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Politicians Avoid Tax Increases Around Elections

Andrew C. Chang*, Linda R. Cohen†, Amihai Glazer‡, and Urbashee Paul§

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Abstract

We use new annual data on gasoline taxes and corporate income taxes from U.S. states to analyze whether politicians avoid tax increases in election years. These data contain 3 useful attributes: (1) when state politicians enact tax laws, (2) when state politicians implement tax laws on consumers and firms, and (3) the size of tax changes. Using a pre-analysis research plan that includes regressions of tax rate changes and tax enactment years on time-to-gubernatorial election year indicators, we find that elections decrease the probability of politicians *enacting* increases in taxes and reduce the size of *implemented* tax changes relative to non-election years. We find some evidence that politicians are most likely to enact tax increases right after an election. These election effects are stronger for gasoline taxes than for corporate income taxes and depend on no other political, demographic, or macroeconomic conditions. Supplemental analysis supports political salience over legislative effort in generating this difference in electoral effects.

Keywords: Corporate Income Taxes; Electoral Cycle; Gasoline Taxes; Pre-analysis Plan; Tax Salience

JEL Codes: D72; D78; H24; H71; K34; P16

*Principal Economist, Division of Research and Statistics, Board of Governors of the Federal Reserve System, 20th St. and Constitution Ave., NW, Washington, DC 20551 USA. +1 (657) 464-3286. a.christopher.chang@gmail.com. https://sites.google.com/site/andrewchristopherchang. ORCID: 0000-0002-9769-789X.

†Department of Economics and School of Law, University of California–Irvine.

‡Department of Economics, University of California–Irvine.

§Department of Economics, Northeastern University.

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1 Introduction

Some taxes are more salient to voters than others. Consider the gasoline tax. In 2011, the Bolivian government announced that it would end subsidies on many fuels, which would lead to a 73 percent increase in the price of gasoline. Five days later, after bus operators went on strike and many took to the streets in outrage, the government withdrew the measure. In 2012, about 12,000 demonstrators clashed with police in front of the Indonesian parliament building in opposition to a 33 percent proposed gasoline and diesel price hike. In the same year, India, Israel, and Pakistan saw mass protests over gas outages and price increases. In 2018, a proposed increase of €0.029 per liter in the fuel tax sparked violent protests across France, pushing President Emmanuel Macron to suspend the proposal. And in 2019, a government-imposed increase in the price of gasoline in Iran led to widespread riots, with more than 100 protesters killed.

The United States has seen calls to raise the federal gas tax to pay for deteriorating infrastructure. In 2019 testimony, U.S. Chamber of Commerce President and Chief Executive Officer Tom Donohue urged the Congress to raise the federal gas tax by 25 cents per gallon, stating

> The federal gas tax, which funds highway and transit programs, is a flat 18.4 cents. The same as it was in 1993. How many people in this room can live off the same paycheck you earned in 1993? No one? Our nation's roads, bridges, and transit systems can't either.[1]

Of course, U.S. politicians are aware that increases in the gas tax are politically unpopular. For example, during the 2008 U.S. presidential campaign

> Both John McCain and Hillary Clinton . . . called for a "gas tax holiday" . . . to offer commuters and vacationers some release from spiraling gas prices. They have urged Congress to suspend the 18.4 cent federal gas tax and 24.4 cent diesel

[1] Donohue (2019).

tax between Memorial Day and Labor Day, a step that could cost the government about $10 billion in revenues.[2]

Fearing voter opposition, politicians may try to reduce the salience of taxes. For example, they may avoid certain nominal tax levels, such as double-digit taxes, that may be more salient. Brunell and Glazer (2001) examine data on U.S. state gasoline taxes, finding that taxes of exactly 10 cents per gallon are disproportionately rare. Politicians may also try to avoid responsibility for effective tax increases by legislating automatic tax rate adjustments or legislating the the more obscure portions of the tax code rather than the statutory rates (Bowman and Mikesell, 1983; Ash, 2016).

Politicians may also game the timing of tax increases to avoid salient, politically unpopular increases close to an election. Berry and Berry (1992); Berry and Berry (1994) find that politicians are most likely to adopt new taxes just after an election, which is presumably when the electoral risks of adoption are smallest. In another example of politicians gaming the timing of taxes, Castanheira, Nicodème and Profeta (2012) find that European Union governments tend to implement labor taxation reforms at the beginning of their mandates.

We use new data on U.S. states and a pre-analysis plan to examine whether elections cause politicians to avoid politically unpopular tax increases in election years and, if this avoidance is the case, why they may do so. Our pre-analysis plan ensures our analysis is devoid of "cherry picking" significant results.[3]

First, we investigate whether the gas tax is so salient to voters that state politicians avoid *implementing* an increase in the gas tax in an election year.[4,5] We then ask whether

[2] Dobbs (2008).

[3] Most pre-analysis plans are used for randomized control trials. We are not aware of any other study in the political economy literature that takes advantage of the analytic rigor of a pre-analysis plan. See Janzen and Michler (2020) for a review of pre-analysis plans for observational studies. See Casey, Glennerster and Miguel (2012) or Banerjee, Duflo, Finkelstein, Katz, Olken and Sautmann (2020) for a further discussion on pre-analysis plans for randomized control trials.

[4] Coibion and Gorodnichenko (2015) provide evidence on the high saliency of gasoline taxes to individuals from the University of Michigan's Surveys of Consumers.

[5] Li, Linn and Muehlegger (2014) find that rising gas taxes are associated with much larger reductions in gasoline consumption than comparable increases in gas prices. Tiezzi and Verde (2016) calculate that a 13.2 cent per gallon tax increase causes, in the long run, a reduction in gasoline demand that is about seven

implementation of the corporate income tax (CIT), which is less politically salient, is also affected by elections.[6] We find that implemented changes in the gasoline tax are smallest in election years. We do not find such election-year dependency for implemented changes in the CIT.

Second, we investigate whether *enactment* of tax increases varies with the electoral cycle. This investigation is possible due to our new data —earlier research largely looks at only tax implementation. Differences between implementation and enactment dates can arise because politicians may enact taxes that will be implemented in the future, possibly to take advantage of budget rules,[7] or to "kick the can" down the road on politically salient tax increases. The literature neglects the difference in political factors, possibly due to data limitations, that can cause differences in the dates on which tax changes are legislated and the dates on which they come into force. We find that both gasoline and corporate tax increases are least likely to be enacted in an election year and the election-year effect is, again, stronger for gasoline taxes.

Third, we investigate whether the causal effect of elections on tax implementation or enactment depends on political, demographic, or macroeconomic conditions. For example, during an economic downturn that coincides with an election or during an election in a more conservative state, politicians may be particularly reluctant to enact or implement taxes. That said, we find little evidence that such dependencies exist.

Lastly, we ask whether fewer taxes are enacted in an election year because politicians are producing less legislation overall (not just tax legislation) in election years, when it is possible that they focus on campaigning rather than legislating. Using a proxy for legislative output

times as big as that induced by an equal market-induced price increase. These authors conjecture that the stronger demand elasticity associated with taxes over market prices is due, in part, to the high salience of gasoline taxes.

[6] Alt, Preston and Sibieta (2010) write "Unlike the personal income tax, corporate taxation is not typically electorally salient. . . . Perhaps corporate tax usually involves technical or complex aspects that elude public understanding." (pg. 32). Laboratory evidence on the lack of salience of business taxes is given by McCaffery and Baron (2006).

[7] For example, the prevalence of statutorily temporary federal taxes could be a response to Congressional Budget Office scoring, which is an analysis of the budgetary effects of legislation that accounts only for budgetary effects over a 10-year window and assumes current law rather than current policy.

—the number of pages of new enacted laws —we find that legislative output is similar in election and non-election years. Therefore, lower overall legislative output does not explain why politicians enact fewer taxes in election years.

Our evidence suggests that smaller and fewer tax changes are implemented and enacted in election years, and these effects are not due to legislative inattention. Furthermore, because we find larger election effects for gasoline taxes than for CITs, we conjecture that the cause of politicians' hesitancy to push for tax increases in election years is due, at least in part, to the relative saliency of taxes to voters.

2 Data on when state politicians enact and implement taxes

We examine new annual state-level tax data from the continental 48 states between 1960 and 2016 for our outcomes of interest. In what follows, I is the indicator function, Δ is the first difference operator, subscript i represents a state, and subscript t is the calendar year.

We have four dependent variables: (1) the implemented change in the gas tax (in cents per gallon), $\Delta GasTax_{it}$; (2) the implemented change in the top statutory CIT rate (in percent), ΔCIT_{it}; (3) an indicator for whether state politicians enact, in calendar year t, an increase in the gas tax for either the current or a future year, denoted by $I[GasTaxBillIncrease_{it} > 0]$; (4) an indicator for whether state politicians enact, during calendar year t, an increase in the CIT for either the current or a future year, denoted by $I[CITBillIncrease_{it} > 0]$. If state politicians enacted multiple bills that affected these taxes within a particular calendar year, then we coded the indicators as the net effect from all bills.

The primary data source for $GasTax_{it}$ and $I[GasTaxBillIncrease_{it} > 0]$ is state session laws. From the session laws, we used all bills that affected flat rate taxes for gasoline used for motor vehicles. These taxes included: environmental fees, petroleum inspection fees, statutorily temporary surcharges, taxes that diverted funds to an apportioned fund, and

proposed taxes subject to a general election.

The session laws allowed us to perfectly construct $I[GasTaxBillIncrease_{it} > 0]$. But in some cases we could not determine $GasTax_{it}$ from the session laws alone (for example, when the gasoline tax was a function of gasoline price or a tax was subject to state revenue targets). In these cases we relied on: the Department of Transportation's *Highway Statistics*,[8] the fuel tax tables from the Tax Policy Center,[9] the fuel tax tables from the Federation of Tax Administrators,[10] and state departments of transportation and revenue as secondary sources for $GasTax_{it}$.[11]

We used session laws to create CIT_{it} and $I[CITBillIncrease_{it} > 0]$. We included any statutorily temporary surcharges in both CIT_{it} and $I[CITBillIncrease_{it} > 0]$. When state politicians enacted a CIT rate change that was implemented in the middle of a calendar year, we coded CIT_{it} as the rate in effect as of December 31 of that calendar year, unless the change was accompanied by a proration clause. In that case we prorated the rate for the partial calendar year accordingly, following Chang (2018*b*).

The novelty of our data is that session laws let us see enactment dates and, hence, create $I[GasTaxBillIncrease_{it} > 0]$ and $I[CITBillIncrease_{it} > 0]$. Earlier research largely considers, and has data on, only implementation dates.[12] But enactment and implementation dates can differ —taxes may be passed but have a delayed implementation,[13] taxes may be indexed to inflation,[14] or tax implementation may be phased in over several years.[15]

[8] See U.S. Department of Transporation, Office of Highway Policy Information (2018).

[9] See Tax Policy Center (2018).

[10] See Federation of Tax Administrators (2018).

[11] Citations vary by state.

[12] Hammar, Löfgren and Sterner (2004) attempt to explain implemented fuel taxes using cross-country data, Li, Linn and Muehlegger (2014) use data from *Highway Statistics* (which are implemented gasoline tax rates), Rivers and Schaufele (2015) study an implemented carbon tax in British Columbia where a validity assumption of their instrumental variables estimation is a sufficiently long lag between enactment and implementation of gas taxes, and Tiezzi and Verde (2016) simulate the effect of implemented taxes.

[13] For example, Arizona session laws 1995, chapter 132, section 3, page 811, adds motor vehicle fuel tax rate at 18 cents per gallon of gasoline. This bill was approved (enacted) on April 17, 1995, but was not effective (implemented) until December 31, 1996.

[14] For example, Massachusetts' session laws 2013, section 44 of chapter 46 adds inflation indexing to the gas tax, effective (implemented) January 1, 2015, but this bill was approved (enacted) on July 24, 2013.

[15] See Tennessee Code Annotated § 67-3-201 (2018): "The rate of the tax imposed by this section shall be: (1) On or after July 1, 2017, through June 30, 2018, twenty-four cents (24 cent(s)) per gallon; (2) On or

Furthermore, the political factors that affect enactment or implementation may differ, so it is useful to study both.

Our main independent variables are indicators for the electoral cycle. $GubElec_{it}$ represents years in which there is a gubernatorial election in a state; $NoGubElec_{it}$ represents years that are more than one year after a gubernatorial election (the omitted category is the year after the election).[16] Gubernatorial election data come from Klarner (2013*a*) through 2015; we separately collected data for 2016.

Our models control for state-level social, economic, and political variables that may affect the enactment and implementation of taxes. Appendix A lists the source data for, and detailed description of, the controls. Table 1 displays summary statistics.

3 Method and pre-analysis plan

Our research follows a pre-analysis research plan[17] that we composed after collecting our data and using a different and smaller, though related, dataset as a pilot.[18] Our plan attempted to ensure that our analysis is devoid of "cherry-picking" significant results.

We are interested in the causal effect of elections on gasoline and CIT implementation and enactment. We employed several models to estimate this causal effect, but our core identification strategy relies on the fact that state gubernatorial elections occur in different years in roughly fixed cycles that should be exogenous to unobserved factors that are correlated with tax implementation or enactment.[19]

Because we used several models, we are concerned about appropriately accounting for multiple hypothesis testing. Therefore, we report two sets of p -values: (1) conventional

after July 1, 2018, through June 30, 2019, twenty-five cents (25cent(s)) per gallon; and (3) On or after July 1, 2019, twenty-six cents (26cent(s)) per gallon."

[16] This coding of the election variables followed Berry and Berry (1992); Berry and Berry (1994).

[17] We registered our preanalysis plan on the Open Science Framework at https://osf.io/f2nr9/.

[18] The pilot dataset had 20 states instead of 48, data from 1963-2010 instead of 1960-2016, and gasoline taxes that excluded environmental fees, petroleum inspection fees, statutorily temporary surcharges, taxes that diverted funds to an apportioned fund, and proposed taxes subject to a general election, instead of including these bills.

[19] This identification strategy has been used before, e.g. Julio and Yook (2012); Jens (2017).

p -values and (2) p -values that adjusted for multiple hypothesis testing by controlling for family-wise error rates (FWER) using the free step-down procedure of Westfall and Young (1993) implemented by Jones, Molitor and Reif (2018)—with 10,000 bootstrap replications per resample.[20] We denote the families of models for FWER adjustment by model number.

In the model descriptions that follow, Φ is the cumulative distribution function for the normal distribution, $I[]$ is the indicator function, Δ represents the first difference operator, $\%\Delta$ is the percent change, subscript i represents the state, and subscript t is the calendar year.

We estimated linear models with ordinary least squares, probit models with maximum likelihood, and clustered standard errors by state.

We are aware of the limitations of our study. One is that we focus on the effect of elections on only two types of taxes. We chose gasoline taxes as an important example of a highly salient tax, and the CIT as one example of a less salient tax. Inferences about the effect of elections on other types of taxes or legislation may lead to different conclusions.

A second limitation is that we modeled the effects of elections as separable linear models. These models abstract from nonlinear effects or covariances in the legislative process between types of taxes. It is possible that gasoline tax bills may have been logrolled together (though not necessarily enacted) with CIT bills.

A third limitation is that we pre-specified that statutorily temporary surcharges, and extensions to those surcharges, were tax increases (and vice versa for tax decreases). Evidence from Chang (2018*a*) suggests that the dynamics for enacting extensions of statutorily temporary taxes may be different than those for enacting statutorily permanent taxes.

[20] In some cases the Westfall and Young (1993) bootstrap did not converge, which we did not expect in our preanalysis plan. In these instances we report the more conservative (relative to Westfall-Young) Sidak-Holm p-values (Guo and Romano, 2007) to account for multiple hypothesis testing.

4 Results

4.1 Main specifications—Is there an effect of elections on tax implementation or enactment? Yes.

Our first set of models, 1a through 1d, is our main specification set for testing whether elections affect tax implementation or enactment:[21]

$$\Delta GasTax_{it} = \alpha_i + \lambda_t + \beta_1 GubElec_{it} + \beta_2 NoGubElec_{it} + \varepsilon_{it} \tag{1a}$$

$$\Delta CIT_{it} = \alpha_i + \lambda_t + \beta_1 GubElec_{it} + \beta_2 NoGubElec_{it} + \varepsilon_{it} \tag{1b}$$

$$I[GasTaxBillIncrease_{it} > 0] = \Phi[\alpha_i + \lambda_t + \beta_1 GubElec_{it} + \beta_2 NoGubElec_{it} + \varepsilon_{it}] \tag{1c}$$

$$I[CITBillIncrease_{it} > 0] = \Phi[\alpha_i + \lambda_t + \beta_1 GubElec_{it} + \beta_2 NoGubElec_{it} + \varepsilon_{it}] \tag{1d}$$

As before, $GubElec_{it} = 1$ in a gubernatorial election year and is 0 otherwise. The indicator variable $NoGubElec_{it} = 1$ in years 2 and 3 following a gubernatorial election. Therefore, the omitted category is the year after the election. Because elections roughly occur in fixed cycles, our models should identify the parameters on these election variables.

Models 1a and 1b investigate the effect of elections on tax implementation. The interpretation of the election coefficients (β_1 and β_2) is how much larger, or smaller, changes in the gasoline tax (in cents per gallon) or the CIT rate are at different points in the electoral cycle.

Models 1c and 1d look at how election years affect tax enactment. As these models are probits, we are interested in (and report) average marginal effects.[22] The interpretation of the

[21] We followed the recommendation of Casey, Glennerster and Miguel (2012) and designated the relative importance of our specifications in our pre-analysis plan.

[22] We calculated average marginal effects using the delta method. This method is the standard method for Stata's "margins" command as of Stata 13.0. For state i's and time j's regressor, $x_{i,j}$, the average marginal effect is $\frac{1}{N}\sum_{i=1}^{N}\frac{dy_i}{dx_{i,j}}$, where N is the total number of states.

electoral variables in the probit models is whether the electoral cycle affects the probability of enacted legislation increasing the gasoline tax or the CIT.

Models 1a through 1d all have state (α_i) and time (λ_t) fixed effects to absorb residual variation that is either cross-sectionally or time-invariant that could confound our estimates.

4.1.1 Elections affect both tax implementation and enactment—but especially for gas taxes.

Figure 1 shows the average marginal effects of the electoral variables from models 1a through 1d. Whiskers are 95 percent conventional confidence intervals. The numbers below each model estimate represent FWER adjusted p-values for the two-sided hypothesis tests that the indicated marginal effect is equal to zero versus the alternative that it is not equal to zero.

Implemented gas tax changes are, on average, 0.21 of a cent per gallon lower in gubernatorial election years (p = 0.05).[23] We find no similar election effect for implemented changes in the CIT rate.

We do find an electoral effect on the enactment of both gasoline and CITs. State politicians are about 9 percentage points less likely to enact an increase in the gasoline tax in an election year, relative to the year just after an election ($p < 0.01$). The same effect for the CIT is about 7 percentage points ($p = 0.01$).

Furthermore, as the estimated effects of $NoGubElec_{it}$ on tax enactment are also negative, as shown in the right two columns of the bottom panel of figure 1, state politicians are most likely to increase the taxes in the year after an election.

Because we find an effect of elections on implementation for the gas tax but not for the CIT, yet we also find an effect of elections on enactment for both taxes, the results suggest that politicians try to game the timing of both taxes but that perhaps the targets of corporate taxes are more sophisticated and react to enactment as opposed to implementation, whereas

[23] Unless otherwise indicated, p-values referenced in the text refer to the FWER-adjusted p-values.

drivers are sensitive to gasoline prices and the actions of politicians.[24]

4.2 Secondary specifications—Election effects persist after adding other controls, especially for gas taxes.

Our second most important set of specifications, 2a through 2d, use additional controls.

$$\Delta GasTax_{it} = \alpha_i + \lambda_t + \gamma X_{it} + \beta_1 GubElec_{it} + \beta_2 NoGubElec_{it} + \varepsilon_{it} \quad (2a)$$

$$\Delta CIT_{it} = \alpha_i + \lambda_t + \gamma X_{it} + \beta_1 GubElec_{it} + \beta_2 NoGubElec_{it} + \varepsilon_{it} \quad (2b)$$

$$I[GasTaxBillIncrease_{it} > 0] = \Phi[\alpha_i + \lambda_t + \gamma X_{it} + \beta_1 GubElec_{it} + \beta_2 NoGubElec_{it} + \varepsilon_{it}] \quad (2c)$$

$$I[CITBillIncrease_{it} > 0] = \Phi[\alpha_i + \lambda_t + \gamma X_{it} + \beta_1 GubElec_{it} + \beta_2 NoGubElec_{it} + \varepsilon_{it}] \quad (2d)$$

The term X_{it} in 2a through 2d captures pre-specified controls that vary in both the cross section and time series: state unemployment rate, mean adjusted Americans for Democratic Action (ADA)[25] score, percent change in gross state product (GSP), resident population, number of licensed drivers, highway gasoline usage (gallons), and per capita income.[26] Because the controls that we add in models 2a through 2d vary in both the cross section and time series, the models can still support state and time fixed effects.

The election effects from these models, shown in figure 2, indicate an electoral pattern that is broadly similar to that observed in our models without additional controls. Implemented gas taxes are smaller in an election year, but there is no such election year effect for

[24] The financial effects of corporate taxes are also complex and implementation has differential effects on firms.

[25] Original ADA scores are scaled such that a senator who votes with the liberal position on every vote receives a score of 100, while a senator who always opposes the liberal position receives a 0. After adjusting scores to remove errors in the original ADA score, following Groseclose, Levitt and Snyder (1999), we found that adjusted scores range from negative 1.51 to 103.11.

[26] See section 2 of our pre-analysis plan or appendix A.3 for a complete list of controls and their descriptions.

implementation of the CIT. Both gas taxes and the CIT are least likely to be enacted in an election year and are most likely to be enacted in the year after an election.

4.3 Tertiary specifications—Election effects persist for gasoline taxes only, after removing fixed effects.

Our main and secondary specifications use both time and state fixed effects, which absorb variation that is either cross-sectionally or time invariant, increasing the validity of the model estimates. That said, fixed effects remove both "good" and "bad" variation. Removing "good" variation can potentially lead to attenuated estimates (Angrist and Pischke, 2008).

Therefore, we pre-specified models 3a through 3d without fixed effects, though we place a lower (pre-specified) emphasis on these models because of their lower internal validity. Instead of fixed effects, the models control for certain variables with low (or no) cross-sectional variation, X_t and other variables with low (or no) time variation, X_i:

$$\Delta GasTax_{it} = \alpha X_i + \lambda X_t + \gamma X_{it} + \beta_1 GubElec_{it} + \beta_2 NoGubElec_{it} + \varepsilon_{it} \tag{3a}$$

$$\Delta CIT_{it} = \alpha X_i + \lambda X_t + \gamma X_{it} + \beta_1 GubElec_{it} + \beta_2 NoGubElec_{it} + \varepsilon_{it} \tag{3b}$$

$$I[GasTaxBillIncrease_{it} > 0] = \Phi[\alpha X_i + \lambda X_t + \gamma X_{it} + \beta_1 GubElec_{it} + \beta_2 NoGubElec_{it} + \varepsilon_{it}] \tag{3c}$$

$$I[\Delta CITBillIncrease_{it} > 0] = \Phi[\alpha X_i + \lambda X_t + \gamma X_{it} + \beta_1 GubElec_{it} + \beta_2 NoGubElec_{it} + \varepsilon_{it}] \tag{3d}$$

Variables in X_i consist of the average share (2003-17) of the population with at least a four-year college degree, the 2010 daily newspaper circulation rate, and the number of legislative terms that can be served by a governor.

Variables in X_t consist of the percent change in the nominal crude oil price, the percent change in nominal non-financial corporate profits before taxes, and transportation usage as measured by the percent change in U.S. vehicle total miles traveled. X_{it} is the same as in

subsection 4.2.[27]

Once we strip the model of fixed effects, but add in their place X_i and X_t, we continue to find an electoral effect on tax implementation and enactment for gasoline taxes, but a much smaller (or no) effect for the CIT.

The estimated average change in the implemented gas tax is 0.39 cents per gallon lower in a gubernatorial election year ($p = 0.07$) compared with the year just after an election (figure 3). The probability of gas tax enactment is 14 percentage points lower during an election year ($p < 0.01$). The election effects for CITs are more muted compared such effects in our main and secondary specifications.

The marginal effects of $NoGubElec_{it}$ are basically zero in figure 3, meaning that tax implementation and enactment are similar in the year after an election compared with at least two years after an election. Only the election year is different.

4.4 Tertiary specifications (continued)-Do tax changes depend on political, demographic, or macroeconomic conditions? No.

For our final set of pre-specified results, we investigated whether the election effects on tax implementation and enactment vary with political, demographic, or macroeconomic conditions.

To do so, we used our models 1a through 1d and added our electoral indicators, $GubElec_{it}$ and $NoGubElec_{it}$, interacted with the following variables that we originally used as controls, removing the relevant fixed effects when necessary: two proxies for voter attentiveness (average percentage of college-educated individuals and daily newspaper circulation), two measures of U.S. macroeconomic conditions (oil prices and corporate profits), and three measures of state-level economic and political conditions (GSP, unemployment rate, and ADA score).

We found that the electoral effect on tax implementation or enactment varied with none of these variables, as the coefficients on the interaction terms were generally 0. Full results

[27] Appendix A contains a detailed description of each of the controls.

and pre-specified models are in appendix B.

5 Does lower legislative output in election years explain our results? Unlikely.

So far, we have shown that taxes are less likely to be enacted or implemented in an election year. We found stronger effects of elections on enactment and implementation for gasoline taxes than for CITs. We suggest that this difference in estimated effect sizes is due to the greater salience of gasoline taxes, relative to that of CITs, to voters in election years.

An alternative explanation for the effect of elections on tax enactment is reduced legislative output in election years. Perhaps, in election years, politicians spend more time and effort on campaigning, rather than legislating. This behavior, however, would not explain our election effect on tax implementation,[28] nor would it explain differential election effects for both tax enactment and implementation between gasoline and CITs (unless gasoline taxes were particularly arduous to enact or implement, relative to CITs, which is unlikely). Lower overall legislative output, however, could explain why elections decrease tax enactment.

To check for this possibility, we conducted an investigation that was not in our pre-analysis plan. Readers should apply the appropriate caution when interpreting these results.

We gathered data on a proxy for legislative output: the number of pages of state session laws. Presumably the legislature is more productive when it enacts more pages of legislation, though our proxy says nothing about the quality of such legislation.

After we collected these data, we estimated the following:

$$SessionLawPages_{it} = \alpha_i + \lambda_t + \beta_1 GubElec_{it} + \beta_2 NoGubElec_{it} + \beta_3 RegularSession_{it} + \varepsilon_{it} \quad (4)$$

In equation 4, $SessionLawPages_{it}$ is the number of pages of state session laws in a given

[28] The implementation date of a bill should be unaffected by time constraints, as legislators may enact a bill and then implement the bill at any future date with no cost.

year, excluding introductory front matter, and $RegularSession_{it}$ is an indicator for whether a state had a regularly scheduled legislative session. Most of the other details of the model are the same as in section 4, except we lack data for Ohio and have data for Wisconsin up through 1990 instead of through 2016. We estimated only one version of equation 4, so we employed no FWER adjustment.

We found an economically small and statistically insignificant effect of elections on legislative output. In gubernatorial election years states had an average of 24 *more* pages of session laws (p = 0.74). In comparison the average size of a state's session laws for a year is 2,090 pages. Furthermore, if politicians were too busy campaigning to legislate, then we would expect a negative effect of elections on legislative output that would lead to fewer pages of session laws, not more pages.

We also estimated equation 4 with X_{it}, defined as in section 4, but the model still showed no effect of elections on legislative output.[29] Therefore, we assert that our findings are not explained by lower legislative output in gubernatorial election years.

6 Conclusion

We used new data on U.S. states and a pre-analysis plan to investigate whether elections cause politicians to avoid enacting or implementing tax increases in election years. We found that enactment of both gasoline and CIT increases is less likely, and the size of implemented gas tax changes is smaller, in an election year. We established causality by leveraging the fact that U.S. gubernatorial election cycles are staggered among states[30] and have followed fixed schedules, making them independent from contemporary macroeconomic conditions that would confound our regression estimates.

We take our finding that the election effects were stronger for gasoline taxes as evidence

[29] The gubernatorial election indicator was 61 ($p = 0.51$).

[30] Thirty four states hold their gubernatorial elections in midterm election years; nine—Delaware, Indiana, Missouri, Montana, New Hampshire, North Carolina, North Dakota, Utah, and Vermont—have them in presidential election years; and five—Kentucky, Louisiana, Mississippi, New Jersey, and Virginia—hold them in odd-numbered years. Incumbents cannot choose when elections take place (Smith and Greenblatt, 2019).

of the electoral salience of and potential voter backlash to increases in gasoline taxes, which aligns with the gas tax salience results of Li, Linn and Muehlegger (2014); Coibion and Gorodnichenko (2015); Tiezzi and Verde (2016). The gas tax may be equally unpopular with voters in off-election years, but our results are consistent with a higher associated electoral penalty to increasing the gas tax in election years —voters, perhaps, are myopic.

While we also found some evidence of an electoral effect on CIT enactment, the electoral pattern was more muted than the pattern for gas tax enactment. We also found no evidence for an electoral effect on CIT implementation. Corporations, perhaps, pay consistent attention to policy formation, making the timing of tax implementation a less relevant outcome of their lobbying and policy influence.

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Appendices

A Appendix: Controls

This section lists the control variables we used in our models. These variables are separated into three categories: variables with limited time-series variation, X_i, variables with limited cross-sectional variation, X_t, and variables that vary in both the cross section and time series, X_{it}.

Under each variable is a sub-list of relevant information, including: the source, the period for which data for this variable are available, the date of the most recent download,[31] and the notation that is used for the relevant variable. Here, subscript i indicates a state and subscript t indicates a calendar year.

A.1 Time-invariant variables (X_i)

These variables vary over states, but not much (or at all) over time.

1. Education - Share of population with a four-year college degree or higher, averaged from 2003 through 2017.
   (a) Source: U.S. Census Bureau (2018), Current Population Survey (CPS). Link: click here
   (b) Data availability: 1998—2017
   (c) Download date: 7/26/2018
   (d) Variable notation: $\overline{Edu_i^{2003-2017}}$
   (e) Note: Period of 2003—17 is due to web availability of CPS data.
2. Newspaper Circulation Total - Daily news - 2010 rate

[31] We report and fix the data versions, as data can revise and lead to different estimation results (Koenig, Dolmas and Piger, 2003; Chang and Li, 2018).

(a) Source: Editor & Publisher Co, New York, NY (2018). Link: click here

(b) Data availability: 1991-2010 for national-level data, 2010 only for state-level data

(c) Download date: 7/18/2018

(d) Variable notation: $News_i$

3. Legislative Terms - Number of years in governor's term (years, not years remaining)

(a) Source: Klarner (2013*a*), "Governors Dataset" (Harvard Dataverse). Link: click here

(b) Data availability: 1956—2004

(c) Download date: 7/20/2018

(d) Variable notation: $Terms_{it}$

Note: This variable has some, but limited, time-series variation, so we include it in the matrix (X_i) but denote it subscript *it*.

A.2 Cross-sectionally invariant variables (X_t)

These variables vary over time, but not much (or at all) across states.

1. Percent Change in Crude Oil Price - Domestic first purchase price per barrel, nominal dollars

(a) Source: U.S. Energy Information Administration (EIA) (2018), Monthly Energy Review, Table 9.1, Annual Averages. Link: click here

(b) Data availability: 1861—2017

(c) Download date: 7/30/2018

(d) Variable notation: $\%\Delta Oil_t$

2. Corporate Profits - Percent change in non-financial corporate profits, before tax (without IVA and CCAdj), nominal dollars

(a) Source: Federal Reserve Economic Data (FRED) (2018*b*). Link: click here

(b) Data availability: 1948—2017

(c) Download date: 7/27/2018

(d) Variable notation: $\%\Delta Prof_t$

3. Transportation Usage - Vehicle total miles in millions per year (sum of Bus, Commuter Bus, and Bus Rapid Transit), percent change

(a) Source: America Public Transportation Association (2018). Link: click here

(b) Data availability: 1926—2006. Data starting in 2007—15 are not comparable to pre-2007 data, so we use data through 2006.

(c) Download date: 7/17/2018

(d) Variable notation: $\%\Delta Trans_t$

A.3 Variables that vary over time and across states (X_{it})

These are variables for which we have acquired panel data approximately covering our desired time period of 1960 through 2016.

1. Gross State Product (GSP) - Percent change, nominal dollars

(a) Source: Bureau of Economic Analysis (BEA) (2018). Link: click here

(b) Data availability: 1963—2017

(c) Download date: 7/12/2018

(d) Variable notation: $\%\Delta GSP_{it}$

Note: There is a discontinuity in the time series of GSP in 1997 since industry codes change from a SIC (Standard Industrial Classification) to NAICS (North American Industry Classification System) standard, though we use the entire panel series.

2. Unemployment Rate—State percentage of people unemployed—Average of monthly, seasonally adjusted rates.

   (a) Source: Federal Reserve Economic Data (FRED) (2018*c*). Link: click here

   (b) Data availability: 1976—2018

   (c) Download date: 7/30/2018

   (d) Variable notation: $UnempRate_{it}$

3. Voter Preferences - Adjusted Americans for Democratic Actions (ADA) scores - average score across all elected officials by state and year

   (a) Source: Independently tabulated by Groseclose, Levitt and Snyder (1999) (for 1947-1998 originally and later extended by Groseclose to 2008) and by Anderson and Habel (2009) (for 1947—2007); updated and reconciled by Justin Briggs (2008—2015). We use the adjusted ADA score that removes errors in the original ADA score using data from Dr. Groseclose. The adjusted scores range from negative 1.51 to 103.11, with higher numbers still indicating the more liberal position. Link: click here

   (b) Data availability: 1947-2015

   (c) Download date: 7/20/2018

   (d) Variable notation: $VoterPref_{it}$

   Note: The original ADA score calculates annual ratings of elected officials based on approximately 20 roll-call votes per year. Scores are scaled such that a senator who votes with the liberal position on every vote receives a score of 100, while a senator who always opposes the liberal position receives a 0. The mean House ADA score is strongly correlated with other possible measures of a state's liberal-conservative position such as the percentage of the state's presidential vote cast for the Democratic Party.

4. Resident Population - All residents (both civilian and Armed Forces) living in the state, Percent Change

   (a) Source: Federal Reserve Economic Data (FRED) (2018*d*). Link: click here

   (b) Data availability: 1900—2018

   (c) Download date: 7/30/2018

   (d) Variable notation: $\%\Delta Pop_{it}$

5. Licensed Drivers - Number of licensed drivers in the state, Percent Change

   (a) Source: U.S. Department of Transportation, Office of Highway Policy Information (2016*a*). Link: click here

   (b) Data availability: 1949—2014 (Alaska since 1959, Hawaii since 1960)

   (c) Download date: 7/24/2018

   (d) Variable notation: $\%\Delta Drivers_{it}$

6. Highway Use of Gasoline - Thousands of gallons, percent change

   (a) Source: U.S. Department of Transportation, Office of Highway Policy Information (2016*b*). Link: click here

   (b) Data availability: 1949—2016 (Hawaii and Alaska since 1959)

   (c) Download date: 7/30/2018

   (d) Variable notation: $\%\Delta GasUsage_{it}$

7. Per Capita Income - Income per person in state, nominal dollars, percent change

   (a) Source: Federal Reserve Economic Data (FRED) (2018*a*). Link: click here

   (b) Data availability: 1929—2017

   (c) Download date: 7/30/2018

   (d) Variable notation: $\%\Delta IncPerCap_{it}$

B Appendix: Do election effects depend on political, demographic, or economic conditions? No. No. And No.

This appendix reports the pre-specified models that we used to investigate whether elections have different effects on tax implementation or enactment based on political, demographic, or macroeconomic conditions. Largely, these models show that this electoral effect does not depend on these factors.

We focus this explanation on the electoral variables, which are our variables of interest. Estimates of the controls are in tables 2 through 6.

B.1 Do the effects of elections on tax implementation or enactment depend on voter attentiveness? No.

The first family (for FWER adjustment) of our heterogeneity-seeking models, 5a through 5d, asks whether the election effect varies with measures of voter's attentiveness to new legislation, which we proxy with the average education level in a state, $\overline{Edu_i^{2003-2017}}$ and the average daily newspaper circulation, $News_i$.

$$\begin{aligned}\Delta GasTax_{it} = \lambda_t + \gamma X_{it} + \beta_1 GubElec_{it} + \beta_2 NoGubElec_{it} \\ + \theta_1 \overline{Edu_i^{2003-2017}} + \theta_2(\overline{Edu_i^{2003-2017}} * GubElec_{it}) + \theta_3(\overline{Edu_i^{2003-2017}} * NoGubElec_{it}) \\ + \pi_1 News_i + \pi_2(News_i * GubElec_{it}) + \pi_3(News_i * NoGubElec_{it}) + \varepsilon_{it}\end{aligned} \tag{5a}$$

$$\Delta CIT_{it} = \lambda_t + \gamma X_{it} + \beta_1 GubElec_{it} + \beta_2 NoGubElec_{it} + \theta_1 \overline{Edu_i^{2003-2017}} + \theta_2(\overline{Edu_i^{2003-2017}} * GubElec_{it}) + \theta_3(\overline{Edu_i^{2003-2017}} * NoGubElec_{it}) + \pi_1 News_i + \pi_2(News_i * GubElec_{it}) + \pi_3(News_i * NoGubElec_{it}) + \varepsilon_{it} \tag{5b}$$

$$I[GasTaxBillIncrease_{it} > 0] = \Phi[\lambda_t + \gamma X_{it} + \beta_1 GubElec_{it} + \beta_2 NoGubElec_{it} + \theta_1 \overline{Edu_i^{2003-2017}} + \theta_2(\overline{Edu_i^{2003-2017}} * GubElec_{it}) + \theta_3(\overline{Edu_i^{2003-2017}} * NoGubElec_{it}) + \pi_1 News_i + \pi_2(News_i * GubElec_{it}) + \pi_3(News_i * NoGubElec_{it}) + \varepsilon_{it}] \tag{5c}$$

$$I[CITBillIncrease_{it} > 0] = \Phi[\lambda_t + \gamma X_{it} + \beta_1 GubElec_{it} + \beta_2 NoGubElec_{it} + \theta_1 \overline{Edu_i^{2003-2017}} + \theta_2(\overline{Edu_i^{2003-2017}} * GubElec_{it}) + \theta_3(\overline{Edu_i^{2003-2017}} * NoGubElec_{it}) + \pi_1 News_i + \pi_2(News_i * GubElec_{it}) + \pi_3(News_i * NoGubElec_{it}) + \varepsilon_{it}] \tag{5d}$$

Focusing first on the main effects of elections in these models, figure 4 shows gubernatorial election years are not significantly associated with changes in the gas tax or in the CIT. Nor are they associated with the probability that legislation is adopted to change the taxes.

If you really want to peer into the tea leaves, then the gubernatorial election point estimates are negative for tax enactment. Economically, these point estimates suggest that gubernatorial election years may temporarily suppress gas and corporate tax rate enactment. That said, these models lack state fixed effects, so there may be confounding factors behind these estimates (which is why we put a lower pre-specified weight on these models).

Finally, the interactions between the election indicators and both education and newspaper circulation are not statistically significant (figures 5 and 6 for gubernatorial election years and years 2 and 3 after a gubernatorial election, respectively). We conclude that voter attentiveness has no effect on the propensity for elections to decrease tax enactment and implementation.

B.2 Do effects of elections on tax implementation or enactment depend on U.S. macroeconomic conditions? No.

We now investigate, using equations 6a through 6d, whether the propensity for state politicians to enact or implement taxes depends on U.S. macroeconomic conditions. We interact oil prices and corporate profits, separately, with our electoral cycle indicators:

$$\begin{aligned}\Delta GasTax_{it} = {} & \alpha_i + \gamma X_{it} + \beta_1 GubElec_{it} + \beta_2 NoGubElec_{it} \\ & + \theta_1 \%\Delta OilPrice_t + \theta_2(\%\Delta OilPrice_t * GubElec_{it}) + \theta_3(\%\Delta OilPrice_t * NoGubElec_{it}) \\ & + \pi_1 \%\Delta CorpProf_t + \pi_2(\%\Delta CorpProf_t * GubElec_{it}) \\ & + \pi_3(\%\Delta CorpProf_t * NoGubElec_{it}) + \varepsilon_{it}\end{aligned} \tag{6a}$$

$$\begin{aligned}\Delta CIT_{it} = {} & \alpha_i + \gamma X_{it} + \beta_1 GubElec_{it} + \beta_2 NoGubElec_{it} \\ & + \theta_1 \%\Delta OilPrice_t + \theta_2(\%\Delta OilPrice_t * GubElec_{it}) + \theta_3(\%\Delta OilPrice_t * NoGubElec_{it}) \\ & + \pi_1 \%\Delta CorpProf_t + \pi_2(\%\Delta CorpProf_t * GubElec_{it}) \\ & + \pi_3(\%\Delta CorpProf_t * NoGubElec_{it}) + \varepsilon_{it}\end{aligned} \tag{6b}$$

$$
\begin{aligned}
I[GasTaxBillIncrease_{it} > 0] = \Phi[&\alpha_i + \gamma X_{it} + \beta_1 GubElec_{it} + \beta_2 NoGubElec_{it} \\
&+ \theta_1 \%\Delta OilPrice_t + \theta_2(\%\Delta OilPrice_t * GubElec_{it}) \\
&+ \theta_3(\%\Delta OilPrice_t * NoGubElec_{it}) \\
&+ \pi_1 \%\Delta CorpProf_t + \pi_2(\%\Delta CorpProf_t * GubElec_{it}) \\
&+ \pi_3(\%\Delta CorpProf_t * NoGubElec_{it}) + \varepsilon_{it}]
\end{aligned} \tag{6c}
$$

$$
\begin{aligned}
I[CITBillIncrease_{it} > 0] = \Phi[&\alpha_i + \gamma X_{it} + \beta_1 GubElec_{it} + \beta_2 NoGubElec_{it} \\
&+ \theta_1 \%\Delta OilPrice_t + \theta_2(\%\Delta OilPrice_t * GubElec_{it}) \\
&+ \theta_3(\%\Delta OilPrice_t * NoGubElec_{it}) \\
&+ \pi_1 \%\Delta CorpProf_t + \pi_2(\%\Delta CorpProf_t * GubElec_{it}) \\
&+ \pi_3(\%\Delta CorpProf_t * NoGubElec_{it}) + \varepsilon_{it}]
\end{aligned} \tag{6d}
$$

Again, focusing first on the main effects, we see in in figure 7 that enacted and implemented taxes are least likely and smallest, respectively, in an election year, particularly for gasoline taxes.

Regarding the interaction terms of our gubernatorial election indicator with oil prices and corporate profits (θ_2 and π_2), figure 8 shows no statistically significant estimates, so we conclude that U.S. macroeconomic conditions have no effect on tax implementation or tax enactment during state gubernatorial election years, though the interaction term in the implemented gas tax model is rather imprecise.

There is some evidence that, when it is at least two years after a gubernatorial election, higher oil prices are associated with higher implemented (but not enacted) taxes. For the gasoline tax, a 1 percentage point increase in oil prices at least two years after a gubernatorial election causes a 0.86 cent per gallon increase in implemented gasoline taxes ($p = 0.03$),

shown in figure 9. The same effect for the CIT is 0.2 percentage points and more imprecise ($p = 0.07$). That said, we placed a lower (pre-specified) weight on these models as well, as equations 6a through 6d lack time fixed effects, so there may be omitted factors that confound these estimates.

B.3 Do the effects of elections on tax implementation or enactment depend on state-level political or economic conditions? No.

In our final family of pre-specified models that investigate whether the propensity for politicians to enact or implement taxes is heterogeneous, equations 7a through 7d interact our electoral cycle indicators with three state-level political and economic indicators: ADA score, gross state product (GSP), and the unemployment rate:

$$\begin{aligned}\Delta GasTax_{it} = {} & \alpha_i + \lambda_t + \beta_1 GubElec_{it} + \beta_2 NoGubElec_{it} \\ & + \theta_1 \%\Delta GSP_{it} + \theta_2(\%\Delta GSP_{it} * GubElec_{it}) + \theta_3(\%\Delta GSP_{it} * NoGubElec_{it}) \\ & + \pi_1 UnempRate_{it} + \pi_2(UnempRate_{it} * GubElec_{it}) + \pi_3(UnempRate_{it} * NoGubElec_{it}) \\ & + \phi_1 VoterPref_{it} + \phi_2(VoterPref_{it} * GubElec_{it}) \\ & + \phi_3(VoterPref_{it} * NoGubElec_{it}) + \varepsilon_{it}\end{aligned} \tag{7a}$$

$$\begin{aligned}\Delta CIT_{it} = {} & \alpha_i + \lambda_t + \beta_1 GubElec_{it} + \beta_2 NoGubElec_{it} \\ & + \theta_1 \%\Delta GSP_{it} + \theta_2(\%\Delta GSP_{it} * GubElec_{it}) + \theta_3(\%\Delta GSP_{it} * NoGubElec_{it}) \\ & + \pi_1 UnempRate_{it} + \pi_2(UnempRate_{it} * GubElec_{it}) + \pi_3(UnempRate_{it} * NoGubElec_{it}) \\ & + \phi_1 VoterPref_{it} + \phi_2(VoterPref_{it} * GubElec_{it}) \\ & + \phi_3(VoterPref_{it} * NoGubElec_{it}) + \varepsilon_{it}\end{aligned} \tag{7b}$$

$$\begin{aligned}I[GasTaxBillIncrease_{it} > 0] = \Phi[& \alpha_i + \lambda_t + \beta_1 GubElec_{it} + \beta_2 NoGubElec_{it} + \theta_1 \%\Delta GSP_{it} \\ & + \theta_2(\%\Delta GSP_{it} * GubElec_{it}) + \theta_3(\%\Delta GSP_{it} * NoGubElec_{it}) \\ & + \pi_1 UnempRate_{it} + \pi_2(UnempRate_{it} * GubElec_{it}) \\ & + \pi_3(UnempRate_{it} * NoGubElec_{it}) \\ & + \phi_1 VoterPref_{it} + \phi_2(VoterPref_{it} * GubElec_{it}) \\ & + \phi_3(VoterPref_{it} * NoGubElec_{it}) + \varepsilon_{it}]\end{aligned} \tag{7c}$$

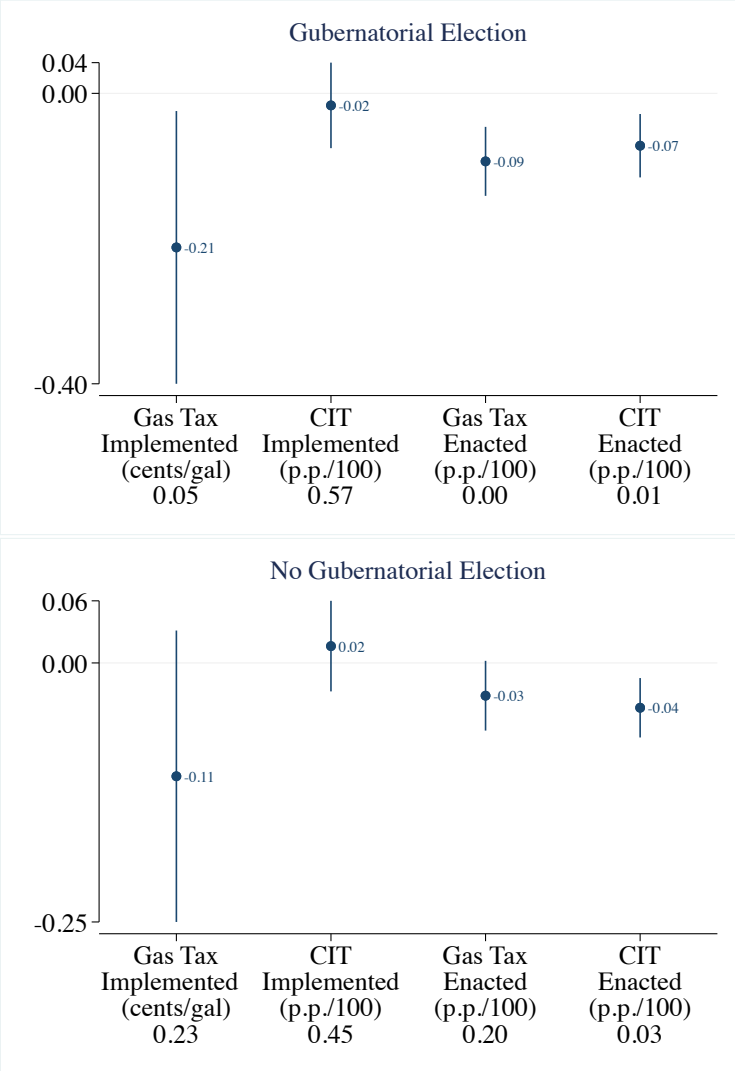
$$\begin{aligned}I[CITBillIncrease_{it} > 0] = \Phi[& \alpha_i + \lambda_t + \beta_1 GubElec_{it} + \beta_2 NoGubElec_{it} + \theta_1 \%\Delta GSP_{it} \\ & + \theta_2(\%\Delta GSP_{it} * GubElec_{it}) + \theta_3(\%\Delta GSP_{it} * NoGubElec_{it}) \\ & + \pi_1 UnempRate_{it} + \pi_2(UnempRate_{it} * GubElec_{it}) \\ & + \pi_3(UnempRate_{it} * NoGubElec_{it}) \\ & + \phi_1 VoterPref_{it} + \phi_2(VoterPref_{it} * GubElec_{it}) \\ & + \phi_3(VoterPref_{it} * NoGubElec_{it}) + \varepsilon_{it}]\end{aligned} \tag{7d}$$

The main electoral effects, shown in figure 10, indicate that none of the models have an

electoral effect. This family of pre-specified models is the only family that finds no electoral effects. Furthermore, in figures 11 and 12, the interaction terms of our electoral variables with GSP, the unemployment rate, and the ADA score are not significant.

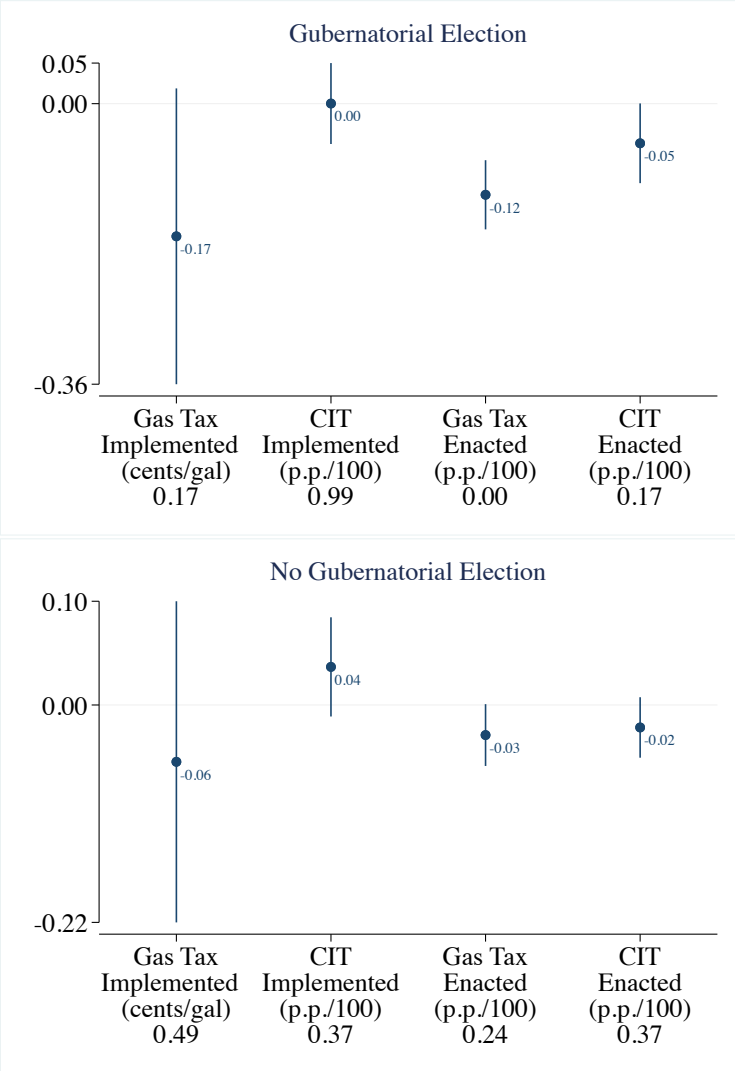
C Figures and Tables

Figure 1: Elections Affect Tax Implementation and Enactment



Description: Average marginal effects from models 1a through 1d of our electoral cycle indicators on tax implementation and tax enactment. The top panel shows results for gubernatorial election years and the bottom panel shows the results for being in years 2 or 3 after a gubernatorial election, so the omitted category is the year after an election. Whiskers are 95 percent conventional confidence intervals. FWER adjusted p-values for the null hypothesis that the marginal effect is equal to zero versus not equal to zero are shown in the bottom row of text for each model in each panel.
Interpretation: Implemented gas taxes are smallest in an election year, there is no similar electoral effect for the CIT. Both the gas tax and the CIT are least likely to be enacted in an election year and are most likely to be enacted in the year after the election.

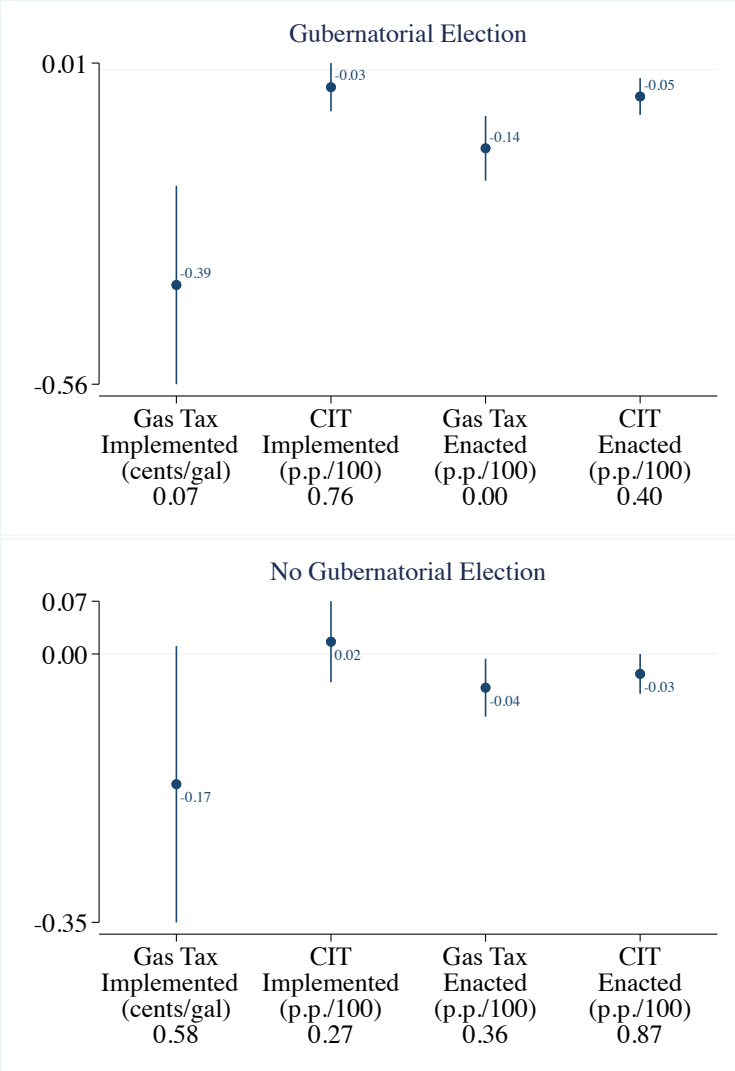
Figure 2: Elections Affect Tax Implementation and Enactment (Continued)



Description: Average marginal effects from models 2a through 2d of our electoral cycle indicators on tax implementation and tax enactment. The top panel shows results for gubernatorial election years and the bottom panel shows the results for being in years 2 or 3 after a gubernatorial election, so the omitted category is the year after an election. Whiskers are 95 percent conventional confidence intervals. FWER adjusted p-values for the null hypothesis that the marginal effect is equal to zero versus not equal to zero are shown in the bottom row of text for each model in each panel.

Interpretation: Our models with additional control variables still show electoral effects on tax implementation and enactment. Implemented gas taxes are smallest in an election year, but we do not find a similar electoral effect for the CIT. Both the gas tax and the CIT are least likely to be enacted in an election year and are most likely to be enacted in the year after an election.

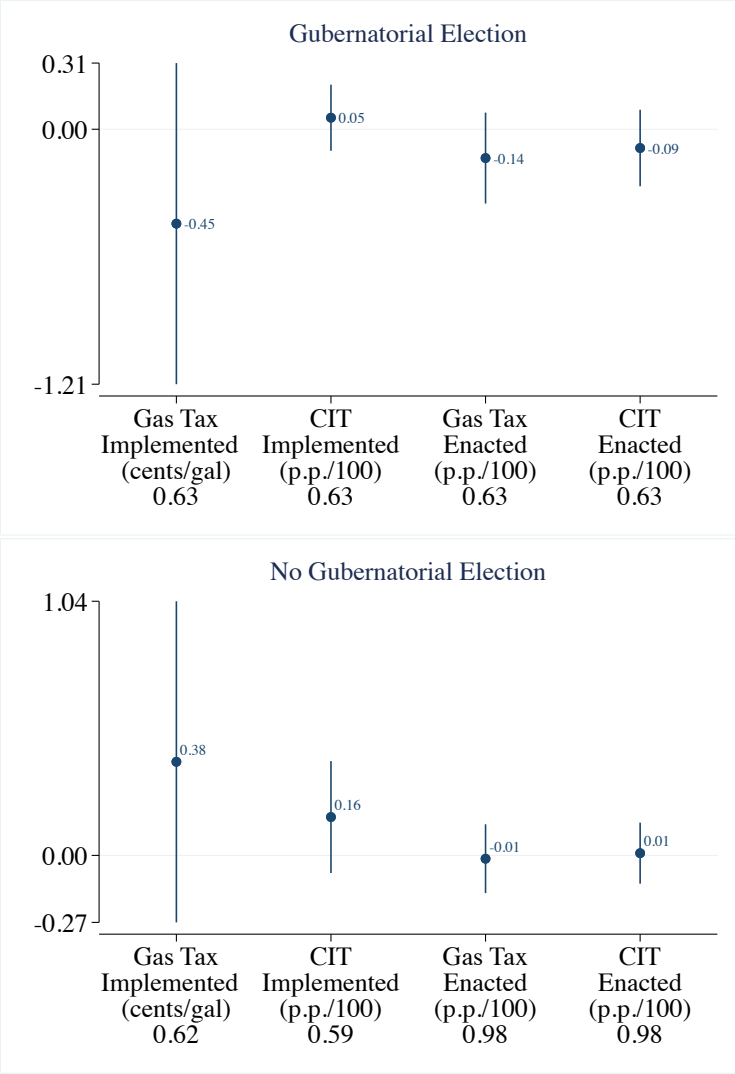
Figure 3: Election Effects Persist after Removing Fixed Effects



Description: Average marginal effects from models 3a through 3d of our electoral cycle indicators on tax implementation and tax enactment. The top panel shows results for gubernatorial election years and the bottom panel shows the results for being in years 2 or 3 after a gubernatorial election, so the omitted category is the year after an election. Whiskers are 95 percent conventional confidence intervals. FWER adjusted p-values for the null hypothesis that the marginal effect is equal to zero versus not equal to zero are shown in the bottom row of text for each model in each panel.

Interpretation: Our models without fixed effects, but that use time or state-invariant variables control instead, still show electoral effects on tax implementation and enactment. But the estimated effects are stronger for gas taxes and weaker for the CIT relative to our main specifications. There is still no evidence of an electoral effect on CIT implementation.

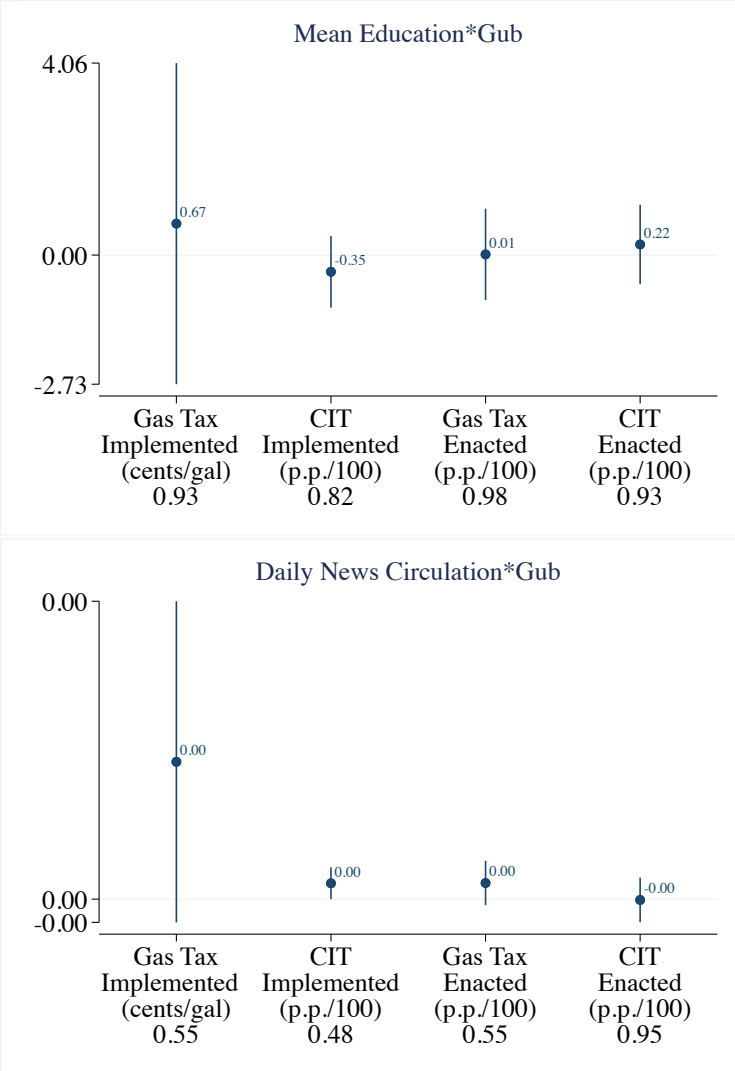
Figure 4: Implemented and Enacted Gas Taxes Are Smallest in an Election Year



Description: Average marginal effects from models 5a through 5d of our electoral cycle indicators on tax implementation and tax enactment. The top panel shows results for gubernatorial election years and the bottom panel shows the results for being in years 2 or 3 after a gubernatorial election, so the omitted category is the year after an election. Whiskers are 95 percent conventional confidence intervals. FWER adjusted p-values for the null hypothesis that the marginal effect is equal to zero versus not equal to zero are shown in the bottom row of text for each model in each panel.

Interpretation: Relative to our main and secondary specifications, the estimates are more imprecise, but the point estimates still suggest implemented and enacted gas taxes are smallest in an election year. We find no similar electoral effect for the CIT.

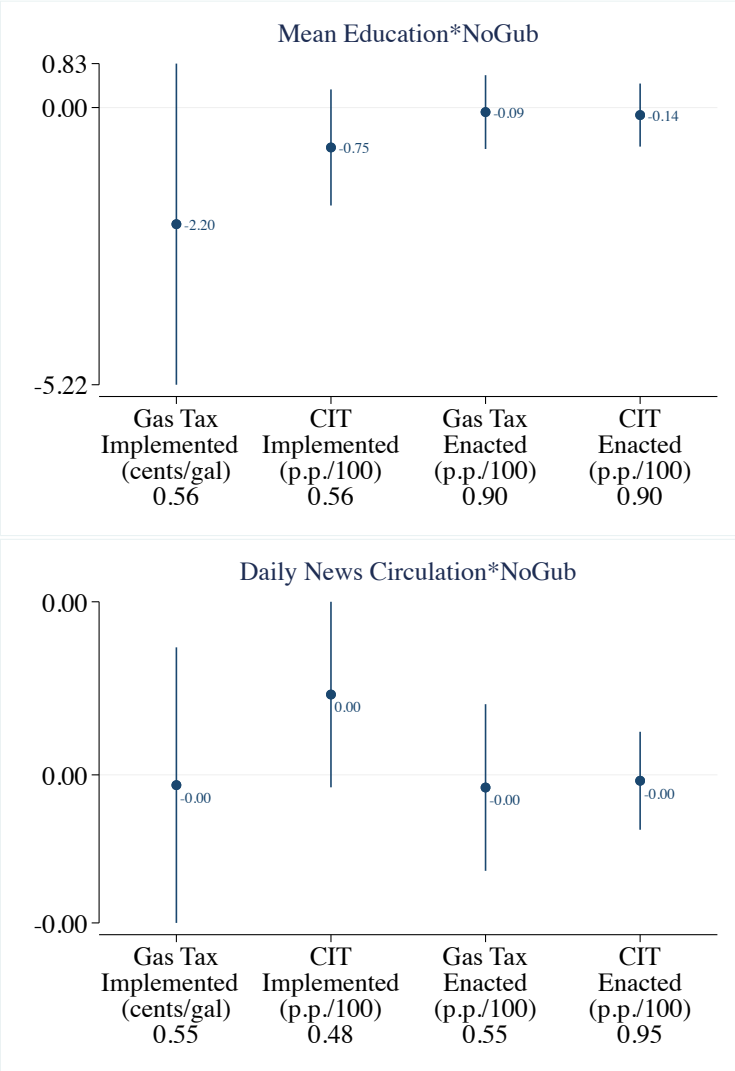
Figure 5: Gubernatorial Election Effect Is Not Dependent on Voter Attentiveness



Description: Average marginal effects from models 5a through 5d of our gubernatorial election indicator interacted with two proxies for voter attentiveness (θ_2 and π_2). The top panel shows results for an interaction with average education level and the bottom panel shows an interaction with average newspaper circulation. Whiskers are 95 percent conventional confidence intervals. FWER adjusted p-values for the null hypothesis that the marginal effect is equal to zero versus not equal to zero are shown in the bottom row of text for each model in each panel.

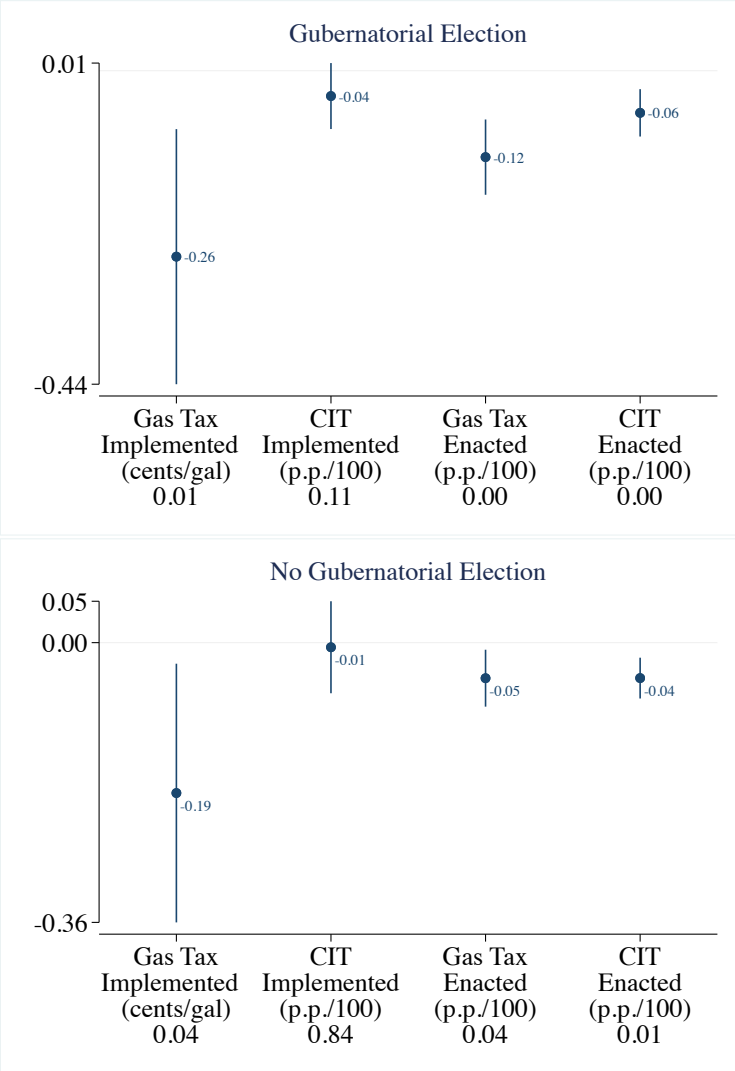
Interpretation: We find that the effect of gubernatorial elections, or lack thereof, on tax enactment or implementation does not depend on a state's voter attentiveness.

Figure 6: Taxes Do Not Depend on Voter Attentiveness More Than Two Years after an Election



Description: Average marginal effects from models 5a through 5d of our indicator for being at least two years from a gubernatorial election interacted with with two proxies for voter attentiveness (θ_3 and π_3). The top panel shows results for an interaction with average education level and the bottom panel shows an interaction with average newspaper circulation. Whiskers are 95 percent conventional confidence intervals. FWER adjusted p-values for the null hypothesis that the marginal effect is equal to zero versus not equal to zero are shown in the bottom row of text for each model in each panel.
Interpretation: We do not find that tax enactment or implementation at least two years after a gubernatorial election depends on a state's voter attentiveness.

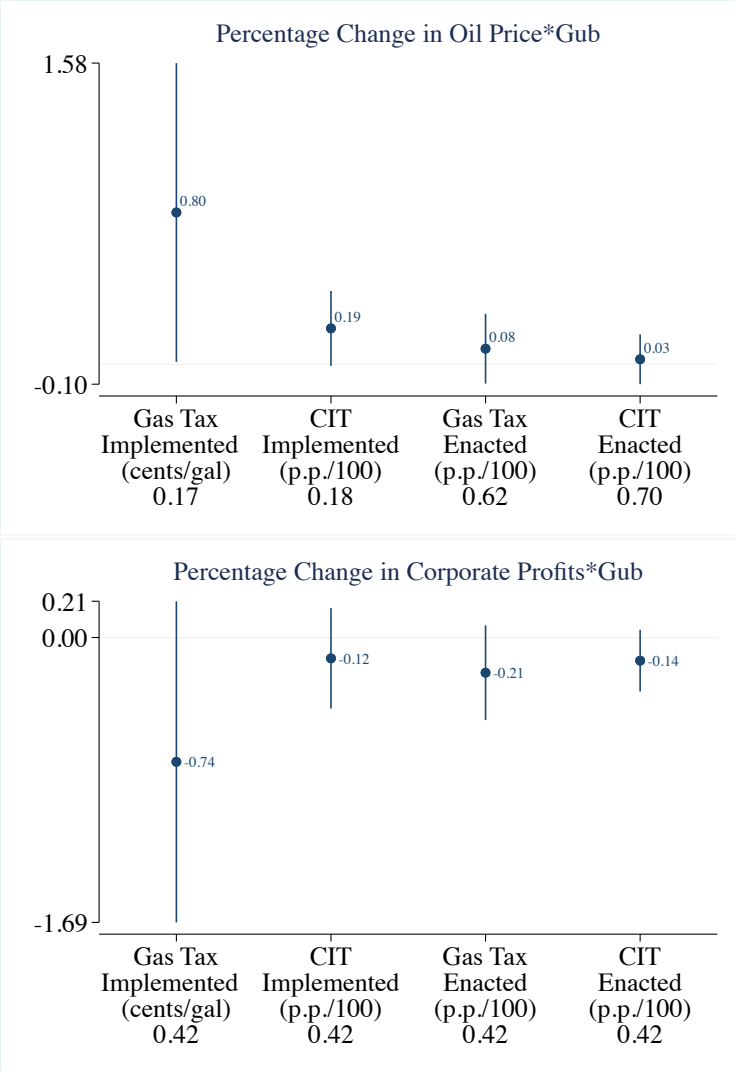
Figure 7: Gas Taxes and the CIT Are Least Likely to Be Enacted in an Election Year



Description: Average marginal effects from models 6a through 6d of our electoral cycle indicators on tax implementation and tax enactment. The top panel shows results for gubernatorial election years and the bottom panel shows the results for being in years 2 or 3 after a gubernatorial election, so the omitted category is the year after an election. Whiskers are 95 percent conventional confidence intervals. FWER adjusted p-values for the null hypothesis that the marginal effect is equal to zero versus not equal to zero are shown in the bottom row of text for each model in each panel.

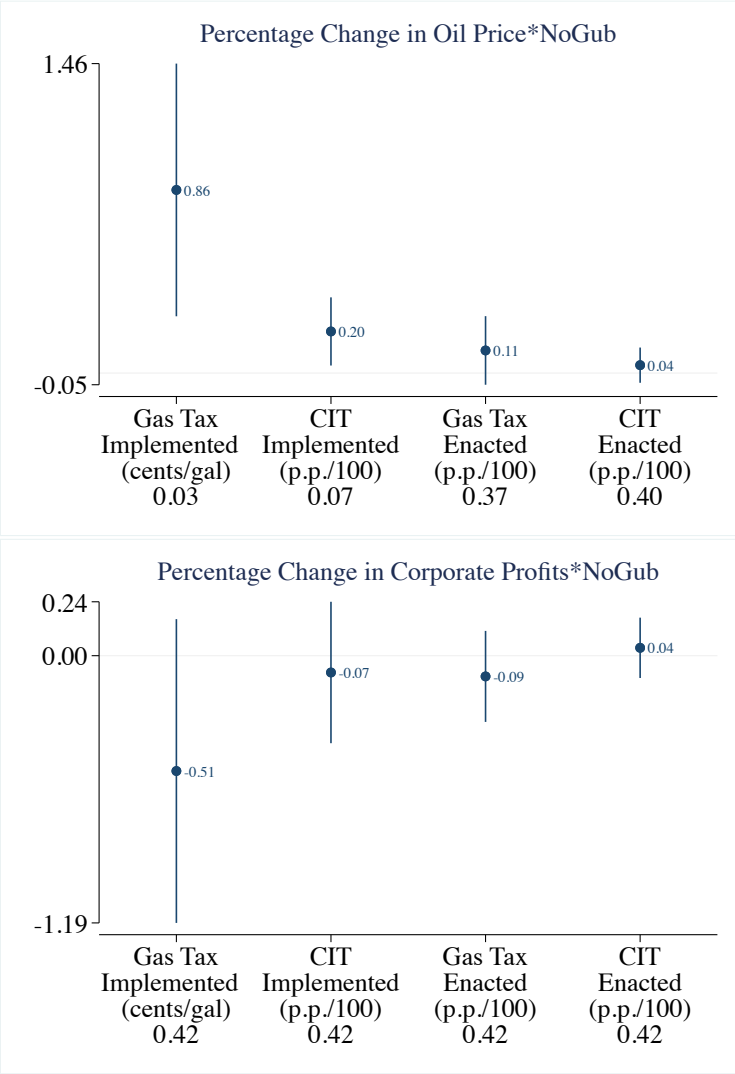
Interpretation: The results from this model are similar to those from our main specifications. Implemented gas taxes are smallest in an election year, but we find no similar electoral effect for the CIT. Both the gas tax and the CIT are least likely to be enacted in an election year and are most likely to be enacted in the year after the election.

Figure 8: Election Year Effect Does Not Depend on U.S. Macroeconomic Conditions



Description: Average marginal effects from models 6a through 6d of our gubernatorial election indicator interacted with two measures of U.S. macroeconomic conditions: oil prices and corporate profits (θ_2 and π_2, respectively) on tax implementation and tax enactment. The top panel shows results for oil prices and the bottom panel shows those for corporate profits. Whiskers are 95 percent conventional confidence intervals. FWER adjusted p-values for the null hypothesis that the marginal effect is equal to zero versus not equal to zero are shown in the bottom row of text for each model in each panel.
Interpretation: The effect of elections on tax implementation or enactment at the state level does not depend on U.S. macroeconomic conditions.

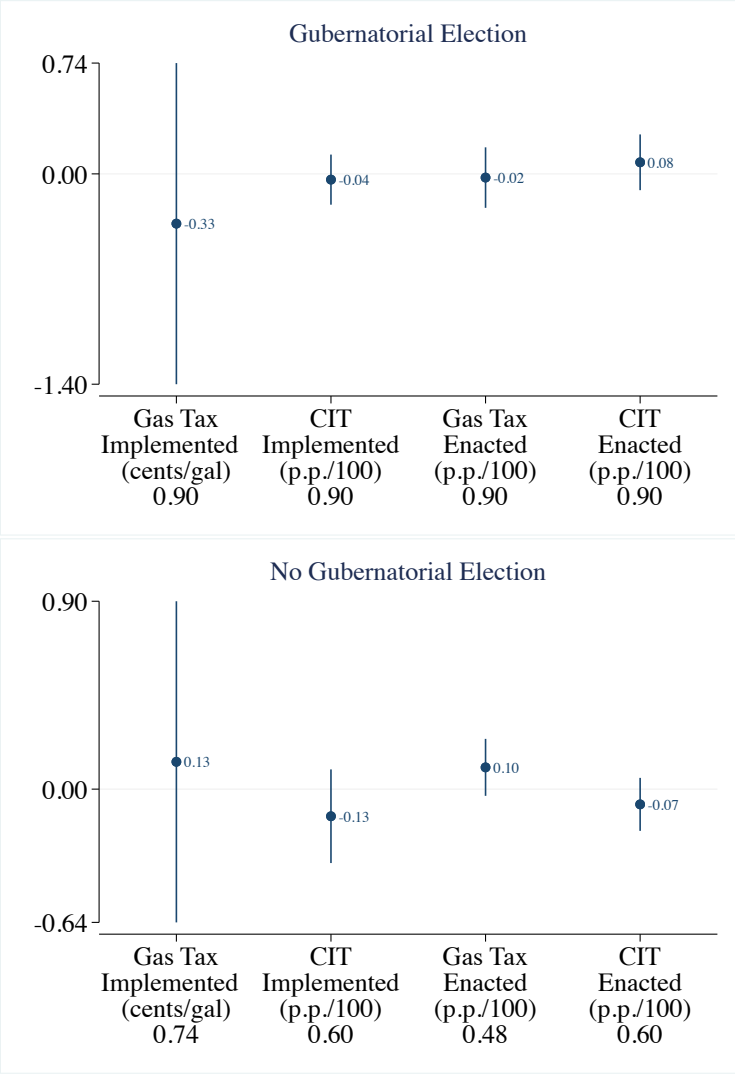
Figure 9: Tax Implementation Two or More Years after an Election May Depend on U.S. Macroeconomic Conditions, but Enactment Does Not



Description: Average marginal effects from models 6a through 6d of our no gubernatorial election indicator interacted with two measures of U.S. macroeconomic conditions: oil prices and corporate profits (θ_3 and π_3, respectively) on tax implementation and tax enactment. The top panel shows results for oil prices and the bottom panel shows those for corporate profits. Whiskers are 95 percent conventional confidence intervals. FWER adjusted p-values for the null hypothesis that the marginal effect is equal to zero versus not equal to zero are shown in the bottom row of text for each model in each panel.

Interpretation: At least two years after a state gubernatorial election, tax implementation is more likely when oil prices are higher. Tax enactment does not depend on U.S. macroeconomic conditions.

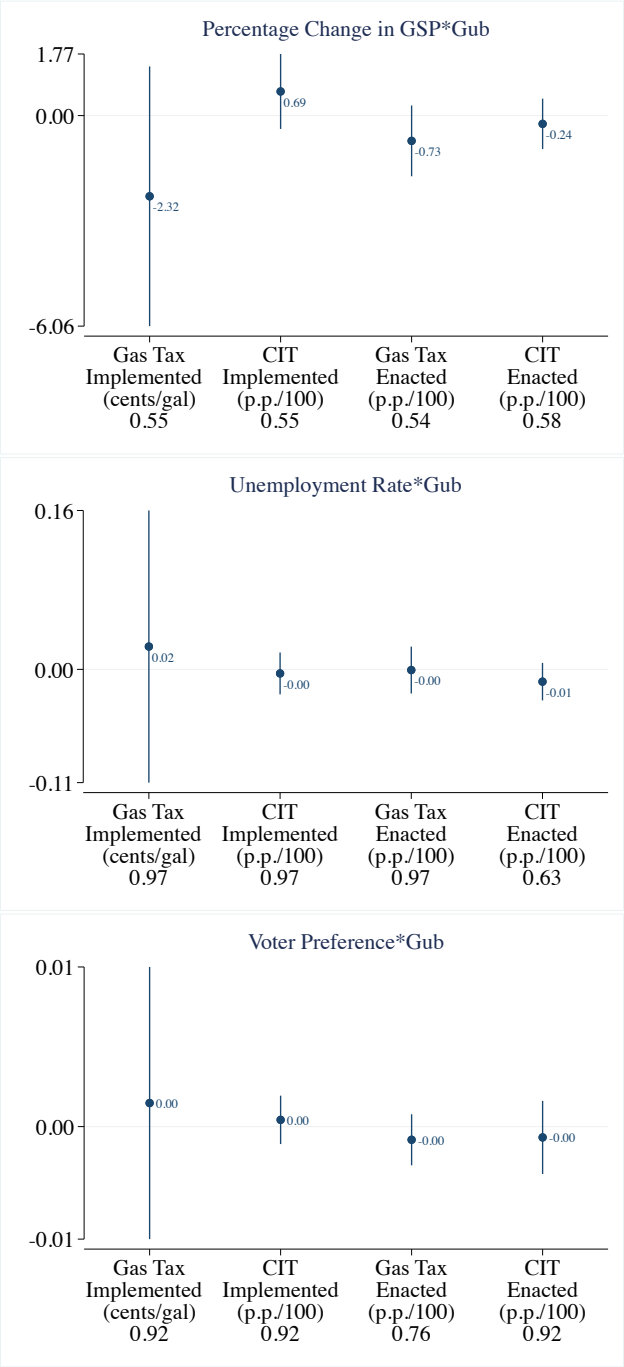
Figure 10: No Electoral Effects on Tax Implementation or Enactment



Description: Average marginal effects from models 7a through 7d of our electoral cycle indicators on tax implementation and tax enactment. The top panel shows results for gubernatorial election years and the bottom panel shows the results for being in years 2 or 3 after a gubernatorial election, so the omitted category is the year after an election. Whiskers are 95 percent conventional confidence intervals. FWER adjusted p-values for the null hypothesis that the marginal effect is equal to zero versus not equal to zero are shown in the bottom row of text for each model in each panel.

Interpretation: Relative to our main and secondary specifications, the estimates are more imprecise. This set of models is the only set where we find no electoral effects on tax implementation or enactment.

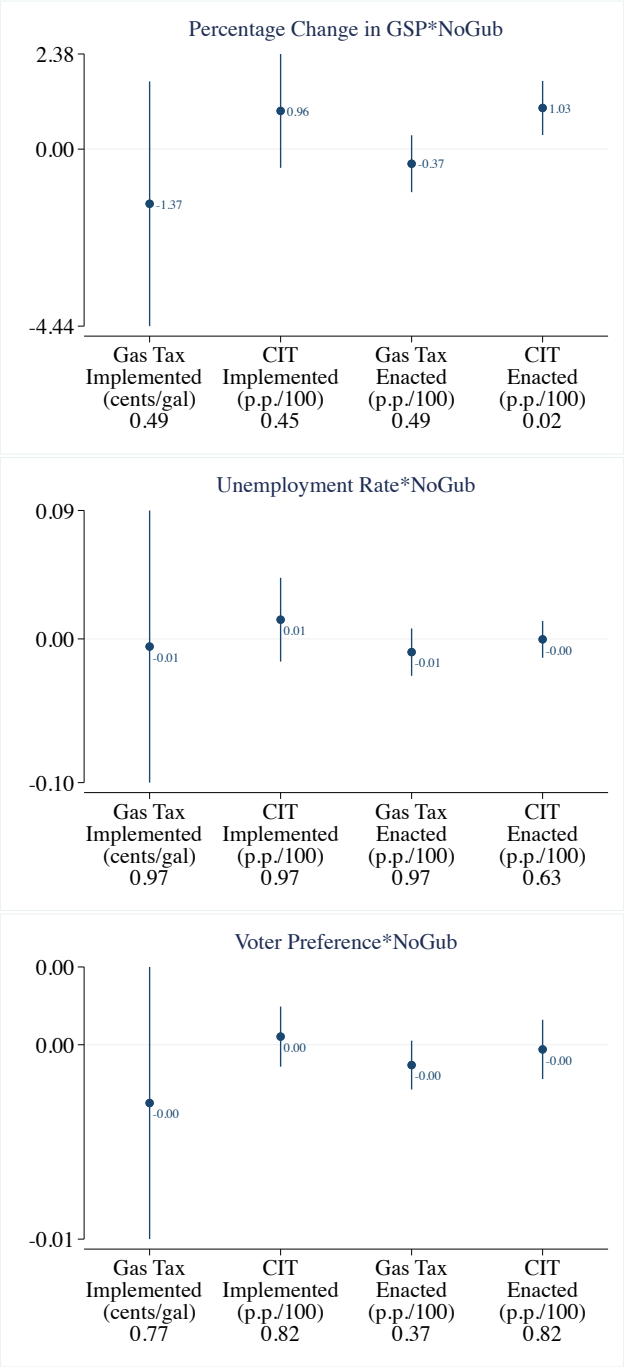
Figure 11: No Effect of State Political or Economic Conditions in Gubernatorial Election Years



Description: Average marginal effects from models 7a through 7d of our gubernatorial election indicator interacted with measures of state economic and political conditions. Whiskers are 95 percent conventional confidence intervals. FWER adjusted p-values for the null hypothesis that the marginal effect is equal to zero versus not equal to zero are shown in the bottom row of text for each model in each panel.

Interpretation: We find that the effect of elections on tax enactment or implementation does not depend on state economic or political conditions.

Figure 12: No Effect of State Political or Economic Conditions on Tax Implementation or Enactment at Least Two Years after an Election



Description: Average marginal effects from models 7a through 7d of our no gubernatorial election indicator interacted with measures of state economic and political conditions. Whiskers are 95 percent conventional confidence intervals. FWER adjusted p-values for the null hypothesis that the marginal effect is equal to zero versus not equal to zero are shown in the bottom row of text for each model in each panel.

Interpretation: We find that neither tax implementation or enactment is affected by state economic or political conditions at least two years after a gubernatorial election.

Table 1: Descriptive Statistics

Variable	**N**	**Mean**	**Stdev**	**Median**	**Min**	**Max**
Gas Tax Level Change (cent/gal)	2,800	0.34	1.26	0.00	-12.00	20.85
CIT Change (p.p./100)	2,800	0.05	0.51	0.00	-5.45	8.00
Gas Tax Bill Increase	2,850	0.13	0.34	0.00	0.00	1.00
CIT Bill Increase	2,850	0.06	0.24	0.00	0.00	1.00
Gubernatorial election year	2,800	0.28	0.45	0.00	0.00	1.00
Democratic Governor	2,850	0.50	0.50	1.00	0.00	1.00
Republican Governor	2,850	0.40	0.49	0.00	0.00	1.00
Education (% College or higher)	2,850	0.20	0.04	0.20	0.14	0.30
News Circulation (total/day)	2,850	912.55	1235.70	521.80	82.27	6040.56
Term Length (years)	2,250	3.75	0.66	4.00	2.00	4.00
%Δ Oil Price	2,800	7.94	26.01	2.23	-49.20	76.61
%Δ Corporate Profits	2,800	7.45	16.04	8.70	-27.91	43.73
%Δ Transportation Usage	2,300	1.04	2.83	0.57	-4.91	9.95
%Δ GSP per capita	2,650	6.85	4.75	6.39	-26.63	55.54
Avg. Unemployment Rate (%)	2,050	6.03	2.06	5.69	2.30	17.79
ADA score ($>$ 50 - more liberal)	2,800	48.80	21.89	47.60	-1.51	103.11
%Δ Population	2,800	1.09	1.22	0.85	-5.99	12.78
%Δ Drivers	2,700	1.80	3.73	1.49	-24.37	35.56
%Δ Gas Usage	2,800	1.69	4.03	1.80	-22.31	27.35
%Δ Income per capita	2,800	5.73	3.68	5.47	-10.28	40.95

Description: Descriptive statistics of all relevant variables are displayed in this table. %Δ is percent change.

Table 2: Secondary Specification Controls

	Gas Tax Imp.	CIT Imp.	Gas Tax Enac.	CIT Enac.
	(1)	(2)	(3)	(4)
	(cents/gal)	(p.p./100)	(p.p./100)	(p.p./100)
%Δ GSP per capita	0.97	-0.05	0.08	0.08
	(0.83)	(0.62)	(0.31)	(0.31)
Avg. Unemployment Rate (%)	0.04	0.00	-0.00	0.02
	(0.03)	(0.01)	(0.01)	(0.01)
ADA score (> 50 - more liberal)	0.00	-0.00	0.00	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)
%Δ Population	0.12	-1.23	-0.08	-0.28
	(3.70)	(1.62)	(1.28)	(1.54)
%Δ Drivers	-0.90	-0.31	-0.24	-0.49
	(0.97)	(0.22)	(0.22)	(0.25)
%Δ Gas Usage	-0.88	0.20	-0.55	-0.02
	(0.74)	(0.30)	(0.32)	(0.30)
%Δ Income per capita	-4.00	-1.00	-1.32	-0.06
	(1.82)	(0.67)	(0.38)	(0.51)
Adjusted R^2	0.04	0.01		
pseudo-R^2			0.15	0.16
Observations	1950	1950	1824	1080

Description: Marginal effects of control variables from equations 2a through 2d. Clustered standard errors by state in parentheses. Statistical significance asterisks omitted.

Table 3: Tertiary Specification Controls

	Gas Tax Imp.	CIT Imp.	Gas Tax Enac.	CIT Enac.
	(1)	(2)	(3)	(4)
	(cents/gal)	(p.p./100)	(p.p./100)	(p.p./100)
Education (% College or higher)	0.25	0.05	-0.32	0.23
	(0.80)	(0.19)	(0.28)	(0.18)
News Circulation (total/day)	-0.00	-0.00	-0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)
Term Length (years)	-0.01	-0.03	-0.02	-0.02
	(0.06)	(0.01)	(0.01)	(0.01)
%Δ Oil Price	-0.08	0.01	-0.03	-0.02
	(0.12)	(0.04)	(0.04)	(0.02)
%Δ Corporate Profits	-0.21	-0.06	-0.03	-0.03
	(0.22)	(0.06)	(0.06)	(0.04)
%Δ Transportation Usage	2.08	-0.45	-0.15	0.07
	(1.44)	(0.30)	(0.40)	(0.23)
%Δ GSP per capita	1.35	0.16	-0.06	0.04
	(1.24)	(0.53)	(0.21)	(0.14)
Avg. Unemployment Rate (%)	0.05	0.02	0.01	0.01
	(0.02)	(0.01)	(0.00)	(0.00)
ADA score (> 50 - more liberal)	0.00	-0.00	0.00	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)
%Δ Population	-3.56	-0.94	-1.21	-1.30
	(3.60)	(1.12)	(0.92)	(0.59)
%Δ Drivers	-1.23	-0.23	-0.29	-0.28
	(1.18)	(0.18)	(0.27)	(0.13)
%Δ Gas Usage	-1.21	-0.08	-0.69	-0.09
	(0.94)	(0.26)	(0.30)	(0.19)
%Δ Income per capita	-2.75	-0.31	-0.05	0.04
	(1.73)	(0.54)	(0.38)	(0.27)
Adjusted R^2	0.02	0.00		
pseudo-R^2			0.05	0.09
Observations	1450	1450	1450	1450

Description: Marginal effects of control variables from equations 3a through 3d. Clustered standard errors by state in parentheses. Statistical significance asterisks omitted.

Table 4: Election Effect Heterogeneity by Voter Attentiveness Controls

	Gas Tax Imp.	CIT Imp.	Gas Tax Enac.	CIT Enac.
	(1)	(2)	(3)	(4)
	(cents/gal)	(p.p./100)	(p.p./100)	(p.p./100)
%Δ GSP per capita	0.63	-0.07	-0.07	-0.00
	(0.88)	(0.62)	(0.20)	(0.22)
Avg. Unemployment Rate (%)	0.01	0.00	-0.01	0.01
	(0.02)	(0.01)	(0.01)	(0.00)
ADA score (> 50 - more liberal)	0.00	-0.00	0.00	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)
%Δ Population	-0.51	-1.27	-0.92	-1.82
	(2.55)	(1.23)	(0.73)	(0.84)
%Δ Drivers	-1.09	-0.29	-0.26	-0.37
	(0.94)	(0.21)	(0.19)	(0.18)
%Δ Gas Usage	-1.06	0.17	-0.46	-0.09
	(0.69)	(0.30)	(0.26)	(0.25)
%Δ Income per capita	-3.72	-0.93	-1.18	0.06
	(1.79)	(0.65)	(0.34)	(0.41)
Education (% College or higher)	0.26	0.52	-0.52	0.52
	(1.33)	(0.36)	(0.29)	(0.32)
News Circulation (total/day)	-0.00	-0.00	-0.00	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)
Adjusted R^2	0.06	0.02		
pseudo-R^2			0.13	0.11
Observations	1950	1950	1900	1350

Description: Marginal effects of control variables from equations 5a through 5d. Clustered standard errors by state in parentheses. Statistical significance asterisks omitted.

Table 5: Election Effect Heterogeneity by Macroeconomic Conditions Controls

	Gas Tax Imp.	CIT Imp.	Gas Tax Enac.	CIT Enac.
	(1)	(2)	(3)	(4)
	(cents/gal)	(p.p./100)	(p.p./100)	(p.p./100)
%Δ GSP per capita	1.51	0.15	0.73	0.23
	(1.01)	(0.53)	(0.25)	(0.18)
Avg. Unemployment Rate (%)	0.04	0.02	0.01	0.02
	(0.02)	(0.01)	(0.00)	(0.00)
ADA score (> 50 - more liberal)	0.00	-0.00	0.00	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)
%Δ Population	-7.00	-0.29	-1.93	-0.42
	(4.23)	(1.29)	(1.04)	(0.88)
%Δ Drivers	-0.81	-0.13	-0.22	-0.22
	(1.02)	(0.21)	(0.25)	(0.14)
%Δ Gas Usage	-0.87	0.01	-0.63	-0.09
	(0.73)	(0.26)	(0.28)	(0.19)
%Δ Income per capita	-0.90	0.05	-0.22	-0.02
	(1.03)	(0.47)	(0.29)	(0.24)
%Δ Oil Price	-0.75	-0.15	-0.14	-0.05
	(0.29)	(0.09)	(0.07)	(0.05)
%Δ Corporate Profits	0.45	0.00	0.04	-0.04
	(0.31)	(0.12)	(0.09)	(0.07)
Adjusted R^2	-0.00	-0.00		
pseudo-R^2			0.07	0.15
Observations	1950	1950	1872	1560

Description: Marginal effects of control variables from equations 6a through 6d. Clustered standard errors by state in parentheses. Statistical significance asterisks omitted.

Table 6: Election Effect Heterogeneity by State-level Political or Economic Conditions Controls

	Gas Tax Imp.	CIT Imp.	Gas Tax Enac.	CIT Enac.
	(1)	(2)	(3)	(4)
	(cents/gal)	(p.p./100)	(p.p./100)	(p.p./100)
%Δ GSP per capita	1.39	-1.04	-0.18	-0.48
	(1.43)	(0.74)	(0.44)	(0.28)
Avg. Unemployment Rate (%)	0.06	0.00	0.01	0.02
	(0.04)	(0.01)	(0.01)	(0.01)
ADA score (> 50 - more liberal)	0.00	-0.00	0.00	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)
Adjusted R^2	0.03	0.01		
pseudo-R^2			0.15	0.17
Observations	2000	2000	1911	1080

Description: Marginal effects of control variables from equations 7a through 7d. Clustered standard errors by state in parentheses. Statistical significance asterisks omitted.