

Attribution Errors in Federalist Systems: When Voters Punish the President for Local Tax Increases

Michael W. Sances, University of Memphis

How do voters attribute blame when policy responsibility is shared? While central to accountability, this question is difficult to answer because “who does what” is often ambiguous. This article exploits a case where policy responsibility is unambiguous: local tax referendums. Although presidents have no control over property taxes or the decision to raise local rates, I find that voters punish the president’s party for tax increases enacted via direct democracy. This effect is robust to adjusting for population-based measures of the local economy, as well as panel and discontinuity designs to account for unobserved factors. The effect varies with the magnitude of the tax increase but not with local economic performance, suggesting that voters react to the change in spending money, as opposed to being “primed” to consider national issues. Thus, voters punish officials not only for events that no one controls but also for policies that voters themselves enact.

Do voters correctly attribute blame to different actors? Although political scientists have found that voters punish officials for events that no one controls, whether voters punish one political actor for the actions of another is a much more difficult question to answer. The reason is that assigning responsibility for policy decisions—as opposed to ruling out responsibility for events that no one controls—is often highly subjective, even for social scientists. As Dahl (2002, 115) asks:

Where are we to place responsibility for the conduct of our government? When we go to the polls, who can we hold accountable for the successes and failures of national policies? The president? The House? The Senate? The unelected Supreme Court? Or, given our federal system, the states, where governments are, in their complexity, a microcosm of the national government? . . . Even for those who spend their lives studying politics, these can be extremely difficult questions to answer.

While such knowledge is hard to obtain, it is also essential for assessing voters’ own capacity for blame attribution.

In this article, I study attribution in a setting where responsibility should be crystal clear to both voters and researchers. In towns in Massachusetts, citizens frequently vote in referendums on whether to raise the local property tax rate. Exploiting this fact, I ask whether voters punish the incumbent president’s party for voter-imposed local tax increases. Because property taxes are solely a local issue, and because the increases are the result of decisions made by voters themselves, this case offers a clear benchmark of what correct policy attribution would be. Namely, correct attribution precludes any effect of these increases on presidential vote share. Moreover, because the president’s lack of responsibility should be so obvious, the results of this test are likely to extend to other cases where assigning policy responsibility is harder.

My results cast doubt on voters’ ability to correctly attribute blame, even in this extremely simple setting. On average, a voter-imposed tax increase leads to a nearly 2 percentage point decline in the incumbent party’s vote share. The effect is robust to adjusting for time-varying covariates, a panel design that accounts for pre-increase trends in vote share, and a regression discontinuity design that exploits the quasi-random nature of close referendum votes. The effect is also not the result of local referendums raising the salience of national issues. Rather, voters appear to be reacting to the change in

Michael W. Sances (msances@memphis.edu) is an assistant professor in the Department of Political Science at the University of Memphis, Memphis, TN 38152.

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their financial situation that tax increases cause, ignoring the fact that they themselves (or, no less worrisome for accountability, their neighbors) caused this change.

These findings have important implications for the study of accountability. For one, they provide the most direct evidence that voters punish some policy makers for the actions of others. Additionally, they add to a growing literature showing that leaders are often judged on events beyond their control. In this case, however, the event in question is an actual policy decision. Given the potentially systemic effects on performance and the incentives it creates for some leaders to actively underperform, this result is in fact more troubling for democracy than punishment for random events.

SHARED POWERS, RETROSPECTIVE VOTING, AND ATTRIBUTION

In the standard model of retrospective voting, voters observe events, judge how much responsibility to assign for each event to different actors, and then use these responsibility attributions to form a vote choice (Anderson 2007; Healy and Malhotra 2013). Assuming that voters perfectly observe events, politicians will be incentivized to take actions to improve voters' welfare, realizing that the likelihood of retaining office is higher when voters are better off. While rarely explicitly incorporated into this model, it is sometimes argued that the presence of multiple actors will enhance accountability. In addition to preventing tyrannical concentrations of power in one branch or level, splitting power across multiple actors is thought to enhance democratic accountability by giving voters multiple venues for participation. Should one actor be unresponsive to the popular will, voters can simply appeal to another (Arceneaux 2006).

For these benefits to be realized, however, voters must be capable of making distinctions between government actors. In the retrospective voting model, this means that voters must correctly assign responsibility for events to different officials. For instance, officials should only be held accountable for the events that they themselves caused and not for the events that other actors caused. As noted by several scholars, this can break down if acquiring responsibility information is too costly for voters or if politicians actively seek to "pass the buck" to other officials (Arceneaux 2006; Cutler 2004; Powell and Whitten 1993). Indeed, at least as long as scholars have celebrated federalism's benefits, so too they have worried about voters' ability, given institutional constraints, to meet its core requirement. Even Alexander Hamilton, who lauded the benefits of shared powers, also worried about its dangers in *Federalist* 70:

It often becomes impossible, amid mutual accusations, to determine on whom the blame or the punishment of a

pernicious measure, or series of pernicious measures, ought really to fall. It is shifted from one to another with so much dexterity, and under such plausible appearances, that the public opinion is left in suspense about the real author.¹

Beyond simply undermining federalism's potential benefits, the difficulty of attributing blame also creates problems of its own. If officials know they are being judged on events outside of their control, they will have less of an incentive to exert effort (Achen and Bartels 2016; Patty and Weber 2007; however, see Ashworth and Bueno de Mesquita 2014). While these problems are well known in single-actor studies of accountability, which often find leaders punished for natural disasters and other random events (Healy and Malhotra 2010; Healy, Malhotra, and Mo 2010), they are only exacerbated when power is shared. If voters incorrectly attribute the actions of one actor to another, then both actors will have an incentive to underperform, knowing that they will be held accountable for events beyond their control.

Indeed, that voters blame policy makers for disasters is arguably much less problematic compared to the possibility that they would blame them for the actions of other policy makers. Unlike disasters, these other policy makers may be members of opposing parties with electoral incentives of their own (Mayhew 1974, 30–31). In such situations, a branch controlled by one party may actually have incentives to harm voters, or at best not help them, if they believe that only the opposite party will be judged on the outcomes they control. Such concerns become especially salient in an era where increasing political competition raises the stakes of partisan obstruction (Lee 2013; Sommer 2012).

Given the potential dangers of attribution errors, numerous studies have explored the nature of voter attribution. However, convincing evidence on whether voters blame certain officials for the actions of others has remained elusive. In the next section, I summarize the three major ways that scholars have studied this question.

DO VOTERS CORRECTLY ATTRIBUTE BLAME FOR POLICY?

The first approach taken by scholars is to test whether voters make any distinctions between government actors (Atkeson and Partin 1995; Cutler 2004, 2008; Stein 1990), an obvious

1. While Hamilton is discussing plural vs. unitary executives in this passage, the problem he identifies is applicable to any system of shared powers. In this article, I use *federalism* as a shorthand for such systems, including those where power is divided between branches as well as those where it is split between levels of government.

prerequisite to correct attribution. Using survey data, Arceneaux (2006) finds that voters do assign different levels of responsibility to state, local, and federal actors for issues such as unemployment and education. Arceneaux concludes that his results “should allay fears ... that federalism is too complex for voters to understand” (748). However, Arceneaux does not claim that voters make the “correct” distinction, only that these distinctions are coherent, leaving open the possibility that these distinctions are systematically biased.

A second group of studies asks whether partisanship and political sophistication affect voter attributions (Brown 2010; Gomez and Wilson 2008; Lyons and Jaeger 2014; Rudolph 2003; Tilley and Hobolt 2011). These studies leverage specific events—such as economic performance, budget negotiations, and natural disaster relief—in order to better assess attribution biases. For example, Malhotra and Kuo (2008) find that Democrats are more likely to blame the Republican-controlled national government for the response to Hurricane Katrina, while Republicans are more likely to hold the Democratic-controlled state and local authorities responsible; they also find that experimentally informing their subjects of different officials’ titles mitigates the effect of partisanship (however, see Lyons and Jaeger 2014). Malhotra and Kuo conclude that voters’ ability to overcome partisan bias is a “reason to be optimistic about the capacity for citizens to make unbiased blame attributions” (131). That voters update attributions based on new information, however, does not entirely rule out bias in their attributions. In some situations, it may genuinely be the case that Democratic officials are responsible for a policy failure and Republican officials are not. Thus, Republicans who blame the Democratic official may be correctly attributing blame, while researchers would interpret this behavior as biased.

A third group of studies seeks to overcome this issue by establishing an explicit benchmark of what correct attributions would imply. Perhaps not surprisingly, given the subjectivity of many benchmarks, these studies tend to reach very different conclusions about voters’ capacity for attribution. For instance, Bowler and Donovan (1995) show that voters who believe that the federal income tax is the worst tax are more likely to disapprove of the federal government, while those who believe that the property tax is the worst are more likely to disapprove of their local government. In contrast, Caplan et al. (2013) show that ordinary voters often assign more or less influence to different actors than do political scientists. For instance, citizens believe that the Federal Reserve has more influence over how the federal budget is spent relative to what political scientists believe. Examining electoral data, both Carsey and Wright (1998) and Rogers (2016) find that the single-most important determinant of gubernatorial and state legislative elections, respectively, is presidential approval: if voters approve (disap-

prove) of the president, they reward (punish) the president’s party in their state elections. These results suggest that state officials are punished for events that they do not control (the president’s actions) but not for events that they do control (their own actions).

While provocative, the normative interpretation of these findings ultimately depends on the validity of the assumed benchmark. Both Carsey and Wright (1998) and Rogers (2016) assume that correct attribution means voters attributing the performance of the state economy to state officials and not to the president. On the other hand, Peltzman (1987, 294) similarly finds a strong relationship between state economic performance and the vote share of the president’s party in state elections. Yet Peltzman’s interpretation is that the state voter sensibly “understands that governors have little influence on the growth of state income, and ... rewards (penalizes) the *party* of the incumbent *president* for good (bad) macro performance” (emphasis in original). If Peltzman is right that governors and other state officials have little influence over the state economy and that presidents have a great deal of influence, then the importance of presidential approval in state elections in fact reflects proper attribution.

Thus, while voters have convincingly been shown to make distinctions between political actors, whether these distinctions are systematically biased is still an open question. As the foregoing examples show, however, resolving this debate is impossible in the absence of a case where responsibility for events can clearly be attributed to a particular political actor.

PROPERTY TAX REFERENDUMS IN MASSACHUSETTS

To provide a novel test of attribution in systems of shared power, I study the case of property tax referendums in Massachusetts cities and towns. In these municipalities, property taxes are the most important funder of local government, accounting for 53% of total revenues.² Property taxes are also economically significant for Massachusetts residents, amounting to about 5% of median family income in my data.

Importantly, the president and other federal officials have no influence over the amount of property taxes that voters pay. Under Massachusetts law, municipal governments are responsible for writing the budget and setting the tax rate. While this power was originally held by the town council,³ voters eventually gained significant control over property tax in-

2. State aid accounts for 25%, local receipts (e.g., utility fees, license and permit fees) for 19%. The remaining portion is categorized as “all other” (Massachusetts Department of Revenue 2015).

3. Today, this typically means the board of selectmen (if a town) or the city council (if a city). Some towns still retain an open town meeting form of government. Town council members are elected on nonpartisan ballots.

creases. In 1980, voters passed Proposition 2 1/2, a ballot initiative that put two limits on property tax growth effective in fiscal year 1982. First, municipalities are prevented from raising property tax revenues in excess of 2.5% of total property wealth. Second, they may not increase property tax revenues beyond 2.5% of the prior fiscal year. To get around these limits, municipalities must gain approval from voters in local referendums (Massachusetts Department of Revenue 2015). These approvals are presented to voters in terms of a subject—what the increase is meant to pay for—and a dollar amount—how much additional revenues the increase is meant to raise. According to Roscoe (2014), 27% of increases are intended to fund public education, 15% are for general operating expenses, 14% are for police and emergency responders, and 13% are for public works.⁴

The effects of Proposition 2 1/2 on tax revenues were severe. According to Cutler, Elmendorf, and Zeckhauser (1999, 317–18), 42% of municipalities saw an immediate decrease in property taxes in the wake of the reform. Statewide, local property taxes grew by about 6% per year on average prior to the reform's enactment in 1982, yet fell by 9% in fiscal year 1982. Voters thus gained significant control over property taxes under this reform: in the absence of voter approval, such decreases would be permanent, and future tax increases would be the consequence of voters' own decisions. As I show in the appendix, available online, these referendums are themselves economically consequential, increasing tax bills by an average of \$130 per year.⁵

Table 1 describes the variation in tax increase referendums between 1990 and 2012.⁶ Importantly, the first column represents the fiscal year that the tax takes effect, not the year the referendum was held. The actual vote to raise taxes is held in the preceding fiscal year, typically in May. Fiscal years run from July to June, such that fiscal year 2012 begins July 2011. For example, I code increases as taking effect in 2004 if the referendum is held in May 2003, as the affected fiscal year runs

4. The remaining portion is meant to fund miscellaneous purposes. The vast majority of referendums are for a single purpose: Roscoe (2014) reports that only 2.4% of votes involve multiple subjects.

5. As a percentage of median income in these towns, this amounts to 0.15 percentage points. (I adjust both the tax bill and median income for inflation prior to computing this ratio.) For a point of comparison, Bartels (2013) reports that a 1 percentage point increase in election-year income growth is associated with a 5.5 point increase in incumbent vote share ($SE = 0.9$). Based only on this estimate, the impact of a tax increase could be around $(0.15 \times 5.5 =) 0.83$ percentage points, or as high as 1.1 points.

6. While referendums were first held in the 1980s, the Department of Revenue data on referendums begins in 1990. See the appendix for information on data sources. Note also that I refer to these votes as “tax increases,” though they are commonly referred to as “overrides” (due to the fact that they override existing tax limitations) by locals and others who have analyzed this institution.

Table 1. Towns Voting to Increase Property Taxes, 1990–2012

Affected Year	Holding Referendum (N)	Passing Referendum (N)	Passage Rate (%)
1990	131	80	61
1991	181	100	55
1992	144	65	45
1993	84	57	68
1994	75	49	65
1995	60	35	58
1996	54	28	52
1997	39	17	44
1998	32	21	66
1999	31	23	74
2000	29	19	66
2001	40	35	88
2002	46	39	85
2003	50	40	80
2004	68	50	74
2005	63	45	71
2006	75	54	72
2007	70	41	59
2008	57	35	61
2009	65	42	65
2010	27	18	67
2011	29	17	59
2012	41	19	46

Note. This table shows the number of towns holding and passing at least one tax increase vote by the year the increase would take effect. Typically, votes to increase taxes take place in the prior fiscal year. The passage rate is calculated as the number of towns passing at least one increase divided by the number of towns holding at least one increase. Presidential election years are shown in italics, as these are the years used in the primary analysis.

July 2003 through June 2004. This means that voters experience the increase for 15 months prior to the presidential elections (July 2003–October 2004).⁷ Taxes are paid quarterly, on the first of the month in August, November, February, and May; or they are incorporated into monthly mortgage payments if homeowners' lenders utilize an escrow account.

The number of towns holding tax referendums is displayed in the second column of table 1, the number of towns approving an increase is displayed in the third column, and the overall passage rate for the year is indicated in the final column. I indicate presidential election years by italicizing the respective rows, as I use these years in the primary analysis. Table 1

7. While we could code increases occurring in May 2004 as affecting the 2004 election, this would mean that voters would only experience three months with the tax increase. Later in this article, I show there is a slight negative impact of these short-term increases, and I discuss the timing of increases in more detail in the appendix.

shows that tax increases were most common in the earlier part of the period, that after 1992 about 50 towns have attempted to increase taxes each year, and that such measures had about a 50% chance of passing on average.⁸

Given that local budgets grew by 6% prior to the reform, it is not surprising that nearly all towns attempted to surpass the 2.5% growth limit at least once over this period. As shown in figure A1 in the appendix, between 1990 and 2012, 298 of the 351 municipalities held at least one referendum, with about four attempts on average, and 239 towns passed at least one increase. Concerning increases that had an impact on presidential election years, 215 towns held at least one referendum, and 132 towns passed at least one increase.⁹ Thus, both considering and passing tax increases are common, and this does not appear to be driven by the presidential election calendar; that only 132 towns passed increases in election years is due to their only being six election years. Nonetheless, I will show below that the results hold whether or not I restrict the analysis to presidential election years.

Property tax increases thus represent an important policy decision with a significant influence on voter welfare. Importantly, this decision is entirely out of the hands of presidents but is entirely in the hands of local actors: the town officials who place the increases on the ballot and the voters who decide whether the increases pass. These events therefore represent an ideal opportunity for testing for attribution errors with multiple actors. If there is an effect of such increases on presidential vote share, voters have made such an error—and indeed, they have done so in a setting where avoiding such errors should be extremely easy.

EFFECT OF VOTER-IMPOSED TAX INCREASES ON PRESIDENTIAL VOTING

I begin with a simplified graphical presentation of the key result, comparing changes in Democratic presidential vote share in towns that did and did not pass tax increases that took effect in presidential years.¹⁰ To account for the fact

that certain years are better or worse for incumbents, I present these differences separately by year, weighted by the total number of voters. I also show these differences by the party of the incumbent, in order to preempt concerns that impacts vary by—or are driven by—years where Democrats or Republicans were in the White House. As noted above, this analysis only uses tax increases that took effect in presidential election years.

Figure 1 plots these simple averages and their 95% confidence intervals, calculated as 1.96 times the standard error of the mean. The left panel compares Democratic vote share between towns that did not and did decide to increase taxes, in years when the incumbent president is a Republican (1992, 2004, and 2008). Depending on the year, the average change is either positive or negative; however, the average change is always higher for towns that passed tax increases, suggesting that voters react to tax increases by punishing the incumbent Republican party. The individual differences range from 0.5 to 3.7, with an average of 1.7.

The right panel conducts this same comparison for years with Democratic incumbents (1996, 2000, and 2012). Again, Democrats do better or worse on average depending on the year; however, it is always the case that they do worse in towns experiencing voter-enacted property tax increases compared to towns that do not. The differences range from -0.14 to -2.2 , with an average difference of -1.4 .

Overall, these patterns are exactly what we would expect if voters punish the incumbent president's party for local tax increases: in years with a Democratic incumbent, Democrats lose votes when tax increases happen; in years with a Republican candidate, in contrast, Democrats gain. Indeed, it is striking that the direction of the difference is consistent with attribution error in every one of the six years. While the magnitudes of the differences do vary depending on the year, splitting the data into six samples increases the probability these differences could arise due to random chance. For these and other reasons, I next use a variety of statistical approaches to more rigorously test for evidence of attribution error.

Are towns passing tax increases different?

While consistent with attribution error, the patterns observed in figure 1 could arise from one of several alternative expla-

8. The average vote margin over this period was about 46%. I show the distribution of referendum vote share in the appendix.

9. Each year, about 8% of towns held more than one referendum, and about 3% passed more than one referendum. In the main analysis, I delete these repeat observations, keeping the referendum with the latest month and day of the year. In the even rarer cases where multiple referendums are held on the same date, I sort observations by a random number and keep the observation with the lowest value of this number. Later in the article, I show that my results are robust to incorporating these repeat votes into the analysis. Across the entire time period, towns held three referendums on average and passed two referendums on average.

10. Both here and in most of the subsequent analyses, the comparison is between towns that hold and pass tax increase referendums, on one hand, to towns that either do not hold a tax increase referendum or hold a

failed referendum, on the other. Even though the treatment is the result of two decisions—first holding, then passing a referendum—I believe that this is the appropriate comparison, as I argue that both holding and passing a referendum are exogenous conditional on the various strategies I am using to rule out confounding variables. Later in the article, I show that the results are unchanged if I exclude towns that do not hold referendums and simply compare those that held and passed to those that held and failed.

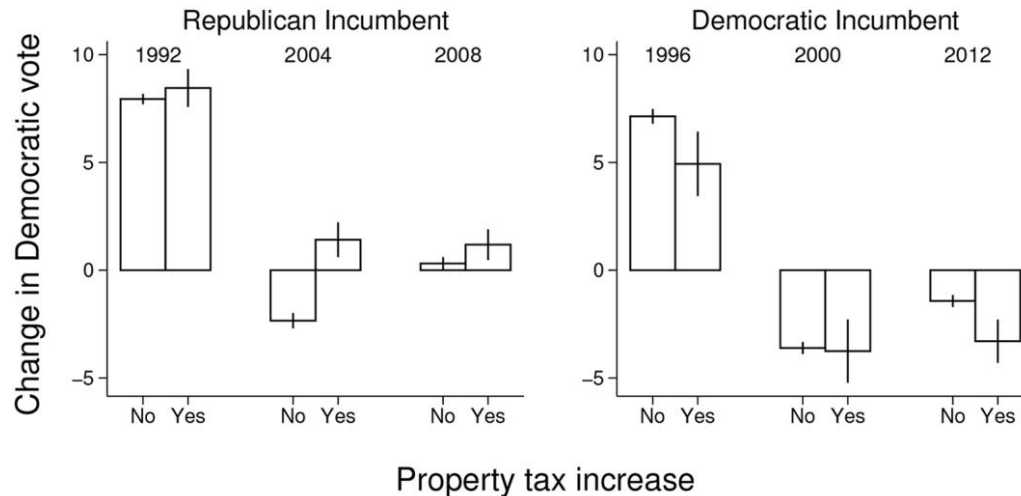


Figure 1. The effect of voter-imposed tax increases on presidential voting, 1992–2012. This figure compares changes in Democratic presidential vote share in towns that do and do not pass tax increases that take effect in election years, separately for years when the president is a Democrat and years when the president is a Republican. Changes in vote share are weighted by the total number of voters. Vertical bars span 95% confidence intervals.

nations. Voters might punish the incumbent president for many sensible reasons, and many of these reasons may co-vary with the decision to raise taxes. In this section, I discuss these alternative explanations, as well as my strategies to account for them.

I first consider whether towns that hold and pass tax increases are somehow different than those that do not, in ways that might also have an impact on presidential voting. As noted above, the majority of towns, 85%, held at least one tax increase referendum between 1990 and 2012; a large majority (68%) also passed at least one. The fact that nearly all towns engage in this behavior at some point makes it less likely that treated and untreated towns differ on fixed characteristics. Additionally, because I examine changes in incumbent vote share, fixed differences between towns should be held constant.¹¹

Other threats to inference may arise from time-varying differences. For instance, towns passing increases may be becoming more ethnically heterogeneous, which could also affect the town's overall ideology (Hopkins 2009). Alternatively, the decision to raise taxes could be related to local economic or fiscal conditions that are themselves a function of national factors. Perhaps towns experience economic stress, for which they blame the president, while at the same time demand for local services grows, leading to a tax increase vote.¹² To take yet another possibility, perhaps towns

increase taxes when state and federal aid declines; voters might punish the president for these declines in aid, while also voting to raise taxes to make up for the difference in revenue.

To explore the extent to which such differences may be a problem, I examine differences in observable characteristics between passers and nonpassers in the appendix. This analysis shows that neither holding nor passing a tax increase referendum is consistently related to any observable characteristic, except the share of white residents and median family income. Importantly, neither of these variables should predict anti-incumbent voting, but both are associated with anti-Democratic voting.¹³ The differences in means are also generally the same in years with or without presidential elections. Nevertheless, to account for the possibility that the tax increase effect is driven by these or other observable differences that change over time, I estimate a series of panel regressions that adjust for these factors in the next section.

When examining panel data, it is also important to account for the possibility that changes between periods are driven by regression to the mean, a more complicated problem that simply including covariates will not address. For instance, it could be that towns passing increases in year t also experienced deviations from their typical voting behavior in year

11. Later in the article, I also show that the tax increase effect is robust to the inclusion of town fixed effects, which eliminates the threat of confounding from any time-invariant factors.

12. On the other hand, voters would theoretically be less likely to approve tax increases when the economy is bad, and existing evidence on direct democracy in other states validates this expectation (Bowler and

Donovan 1998). Additionally, Roscoe (2014) finds a positive relationship between town income and the propensity to pass increases, further suggesting that increases are not a proxy for economic distress. However, I directly test for this possibility rather than ruling it out a priori.

13. Both median income and the share of white voters are negatively correlated with Democratic vote share (in my sample, the correlations are -0.27 and -0.63 , respectively) but are uncorrelated with incumbent vote share (the relevant correlations are 0.06 and 0.03).

$t - 4$. If such deviations are also related to a tendency to pass tax increases, then this will lead to biased estimates. I therefore estimate regressions that include lagged Democratic vote share, which should account for regression to the mean (Angrist and Krueger 1999, 1295; Finkel 1995). As a robustness check, I also experiment with multiple lags of Democratic vote share. Additionally, in the appendix, I explicitly test whether the tendency to pass tax increases is related to deviations from normal voting in the previous election; I find no evidence that this is the case.

Finally, in an observational study, there also exists the threat of confounding from time-varying but unobservable factors. These could include strategic behavior on the part of town or federal officials, the omission of an important control variable, or the failure to properly measure important confounders, such as economic performance. I therefore also employ two strategies to rule out the threat of unobservable time-varying confounders, namely, dynamic panel estimation and a regression discontinuity design. I explain these strategies in more detail below.

Panel analysis

The basic regression specification I employ captures the logic of figure 1: in years when the incumbent president is a Democrat, tax increases are expected to hurt Democratic candidates; yet when the incumbent is a Republican, Democrats are expected to gain. Thus, the regression takes the following form:

$$\begin{aligned} \text{Democratic Vote Share}_{jt} = & \beta_1(\text{Tax Increase}_{jt} \times \text{Democratic Incumbent}_{jt}) \\ & + \beta_2(\text{Democratic Vote Share}_{j,t-4}) + \text{Year}_t + \sum_{k=1}^K \alpha_k(x_{kjt}) \\ & + \varepsilon_{jt}, \end{aligned}$$

where the outcome variable is the proportion of major party votes received by the Democratic presidential candidate in town j and year t . As is standard in studies of economic voting (Kramer 1971; Peltzman 1987), the key independent variable is an interaction between events (here, a voter-imposed tax increase) and a variable coded 1 if the incumbent president is a Democrat and -1 if Republican. This coding reflects the expectation that a tax increase will hurt the incumbent party: a negative β_1 means that a tax increase hurts Democrats when they are the incumbent party and helps them when Republicans are the incumbent party. In all specifications, I include the Democratic candidate's vote share in the previous election, as well as indicators for year. The x_k variables represent measurable time-varying covariates intended to account for the alternative explanations mentioned above and

are described below. I cluster standard errors at the town level to account for dependence within towns over time, and I weight observations by the number of voters in each town.

Column 1 of table 2 presents results from the simplest specification, including only the tax increase variable, year effects, and lagged vote share. The point estimate indicates that a voter-imposed tax increase decreases the incumbent's vote share by 1.8 percentage points ($SE = 0.3$ percentage points), which is similar to the overall averages computed from figure 1.

I next add controls for local economic conditions. Standard Census-based measures of income and unemployment can attenuate the true impact of the local economy on election outcomes, as these measures are either available only every 10 years for the population (in the case of the Census data) or are estimated based on 0.1% of the US population (in the case of the Bureau of Labor Statistics data; see Healy and Lenz 2014). I therefore follow Healy and Lenz (2014) and include measures of wage and employment growth from the Quarterly Census of Employment and Wages (QCEW), an annual population-based measure generated by employers' Unemployment Insurance filings and produced by the Bureau of Labor Statistics. While these measures are at the employer level—thus only measuring employment and wages for businesses located in, rather than for employees residing, in these towns—they should still pick up any localized economic shocks that could correlate with the decision to raise property taxes.

In column 2, I add wage growth from the QCEW, also interacted with Democratic incumbency. The estimate for wages is positive, at 0.3, but is statistically insignificant, with a standard error of 0.8. More importantly, the estimate for tax increases is unchanged compared to column 1. The same is true when I add QCEW employment growth in column 3, where the estimate on employment is actually negative but statistically insignificant. Thus, even controlling for the best available measures of the local economy, tax increases are associated with a decline in incumbent vote share.¹⁴

In column 4, I add an additional measure of the local economy: annual changes in total property values. As with the QCEW, this measure is based on a population, in this case towns reporting to the state Department of Revenue. However, the values are missing for some towns in some years, causing a slight drop in sample size from about 2,000 to about 1,900 (or 351 to 341 unique towns). Like the QCEW, this measure should pick up any local economic shocks, but unlike

14. The sample size declines slightly from col. 1 to cols. 2 and 3, because I have coded as missing observations less than the 1st percentile or greater than the 99th percentile on the QCEW measures. Results are unchanged when including these outlier observations.

Table 2. The Effect of Voter-Imposed Tax Increases on Presidential Voting, 1992–2012

	(1)	(2)	(3)	(4)	(5)	(6)
Tax increase × Democratic incumbent	−1.85*** (.30)	−1.85*** (.30)	−1.84*** (.30)	−1.88*** (.33)	−1.88*** (.33)	−1.84*** (.37)
Average wages × Democratic incumbent		.32 (.79)	.30 (.79)	−.05 (.83)	−.05 (.84)	.25 (.79)
Employment × Democratic incumbent			−1.76 (1.17)	−2.26 (1.23)	−2.26 (1.23)	−1.72 (1.24)
Property values × Democratic incumbent				−.02 (1.25)	−.03 (1.25)	−.18 (1.34)
Unemployment × Democratic incumbent					−.38 (1.48)	−.31 (1.49)
Federal grants × Democratic incumbent						.49 (1.36)
State aid × Democratic incumbent						2.69 (2.00)
Population						−5.12*** (.73)
White						−5.98*** (.65)
Age 65+						.88 (.53)
Renter						2.33*** (.68)
Median income						.03 (.80)
Constant	6.69*** (.68)	−5.25*** (.81)	−4.24*** (1.06)	8.64*** (1.36)	−3.38* (1.64)	6.35** (2.19)
Lagged Democratic vote share	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,106	2,054	2,035	1,893	1,885	1,885
Towns	351	351	351	341	341	341

Note. Standard errors clustered by town are in parentheses.

* $p < .05$.

** $p < .01$.

*** $p < .001$.

the QCEW, this measure includes town residents as opposed to businesses. Again, however, the estimate of tax increases is substantively unchanged, in fact increasing to 1.9 points (SE = 0.3 points), and property values themselves have no impact on vote share.

In column 5, I add estimated annual town unemployment, based on the Local Area Unemployment Statistics from the federal Bureau of Labor Statistics (BLS). According to the BLS, these estimates are generated by synthesizing data from the QCEW, the American Community Survey, and the 0.1% samples from the Current Population Survey (Bureau of Labor Statistics 2016). While the sign on this coefficient is in the expected direction, suggesting that unemployment hurts Democratic candidates when they are the in-

cumbent party, it is again not statistically significant. Again, however, the more important point is that the estimate on tax increases retains its magnitude and significance.

In column 6, I add measures of federal and state aid, as well as a series of demographic controls from the Census, including population, the percentage of white residents, the percentage of senior citizens, the percentage of renters, and logged median family income. The measures of aid are available every year, while the Census variables are available only every 10 years, so I linearly interpolate them between Census years. For the measures of aid, I again interact the variables with the party of the incumbent, under the assumption that effects may vary depending on who is in power; I do not interact the Census measures, though the results are robust to doing so. Aside from

the noneffect of state and federal aid, what is notable in column 6 is that the impact of tax increases is still about 1.8, with a standard error of 0.4.

Robustness tests

To further probe the robustness of the tax increase effect, I perform a series of additional tests in table 3. The first row of this table repeats the estimate of a 1.85 point decline from table 2, column 1. In the second row, I add town fixed effects. This specification exploits within-town variation in tax increases, effectively comparing how presidential vote share changes when the same town does and does not increase taxes, and it rules out the influence of any confounding factors that do not vary across time. The estimate is substantively very similar to the baseline, at -1.76 ($SE = 0.35$). Adding an additional lag for Democratic vote share (i.e., Democratic vote share in the election eight years prior) in the third row also does not affect the results: the point estimate is -1.79 ($SE = 0.29$).

The next two rows show the effect is not driven by particular groups of observations. In the fourth row, I sequentially drop each of the six election years and re-estimate the main regression from table 2. Even though this reduces the sample size by one-sixth, the smallest effect observed from these six separate regressions is still -1.12 , with a standard error of 0.33. In the fifth row, I perform the same analysis for counties, of which there are 14 in the state with an average of 25 towns per county. Of these 14 estimates, each dropping a particular county, the smallest estimate is -1.68 ($SE = 0.33$). Thus, the results are not driven by any particular time or place.

The next test further addresses the possibility that local tax increases may correlate with other factors that may, in turn, be plausibly attributed to the president. For instance, perhaps poorly performing schools lead towns to pass tax increases, and voters at least partly blame the president for poor schools. In this scenario, the observed effect of tax increases does not represent voters reacting to an increase in taxes, but a deteriorating school system that they may attribute to the president's education policies. While school quality is likely proxied for by median income and property wealth in table 2, in the sixth row, I perform a more direct test by dropping education-related votes from the analysis.¹⁵ Dropping these votes leaves the estimate substantively unchanged, at -2.02 ($SE = 0.41$).

15. According to Roscoe (2014), 27% of referendums held between 1990 and 2007 involved education. In my data, which run until 2012 and are restricted to presidential election years for this analysis, the proportion is 33%. I code votes as education-related if the ballot description includes the phrases "school" or "educ."

Table 3. Robustness of Tax Increase Effect: Alternative Specifications

	Estimate
1. Estimate from table 2, column 1	-1.85^{***} (.30)
2. Add town fixed effects	-1.76^{***} (.35)
3. Add Democratic vote share in the election eight years prior	-1.79^{***} (.29)
4. Smallest effect dropping individual years and re-estimating	-1.12^{***} (.33)
5. Smallest effect dropping individual counties and re-estimating	-1.68^{***} (.33)
6. Exclude education-related referendums	-2.02^{***} (.41)
7. Holders only	-1.10^{**} (.40)
8. Placebo treatment: referendums that failed	$-.35$ (.28)

Note. Standard errors clustered by town are in parentheses.

* $p < .05$.

** $p < .01$.

*** $p < .001$.

The sixth row estimates a specification where I drop all towns that do not hold referendums. Thus, rather than compare (a) towns that hold and pass referendums to (b) towns that either do not hold or hold and reject referendums, this specification compares "holders" only: towns that hold and pass increase referendums to towns that hold and reject. Here the estimate is -1.1 , with a standard error of 0.4. That this estimate is qualitatively similar to the others supports the view that there is no bias resulting from towns first needing to select into holding referendums, and the decline in magnitude is likely due to the large decrease in sample size (from about 2,100 to about 400) when dropping towns not holding referendums.

Finally, I re-estimate the result from table 2 by substituting the tax increase variable with a "placebo" treatment that should, according to the attribution hypothesis, have no impact on presidential vote share: failed tax increases. This test serves two purposes. First, it partly addresses the possibility that tax votes, regardless of their outcome, may "prime" certain issues in voters' minds, leading them to evaluate the president based on taxes that the president might plausibly be responsible for, such as the federal income tax (Donovan, Tolbert, and Smith 2008; Nicholson 2005). Second, it tests whether

local fiscal conditions as a result of the tax increase may be influencing voter behavior. Perhaps voters observe their property taxes increase and infer something about the local economy, which they then attribute to the president. If this were the case, we should see an even stronger effect when tax increases fail. Tax increases are typically proposed precisely to address and prevent local fiscal stress, and they have been found to increase fiscal stress even further when they fail (Makowsky and Stratmann 2009). Contrary to these two explanations, however, failed increases have no association with presidential vote share: the estimate in the seventh row is -0.35 percentage points with a standard error of 0.28 .

Dynamic panel estimates

As an additional robustness check, I next check for evidence of pre-tax increase trends in Democratic vote share by estimating the following dynamic panel regression:

$$\begin{aligned} \text{Democratic Vote Share}_{jt} &= \sum_{s=-3}^3 \alpha_s (\text{Tax Increase}_{j,t+s} \times \text{Democratic Incumbent}_{jt}) \\ &\quad + \beta (\text{Democratic Vote Share}_{j,t-4}) + \text{Year}_t + \varepsilon_{jt}. \end{aligned}$$

In other words, I add three “leads” and three “lags” of the tax increase variable to the regression specification used in table 2. The lead variables should detect any differences in incumbent vote share between towns that passed tax increases and those that did not, prior to the actual tax increase. If such differences are evident, it suggests that towns passing increases would have seen reductions in incumbent voting, even in the absence of a tax increase. The lag variables, in contrast, estimate the long-term impact of the increase on vote share.

I plot the coefficient estimates and their 95% confidence intervals in figure 2. Reassuringly, this figure shows that there is no impact of being three or two years prior to a tax increase on presidential vote share. The slightly negative, but insignificant, estimate for being one year prior is likely due to the fact that, given the imperfect match between fiscal and calendar years, being “one year prior” to an increase means voters experience three months of the tax increase before the election.¹⁶ The lag coefficients, meanwhile, exhibit a sensible

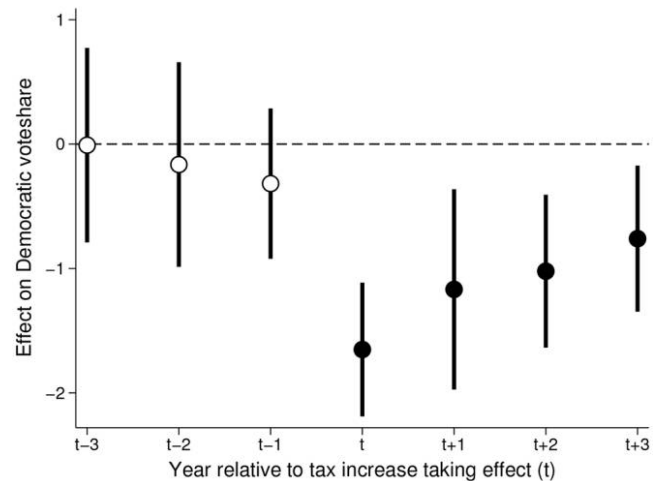


Figure 2. Effect of tax increases on presidential vote: accounting for pre-trends. This figure plots coefficient estimates (with vertical lines spanning 95% confidence intervals) for indicators for being three, two, and one years prior to a tax increase ($t-3$, $t-2$, $t-1$), being in the year of a tax increase (t), and being one, two, and three years after a tax increase ($t+1$, $t+2$, $t+3$).

pattern of gradually declining magnitude as the tax increase recedes further into the past.

Regression discontinuity design

To isolate the effect of tax increase votes from other factors, a natural strategy in this setting is the regression discontinuity design (RDD). That is, we may seek to exploit the fact that the tax increase treatment changes discontinuously when referendum vote share reaches 50%, similar to a variety of studies that have estimated election effects in political science (Caughey and Sekhon 2011; Eggers et al. 2015). Because the treatment jumps sharply once referendum votes reach a majority, and because close referendums are essentially determined by a coin flip, comparing the difference in outcomes between bare-winners and bare-losers is akin to a randomized experiment (Lee and Lemieux 2010). That is, bare-winners and bare-losers should be comparable on any characteristics that might also affect the outcome, and so any difference in outcomes can plausibly be attributed to the treatment.¹⁷

While promising, a discontinuity design faces several obstacles in this setting. First, it is well known that discontinuity designs lack statistical power compared to other designs (Schochet 2009). While detecting an effect in an RDD requires more data than other designs, in my case I lose over four-fifths of the data because only towns that hold refer-

16. To repeat the example given previously: if voters pass a referendum in May 2003, it takes effect in July 2003, the start of fiscal year 2004. Thus at the next presidential election, voters have experienced 15 months of the increase. Such increases are coded as occurring at time t in my analysis. In contrast, referendums passed in May 2004 and effective July 2004 (hence giving voters three months of the tax increase before the election) are coded as time $t-1$ in fig. 2. I explain timing in more detail in the appendix.

17. In this setting, the RD is therefore “sharp”—all units above the cutoff are treated—as opposed to “fuzzy”—when only the probability of treatment changes at the cutoff.

endums have values of the forcing variable.¹⁸ To address this problem, I incorporate the non-presidential election years into the analysis by assigning the observations the value of presidential vote share for the approaching election. For example, a town holding a referendum in 1993, but not in 1994, 1995, or 1996, is assigned its presidential vote from 1996.¹⁹ This imputation strategy not only increases power, but it also addresses concerns that results presented earlier are driven by election year tax increases being different from non-election year increases, or by towns deciding to hold referendums being different from towns that do not. To further increase precision, I also demean observations across time and within towns, adjust for lagged vote share, and weight observations by the total number of voters.

Second, an RDD in this setting must account for the fact that towns sometimes hold multiple referendums in the same year. Because towns can be treated if any one of several vote share variables exceeds the 50%, distinguishing between treated and control units is not as straightforward as in a typical RDD (Wong, Steiner, and Cook 2013). I therefore adopt what Wong et al. (2013) call the “centering” approach, assigning each town the highest vote share it received in any given year. For instance, a town holding two votes is counted as treated if either the first or second referendum passed and as a control otherwise. Such a strategy likely works against my finding an effect, as some of the “control” units will actually have experienced a tax increase.²⁰

With these caveats in mind, I present the graphical result of this strategy in figure 3. The vertical axis represents the (demeaned) change in Democratic presidential vote share, while the horizontal axis represents normalized referendum vote share—as before, multiplied by -1 if the president is a

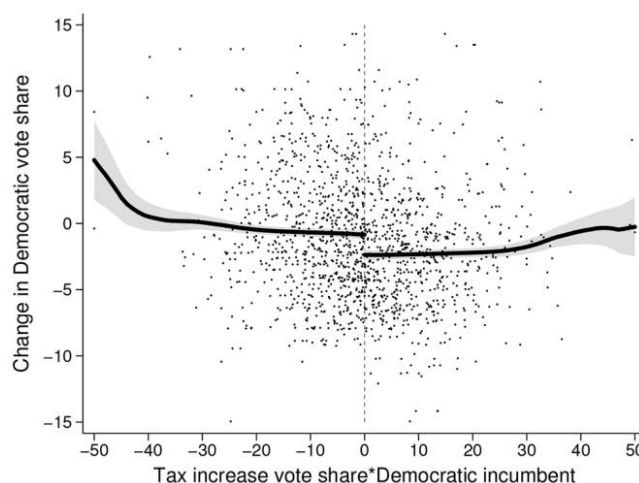


Figure 3. The effect of voter-imposed tax increases on presidential voting: regression discontinuity design. This figure compares changes in Democratic presidential vote share in towns where tax increases barely failed versus where they barely passed. The vertical axis represents changes in presidential vote share, demeaned by town and year and weighted by population, and imputed for nonelection years, as discussed in the text. The horizontal axis represents referendum vote share, multiplied by $+1$ if the incumbent president is a Democrat and -1 if the incumbent president is a Republican. The thick horizontal lines represent local polynomial fits estimated separately for either side of the threshold; shaded areas represent 95% confidence intervals.

Republican and $+1$ if a Democrat. Each point represents an observation, while the solid lines represent separate local polynomial estimates and their 95% confidence intervals. Even just keeping to the raw data, there is a noticeable drop in the distribution of Democratic vote share when the horizontal axis crosses zero. As suggested by the solid lines, on average incumbent vote share drops by 1.5 points at the threshold, a decrease that is similar to the estimates presented earlier.²¹

In the appendix, I report formal estimates and standard errors from linear regressions conducted using various windows of vote share, with standard errors calculated using the block bootstrap. These estimates show that the effect is relatively stable regardless of the window used: using all the data yields an estimated decline of 1.34 points ($SE = -.39$); using only the data within a 10 point window yields an estimate of -1.22 ($SE = .59$); and using a 1 point window yields a more uncertain, but substantively similar, estimate of -1.76 ($SE = 1.54$). Using an estimator that includes quadratic referendum vote share gives similarly negative, if somewhat noisier, estimates. Also in the appendix, I show that referendum vote share is approximately normally distributed around 50% and that there is no noticeable jump in the density at the threshold that would indicate manipulation.

18. Between 1992 and 2012, 215 unique towns considered raising taxes effective in presidential election years. Because some towns held more than one referendum over this period, the total number of observations with values of referendum vote share over this period is 393. In contrast, there are 2,106 observations included in the panel analysis above. Note that unlike most of the analyses presented above, the comparison in the RD is between towns that hold and pass referendums to towns that hold and reject referendums. Previously, I compare towns that hold and pass to towns that do not hold OR hold and reject. I obtain similar estimates regardless of the comparison used.

19. If a town were to hold referendums effective 1993, 1994, 1995, and 1996, then its presidential vote share in 1996 is assumed to be a function of referendum passage in each year; thus the town's 1996 vote share is counted four times. Results are very similar if, alternatively, I instead assign towns past referendum vote share instead of future presidential vote share. For example, a town holding a referendum in 1993 but not 1994, 1995, or 1996 is assigned its 1993 referendum vote share in 1996.

20. The result is robust to other potential codings, such as using the earliest referendum voted on or taking the minimum of all possible vote shares.

21. The total number of observations in fig. 3 is 1,491.

WHY DO INCREASES MATTER? RETROSPECTION, PRIMING, OR TURNOUT

The evidence presented so far indicates that voters punish the president for the actual change in their tax bills and not some other factor that co-varies with tax increases, such as the local economy. In this section, I explore possible mechanisms behind this effect. First, voters may simply observe that their income has changed and fail to separate out the local versus national sources of that change before they attribute it to the president. In this scenario, voters are unfairly punishing the president because their taxes went up. Alternatively, the shock of the tax increase may “prime,” or raise the salience of, other factors that voters attribute to the president. Finally, tax increases may alter the composition of the electorate by affecting voter turnout.

The retrospection explanation implies that voters only notice their income has decreased yet fail to separate out the amount of the decrease due to local taxes. This does not mean that voters do not know that a tax increase has passed, only that it does not enter into their voting decision. By the time voters make that decision, the only information they bring to bear is the change in their personal income, regardless of which particular policy decisions led to that change (Fiorina 1981). This pocketbook effect constitutes a noisy signal of the president’s performance, encompassing presidential actions as well as local tax increases, and it is costly for voters to extract the component of this signal that actually reflects presidential competence.

If the effect does occur via voter’s pocketbooks, this implies that the effect of tax hikes should be increasing in the actual change in voters’ tax bills. That is, if voters are simply using the change in their personal incomes as a signal for presidential performance, failing to subtract the change due to property taxes, then presidents should suffer more the larger the tax increase. To test this, I interact the tax increase indicator with the size of the increase, measured using changes in average single-family tax bills by town. Again, I multiply the increase variable by -1 ($+1$) if the incumbent president is a Republican (Democrat), such that increases are expected to hurt Democrats when they are the incumbent and help them when they are not. For presentational purposes only, I also scale the tax increase by town-level median income.²²

22. I adjust tax bills for inflation such that they are expressed in terms of real 2012 dollars. I also delete observations below the 1st or above the 99th percentile on the tax bill change variable, as the unadjusted range is between roughly $-1,700$ and $1,700$ dollars (or about -2% and $+2\%$ of median income). Results are robust to expressing bills in absolute dollar amounts.

In figure 4, I show the relationship between changes in Democratic vote share, net of year effects, and changes in tax bills as a percentage of median income, in towns that passed tax increases only.²³ Consistent with the retrospective voting explanation, there is a clear negative relationship between vote share and the magnitude of the tax increase, which can be as large as 0.6% of median income. The regression line suggests that the largest tax increases under Republican presidents lead to an increase in Democratic vote share of about 5 points, whereas the largest increases under Democratic presidents lead to a decline of about 4 points.

To formally test for this relationship, I estimate the following regression:

$$\begin{aligned} \text{Democratic Vote Share}_{jt} &= \beta_1(\text{Tax Increase}_{jt}) + \beta_2(\Delta\text{Tax Bill}_{jt} \times \text{Democratic Incumbent}_{jt}) \\ &+ \beta_3[\text{Tax Increase}_{jt} \times (\Delta\text{Tax Bill}_{jt} \times \text{Democratic Incumbent}_{jt})] \\ &+ \beta_4(\text{Democratic Vote Share}_{j,t-4}) + \text{Year}_t + \sum_{k=1}^K \alpha_k(x_{kjt}) + \varepsilon_{jt}. \end{aligned}$$

As in figure 4, the key independent variable in this regression is the annual change in tax bills, interacted with incumbency such that increases in bills are predicted to hurt Democrats when they are the incumbent and help them when they are not. To test whether the effect of tax increases varies with the magnitude of the increase, I also include an indicator for tax increases and an interaction between tax increases and the pre-interacted tax increase amounts variable. Because of this coding, β_1 represents the impact of the largest voter-approved tax increase under Republican presidents, β_2 represents the impact of the largest non-voter-approved tax increase under Democratic presidents, and β_3 represents the difference in the impact of the largest tax increases under Democratic presidents, where the difference is between voter- and non-voter-approved increases.

I show the results in table 4. Column 1 includes the tax increase indicator, the magnitude of the bill change, and the interaction between the two, whereas column 2 adds the complete set of control variables from table 2, column 6; both specifications include year fixed effects and lagged Democratic vote share, and the tax bill change variable is coded such that zero represents the minimum value and one the maximum. The results are consistent with figure 4: the coefficient in the first row indicates that the largest tax increases under Republican presidents lead to about a

23. I exclude towns that do not pass tax increases from fig. 4, as they predictably exhibit much less variation in tax bills. I later incorporate these towns in a regression specification.

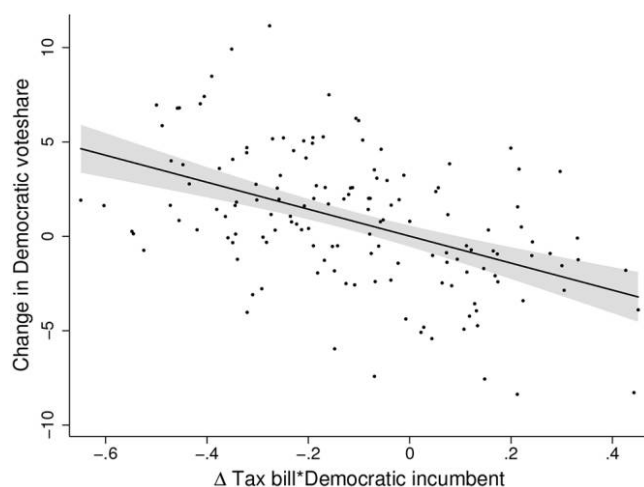


Figure 4. Effects of tax increases are larger for higher tax increase amounts. This figure shows how the effect of tax increases varies as a function of the actual change in tax bills resulting from the increase, for towns passed increases only. Changes in vote share are adjusted for election-specific effects by regressing vote share on year indicators and generating residuals, and Δ Tax Bill is expressed as a percentage of median income. Solid lines represent linear regressions, with the shaded area spanning the 95% confidence interval.

4 point increase in Democratic vote share (standard errors of about 0.8 points), the second row indicates a slight negative impact of non-voter-approved tax increases, and summing the first and third rows implies about a 2.7 to 2.9 point decrease in Democratic vote share for the largest tax increases under Democratic presidents (SE = 0.77 for both specifications).²⁴

In the remaining four columns of the table, I substitute measures of local economic conditions for tax bills. If tax increases prime the local economy, then it should be the case that the impact of these increases also varies with measures of the local economy: there will be more information to prime in voters' minds. To conserve space, I present results only using wages and employment from the Quarterly Census of Employment and Wages, but the results are robust to whatever measure I use. Columns 3 and 4 show that the impact of tax increases does not vary by wages as priming would predict: the interactive coefficients are actually negatively signed, though insignificant, suggesting that tax increases have

a more negative impact the stronger the local economy. Similarly, columns 5 and 6 show that the impact of tax increases does not vary substantively or significantly by employment growth. In sum, the evidence strongly suggests that voters are reacting to the actual changes in economic well-being that result from tax increases and not to any heightened salience of presidentially relevant issues.

Finally, I consider whether the results are driven by differential turnout. Recent evidence suggests that out-party voters react to positive changes in income by staying home rather than by voting for the incumbent, while members of the president's party will respond by turning out more (Chen 2013). In the appendix, I estimate a regression similar to that presented in table 2, except that I use presidential turnout as the outcome. I find there is a small, but statistically significant, relationship between tax increase passage and turnout: the estimate suggests that a tax increase causes an increase in turnout of 0.44 percentage points (SE = 0.20 points). However, I find no evidence that this effect varies based on the interaction between voter and presidential partisanship.

POLITICS AND BLAME IN MASSACHUSETTS TOWNS

When studying aggregate data, it is useful to explore, as much as possible, the underlying politics. One potential concern when examining figure 2 is that the effects are strongest in 1996 and 2004—both years following elections with robust third party voting in Massachusetts. In 1992 and 2000, Ross Perot and Ralph Nader received, respectively, 22.8% and 6.4% of the popular vote. Perhaps Perot-leaning towns in 1992 usually voted Republican and so in 1996 swung back to the Republican party; this return to normalcy could be confused with a surge in “anti-incumbent” voting. Similarly, Nader towns in 2000 may have swung back to the Democrats in 2004. Such swings are problematic only if they co-vary with tax increases. As I show in the appendix, they do not; moreover, while Nader-leaning towns did swing back to the Democrats in 2004, Perot-leaning towns in 1992 were in fact more likely to swing Democratic (pro-incumbent) in 1996.

A more plausible explanation for variability in effects is offered by figure 4 and table 4: in towns and years where the impact on voters' pocketbooks is higher, so too is the electoral impact. Why would voters punish the president for an economic pain of their own making? The literature on “blind” retrospection offers two possible explanations. First, events may subconsciously have an impact on voters' moods, and these moods may affect voting independent of any outside actor. This explanation is more likely in cases where the event is something that “voters themselves would recognize as politically irrelevant,” such as sporting matches (Achen and Bartels 2016, 138), and so it can be ruled out here. Alternatively,

24. The impact of the largest tax increases under Democratic presidents is:

$$\begin{aligned} E[\text{Democratic Vote Share}_{jt} | \text{Tax Increase}_{jt} = 1, \\ \Delta \text{Tax Bill}_{jt} \times \text{Democratic Incumbent}_{jt} = 1] \\ - E[\text{Democratic Vote Share}_{jt} | \text{Tax Increase}_{jt} = 0, \\ \Delta \text{Tax Bill}_{jt} \times \text{Democratic Incumbent}_{jt} = 1] \\ = (\beta_1 + \beta_2 + \beta_3) - (\beta_2) = \beta_1 + \beta_3. \end{aligned}$$

Table 4. Effects of Tax Increases Vary by Tax Increase Amount, but Not by Local Economic Conditions

	Tax Bills		Wages		Employment	
	(1)	(2)	(3)	(4)	(5)	(6)
(1) Tax increase	3.96*** (.77)	4.14*** (.76)	2.06* (.95)	1.49 (1.05)	2.61 (1.34)	1.57 (1.45)
(2) Moderator	-1.88* (.87)	-1.63* (.80)	-.04 (.63)	-.31 (.57)	-.97 (.73)	-.98 (.76)
(3) Increase \times moderator	-6.64*** (1.44)	-7.04*** (1.42)	-2.48 (2.07)	-1.08 (2.20)	-3.36 (2.83)	-1.12 (2.99)
(1) + (3)	-2.68*** (.77)	-2.90*** (.77)	-.42 (1.21)	.41 (1.26)	-.74 (1.56)	.45 (1.63)
Controls	No	Yes	No	Yes	No	Yes
Observations	1,930	1,858	2,007	1,851	2,015	1,850

Note. For each pair of columns, Moderator refers to the coefficient on the variable in the column group title (Tax Bills, Wages, or Employment) and Interaction refers to the interaction between tax increases and the variable denoted by Moderator. Tax bills are expressed as a annual changes in real dollars divided by median income in real dollars, multiplied by 100. All specifications include year fixed effects and lagged Democratic vote share. "Controls" include all control variables from table 2, col. 6. Standard errors clustered by town are in parentheses.

* $p < .05$.

** $p < .01$.

*** $p < .001$.

voters may be persuaded, via what Achen and Bartels term the "social construction of blame," that government is at fault. In the famous shark attacks case, it was rumored that German submarines had induced the sharks to attack; as well, both President Wilson and some of his top aides resided in the area, "reinforcing the notion that the federal government should have done *something*" (Achen and Bartels 2016, 120; emphasis in the original).

To explore possible social construction in these towns, I reviewed local newspaper coverage in the 18 towns that successfully passed tax increases in 2012.²⁵ While it would be unwise to draw strong conclusions from a small number of cases, several interesting patterns emerge. First, there is never any ambiguity as to who is to blame for the referendum outcome: the voters. Second, while there are often conflicting views about why a town must hold a referendum, any links to the federal government are weak and are made solely by supporters of the increase, who are presumably pleased with the referendum outcome and thus in no mood to punish. More often, supporters point to state aid, the economy, and the need to maintain services. Opponents, in contrast, always blame town officials for being fiscally irresponsible. No one mentions the president.

Third, impacts on voters' pocketbooks are consistently emphasized, especially by opponents. For instance, the *Boston Herald* reported that proposed increases in several towns threatened "cash-strapped residents try[ing] to rebound from the sluggish economy" (Fargen 2011). In Arlington, the anti-increase *Coalition for Responsible Spending* "said the town should not be seeking a tax increase while many are still feeling the effects of a down economy" (Parker 2011); the front page of said *Coalition's* web site promised a "devastating blow to the cost of living for our neighbors" (*Coalition for Responsible Spending* 2011). In Georgetown, one voter wondered whether "a certain clique of soccer moms and dads" will "some day, perhaps 20 years from now, look back at today's vote, and realize what pain they inflicted upon the less fortunate in our town" (Brennan 2011).

Thus, a socially constructed link between tax increases and the president is not to be found. Instead, what is found in these towns is a clear link between tax increases and changes in voters' pocketbooks. Come presidential election time, after a year of enduring the pain of a tax increase, the causal links between pocketbooks and local actors have, presumably, evaporated, leaving only the pain itself. Should they seek an explanation for their pain during the presidential race, voters then presumably find the ever-present, socially constructed link between pocketbooks and presidents, and it is this link that likely explains the effect.

25. I provide more details about this inquiry in the appendix.

CONCLUSION

I use local tax referendums in Massachusetts to provide a novel test of whether voters punish presidents for the actions of other policy makers. Because presidents have no control over property taxes and because voters themselves obviously determine their increase, there should be no relationship between these increases and presidential vote share. Yet there is a very robust, negative relationship between tax increases and incumbent vote share. Because responsibility is relatively easy to pinpoint in this setting, these findings raise serious doubts about voters' capacity to properly attribute blame in more complicated situations.

Beyond shared power in general, my results also have implications for direct democracy. A more pessimistic interpretation of these findings is that voters are blaming politicians for their own choices. However, an equally plausible interpretation is that voters understand the president is not to blame but that voters in the national election, uninterested in local issues, do not. While both scenarios imply attribution error, future work could examine which of these explanations is more accurate.

My finding about an "irrelevant" event affecting presidential voting is consistent with studies of natural disasters, sporting matches, and experimentally assigned lotteries (Achen and Bartels 2016; Healy et al. 2010; Huber, Hill, and Lenz 2012). As Healy and Malhotra (2010) note, however, in many of these studies, voters may plausibly be blaming leaders for failing to anticipate, or adequately respond to, seemingly irrelevant events. In only one other observational study have incumbents been shown to be punished in response to events for which anticipation and response are utterly implausible (Fowler and Montagnes 2015; Healy et al. 2010).²⁶

While similar to findings regarding "random" events, the behavior observed in the present study is potentially even more troubling for democracy. Judging the executive on the basis of sports matches may decrease his/her effort, but judging him/her on the basis of legislative performance may give the legislature an incentive to enact harmful policies. In addition to documenting voters' behavior, future studies should also begin to focus on how such behavior actually changes the incentives of elites. Do policy makers react to voters' attribution behavior by decreasing their effort? While officials' reactions to voters' "failure to filter" in single-actor settings has been studied theoretically (Ashworth and Bueno

de Mesquita 2014), how such behavior affects elite behavior in multi-actor settings now merits greater theoretical and empirical inquiry.

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26. As in the "random events" studies, voters' reactions to tax increases may not necessarily be "irrational." As Huber et al. (2012) note, "Rational voters [may] act on the basis of a knowingly imperfect heuristic because doing otherwise is too costly" (731).

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