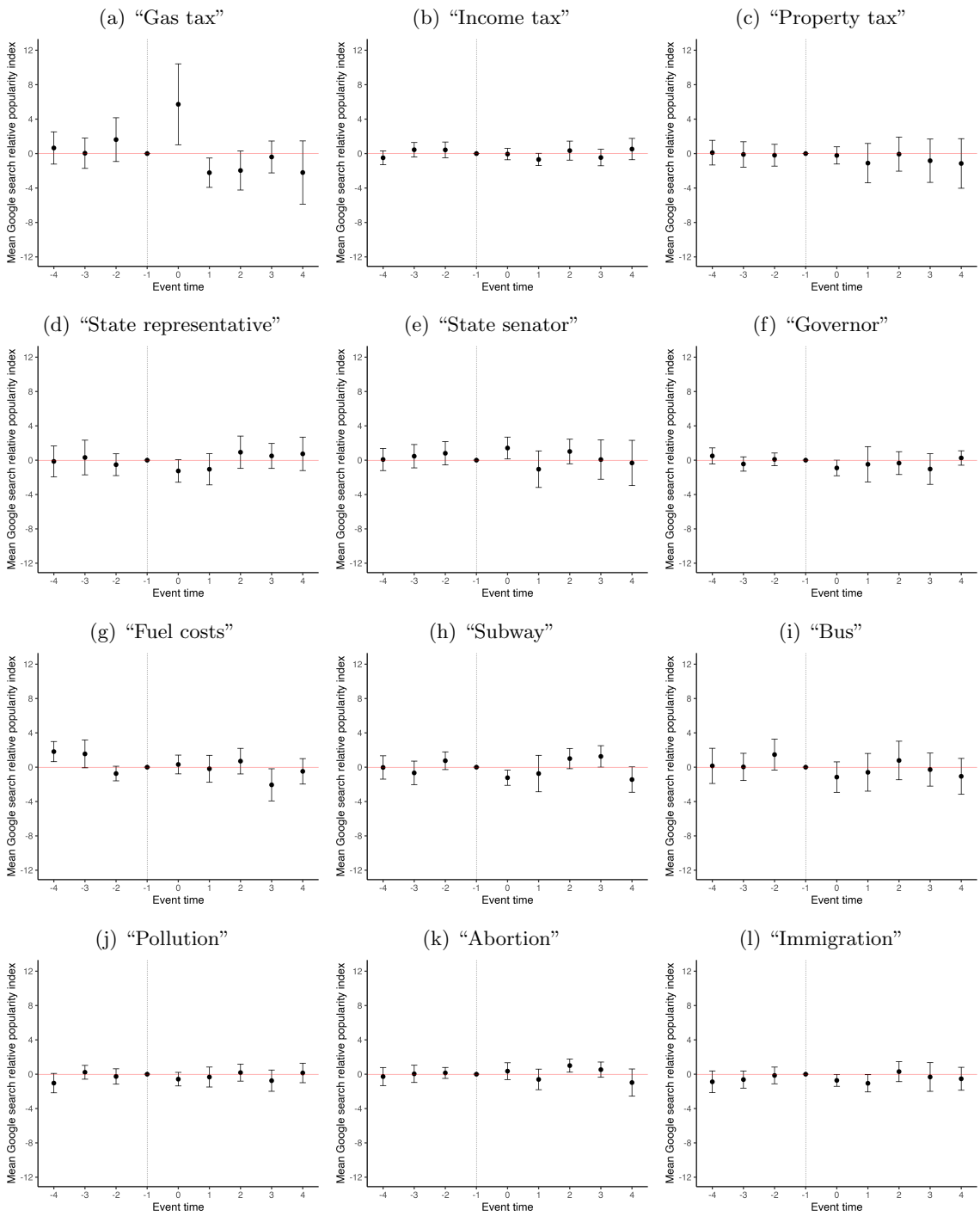
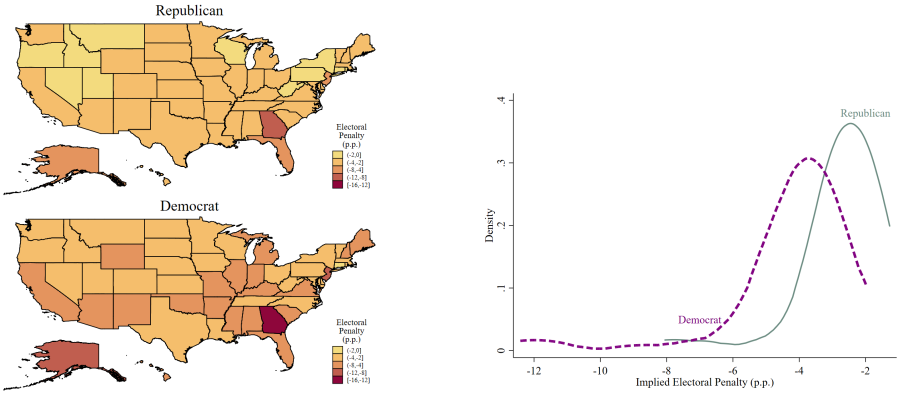


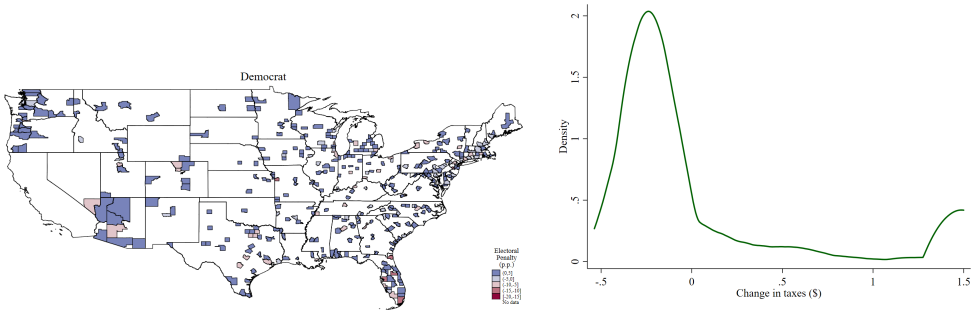
Figure A5: Gas tax changes and Google searches

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) Penalties from national optimal tax from Parry and Small (2005)



(b) Optimal county taxes from Nehiba (2022)



(c) Penalties from optimal county taxes from Nehiba (2022)

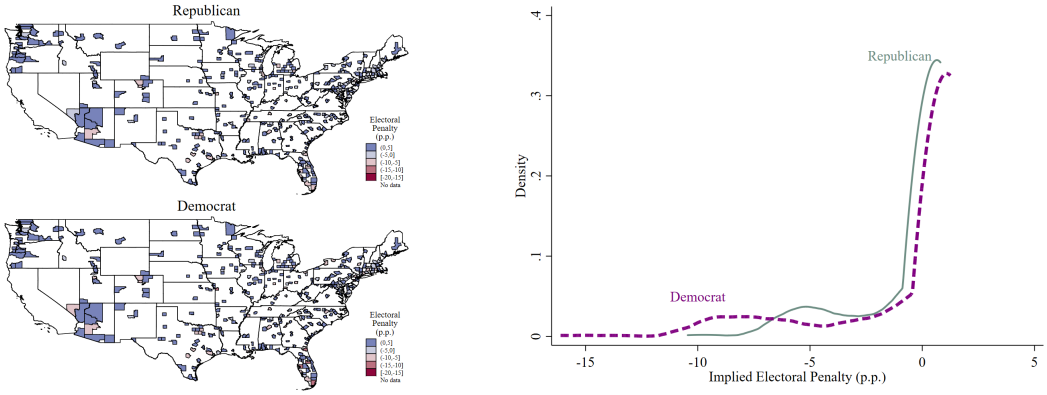


Figure A6: Back-of-the-envelope calculations for electoral penalties

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 (A6). More detail on calculation provided in Appendix D. 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 D.

Table A1: 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 A2: Gas tax summary statistics in cents

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

Notes: This table shows summary statistics for state gasoline taxes. 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 A3: 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 A4: Factors that explain gas tax changes

	(1)	(2)	(3)	(4)	(5)	(6)
Log road mileage per capita	0.042	0.117	-0.020	-0.001	0.047	0.134
	(0.033)	(0.144)	(0.036)	(0.178)	(0.188)	(0.190)
Log licensed drivers per capita	0.018	-0.022	0.069	-0.068	0.162	-0.275
	(0.191)	(0.270)	(0.194)	(0.276)	(0.376)	(0.327)
Log autos per capita	0.122*	0.050	0.049	0.041	0.050	0.075
	(0.072)	(0.101)	(0.080)	(0.094)	(0.132)	(0.102)
Log vehicle miles traveled per capita	-0.217*	0.076	-0.239*	0.064	0.184	0.002
	(0.115)	(0.259)	(0.126)	(0.333)	(0.382)	(0.396)
Log real personal income per capita	0.372*	0.295	-0.005	-0.046	0.047	-0.010
	(0.219)	(0.501)	(0.254)	(0.663)	(0.661)	(0.906)
Log tax-exclusive gas price	-0.035	-0.017	-0.720***	-1.277***	-1.163***	-1.057***
	(0.047)	(0.052)	(0.218)	(0.254)	(0.303)	(0.322)
Gas tax indexing	0.505***	0.495***	0.498***	0.489***	0.506***	0.404***
	(0.079)	(0.096)	(0.071)	(0.091)	(0.123)	(0.100)
Log average family size	-0.212	-0.068	-0.376	-0.111	-0.072	-0.011
	(0.291)	(0.403)	(0.285)	(0.399)	(0.417)	(0.444)
Fraction of adults graduating high school	0.006	0.019	0.010**	0.016	0.016	0.037*
	(0.004)	(0.013)	(0.004)	(0.015)	(0.015)	(0.019)
Fraction of adults with bachelor's	0.000	0.017	-0.002	0.005	0.006	0.015
	(0.005)	(0.017)	(0.005)	(0.020)	(0.027)	(0.025)
Urban population share	-0.017	-0.953	-0.272*	-1.454	-1.511	-1.091
	(0.129)	(1.623)	(0.149)	(1.527)	(1.801)	(2.441)
Log GDP per capita	-0.242*	-0.604**	0.204	-0.053	-0.322	-0.216
	(0.124)	(0.270)	(0.199)	(0.464)	(0.472)	(0.728)
Manufacturing share of GDP	0.157	0.431	-0.252	-0.016	-0.085	1.078
	(0.276)	(0.559)	(0.297)	(0.768)	(0.912)	(1.096)
Mining share of GDP	-0.255	0.817	-0.811**	-0.138	0.262	-0.330
	(0.246)	(0.576)	(0.373)	(0.895)	(1.090)	(1.042)
Percent state budget surplus	0.064	0.049	0.123*	0.118**	0.292	0.083
	(0.048)	(0.045)	(0.067)	(0.056)	(0.185)	(0.052)
Unemployment	0.012	0.010	0.013	0.015	0.025	0.009
	(0.010)	(0.013)	(0.015)	(0.020)	(0.022)	(0.022)
Democratic vote share	-0.029	0.012	-0.055	-0.030	-0.073	0.028
	(0.074)	(0.074)	(0.085)	(0.090)	(0.107)	(0.087)
Democrat control of state house	0.040	0.043	0.032	0.025	0.026	0.032
	(0.043)	(0.032)	(0.040)	(0.030)	(0.034)	(0.032)
Democrat control of state senate	0.010	-0.020	0.022	-0.023	-0.009	-0.022
	(0.043)	(0.045)	(0.041)	(0.043)	(0.043)	(0.046)
Average house LCV rating	0.000	0.000	0.000	-0.001	-0.001	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Average senate LCV rating	-0.001	0.000	0.000	0.000	0.000	0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Democrat governor	-0.002	-0.019	-0.021	-0.024	-0.023	-0.018
	(0.021)	(0.026)	(0.020)	(0.025)	(0.027)	(0.027)
Population share in MSA with rail					0.452	
					(0.628)	
Log transit ridership per capita						0.055
						(0.047)
Year FEs			X	X	X	X
State FEs		X		X	X	X
Sample period	1983–2016	1983–2016	1983–2016	1983–2016	1983–2008	1991–2016
Observations	1,600	1,600	1,600	1,600	1,208	1,237

Notes: All specifications are linear probability models 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. "GDP" is the state-level gross domestic product. "LCV rating" is the League of Conservation Voters' annual score card of environmental voting records for members of the U.S. Congress. Columns (5) and (6) include measures of public transit use and access that have varying temporal coverage during our sample. 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 A5: Regression discontinuity estimates: effect of winning close election on gasoline tax after election t

Panel (a). Republican party	(1)	(2)	(3)	(4)
Win Election ($\mathbf{1}\{M_{it} \geqslant 0\}$)	-0.382	0.030	-0.712	-0.771
	(1.615)	(1.684)	(0.764)	(0.718)
Year FEs			X	X
State FEs			X	X
Covariates			X	X
Right Bandwidth	0.198	0.188	0.143	0.210
Left Bandwidth	0.082	0.130	0.069	0.102
Kernel	Uniform	Triangular	Uniform	Triangular
Observations	295	306	228	319
Panel (b). Democratic party	**(1)**	**(2)**	**(3)**	**(4)**
Win Election ($\mathbf{1}\{M_{it} \geqslant 0\}$)	-1.053	0.199	-1.231	-0.940
	(1.645)	(1.550)	(0.874)	(0.682)
Year FEs			X	X
State FEs			X	X
Covariates			X	X
Right Bandwidth	0.165	0.156	0.124	0.171
Left Bandwidth	0.052	0.071	0.046	0.062
Kernel	Uniform	Triangular	Uniform	Triangular
Observations	278	269	190	284

Notes: Here we provide a test for whether political outcomes may result in gas tax increases. We present estimates from 8 regressions where the dependent variable is the gasoline tax 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. 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. 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: 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 A7: 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 A8: 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 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: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.