

# How Electoral Institutions Affect Political Accountability: Evidence from All-Mail Elections\*

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## Abstract

A central topic in the study of democratic governance concerns the conditions under which voters make informed choices at the ballot box and are able to hold elected officials accountable. In this paper, I exploit the staggered implementation of all-mail elections in Washington to study the effects of electoral institutions on political accountability. I find that all-mail elections cause a decrease in taxing and spending in the state's municipalities. This is consistent with my argument that the reform causes voters to gather more information about politics than they would when voting at a polling place and, as a result, that elected officials are more competent and responsive after the implementation of vote-by-mail. I present additional evidence to support my theory. First, I show that vote-by-mail causes a decrease in violations on accountability audits and an increase in turnout in municipal elections. Second, using data on the characteristics of voters in municipal elections in Washington I show that the municipal finance results cannot be explained by changes in the composition of the electorate induced by all-mail elections.

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An enduring problem in democracies is determining how to overcome the informational asymmetries between elected officials and the public in order to ensure that policy makers are faithful agents of the electorate. Voters often do not have sufficient knowledge to make informed choices while voting because relevant political information is simply unavailable or citizens choose not to seek it out. The task that voters are faced with is especially difficult in democracies where there are many layers of government and, consequently, many candidates that voters must evaluate in any given election. This lack of political knowledge among voters can result in the electorate selecting incompetent politicians or ineffectively monitoring incumbents, which causes a breakdown in the accountability-inducing mechanism of elections. However, a large formal theory literature on elections suggests that institutions that increase voters' access to political information can result in politicians working harder for and more in line with the interests of their constituents (Ashworth 2012, Besley 2006).

I propose that electoral institutions shape how voters gather information about politics and evaluate incumbents, which affects the behavior of elected officials. Particularly, I argue that all-mail elections<sup>1</sup> result in voters obtaining more information about politics than they do when voting in a traditional polling place. A voter is able to gain relevant political information if he is voting in an all-mail election because when filling out his ballot he has the ability and time to learn more about the races that he knew nothing or little about before receiving his ballot. However, if an individual comes across a race in which he knows nothing about the candidates when voting at a polling place, he is unable to obtain additional information before casting his vote. Increases in information among voters strengthens the electorate's ability to hold politicians accountable through elections (Snyder and Strömberg 2010). Therefore, I expect that elected officials will be more competent and responsive to the preferences of their constituents when they are selected with all-mail elections instead of polling place elections.

In this paper, I examine the impact of vote-by-mail on political accountability and policy outcomes by exploiting the staggered implementation of the institution in an American state with a difference-in-differences research design. I also trace out the effects of vote-by-mail on additional dimensions of elected official behavior and voter behavior to corroborate my interpretation of the main findings. In particular, I leverage a law passed in the state of Washington that allowed individual counties to decide if and when they wanted to switch to all-mail elections. In the main analyses, I examine the effects of vote-by-mail on mu-

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<sup>1</sup>In this paper, I use the terms vote-by-mail and all-mail elections interchangeably to refer to an election where ballots are delivered to all voters by mail, which is also known as postal ballot delivery.

municipal fiscal policy in Washington. Based on previous work that finds voters generally dislike government spending (Peltzman 1992), I expect that there will be a decrease in revenue and expenditures in Washington's municipalities after switching to vote-by-mail because voters will be more informed. Consistent with this hypothesis, I find that vote-by-mail causes a decrease in the levy amount for property taxes that is determined by elected municipal officials. This translates into a decrease in revenue from property taxes and a decrease in total revenue and expenditures in municipalities. In an additional set of analyses I provide evidence consistent with my theory that the decrease in taxing and spending is caused by voters gathering additional information about politics after voting by mail. First, I show that after the switch to vote-by-mail there is a decrease in the number of negative findings on accountability audits of municipal governments. These estimates, however, are not precisely estimated. Second, analysis of voter behavior data supports my interpretation that the effects of vote-by-mail on policy outcomes are the result of voters gathering more information about politics. In particular, I find that vote-by-mail causes an increase in the number of ballots cast in municipal elections. In addition, I find that the electorate is not more conservative after vote-by-mail is implemented, which rules out a potential alternative explanation for the municipal fiscal policy findings.

This paper makes contributions to a number of different literatures. First, the findings have implications for our understanding of responsiveness in local governments in the United States. As Trounstein (2010) notes, there is a lack of research on responsiveness and accountability at the local level. This paper builds on more recent work that demonstrates how fiscal policy outcomes in cities and counties are influenced by the ideology of its citizens (Einstein and Kogan 2016, Tausanovitch and Warshaw 2014), whether or not government officials are appointed or elected (Sances 2016), and the party identification of politicians (de Benedictis-Kessner and Warshaw 2016; 2019) by showing how vote-by-mail also shapes local government spending. Second, I show that vote-by-mail, like other electoral institutions including election timing (Anzia 2011), ballot technology (Fujiwara 2015), and compulsory voting (Bechtel, Hangartner and Schmid 2016, Fowler 2013), affects policy outcomes. Third, although there is a large literature on the effects of vote-by-mail in the United States (Bergman and Yates 2011, Berinsky, Burns and Traugott 2001, Gerber, Huber and Hill 2013, Kousser and Mullin 2007, Magleby 1987, Richey 2008), it focuses almost exclusively on how the institution affects voter turnout and the composition of the electorate.<sup>2</sup> More recent work, however,

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<sup>2</sup>One notable exception is Meredith and Malhotra (2011) who use the 2008 presidential primary in California to show that when information is revealed close to Election Day vote-by-mail and polling place voters will vote differently. In addition, other work has examined the impact of vote-by-mail on ballot roll-off (Alvarez, Beckett and Stewart 2013, Dubin and Kalsow 1996, Hanmer and Traugott 2004, Marble 2017, Menger, Stein and Vonnahme 2018, Szweczyk 2019).

directly examines the impact of all-mail elections on voter knowledge and finds that the reform increases the amount of information that voters have about politics (Szewczyk 2019). I expand our understanding of vote-by-mail by building on this research and examining downstream consequences of the reform on elected official behavior. In all, I demonstrate how this electoral reform has profound effects on electoral accountability and public policy.

## **Vote-by-Mail, Political Information, and Electoral Accountability**

The central theoretical argument in this paper is that holding elections by mail causes a change in voter behavior that gives voters the ability to gather more information about politics than they would when voting at a polling place. As a result, elected officials will implement policies that are more in line with citizens' policy preferences. The mechanism that increases the probability that voters who cast their ballots in all-mail elections will obtain information about elected officials is simple: voters have the ability and time to gather more information about candidates and races for which they would otherwise have little or no information when they participate via vote-by-mail rather than at a polling place. This builds on previous work that suggests that vote-by-mail may increase the likelihood voters gather political information (Marble 2017, Menger, Stein and Vonnahme 2018, Szewczyk 2019). In this section, expand on this intuition and discuss how voters and elected officials in Washington changed their behavior after the switch to vote-by-mail.

Due to the large number of elected offices and multiple layers of governments in the United States, there are many officials that voters are tasked with holding accountable through elections (Berry and Gersen 2009). However, most citizens do not have sufficient information or interest in a particular office to evaluate all of these elected officials when casting a ballot. There are typically many races on the ballot in a given election for which voters have little information about the candidates running for office or the performance of the incumbent if she is running for reelection. Moreover, if an individual comes across a race that he knows nothing about when casting a ballot at a polling place, he has neither the time nor the ability to obtain additional information about these races.

However, when elections in which all voters are mailed a ballot are held, the process of voting is much different. Voters receive their ballots multiple weeks before an election and are able to examine the ballot for an extended period of time before deciding how to vote. Thus, voters have the ability to gather information about the candidates running for office and relevant policy issues. This informational effect will be partic-

ularly large for the numerous races for local office that are not very salient. Anecdotal evidence suggests that individuals do indeed seek out political information while filling out their ballot when they participate in a vote-by-mail election. For example, one voter in Washington said, “When I voted at the polls, I would not read up as much. Now that I have the absentee, I have the time to really read things” (Reed 2007). This quote illustrates that voters who otherwise would not look up information about a low-salience race on the ballot when voting in a polling place choose to do so when they have more time with their ballot when voting by mail.

Further, others have argued that all-mail elections add a new social aspect to voting that results in discussion about politics, which will also increase levels of voter knowledge (Eveland 2004). For example, an opinion piece in the newspaper *The Columbian* observes that under vote-by-mail, “Many voters enjoy gathering with children to share the research, deliberation and voting experiences” (*In Our View: A Good Way to Vote* 2010). Thus, not only are voters taking more time to conduct research about politics under a vote-by-mail system but they are also building additional discussion about the issues into their voting experience. This should result in vote-by-mail voters learning more about the candidates running for office.

Whether they are researching on their own or discussing the election with family, there are a number of tools that voters can use to gather information about candidates or ballot measures when they are voting at home. For example, voters have a great deal of information available to them through newspaper coverage and the Internet. Voters can use the extra time with their ballot to conduct Google searches about the races that appear on the ballot. Importantly, all households in the state of Washington are also mailed a Voters’ Pamphlet that contains biographical information about the candidates running for office and detailed descriptions of ballot measures. At a minimum, this source of information can help voters select more experienced and competent candidates based on their background and previous experiences that are outlined in the pamphlet. In sum, the core argument of this paper is that when voting in an all-mail election voters have access to information about the election while voting and naturally have the time to conduct additional research about candidates on the ballot. Consequently, under a vote-by-mail electoral system voters will be more informed, on average, than they would be when voting in a traditional polling place systems, and this increase in knowledge will cause changes in the behavior of elected officials.

For the effects of vote-by-mail on voter behavior to also affect the behavior and incentives of politicians, they must be aware of the changes in voter behavior that are the result of the reform. In the case of Washington, elected officials, indeed, are cognizant of the effects that vote-by-mail has on the voting experience

and that voters will be more informed than when they voted at a polling place. For example, the Secretary of State in Washington, an elected office, says, “He...believes voters probably make better-informed choices, sitting down at the dining room table with voters’ guides, editorials and Web sites available” (*Is Online Voting Next for Washington Voters?* 2006). Incumbent politicians, therefore, understand that when the jurisdiction that they are representing switches to a vote-by-mail electoral system that voters will be gathering more information about their performance in office.

Because incumbents are aware of this change in voter behavior, vote-by-mail should also induce changes in behavior among elected officials. A rich theoretical literature on political agency models sheds light on the principal-agent relationship between citizens and their elected officials by looking at how the institutional context of elections structures the interactions between voters and politicians (Ashworth 2012). For example, Besley (2006) presents a two-period model where voters evaluate the incumbent based on her performance in office and the incumbent’s behavior is conditioned by her anticipation of the voters’ decision rule. As in all agency models, the key strategic tension in this model is that elected officials have an informational advantage over citizens about the ideal policy, which creates a number of problems for monitoring incumbents and the selection of competent policy makers. Voters are likely to be poorly informed about the optimal policy that should be implemented as the government has more knowledge about the nuances of the policy environment. Moreover, it may also be the case that voters do not observe what policies are implemented and the consequences of those policy choices, which also contributes to this informational asymmetry. When this is the case, elected officials are not necessarily incentivized to work in the best interest of their constituents.

However, changes in voter information levels, like those induced by all-mail elections, should affect the behavior of incumbents and policy outcomes. Indeed, a large class of models suggests that increasing policy relevant information about the incumbent politician increases the responsiveness of elected officials (Besley 2006). An institution like vote-by-mail decreases the amount of noise in voters’ evaluations of incumbents because it makes it easier for voters to gather political information. Therefore, I expect that vote-by-mail should change the incentives of elected officials and cause incumbents to be more responsive to voters. In addition, any observed changes of all-mail elections on policy outcomes could be the result of selection of new elected officials. That is, voters will be better able to select more competent politicians after vote-by-mail is implemented because they will be more informed about the candidates running for office.

In sum, I argue that vote-by-mail affects the behavior of voters and, as a result, the strategic context of elections. Holding all-mail elections results in voters gathering more information about politics and an electorate that is more informed than they would be if they voted at traditional polling places. Based on the theoretical literature on electoral accountability, I posit that this change in voter behavior will affect the ability of the electorate to select competent elected officials and the incentives of incumbent politicians. As a result, vote-by-mail has the potential to cause elected officials to be more responsive to the preferences of voters and to be more competent.

## **Observable Implications**

To test the implications of this theory I estimate the effects of vote-by-mail on fiscal policy in Washington's municipalities. Based on previous research, I expect that, because voters will be more informed, vote-by-mail will cause a decrease in municipal taxing and spending. There are two mechanisms through which this could happen. First, if elected officials know that their actions are more likely to be observed by their constituents they will put more effort into their jobs. The relevant political agency literature argues that incumbents who exert relatively higher levels of effort will provide services at a lower cost (Persson and Tabellini 2000). Therefore, the increased information that voters gather as a result of vote-by-mail will cause a decrease in revenue and property tax levels in Washington's municipalities.

Second, although individual voters may differ with regard to which policy they think is optimal among a host of different choices, most voters prefer that government services are provided to the public efficiently and at a low cost. Indeed, previous research finds that voters punish incumbents for high levels of taxing and spending (Besley and Case 1995*b*) and when governors in U.S. states face term limits taxing and spending increases (Besley and Case 1995*a*, Alt, De Mesquita and Rose 2011). Further, there is evidence that when policymaking officials are elected rather than appointed that tax revenue decreases. For example, Sances (2016) shows that when towns in New York switch from appointed to elected tax assessors that there is a decrease in assessed property values and, consequently, tax rates. This is also consistent with research that finds when voters are faced with ballot measures, and policy is more likely to reflect the preference of the median voter, that levels of government spending and tax rates decrease (Matsusaka 2004).

I corroborate my interpretation that the effects of vote-by-mail on taxing and spending are the result of increases in voters' political knowledge by examining an additional set of dependent variables. Finding the

expected relationship between all-mail elections and these outcomes should bolster our confidence in this theoretical mechanism. First, I expect that under a vote-by-mail electoral system elected officials will avoid misappropriating resources or breaking laws, which hurts their reelection chances, as they anticipate these actions are more likely to be observed by voters. This is consistent with previous work that examines how electoral incentives affect corrupt behavior among politicians (Ferraz and Finan 2011). I hypothesize that in political jurisdictions with vote-by-mail elections there will be more safeguards of public resources and fewer breaches of good governance policies. Second, I expect that there will be a decrease in ballot roll-off on municipal races after the switch to vote-by-mail (Marble 2017, Menger, Stein and Vonnahme 2018, Szewczyk 2019). To test this hypothesis, I estimate the effect of vote-by-mail on the number of ballots cast for municipal elections and compare it to the impact of the reform on total turnout. The logic of this analysis is that if voters are paying more attention to races for municipal office after the implementation of all-mail election, they should be more likely to cast a vote in these races.

## **Estimating the Effects of Vote-by-Mail**

Identifying the causal effects of an electoral reform is challenging due to the absence of a counterfactual unit that is exactly the same as the one observed except for the absence or presence of the institution of interest. I cannot simply compare places that do and do not hold all-mail elections at a given point in time using cross-sectional data. This would lead to biased estimates of the effects of vote-by-mail if, for example, citizens in jurisdictions with vote-by-mail have an underlying propensity to be more engaged with politics because it could affect whether or not a jurisdiction adopts vote-by-mail and the outcome variables of interest. I overcome these issues by leveraging a policy intervention that resulted in a staggered roll-out of vote-by-mail in Washington state.

## **Municipal Elections in Washington**

Elections for municipal office in Washington are low-information environments that are held off-cycle in November of odd-numbered years. Although it may be the case that holding local elections off-cycle results in more attention being paid to local politics rather than voters solely focusing on national races, off-cycle elections are typically low-salience and low-turnout contexts (Berry and Gersen 2010, Hajnal and Lewis 2003). Indeed, previous research has shown that local elections are indicative of an electoral environment



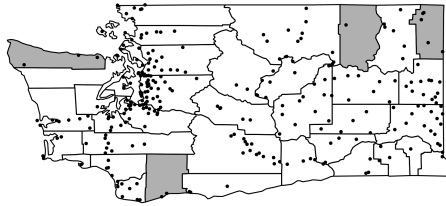
in which incumbents are not held accountable because voters do not use available political information. For example, Kogan, Lavertu and Peskowitz (2016) find that the implementation of school performance measures in Ohio had little effect on retrospective evaluations of elected school board officials in the state. Thus, the theoretical framework that I have presented is especially applicable for thinking about accountability in local elections as there are relatively low levels of information about elected officials and policy outcomes.

Washington's municipal elections in particular lack a number of features that typically send informational signals to voters. Specifically, these local elections are non-partisan and there is no marking of who the incumbent is on the ballot. Party identification serves as a strong heuristic for voters and the absence of this shortcut decreases the informational content of the ballot (Schaffner, Streb and Wright 2001). Moreover, because there is no signal of who the incumbent is on the ballot, voters who have relatively low levels of political information and do not know the name of the incumbent will not be able to perform a simple retrospective evaluation of her performance. Therefore, an institutional change, like vote-by-mail, that affects how voters' access political information should have a significant effect on electoral accountability in this type of environment.

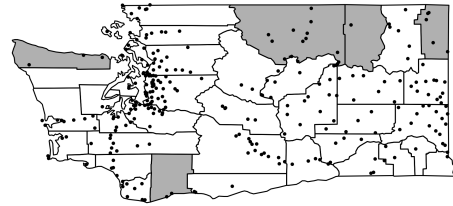
### **The Staggered Implementation of Vote-by-Mail in Washington**

Most importantly for this study, Washington presents an excellent case for analyzing the effects of vote-by-mail on electoral accountability due to the staggered implementation of the institution in the state. Washington experienced a gradual transition from traditional polling places to vote-by-mail that I exploit in order to estimate the causal effects of vote-by-mail on political behavior and policy outcomes. Starting as early as 1915 Washington allowed for absentee voting using a mail-in ballot in which voters were able to apply to receive their ballot in the mail rather than going to a polling place if they could provide a reason as to why they could not vote on election day (Reed 2007). The state passed a law that allowed voters to apply for permanent absentee status in 1993, which gave voters the ability to receive their ballot in the mail without an excuse.

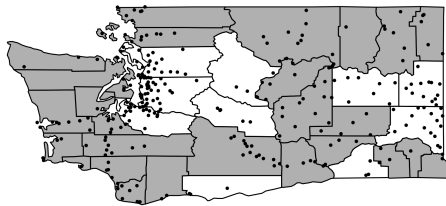
There were also a number of reforms in Washington that gradually forced citizens to use vote-by-mail, which is the source of treatment in this study, rather than individual voters being able to choose to receive their ballot in the mail. First, in 1967 the state allowed county auditors in the state's 39 counties to assign precincts with fewer than 200 registered voters to hold all-mail elections, and some rural counties with small populations redrew their precincts so they were small enough to allow the entire county to hold all-



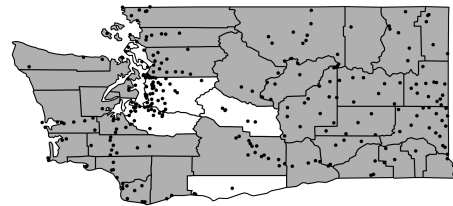
(a) 2003



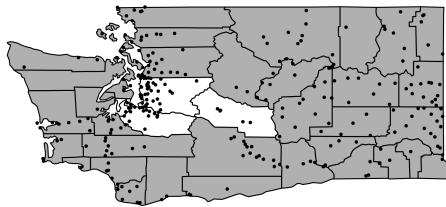
(b) 2004



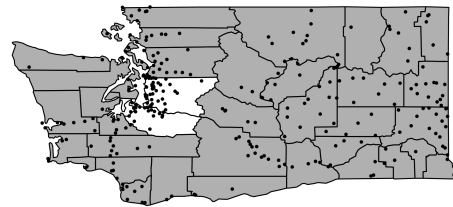
(c) 2005



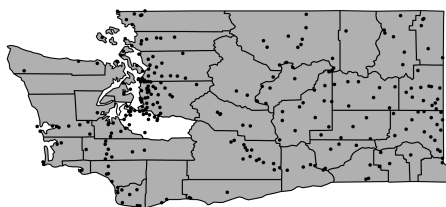
(d) 2006



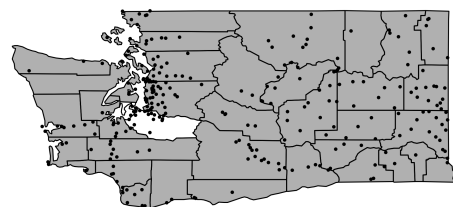
(e) 2007



(f) 2008



(g) 2009



(h) 2010

Figure 1: Maps of Washington's counties and municipalities (black dots). Grey shading indicates that a county held vote-by-mail elections in a given year.

mail elections (Reed 2007). Second, in 2005 the state legislature passed a law, HB 1754, allowing individual counties to decide if they wanted to implement all-mail elections in their jurisdictions. Counties, under the direction of their county councils, gradually switched to vote-by-mail elections in the years following HB 1754. Finally, in 2011 only Pierce County had yet to implement all-mail elections, and the state legislature enacted a law forcing the county to implement vote-by-mail.

Figure 1 displays maps that depict the transition to vote-by-mail in select years. The outline of each county is displayed on the map along with black dots representing each municipality in the state. Grey shading on the map indicates that a particular county held all-mail elections in a given year while the counties shown in white held polling place elections. Prior to 2005, a total of five counties had sufficiently small precincts to allow vote-by-mail elections for the entire county. After the state allowed individual counties to switch to vote-by-mail in 2005, many counties decided to immediately discontinue polling place elections. Then, almost every year after 2005 more counties switched to vote-by-mail until the state legislature required that all elections in the state were held using vote-by-mail in 2011. In Figure F.1 in the Online Appendix, I show the number of counties and municipalities that held vote-by-mail elections in each year.

## **Estimating Equation**

My estimation strategy follows Gerber, Huber and Hill (2013) who examine the effects of the transition to all-mail elections on voter turnout in Washington. In particular, I exploit the staggered implementation of vote-by-mail in Washington with a difference-in-differences research design. This design allows me to compare changes in municipalities and counties where officials were elected using vote-by-mail, the treatment group, to changes in municipalities and counties where officials were elected using a traditional polling place election, the control group. I classify a county and all municipalities within it as being in treatment for all of the years in which the county held all-mail elections and the control group consists of all other counties and municipalities that have not yet switched to vote-by-mail in a given year. It is possible that counties where the local election officials intentionally redrew precincts to force vote-by-mail elections prior to 2005 follow different pretreatment trends in the outcomes of interest, which would violate the parallel trends assumption necessary for a causal interpretation of the difference-in-differences estimates. Therefore, I estimate all of the empirical models using the full sample of political units in the state as well as a restricted sample that is only the subset of counties or municipalities that switched to vote-by-mail in 2005 or later. The interpretation of the estimated effects from the restricted sample is the impact of all-mail

elections in Washington on the outcomes of interest among counties or municipalities that implemented the electoral reform because of HB 1754. Meanwhile, with the full sample I estimate the effects among the universe of municipalities or counties in the state.<sup>3</sup>

I generally estimate OLS models of the following form,

$$Y_{i,t} = \alpha_i + d_t + \beta VBM_{i,t} + \delta^T \mathbf{X}_{i,t} + \gamma_i t + \epsilon_{i,t} \quad (1)$$

where  $Y_{i,t}$  represents the outcome variables of interest. On the right hand side of the equation,  $\alpha_i$  represents either county or municipality fixed effects, depending on the model, to control for time invariant differences between counties or municipalities. To control for year specific shocks I include year specific fixed effects,  $d_t$ .  $\mathbf{X}_{i,t}$  is a vector of unit specific control variables that vary over time.  $VBM_{i,t}$  is a dummy variable that is equal to 1 after a county or the municipalities within it switch to vote-by-mail and  $\beta$  is the causal effect of interest. I also display the results of models for all dependent variables that include a municipality-specific or county-specific linear time trend,  $\gamma_i t$ . I report robust standard errors clustered by county.

## Data

I draw on a wide variety of sources to gather data for the analyses in this paper. I now discuss each in turn.

**Municipal Finances.** First, I collect information on municipal property tax levies from 1999 to 2010 from the Washington State Department of Revenue. The property tax levy is the total amount of money that elected officials in a municipality determine must be raised from property taxes in order to fund the budget in that year. Levies are reported by the Department of Revenue in each year as one value for all of the municipalities in a given county. So, although this variable,  $LogLevyPerCapita_{c,t}$ , represents the property tax amount levied by municipalities, it varies at the county level.

In addition to property tax levy amounts, I use data from 2001 to 2010 on the actual level of revenue and expenditures in municipalities from the Washington State Auditor's Office. The Auditor's Office collects detailed revenue and expenditure data from each municipality in the state at the end of every calendar year. This gives me the ability to examine municipal fiscal policy at the municipality-level rather than relying only on property tax levies aggregated up to the county-level. I create variables measuring property tax

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<sup>3</sup>However, I omit the six municipalities in Washington that cross county borders and one municipality that was incorporated during the time period of interest from the analysis.

revenue,  $\text{LogPropertyTaxRevenuePerCapita}_{m,t}$ , total revenue,  $\text{LogRevenuePerCapita}_{m,t}$ , and total spending,  $\text{LogExpendituresPerCapita}_{m,t}$ , in municipalities. All of the fiscal policy variables are generated by taking the total amount of money in a given category and then normalizing the amount to 2001 real dollars using the consumer price index. Then, I divide the totals by the population in the municipality or county and log this value in order to adjust for skew in the data.

**Municipal Audits.** Municipalities are regularly subject to “accountability audits” and “financial audits” that are completed by representatives of the Washington State Auditor’s Office. Accountability audits examine if municipalities are following necessary policies and laws, while financial audits assess if municipalities’ financial statements are accurate. Accountability audits are completed at least once every three years in each municipality and financial audits are done yearly for municipalities that have revenues that exceed a threshold. After an audit is completed, the auditor summarizes any bad behavior in the municipality by noting if there are any “findings.” Each finding explicitly states a specific breach of law or protocol by a municipal official. These reports include the misconduct of both career government employees as well as elected officials. But, elected officials are in charge of managing the government so I expect that a shock, such as vote-by-mail, that induces elected officials to act more responsibly will affect the outcomes of these audits. Figure 2 displays an example of an activity for which a municipality could receive a finding.

| <b>Schedule of Audit Findings</b>  |   |
|--|---|
| <p><b>City of Spokane</b><br/> <b>Spokane County</b><br/> <b>November 15, 2006</b></p> |   |
| <b>2.</b>  | <b>The City of Spokane did not follow competitive bid law.</b>  |
|  | <b>Description of Condition</b>   |
|  | The City is required to formally bid public works projects in excess of \$70,000 unless an allowable exception to the bidding requirements is identified. The City paid \$2,317,639 to a contractor to build a gondola in Riverfront Park without going through a formal bid process. |

Figure 2: Example of an audit finding in a municipality in Washington.

I obtain the audit reports from the Washington State Auditor’s website where a PDF of each report from January 2005 to present is publicly available. The audit periods covered in my data, however, begin prior to 2005 because the audit reports are released a few years after the time period that is being audited. The type of audit, the time period for which the government’s activities were being audited, and the number of “findings” that the auditor discovers are collected from the reports. This information is used to create the

dependent variable,  $AvgFindings_{m,t}$ , which is the average number of findings for a municipality in a given time period. Higher values of this variable indicate that more laws and regulations were broken or public resources were misappropriated in the municipality.

**Election Results.** I gather election data from two sources. First, I collect the results of municipal elections in Washington from individual county election offices for all odd-year November general elections from 2001 to 2011. I create the variable  $MunicipalTurnout_{m,t}$  by taking the total number of ballots cast for the race in each municipality that received the most votes in a given year and dividing it by the Citizen Voting Age Population (CVAP) from the U.S. Census. Second, the variable  $CountywideTurnout_{c,t}$  is generated by taking the total number of ballots cast in odd-year general elections from 2001 to 2011 in each county and dividing it by the CVAP at the county level. I collect this data from the Washington Secretary of State.

**Demographic Controls.** I control for demographic characteristics of Washington's counties and municipalities that may affect both the timing of the switch to vote-by-mail and the outcomes of interest. This data comes from the 2000 Decennial U.S. Census and the 2009, 2010, and 2011 American Community Survey's 3 Year Estimates. I linearly interpolate the Census data for each missing year of the panel. I control for total population as well as the distribution of racial characteristics, educational attainment, age, income, and rural residents in each jurisdiction.

**Vote-By-Mail.** The main independent variable of interest in my analyses,  $VBM_{i,t}$ , is created using information from the Washington Secretary of State's Office about when each county switched to vote-by-mail. It is a dummy variable equal 1 in all years when a county or municipality holds all-mail elections and 0 otherwise.

## Vote-by-Mail Causes a Decrease in Taxing and Spending

In this section, I examine the effects of vote-by-mail on revenue and expenditure levels in Washington's municipalities. I first estimate the effects of all-mail-elections on municipal property tax levies. Washington uses a budget-based system to calculate property taxes, which are determined using the levy, assessed property value, and levy rate. Each year taxing districts in the state, including municipalities, set a budget and determine how much property tax revenue is needed to fund the budget. This is known as the property tax levy, which is the policy lever regarding property taxes that elected officials in municipalities have direct

control over. The amount of money that residents pay in property taxes, the levy rate, is determined based on their property value and the size of the municipal property tax levy.

The results of models in the form of Equation 1 with log property tax levy per capita as the dependent variable are shown in Table 1. Recall that this variable measures municipal property tax levies aggregated up to the county level. All of the models include county and year fixed effects. In Columns 2 and 3 I add demographic control variables and in Column 3 I add county-specific time trends to the models. Looking first at the top panel, which displays the estimates with the full sample of data, I find evidence that the implementation of vote-by-mail causes a decrease in the amount of the municipal property tax levy in Washington. After taking into account the log transformation of the dependent variable, I estimate that the electoral reform results in a 3.7% to 4.4% decrease in municipal property tax levies. The results with the restricted sample, in the bottom panel of Table 1, are substantively similar although the coefficient on the vote-by-mail variable is not statistically significant in the model with demographic controls in Column 2. These results provide evidence in support of my hypothesis that switching to a vote-by-mail electoral system will cause a decrease in taxes.

Table 1: The Effects of Vote-by-Mail on Municipal Property Tax Levies

|                           | Property Tax Levy  |                     |                      |
|---------------------------|--------------------|---------------------|----------------------|
|                           | (1)                | (2)                 | (3)                  |
| <i>Full Sample:</i>       |                    |                     |                      |
| Vote-by-Mail              | −0.037*<br>(0.019) | −0.041**<br>(0.019) | −0.044***<br>(0.017) |
| Observations              | 468                | 468                 | 468                  |
| Adjusted R <sup>2</sup>   | 0.984              | 0.985               | 0.991                |
| <i>Restricted Sample:</i> |                    |                     |                      |
| Vote-by-Mail              | −0.042*<br>(0.023) | −0.035<br>(0.024)   | −0.041**<br>(0.021)  |
| Observations              | 408                | 408                 | 408                  |
| Adjusted R <sup>2</sup>   | 0.980              | 0.981               | 0.988                |
| County FE                 | Yes                | Yes                 | Yes                  |
| Year FE                   | Yes                | Yes                 | Yes                  |
| Controls                  | No                 | Yes                 | Yes                  |
| County Time Trends        | No                 | No                  | Yes                  |

*Note:* Robust standard errors clustered by county in parentheses.  
 \*p<0.1; \*\*p<0.05; \*\*\*p<0.01. Control variables include the following demographic controls at the county level: percent Black, percent Latino, median income, percent with high school degree or higher, percent urban and total population.

Although one may expect that changes in property tax rates will be obvious to voters regardless of how informed they are because it is easy for voters to observe how much they have to pay each year, property taxes in the state of Washington are some of the most complicated in the country. Annual property tax levy rates in Washington are capped at \$10 across all taxing districts for every \$1,000 in property value. Municipalities are typically able to have a levy rate up to \$3.375, for every \$10. In addition, the increase in the levy from the previous year cannot exceed 1% and a municipality's levy can be decreased depending on the levies of other taxing districts that overlap with the municipality. The ability of citizens to hold elected officials accountable is hindered by confusion about government responsibilities (de Benedictis-Kessner 2018), so uninformed voters in Washington may be unable to effectively punish the correct politicians for tax increases. That is, the calculation of property taxes in Washington is incredibly complex and it is challenging for voters with low levels of political information to assign credit or blame for changes in property tax rates without seeking out additional information.

The results thus far show that all-mail elections in Washington caused municipalities to lower their property tax levies. This is evidence that elected officials in municipalities are responsive to this electoral reform because this is the piece of the property tax equation that they have direct control over. To tease out the consequence of this decrease in property tax levies, I next examine the effects of vote-by-mail on the total amount of revenue and expenditures in Washington's municipalities. I also estimate the effects of vote-by-mail on the actual property tax revenue obtained by municipalities because, unlike the property tax levy data, the revenue data is observed at the municipality level. This allows for a more natural substantive interpretation of the point estimates and gives me the ability to control for municipality specific demographic variables and include municipality fixed effects in the models. Table 2 displays the results of models with log property tax revenue per capita (Columns 1-3), log total revenue per capita (Columns 4-6), and log total expenditures per capita (Columns 7-9) as the dependent variables. As before, the top panel displays the effects of vote-by-mail with the full sample of data and the results with the restricted sample are shown in the bottom panel.

First, the results with property tax revenue as the dependent variable corroborate the findings in Table 1. The effect of vote-by-mail on property tax revenue is negative and at least marginally statistically significant across all of the specifications of the model. The estimated effect ranges from a 2.7% to 4.0% decrease, which is similar to that for the property tax levy. Further, looking at the models with log total revenue per capita and log total expenditures per capita as the dependent variables, there is a consistent theme:



revenue and expenditures decrease when a municipality switches to vote-by-mail. The results maintain standard levels of statistical significance across nearly all specifications of the model. For example, in the model with the full sample of municipalities and controls for demographic characteristics, switching to vote-by-mail leads to a 5.6% decrease in revenue per capita (top panel, Column 5). Although, this estimate is only marginally significant ( $p < 0.1$ ), in the models estimated with the restricted sample of data the coefficients on the vote-by-mail variable achieve traditional levels of statistical significance ( $p < 0.05$ ). In the analogous model with log expenditures per capita as the dependent variable the coefficient on the vote-by-mail variable is statistically significant and equal to -0.066 (top panel, Column 8), which indicates that vote-by-mail elections cause a 6.6% decrease in total expenditures per capita. However, the point estimate on the vote-by-mail variable shrinks considerably after municipality time trends are included in the model for both dependent variables. Nevertheless, this provides strong evidence that vote-by-mail causes elected officials to implement more conservative fiscal policies in Washington's municipalities.

Table 2: The Effects of Vote-by-Mail on Municipal Revenue and Expenditures

|                           | Property Tax Revenue |                      |                     | Total Revenue       |                     | Total Expenditures |                      |                      |                   |
|---------------------------|----------------------|----------------------|---------------------|---------------------|---------------------|--------------------|----------------------|----------------------|-------------------|
|                           | (1)                  | (2)                  | (3)                 | (4)                 | (5)                 | (6)                | (7)                  | (8)                  | (9)               |
| <i>Full Sample:</i>       |                      |                      |                     |                     |                     |                    |                      |                      |                   |
| Vote-by-Mail              | -0.038***<br>(0.010) | -0.036***<br>(0.009) | -0.029**<br>(0.012) | -0.049*<br>(0.029)  | -0.056*<br>(0.032)  | -0.037<br>(0.033)  | -0.061**<br>(0.026)  | -0.066**<br>(0.031)  | -0.029<br>(0.032) |
| Observations              | 2,566                | 2,566                | 2,566               | 2,567               | 2,567               | 2,567              | 2,567                | 2,567                | 2,567             |
| Adjusted R <sup>2</sup>   | 0.933                | 0.935                | 0.958               | 0.699               | 0.703               | 0.743              | 0.691                | 0.696                | 0.739             |
| <i>Restricted Sample:</i> |                      |                      |                     |                     |                     |                    |                      |                      |                   |
| Vote-by-Mail              | -0.040***<br>(0.011) | -0.038***<br>(0.010) | -0.027*<br>(0.014)  | -0.056**<br>(0.026) | -0.067**<br>(0.028) | -0.051*<br>(0.028) | -0.073***<br>(0.021) | -0.081***<br>(0.025) | -0.043<br>(0.027) |
| Observations              | 2,359                | 2,359                | 2,359               | 2,360               | 2,360               | 2,360              | 2,360                | 2,360                | 2,360             |
| Adjusted R <sup>2</sup>   | 0.931                | 0.932                | 0.956               | 0.700               | 0.703               | 0.744              | 0.691                | 0.695                | 0.740             |
| Munic FE                  | Yes                  | Yes                  | Yes                 | Yes                 | Yes                 | Yes                | Yes                  | Yes                  | Yes               |
| Year FE                   | Yes                  | Yes                  | Yes                 | Yes                 | Yes                 | Yes                | Yes                  | Yes                  | Yes               |
| Controls                  | No                   | Yes                  | Yes                 | No                  | Yes                 | Yes                | No                   | Yes                  | Yes               |
| Munic TT                  | No                   | No                   | Yes                 | No                  | No                  | Yes                | No                   | No                   | Yes               |

*Note:* Robust standard errors clustered by county in parentheses. \* $p < 0.1$ ; \*\* $p < 0.05$ ; \*\*\* $p < 0.01$ . Control variables include the following demographic controls at the municipality level: percent Black, percent Latino, median income, percent with high school degree or higher, percent urban and total population.

These are substantively significant decreases in taxing and spending. In the average municipality the property tax revenue per capita is \$204.84 and vote-by-mail causes a \$5.45 to \$8.27 decrease in property tax revenue per capita, depending on the model. This appears to be a somewhat small effect but the property

tax burden is not distributed evenly across the population so some voters may see substantial decreases in their property tax payments while voters who do not own a home will not be effected by this change. In addition, in terms of total property tax revenue raised by the average municipality this is equal to a decrease in \$115,589.90 of funding, at a minimum. This indicates that there is a substantially large amount of money that municipal governments no longer have access to after the switch to vote-by-mail. Further, the effect of vote-by-mail on total expenditures is even larger. In the average municipality there is about a \$132.42 decrease in expenditures per capita after the switch to vote-by-mail. It is also important to note that these estimates are likely a lower bound on the potential effects of vote-by-mail on taxing and spending, because most municipalities switched to vote-by-mail between 2005 and 2011. So, in municipalities that did not switch to vote-by-mail early on, elected officials may have expected that the county that they are located in would implement all-mail elections in the near future causing them to lower taxing and spending prior to the actual switch to vote-by-mail.

### **Placebo Tests and Additional Results**

The identifying assumption of the models presented in Table 2 is that the timing of the implementation of vote-by-mail is uncorrelated with time-varying municipality specific factors after controlling for the demographic characteristics of municipalities that change over time as well as municipality and year specific shocks. Because the decision to switch to all-mail elections in Washington was decided at the county level it is unlikely that vote-by-mail timing is correlated with the political characteristics or financial dynamics of municipalities. I further alleviate these concerns by estimating placebo tests in which I include leads of the vote-by-mail indicator variable to examine if the future implementation of all-mail elections causes changes in the municipal finance outcomes. The results of these models are in Tables B.1 and B.2 in the Online Appendix. For the property tax levy and property tax revenue dependent variables there is no evidence that future switches to vote-by-mail cause changes property tax levels (Table B.2). However, when total revenue and total expenditures are the dependent variables the coefficients on the lead variables are uniformly negative across all of the specifications of the model and in one specification (Column 6, Table B.1) a lead variable is a marginally significant ( $p < 0.1$ ) predictor of total expenditures. One potential explanation for this finding is anticipatory changes in spending patterns among policymakers. That is, it is possible that municipal officials decrease spending in the year prior to when their county adopts vote-by-mail because they expect that the reform will be implemented soon.

Next, I interrogate the main municipal fiscal policy findings by testing hypotheses derived from the theory that should hold if the observed effects of vote-by-mail on municipal finances are not false positives. The results of these analyses are displayed in Section B.2 of the Online Appendix. First, I analyze how municipal finance outcomes vary across cities with mayor-council governments compared to those with council-manager governments. Scholars argue that in cities with strong mayors policies better reflect the preferences of citizens (Sharp 1997, but see Tausanovitch and Warshaw 2014), so I expect that mayor-council governments will be more responsive to the switch to vote-by-mail. Indeed, in Table B.3 I show that the effects of vote-by-mail on the municipal finance outcomes of interest are driven by municipalities that have an elected mayor. These results, however, should be interpreted with caution because the governance structures of Washington's municipalities do not vary in the data so I cannot include municipality fixed effects in the models to control for municipality specific factors that do not change over time. Second, I expect that electoral competition will magnify the effects of vote-by-mail, because incumbents who believe that they will face strong challengers will be more likely to be responsive to changes in electoral systems and the behavior of voters. I display models in Table B.3 where I interact a measure of electoral competitiveness with the vote-by-mail indicator and find that the effects of vote-by-mail on total revenue and total expenditures become larger as competitiveness increases.

Finally, a potential concern with the analysis is that the vote-by-mail variable is capturing changes in taxing and spending determined in the year before the reform, which would indicate that is not actually all-mail elections causing these changes. I address this concern by estimating models with a series of lags of the vote-by-mail variable in Table B.4. I find that across all of the dependent variables the effects of vote-by-mail begin in the year after municipalities switch to the institution. In all, these results provide additional evidence that the municipal revenue and expenditures results documented in this paper are indeed the result of switching to vote-by-mail.

## **Evidence in Support of the Theoretical Mechanism**

I have established that implementing all-mail elections in Washington caused a decrease in taxing and spending in the state's municipalities. I argue that this is because voters gather additional information about politics when voting by mail. As a result, voters are able to select better elected officials or induce incumbents to act in their interest. One may be skeptical that these effects are actually the result of voters becoming

more informed about politics. For example, it is possible that vote-by-mail also affects the composition of the electorate and results in more conservative individuals turning out to vote, which could also cause these changes in policy outcomes. In order to bolster support for my proposed theoretical mechanism and address alternative explanations, I present a series of additional results. In particular, I analyze another dimension of the behavior of elected officials, audit outcomes, that is unrelated to municipal fiscal policy but could also be affected by increased voter knowledge. Then, I estimate the effects of vote-by-mail on voter turnout and the composition of the electorate.

### **Vote-by-Mail Causes a Decrease in Negative Accountability Audit Findings**

In this section, I examine the effects of implementing all-mail elections on the number of negative findings on accountability audits in Washington's municipalities. Findings on an audit represent government officials engaging in incompetent or unethical behavior. I expect that there will be a decrease in this quantity when a municipality switches to vote-by-mail, because voters are more likely to observe this bad behavior by their elected officials. This set of results provides additional support for my theory that vote-by-mail increases voter knowledge, because it cannot be explained by other mechanisms like changes in the composition of the electorate.

There are a number of challenges to analyzing the municipal audit data from Washington because audits do not occur every year and some audits take place over multiple years. To overcome these issues, I attribute the results of audits to the year in which the audit was completed and then take the average of this value if multiple audits were completed in the same year. I use this data to estimate models in the form of Equation 1 with the average number of findings in each year that an audit ended as the dependent variable. The data is not a balanced panel because there are not audits completed in each year for every municipality, which could cause biased estimates of the effects of vote-by-mail if the frequency of audits is correlated with the number of findings and the timing of the switch to all-mail elections. I estimate a series of non-parametric difference-in-differences models in Section D of the Online Appendix to address these concerns.

The results of the models estimated with the panel audit data are displayed in Table 3. Across all specifications of the model, the estimated effect of vote-by-mail on the average number of audit findings is negative, which is in the expected direction. In the models that do not include control variables (Column 1) and the models that include municipality time trends (Column 3) the coefficients are not statistically significant for the the full sample and the restricted sample. However, after the inclusion of control variables

Table 3: The Effects of Vote-by-Mail on Municipal Audit Findings

|                           | Number of Audit Findings |                     |                   |
|---------------------------|--------------------------|---------------------|-------------------|
|                           | (1)                      | (2)                 | (3)               |
| <i>Full Sample:</i>       |                          |                     |                   |
| Vote-by-Mail              | −0.060<br>(0.039)        | −0.077**<br>(0.039) | −0.062<br>(0.041) |
| Observations              | 1,552                    | 1,552               | 1,552             |
| Adjusted R <sup>2</sup>   | 0.302                    | 0.308               | 0.388             |
| <i>Restricted Sample:</i> |                          |                     |                   |
| Vote-by-Mail              | −0.058<br>(0.041)        | −0.072*<br>(0.041)  | −0.061<br>(0.042) |
| Observations              | 1,456                    | 1,456               | 1,456             |
| Adjusted R <sup>2</sup>   | 0.307                    | 0.312               | 0.395             |
| Munic FE                  | Yes                      | Yes                 | Yes               |
| Year FE                   | Yes                      | Yes                 | Yes               |
| Controls                  | No                       | Yes                 | Yes               |
| Munic TT                  | No                       | No                  | Yes               |

*Note:* Robust standard errors clustered by county in parentheses. \*p<0.1; \*\*p<0.05; \*\*\*p<0.01. Control variables include the number of audits, audit type, and the following demographic controls at the municipality level: percent Black, percent Latino, median income, percent with high school degree or higher, percent urban and total population.

(Column 2) the point estimates are larger and at least marginally significant. In addition to the time variant municipality characteristics, these models include variables measuring the number of audits that ended in a year and the type of audit taking place. In these specifications, I estimate that vote-by-mail causes a .072 to .077 decrease in the average number of findings on an accountability audit. Although these results are substantively small, in the nonparametric models presented in Figure D.1 in the Online Appendix, I find that vote-by-mail causes a 0.261 decrease in the average number of findings for municipalities that switched to vote-by-mail in 2006. This provides some suggestive evidence that vote-by-mail causes municipal elected officials to be less likely to engage in behavior that breaks laws or protocol in Washington. That being said, these results should be interpreted with care considering the uncertainty surrounding the estimates.

Audit report findings in Washington represent a substantial breach of law, so it is perhaps not very surprising that the effects are substantively quite small and are not robust to different specifications of the model. It may be the case that the types of issues that are documented in audit reports are difficult for politicians to fix in the relatively short time period after the switch to all-mail elections. Furthermore, if audit findings are being triggered by the behavior of career civil servants or if competence is a stable characteristic of elected officials, it may take a number of years for these individuals to leave their position and allow more competent employees to be hired or for more competent politicians to be elected. In all, these results provide some suggestive evidence that vote-by-mail causes a decrease in negative findings on audit reports. This indicates that elected officials are behaving more in line with the best interests of their constituents after the switch to vote-by-mail. Importantly, these results cannot be explained by any potential changes in the composition of the electorate that are a result of vote-by-mail.

### **Vote-by-Mail Causes an Increase in Municipal Election Turnout**

The causal mechanism that I propose to explain why vote-by-mail causes the documented changes in policy outcomes and elected official behavior is that the institution causes voters to gather more information about politics. Due to limitations of existing survey data in Washington I cannot credibly estimate the effect of vote-by-mail on voter knowledge in this context. However, I provide evidence consistent with this theory by estimating the effects of vote-by-mail on turnout and ballot roll-off. Recall, I expect that all-mail elections will decrease roll-off because voters will gather more information about low-salience races.

I examine at the effects of vote-by-mail on voter turnout in municipal elections and compare it to the effect of vote-by-mail on countywide voter turnout. I expect that there will be a larger increase in turnout

in municipal elections than in countywide turnout as a result of vote-by-mail. Ideally, I would examine the relationship between vote-by-mail and ballot roll-off on municipal races, but the total number of ballots cast by individuals residing in a given municipality is not uniformly available over time or across jurisdictions, which is necessary to create the measure of ballot roll-off.

Table 4: The Effects of Vote-by-Mail on Voter Turnout

|                           | Municipal Turnout   |                     |                     | Countywide Turnout  |                     |                     |
|---------------------------|---------------------|---------------------|---------------------|---------------------|---------------------|---------------------|
|                           | (1)                 | (2)                 | (3)                 | (4)                 | (5)                 | (6)                 |
| <i>Full Sample:</i>       |                     |                     |                     |                     |                     |                     |
| Vote-by-Mail              | 4.271***<br>(0.826) | 4.243***<br>(0.771) | 3.638***<br>(1.075) | 3.200***<br>(0.581) | 3.068***<br>(0.607) | 3.010***<br>(0.652) |
| Observations              | 1,546               | 1,546               | 1,546               | 234                 | 234                 | 234                 |
| Adjusted R <sup>2</sup>   | 0.677               | 0.693               | 0.767               | 0.908               | 0.916               | 0.928               |
| <i>Restricted Sample:</i> |                     |                     |                     |                     |                     |                     |
| Vote-by-Mail              | 4.403***<br>(0.844) | 4.349***<br>(0.810) | 3.849***<br>(1.231) | 3.452***<br>(0.554) | 3.369***<br>(0.621) | 3.400***<br>(0.675) |
| Observations              | 1,422               | 1,422               | 1,422               | 204                 | 204                 | 204                 |
| Adjusted R <sup>2</sup>   | 0.689               | 0.706               | 0.779               | 0.912               | 0.920               | 0.931               |
| Municipality FE           | Yes                 | Yes                 | Yes                 | No                  | No                  | No                  |
| County FE                 | No                  | No                  | No                  | Yes                 | Yes                 | Yes                 |
| Year FE                   | Yes                 | Yes                 | Yes                 | Yes                 | Yes                 | Yes                 |
| Controls                  | No                  | Yes                 | Yes                 | No                  | Yes                 | Yes                 |
| Time Trends               | No                  | No                  | Yes                 | No                  | No                  | Yes                 |

*Note:* Robust standard errors clustered by county in parentheses. \*p<0.1; \*\*p<0.05; \*\*\*p<0.01. Control variables include an indicator for if there was a mayoral race (Columns 1, 2, and 3) as well as the following demographic controls at the municipality (Columns 1, 2, and 3) or county (Columns 4, 5, and 6) level: percent Black, percent Latino, median income, percent with high school degree or higher, percent urban and total population.

In Column 1-3 of Table 4 display the results of models in the form of Equation 1 with municipal turnout as the dependent variable.<sup>4</sup> All of the models include municipality and year fixed effects and the models in Columns 2 and 3 include a set of demographic and municipal race specific control variables. In Column 3, I add municipality time trends to the model. The results indicate that vote-by-mail causes a 4.271% to 3.638% increase in turnout in Washington's municipal elections. On the other hand, in Columns 4, 5, and 6 of Table 4 I find that there is a 3.010% to 3.452% increase in countywide turnout in odd-year elections after the switch to vote-by-mail.<sup>5</sup> This indicates that the increase in turnout in municipal races as a percentage of the CVAP is substantially larger than the increase in countywide turnout as a percentage of CVAP after

<sup>4</sup>In Section E of the Online Appendix, I present placebo tests of the voter turnout and ballot roll-off results.

<sup>5</sup>These estimates relate most closely to the results in Table A3, Column 11 of Gerber, Huber and Hill (2013) who also estimate the effects of vote-by-mail on turnout in Washington.

switching to all-mail elections.<sup>6</sup> Importantly, the larger magnitude of the effect of vote-by-mail on turnout in municipal elections is consistent with my hypothesis that voters will be more likely to vote in lower salience elections when casting their ballot by mail. Further, these results are similar to previous research that finds all-mail elections cause a decrease in ballot roll-off in Washington (Marble 2017, Szewczyk 2019) and Colorado (Menger, Stein and Vonnahme 2018).

### **Changes in the Composition of the Electorate do not Explain the Findings**

I now address an alternative explanation for the municipal finance results. Vote-by-mail may change the composition of the electorate and elected officials could be decreasing taxing and spending because of who votes, not how informed voters are. This would be a plausible explanation if vote-by-mail increases the socioeconomic bias or proportion of conservatives in the electorate, which others have argued may be an unexpected consequence of the reform (Berinsky 2005). There are a number of channels through which vote-by-mail could cause an increase in the share of conservative voters who prefer low spending. For example, vote-by-mail may increase the political power of homeowners and those with more permanent living situations, because mail-in ballots may not reach voters who frequently move. Or, it may be the case that in off-cycle municipal elections in Washington that the electorate is dominated by special interests that prefer high levels of government spending (Anzia 2011), and the increase in turnout after all-mail elections are implemented decreases the size of this special interests' voting block.<sup>7</sup>

The evidence on the effects of vote-by-mail on the composition of the electorate in Washington does on support this alternative theory. First, using data from the voter file in Washington, Gerber, Huber and Hill (2013) find that vote-by-mail causes the largest increases in turnout among individuals that are younger and who are not regular voters. These groups of voters are less likely to vocally support a decrease in property tax levels, so it is unlikely that changes in the composition of the electorate explain the decrease in taxes caused by vote-by-mail.

Second, I provide additional evidence contrary to this alternative explanation by examining the effects of vote-by-mail on the composition of the electorate in Washington's municipal elections from 2001 to 2011 using data from the Catalist voter file. Catalist merges official voter files with a variety of commercial and

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<sup>6</sup>This differential effect of vote-by-mail across election types is consistent with previous work examining the effects of vote-by-mail on voter turnout that finds turnout increases are larger in larger in local special elections than in statewide general elections (Kousser and Mullin 2007).

<sup>7</sup>However, Kogan, Lavertu and Peskowitz (2018) find that the impact of election timing on the proportion of school employees in school district elections, the main interest group in these elections, is quite small.



Census data using a proprietary matching algorithm (Ansolabehere and Hersh 2012), which allows me to enumerate the number of voters in Washington’s municipal elections from a variety of demographic groups. Other research uses this data to measure the composition of the electorate in local elections over a similar time period (Kogan, Lavertu and Peskowitz 2018). I estimate the effects of vote-by-mail on the ideological and partisan composition of the electorate as well as on the proportion of homeowners, individuals with incomes greater than \$100,000 per year, and individuals older than 65 in the electorate. A detailed description of the data and results is relegated to Online Appendix C. To summarize, there is no evidence to support the argument that vote-by-mail causes the electorate to become more conservative. In all, the results provide compelling evidence that the effects of vote-by-mail on municipal revenue and expenditures documented in this paper are not the result of changes to the composition of the electorate.

## **Conclusion**

The rules that govern how elections are run can have profound implications on the incentives of elected officials and electoral accountability (Anzia 2011, Bechtel, Hangartner and Schmid 2016, Fowler 2013, Fujiwara 2015). Consequently, who gets to vote and how a voter casts his ballot affects the types of policies that are implemented in democracies. This paper examines how a particular electoral institution, vote-by-mail, affects electoral accountability in the context of Washington state. I find that vote-by-mail results in voters being more likely to cast a vote in lower salience races for municipal office. Further, the institution causes as a decrease in revenue, property taxes, and expenditures in Washington’s municipalities. Exploiting a law that resulted in staggered over time changes in the use of vote-by-mail in Washington’s counties with a difference-in-differences research design, these findings provide compelling evidence that vote-by-mail induces elected officials to be more responsive to their constituents.

As more political jurisdictions begin to adopt all-mail elections it is imperative that scholars understand the consequences of this institution. Since Oregon switched to vote-by-mail in 1998 there are now three states in the United States that hold exclusively vote-by-mail elections. California, Utah, and Hawaii are also currently in the process of implementing mail-in ballot based election systems. Thus, it is clear that we should have solid understanding of how this institution shapes the incentives and behavior of elected officials. There has been much debate about the effects of vote-by-mail on voter turnout (Bergman and Yates 2011, Berinsky, Burns and Traugott 2001, Gerber, Huber and Hill 2013, Karp and Banducci 2000, Kousser

and Mullin 2007, Magleby 1987, Richey 2008), but scholars have generally ignored other potential effects of the institution. The findings documented in this paper, consequently, broaden this literature by examining how vote-by-mail affects the information environment of elections, shapes the incentives of elected officials, and impacts public policy.

However, many questions about the effects of vote-by-mail still remain. In particular, future research on this topic should prioritize the collection of data in contexts where it is possible to credibly estimate the effects of vote-by-mail levels of voter knowledge. In addition, other research finds that in complex electoral environments voters are more likely to use racial stereotypes as heuristics, which hurts the chances that candidates that are members of racial minority groups are elected to office (Crowder-Meyer et al. 2018). Therefore, to the extent that vote-by-mail causes voters to gather more information about politics, does the electoral reform also subdue the effects of racial bias in voter decision making?

More broadly, these results speak to the nature of electoral accountability in the United States. It is well documented that the American electorate is biased in favor of those with a higher socioeconomic status (Schlozman, Verba and Brady 2012) and that public policy favors the affluent (e.g. Bartels 2016). Other work finds that institutions that induce accountability among local policymakers actually further biases policy outcomes away from the interests of individuals of a low socioeconomic status (Sances 2016), and I find that vote-by-mail, which many proponents describe as dramatically reducing the cost of voting, only results in small changes to the composition of the electorate. Thus, although the institution of vote-by-mail induces elected officials to implement policies more in line with their constituents interests, this results in public policy - lower property taxes and lower levels of spending - that actually further biases outcomes in favor of high socioeconomic status individuals. Do other electoral reforms in the United States that allegedly increase access to the ballot actually result in more biased policy outcomes? There is still a great deal of work to be done to understand how the patchwork of electoral rules across the United States affects which groups of citizens policy outcomes favor.

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# Online Appendix

|  |           |
|--|-----------|
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## A Summary Statistics

Table A.1: Summary Statistics of Municipal Election Data

| Statistic                | N     | Mean       | St. Dev.   | Min       | Max         |
|--------------------------|-------|------------|------------|-----------|-------------|
| <i>Full Sample</i>       |       |            |            |           |             |
| Turnout                  | 1,546 | 29.149     | 11.726     | 2.086     | 90.769      |
| Vote-by-Mail             | 1,546 | 0.565      | 0.496      | 0         | 1           |
| Mayoral Race             | 1,546 | 0.431      | 0.495      | 0         | 1           |
| Percent African American | 1,546 | 1.458      | 2.541      | 0.000     | 18.466      |
| Percent 65 or Older      | 1,546 | 14.126     | 7.107      | 1.300     | 79.200      |
| Percent Latino           | 1,546 | 12.517     | 18.932     | 0.000     | 99.549      |
| Median Income            | 1,546 | 49,604.600 | 24,130.650 | 4,671.000 | 205,625.000 |
| Percent High School      | 1,546 | 83.426     | 12.963     | 19.100    | 100.000     |
| Percent Urban            | 1,546 | 62.306     | 46.578     | 0.000     | 100.000     |
| Total Population         | 1,546 | 14,081.630 | 43,572.160 | 24.000    | 603,174.000 |
| <i>Restricted Sample</i> |       |            |            |           |             |
| Turnout                  | 1,422 | 28.946     | 11.759     | 2.086     | 90.769      |
| Vote-by-Mail             | 1,422 | 0.551      | 0.498      | 0         | 1           |
| Mayoral Race             | 1,422 | 0.421      | 0.494      | 0         | 1           |
| Percent African American | 1,422 | 1.550      | 2.624      | 0.000     | 18.466      |
| Percent 65 or Older      | 1,422 | 13.766     | 6.969      | 1.300     | 79.200      |
| Percent Latino           | 1,422 | 12.792     | 19.256     | 0.000     | 99.549      |
| Median Income            | 1,422 | 51,028.340 | 24,547.980 | 4,671.000 | 205,625.000 |
| Percent High School      | 1,422 | 83.620     | 13.267     | 19.100    | 100.000     |
| Percent Urban            | 1,422 | 65.542     | 45.535     | 0.000     | 100.000     |
| Total Population         | 1,422 | 15,112.730 | 45,272.080 | 24.000    | 603,174.000 |



Table A.2: Summary Statistics of County Election Data

| Statistic                | N   | Mean        | St. Dev.    | Min        | Max           |
|--------------------------|-----|-------------|-------------|------------|---------------|
| <i>Full Sample</i>       |     |             |             |            |               |
| County Turnout           | 234 | 40.598      | 8.040       | 23.153     | 66.608        |
| Vote-by-Mail             | 234 | 0.624       | 0.485       | 0          | 1             |
| Percent African American | 234 | 0.415       | 0.796       | 0.000      | 6.230         |
| Percent 65 or Older      | 234 | 15.527      | 4.835       | 7.200      | 31.300        |
| Percent Latino           | 234 | 3.563       | 6.530       | 0.011      | 41.928        |
| Median Income            | 234 | 43,808.440  | 8,086.543   | 29,008.330 | 70,567.000    |
| Percent High School      | 234 | 85.694      | 6.953       | 63.678     | 95.800        |
| Percent Urban            | 234 | 0.525       | 0.315       | 0.000      | 0.969         |
| Total Population         | 234 | 161,159.500 | 318,786.400 | 2,129.000  | 1,908,379.000 |
| <i>Restricted Sample</i> |     |             |             |            |               |
| County Turnout           | 204 | 40.424      | 8.369       | 23.153     | 66.608        |
| Vote-by-Mail             | 204 | 0.593       | 0.492       | 0          | 1             |
| Percent African American | 204 | 0.457       | 0.842       | 0.000      | 6.230         |
| Percent 65 or Older      | 204 | 15.302      | 4.856       | 7.200      | 31.300        |
| Percent Latino           | 204 | 3.790       | 6.879       | 0.021      | 41.928        |
| Median Income            | 204 | 44,617.470  | 8,111.626   | 29,008.330 | 70,567.000    |
| Percent High School      | 204 | 85.732      | 7.330       | 63.678     | 95.800        |
| Percent Urban            | 204 | 0.577       | 0.291       | 0.000      | 0.969         |
| Total Population         | 204 | 180,792.500 | 336,958.200 | 2,129.000  | 1,908,379.000 |

Table A.3: Summary Statistics of Municipal Revenue and Expenditure Data

| Statistic                           | N     | Mean       | St. Dev.   | Min        | Max         |
|-------------------------------------|-------|------------|------------|------------|-------------|
| <i>Full Sample</i>                  |       |            |            |            |             |
| Log Revenue Per Capita              | 2,567 | 7.439      | 0.601      | 4.834      | 10.583      |
| Log Property Tax Revenue Per Capita | 2,566 | 5.147      | 0.587      | 3.134      | 6.989       |
| Log Expenditures Per Capita         | 2,567 | 7.409      | 0.610      | 4.256      | 10.441      |
| Vote-By-Mail                        | 2,567 | 0.487      | 0.500      | 0          | 1           |
| Percent African American            | 2,567 | 1.454      | 2.538      | 0.000      | 17.566      |
| Percent 65 or Older                 | 2,567 | 14.129     | 6.570      | 1.500      | 47.600      |
| Percent Latino                      | 2,567 | 11.723     | 18.002     | 0.000      | 98.536      |
| Median Income                       | 2,567 | 49,383.220 | 23,578.540 | 14,576.000 | 204,375.000 |
| Percent High School Degree          | 2,567 | 83.749     | 12.459     | 20.011     | 100.000     |
| Percent Urban                       | 2,567 | 61.507     | 46.675     | 0.000      | 100.000     |
| Total Population                    | 2,567 | 13,931.900 | 43,653.780 | 55.000     | 595,240.000 |
| Competitiveness                     | 2,145 | 0.290      | 0.257      | 0.000      | 0.933       |
| Mayor-Council                       | 2,567 | 0.808      | 0.394      | 0          | 1           |
| <i>Restricted Sample</i>            |       |            |            |            |             |
| Log Revenue Per Capita              | 2,360 | 7.438      | 0.598      | 4.834      | 10.583      |
| Log Property Tax Revenue Per Capita | 2,359 | 5.170      | 0.582      | 3.134      | 6.989       |
| Log Expenditures Per Capita         | 2,360 | 7.408      | 0.608      | 4.256      | 10.441      |
| Vote-By-Mail                        | 2,360 | 0.460      | 0.499      | 0          | 1           |
| Percent African American            | 2,360 | 1.548      | 2.623      | 0.000      | 17.566      |
| Percent 65 or Older                 | 2,360 | 13.773     | 6.359      | 1.500      | 47.600      |
| Percent Latino                      | 2,360 | 11.844     | 18.199     | 0.000      | 98.536      |
| Median Income                       | 2,360 | 50,863.360 | 23,935.230 | 14,576.000 | 204,375.000 |
| Percent High School Degree          | 2,360 | 84.027     | 12.688     | 20.011     | 100.000     |
| Percent Urban                       | 2,360 | 64.609     | 45.736     | 0.000      | 100.000     |
| Total Population                    | 2,360 | 14,957.220 | 45,371.500 | 55.000     | 595,240.000 |
| Competitiveness                     | 1,980 | 0.299      | 0.259      | 0.000      | 0.933       |
| Mayor-Council                       | 2,360 | 0.799      | 0.401      | 0          | 1           |

Table A.4: Summary Statistics of Property Tax Levy Data

| Statistic                        | N   | Mean        | St. Dev.    | Min        | Max           |
|----------------------------------|-----|-------------|-------------|------------|---------------|
| <i>Full Sample</i>               |     |             |             |            |               |
| Log Property Tax Levy Per Capita | 468 | 4.261       | 0.660       | 2.352      | 6.036         |
| Vote-By-Mail                     | 468 | 0.481       | 0.500       | 0          | 1             |
| Percent African American         | 468 | 0.556       | 0.956       | 0.000      | 7.000         |
| Percent 65 or Older              | 468 | 14.941      | 4.416       | 7.200      | 32.400        |
| Percent Latino                   | 468 | 4.768       | 7.821       | 0.011      | 47.100        |
| Median Income                    | 468 | 42,452.140  | 7,707.546   | 28,584.000 | 68,065.000    |
| Percent High School Degree       | 468 | 85.186      | 6.992       | 63.300     | 95.700        |
| Percent Urban                    | 468 | 0.522       | 0.314       | 0.000      | 0.969         |
| Total Population                 | 468 | 158,664.100 | 314,122.500 | 2,129.000  | 1,879,189.000 |
| <i>Restricted Sample</i>         |     |             |             |            |               |
| Log Property Tax Levy Per Capita | 408 | 4.356       | 0.606       | 2.428      | 6.036         |
| Vote-By-Mail                     | 408 | 0.444       | 0.497       | 0          | 1             |
| Percent African American         | 408 | 0.612       | 1.009       | 0.000      | 7.000         |
| Percent 65 or Older              | 408 | 14.752      | 4.446       | 7.200      | 32.400        |
| Percent Latino                   | 408 | 5.072       | 8.233       | 0.020      | 47.100        |
| Median Income                    | 408 | 43,243.890  | 7,736.962   | 28,584.000 | 68,065.000    |
| Percent High School Degree       | 408 | 85.244      | 7.367       | 63.300     | 95.700        |
| Percent Urban                    | 408 | 0.575       | 0.290       | 0.000      | 0.969         |
| Total Population                 | 408 | 177,968.400 | 332,003.700 | 2,129.000  | 1,879,189.000 |

Table A.5: Summary Statistics of Audit Data

| Statistic                  | N     | Mean       | St. Dev.   | Min        | Max         |
|----------------------------|-------|------------|------------|------------|-------------|
| <i>Full Sample</i>         |       |            |            |            |             |
| Number of Audit Findings   | 1,552 | 0.198      | 0.451      | 0.000      | 4.000       |
| Vote-By-Mail               | 1,552 | 0.662      | 0.473      | 0          | 1           |
| Number of Audits           | 1,552 | 1.817      | 0.411      | 1          | 3           |
| Accountability Audit       | 1,552 | 0.892      | 0.311      | 0          | 1           |
| Percent African American   | 1,552 | 1.748      | 2.837      | 0.000      | 18.466      |
| Percent 65 or Older        | 1,552 | 13.648     | 6.450      | 1.300      | 54.000      |
| Percent Latino             | 1,552 | 12.947     | 18.451     | 0.000      | 99.549      |
| Median Income              | 1,552 | 51,589.660 | 24,659.080 | 17,921.110 | 204,375.000 |
| Percent High School Degree | 1,552 | 84.659     | 12.257     | 19.100     | 100.000     |
| Percent Urban              | 1,552 | 72.862     | 42.315     | 0.000      | 100.000     |
| Total Population           | 1,552 | 19,209.020 | 50,498.200 | 39.000     | 603,174.000 |
| <i>Restricted Sample</i>   |       |            |            |            |             |
| Number of Audit Findings   | 1,456 | 0.202      | 0.459      | 0.000      | 4.000       |
| Vote-By-Mail               | 1,456 | 0.640      | 0.480      | 0          | 1           |
| Number of Audits           | 1,456 | 1.827      | 0.403      | 1          | 3           |
| Accountability Audit       | 1,456 | 0.896      | 0.306      | 0          | 1           |
| Percent African American   | 1,456 | 1.838      | 2.904      | 0.000      | 18.466      |
| Percent 65 or Older        | 1,456 | 13.339     | 6.253      | 1.300      | 54.000      |
| Percent Latino             | 1,456 | 13.068     | 18.541     | 0.000      | 99.549      |
| Median Income              | 1,456 | 52,821.530 | 24,892.900 | 20,640.670 | 204,375.000 |
| Percent High School Degree | 1,456 | 84.836     | 12.389     | 19.100     | 100.000     |
| Percent Urban              | 1,456 | 75.341     | 40.858     | 0.000      | 100.000     |
| Total Population           | 1,456 | 20,260.200 | 51,950.370 | 39.000     | 603,174.000 |

Table A.6: Summary Statistics of Catalist Data

| Statistic                                | N     | Mean   | St. Dev. | Min    | Max     |
|--|-------|--------|----------|--------|---------|
| <i>Full Sample</i>                       |       |        |          |        |         |
| Percent Liberal                          | 1,649 | 39.281 | 5.051    | 20.005 | 64.564  |
| Percent Democrat                         | 1,649 | 47.873 | 10.338   | 7.505  | 88.681  |
| Proportion Homeowner or Likely Homeowner | 1,649 | 52.765 | 29.283   | 0.000  | 100.000 |
| Proportion Renter or Likely Renter       | 1,649 | 3.515  | 4.337    | 0.000  | 34.818  |
| Proportion Homeowner                     | 1,649 | 38.488 | 26.856   | 0.000  | 100.000 |
| Proportion Likely Homeowner              | 1,649 | 14.277 | 10.308   | 0.000  | 75.000  |
| Proportion Renter                        | 1,649 | 0.033  | 0.083    | 0.000  | 1.005   |
| Proportion Likely Renter                 | 1,649 | 3.483  | 4.324    | 0.000  | 34.818  |
| Proportion Income > 100,000              | 1,649 | 16.777 | 21.304   | 0.000  | 100.000 |
| Proportion Over 65                       | 1,649 | 48.011 | 10.548   | 0.000  | 100.000 |
| Vote-by-Mail                             | 1,649 | 0.546  | 0.498    | 0      | 1       |
| <i>Restricted Sample</i>                 |       |        |          |        |         |
| Percent Liberal                          | 1,511 | 39.311 | 5.145    | 20.005 | 64.564  |
| Percent Democrat                         | 1,511 | 47.935 | 10.402   | 7.505  | 88.681  |
| Proportion Homeowner or Likely Homeowner | 1,511 | 55.143 | 28.875   | 0.000  | 100.000 |
| Proportion Renter or Likely Renter       | 1,511 | 3.617  | 4.426    | 0.000  | 34.818  |
| Proportion Homeowner                     | 1,511 | 40.655 | 26.826   | 0.000  | 100.000 |
| Proportion Likely Homeowner              | 1,511 | 14.488 | 10.357   | 0.000  | 75.000  |
| Proportion Renter                        | 1,511 | 0.034  | 0.084    | 0.000  | 1.005   |
| Proportion Likely Renter                 | 1,511 | 3.583  | 4.414    | 0.000  | 34.818  |
| Proportion Income > 100,000              | 1,511 | 17.840 | 21.888   | 0.000  | 100.000 |
| Proportion Over 65                       | 1,511 | 47.587 | 10.514   | 0.000  | 100.000 |
| Vote-by-Mail                             | 1,511 | 0.527  | 0.499    | 0      | 1       |

## B Robustness Checks and Additional Results: Municipal Fiscal Policy

### B.1 Robustness Checks

I estimate a series of placebo tests similar to the Granger test for causality in order to examine the validity of the parallel trends assumption, which is the necessary identifying assumption for a difference-in-differences design (Angrist and Pischke 2009). Specifically, I estimate a series of models of the following form,

$$Y_{m,t} = \sum_{\tau=1}^q \beta_{\tau} VBM_{m,t+\tau} + \beta_0 VBM_{m,t} + \delta^T \mathbf{X}_{m,t} + \lambda_m + \tau_t + \epsilon_{m,t} \quad (2)$$

where, as before,  $VBM_{m,t}$  takes on a value of 1 in the years after a municipality switches to all-mail elections,  $Y_{m,t}$  are the municipal fiscal policy dependent variables,  $\mathbf{X}_{m,t}$  is a vector of demographic controls, and  $\lambda_m$  and  $\tau_t$  are municipal and year fixed effects. The variables represented by  $VBM_{m,t+\tau}$  are equal to 1 if a municipality will switch to vote-by-mail  $\tau$  years in the future and 0 otherwise. These variables measure potential anticipatory effects. We may be concerned that the parallel trends assumption does not hold if these leads of the vote-by-mail variable are statistically significant.

Table B.1 displays the results of these models with total revenue (Columns 1-3) and total expenditures (Columns 4-6) as the dependent variables. I show models with different combinations of the lead variables. I find that the coefficient on the variable indicating a switch to vote-by-mail in the next year ( $VBM_{t+1}$ ) is marginally significant ( $p < .1$ ) in one specification of the model when log total expenditures per capita is the dependent variable (bottom panel, Column 6).

The fact that this estimate comes from analysis of the restricted sample of municipalities that switched to vote-by-mail in 2005 or later suggests that municipal officials that switched to vote-by-mail later may have anticipated the implementation of the reform. The regression estimates with property tax levy (Columns 1-3) and property tax revenue (Columns 4-6) as the dependent variables are shown in Table B.2. Across all of the models, the lead variables are not statistically significant and are typically not negative.

Table B.1: The Effects of Vote-by-Mail on Municipal Revenue and Expenditures

|                           | Total Revenue        |                      |                      | Total Expenditures   |                      |                      |
|---------------------------|----------------------|----------------------|----------------------|----------------------|----------------------|----------------------|
|                           | (1)                  | (2)                  | (3)                  | (4)                  | (5)                  | (6)                  |
| <i>Full Sample:</i>       |                      |                      |                      |                      |                      |                      |
| $VBM_{t+3}$               |                      |                      | -0.054<br>(0.052)    |                      |                      | -0.044<br>(0.053)    |
| $VBM_{t+2}$               |                      | -0.047<br>(0.035)    | -0.077<br>(0.060)    |                      | -0.039<br>(0.039)    | -0.063<br>(0.064)    |
| $VBM_{t+1}$               | -0.028<br>(0.035)    | -0.054<br>(0.045)    | -0.086<br>(0.065)    | -0.045<br>(0.028)    | -0.066<br>(0.040)    | -0.091<br>(0.061)    |
| $VBM$                     | -0.068*<br>(0.039)   | -0.105**<br>(0.041)  | -0.150**<br>(0.060)  | -0.093***<br>(0.032) | -0.119***<br>(0.044) | -0.155**<br>(0.067)  |
| Observations              | 2,567                | 2,567                | 2,567                | 2,567                | 2,567                | 2,567                |
| Adjusted $R^2$            | 0.703                | 0.703                | 0.703                | 0.696                | 0.696                | 0.696                |
| <i>Restricted Sample:</i> |                      |                      |                      |                      |                      |                      |
| $VBM_{t+3}$               |                      |                      | -0.069<br>(0.053)    |                      |                      | -0.063<br>(0.055)    |
| $VBM_{t+2}$               |                      | -0.051<br>(0.033)    | -0.091<br>(0.058)    |                      | -0.038<br>(0.038)    | -0.075<br>(0.064)    |
| $VBM_{t+1}$               | -0.020<br>(0.030)    | -0.047<br>(0.041)    | -0.088<br>(0.061)    | -0.038<br>(0.027)    | -0.059<br>(0.037)    | -0.097*<br>(0.058)   |
| $VBM$                     | -0.079***<br>(0.030) | -0.113***<br>(0.035) | -0.172***<br>(0.057) | -0.104***<br>(0.026) | -0.129***<br>(0.041) | -0.184***<br>(0.068) |
| Observations              | 2,360                | 2,360                | 2,360                | 2,360                | 2,360                | 2,360                |
| Adjusted $R^2$            | 0.703                | 0.703                | 0.704                | 0.695                | 0.695                | 0.695                |
| Municipality FE           | Yes                  | Yes                  | Yes                  | Yes                  | Yes                  | Yes                  |
| Year FE                   | Yes                  | Yes                  | Yes                  | Yes                  | Yes                  | Yes                  |
| Controls                  | Yes                  | Yes                  | Yes                  | Yes                  | Yes                  | Yes                  |

*Note:* Robust standard errors clustered by county in parentheses. \* $p < 0.1$ ; \*\* $p < 0.05$ ; \*\*\* $p < 0.01$ . Control variables include the following demographic controls at the municipality level: percent Black, percent Latino, median income, percent with high school degree or higher, percent urban and total population.

Table B.2: The Effects of Vote-by-Mail on Municipal Tax Revenue

|                           | Property Tax Levy  |                   |                   | Property Tax Revenue |                     |                    |
|---------------------------|--------------------|-------------------|-------------------|----------------------|---------------------|--------------------|
|                           | (1)                | (2)               | (3)               | (4)                  | (5)                 | (6)                |
| <i>Full Sample:</i>       |                    |                   |                   |                      |                     |                    |
| $VBM_{t+3}$               |                    |                   | 0.003<br>(0.022)  |                      |                     | 0.010<br>(0.011)   |
| $VBM_{t+2}$               |                    | 0.011<br>(0.020)  | 0.013<br>(0.026)  |                      | -0.001<br>(0.016)   | 0.005<br>(0.021)   |
| $VBM_{t+1}$               | -0.014<br>(0.017)  | -0.009<br>(0.026) | -0.007<br>(0.031) | -0.026<br>(0.019)    | -0.026<br>(0.026)   | -0.020<br>(0.030)  |
| $VBM$                     | -0.049*<br>(0.027) | -0.043<br>(0.036) | -0.041<br>(0.042) | -0.051***<br>(0.018) | -0.052*<br>(0.026)  | -0.044<br>(0.033)  |
| Observations              | 468                | 468               | 468               | 2,566                | 2,566               | 2,566              |
| Adjusted R <sup>2</sup>   | 0.985              | 0.984             | 0.984             | 0.935                | 0.935               | 0.935              |
| <i>Restricted Sample:</i> |                    |                   |                   |                      |                     |                    |
| $VBM_{t+3}$               |                    |                   | 0.019<br>(0.018)  |                      |                     | 0.004<br>(0.011)   |
| $VBM_{t+2}$               |                    | 0.019<br>(0.030)  | 0.031<br>(0.029)  |                      | -0.013<br>(0.015)   | -0.011<br>(0.019)  |
| $VBM_{t+1}$               | -0.012<br>(0.026)  | 0.0004<br>(0.043) | 0.013<br>(0.042)  | -0.031<br>(0.020)    | -0.038<br>(0.027)   | -0.036<br>(0.032)  |
| $VBM$                     | -0.043<br>(0.040)  | -0.030<br>(0.056) | -0.015<br>(0.056) | -0.057***<br>(0.020) | -0.066**<br>(0.028) | -0.062*<br>(0.035) |
| Observations              | 408                | 408               | 408               | 2,359                | 2,359               | 2,359              |
| Adjusted R <sup>2</sup>   | 0.981              | 0.981             | 0.981             | 0.933                | 0.933               | 0.933              |
| Municipality FE           | No                 | No                | No                | Yes                  | Yes                 | Yes                |
| County FE                 | Yes                | Yes               | Yes               | No                   | No                  | No                 |
| Year FE                   | Yes                | Yes               | Yes               | Yes                  | Yes                 | Yes                |
| Controls                  | Yes                | Yes               | Yes               | Yes                  | Yes                 | Yes                |

*Note:* Robust standard errors clustered by county in parentheses. \*p<0.1; \*\*p<0.05; \*\*\*p<0.01. Control variables include the following demographic controls at the municipality level: percent Black, percent Latino, median income, percent with high school degree or higher, percent urban and total population.

## B.2 Additional Municipal Fiscal Policy Results

In this section, I present additional analyses of the municipal finance data. First, I examine if the structure of municipal governments affects the relationship between vote-by-mail and municipal revenue and expenditures. I expect that the effects will be driven by mayor-council municipalities because there is a single elected executive that voters can more easily hold accountable. I exclude the property tax levy dependent variable from this analysis because I do not observe the data at the municipality level. I examine this hypothesis by estimating the following equation,

$$\begin{aligned} Y_{m,t} = & \beta_1 VBM_{m,t} + \beta_2 MayorCouncil_m + \\ & \beta_3 VBM_{m,t} \times MayorCouncil_m + \\ & \delta^T \mathbf{X}_{m,t} + \tau_t + \epsilon_{m,t} \end{aligned} \quad (3)$$

where  $MayorCouncil_m$  is a dummy variable equal to 1 if a municipality has a mayor-council government and  $Y_{m,t}$  represents the various municipal finance dependent variables. I exclude municipality fixed effects from these analyses because mayor-council status is time invariant. Therefore, the estimates from these analyses should not have a causal interpretation, but nevertheless can shed light on the consequences of the implementation of vote-by-mail in Washington. The results of these models are displayed in Columns 1 (revenue), 3 (expenditures), and 5 (property taxes) of Table B.3. The results show that the coefficient on the vote-by-mail indicator is consistently positive but not statistically significant. Notably, across all of the dependent variables the interaction between vote-by-mail and the mayor-council dummy variable is negative and statistically significant. This indicates that the expected level of revenue and expenditures is lower in vote-by-mail municipalities with mayor-council governments compared to those with council-manager governments, which is consistent with the logic that policy outcomes in municipalities with mayor-council governments are more responsive to electoral incentives.

I next examine if the competitiveness of elections for municipal office conditions the effects of vote-by-mail. To measure electoral competitiveness in each municipality, I use the data on municipal elections that is discussed in the text of the paper. The variable is created by dividing the number of votes for the second highest vote getter in each race by the total number of votes for the candidate who won. Then, I take the average of this proportion across all races in a given municipality for each election year. Thus, the



Table B.3: The Effects of Vote-by-Mail on Municipal Revenue and Expenditures

|                           | Total Revenue        |                   | Total Expenditures   |                     | Property Tax Revenue |                      |
|---------------------------|----------------------|-------------------|----------------------|---------------------|----------------------|----------------------|
|                           | (1)                  | (2)               | (3)                  | (4)                 | (5)                  | (6)                  |
| <i>Full Sample:</i>       |                      |                   |                      |                     |                      |                      |
| Competitiveness           |                      | 0.004<br>(0.053)  |                      | −0.014<br>(0.048)   |                      | −0.025<br>(0.021)    |
| Vote-by-Mail              | 0.121<br>(0.076)     | −0.027<br>(0.041) | 0.111<br>(0.085)     | −0.047<br>(0.039)   | 0.022<br>(0.042)     | −0.031***<br>(0.011) |
| Mayor-Council             | 0.134<br>(0.111)     |                   | 0.133<br>(0.121)     |                     | 0.112<br>(0.068)     |                      |
| Vote-by-Mail*Mayor        | −0.229***<br>(0.064) |                   | −0.229***<br>(0.074) |                     | −0.088*<br>(0.046)   |                      |
| Vote-by-Mail*Comp         |                      | −0.104<br>(0.073) |                      | −0.037<br>(0.068)   |                      | 0.035<br>(0.030)     |
| Observations              | 2,567                | 2,145             | 2,567                | 2,145               | 2,566                | 2,144                |
| Adjusted R <sup>2</sup>   | 0.180                | 0.725             | 0.193                | 0.724               | 0.455                | 0.944                |
| <i>Restricted Sample:</i> |                      |                   |                      |                     |                      |                      |
| Competitiveness           |                      | 0.007<br>(0.052)  |                      | −0.013<br>(0.048)   |                      | −0.023<br>(0.021)    |
| Vote-by-Mail              | 0.073<br>(0.072)     | −0.045<br>(0.039) | 0.066<br>(0.082)     | −0.072**<br>(0.036) | 0.027<br>(0.037)     | −0.031**<br>(0.013)  |
| Mayor-Council             | 0.147<br>(0.107)     |                   | 0.145<br>(0.118)     |                     | 0.114*<br>(0.065)    |                      |
| Vote-by-Mail*Mayor        | −0.207***<br>(0.054) |                   | −0.210***<br>(0.065) |                     | −0.070<br>(0.043)    |                      |
| Vote-by-Mail*Comp         |                      | −0.078<br>(0.073) |                      | −0.002<br>(0.067)   |                      | 0.032<br>(0.032)     |
| Observations              | 2,360                | 1,980             | 2,360                | 1,980               | 2,359                | 1,979                |
| Adjusted R <sup>2</sup>   | 0.187                | 0.731             | 0.200                | 0.728               | 0.479                | 0.943                |
| Municipality FE           | No                   | Yes               | No                   | Yes                 | No                   | Yes                  |
| Year FE                   | Yes                  | Yes               | Yes                  | Yes                 | Yes                  | Yes                  |
| Controls                  | Yes                  | Yes               | Yes                  | Yes                 | Yes                  | Yes                  |

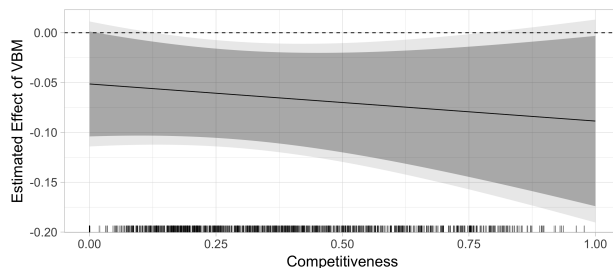
*Note:* Robust standard errors clustered by county in parentheses. \*p<0.1; \*\*p<0.05; \*\*\*p<0.01. Control variables include the following demographic controls at the municipality level: percent Black, percent Latino, median income, percent with high school degree or higher, percent urban and total population.

measure of competitiveness is equal to 0 when all races in a municipality are uncontested and 1 if the top two candidates tied in all races. Due to the timing of municipal elections in Washington, this variable is only observed every other year. I expect that electoral competition will magnify the effects of vote-by-mail, because incumbents who believe that they will face strong challengers may be more likely to be responsive to changes in electoral systems and their constituents. I test this hypothesis by estimating equations of the following form,

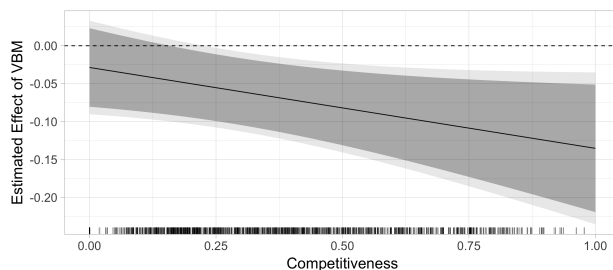
$$\begin{aligned}
Y_{m,t} = & \beta_1 VBM_{m,t} + \beta_2 Competitiveness_{m,(t-1|t-2)} + \\
& \beta_3 VBM_{m,t} \times Competitiveness_{m,(t-1|t-2)} + \\
& \delta^T \mathbf{X}_{m,t} + \lambda_m + \tau_t + \epsilon_{m,t}
\end{aligned} \tag{4}$$

where  $Competitiveness_{m,(t-1|t-2)}$  measures the average division of votes in city council and mayoral elections in the previous election cycle, and the other variables are the same as before. As in the municipal institutions models, I do not estimate these models with the property tax levy dependent variable. The results, displayed in Table B.3, show that for the total revenue (Column 2) and total expenditures (Column 4) dependent variables that the interaction between the vote-by-mail dummy and the competitiveness variable is negative. This indicates that the impact of vote-by-mail becomes more negative as electoral competition increases. However, the interaction term in the model with property tax revenue as the dependent variable (Column 6) is positive, which is opposite of what is expected.

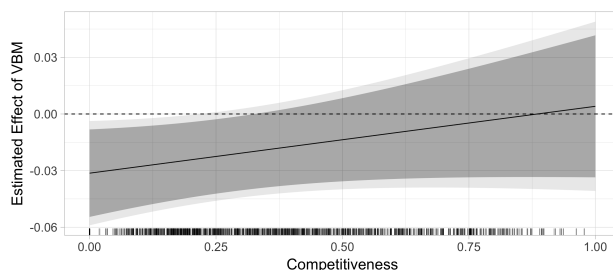
In order to aid in the interpretation of the interaction variables, Figure B.1 displays the marginal effect of vote-by-mail on the dependent variables of interest across different levels of competitiveness using the results from the models with the full sample of municipalities. The solid black line in the figures represents the estimated marginal effect of switching to vote-by-mail at different values of the competitiveness variable. The light shaded region around the line is a 95% confidence interval on these estimates and the dark shaded region is an 90% confidence interval. The marginal effects plots indicate that as competitiveness increases the negative effect of vote-by-mail on total expenditures (Panel A) and total revenue (Panel B) becomes larger. This relationship appears to be strongest for the total revenue dependent variable. Somewhat surprisingly Panel C of Figure B.1 shows that the relationship between electoral competitiveness and revenue from



(a) Log Total Expenditures Per Capita



(b) Log Total Revenue Per Capita



(c) Log Property Tax Revenue Per Capita

Figure B.1: Marginal effects of vote-by-mail on municipal revenue and expenditures. 95% and 90% confidence intervals are displayed in light and dark grey, respectively. Observed values of the competitiveness variable are marked on the x-axis.

property taxes is in the opposite direction. Specifically, as competition increases the estimated marginal effect of vote-by-mail becomes statistically indistinguishable from zero.

Nevertheless, these results indicate that the relationship between vote-by-mail and levels of taxing and spending in Washington's municipalities varies with municipal institutions as well as the competition of elections. When there are institutions in place that increase accountability or competitive elections, municipal fiscal policy outcomes are generally more responsive to the constituent interests after the switch to vote-by-mail. These results provide additional support for the theory put forth in this paper that vote-by-mail induces responsiveness among politicians through electoral incentives.

I now examine the timing of the effects of vote-by-mail on municipal revenue and expenditures, because elected officials cannot immediately change the outcomes of interest. I do this by including a series of

lags of the vote-by-mail indicator. This strategy is similar to the models estimated with Equation 2, but instead of including lead variables I add lags of the vote-by-mail indicator to the model. The results of these models are displayed in Table B.4. The analysis shows that the effects of vote-by-mail on the municipal finance dependent variables happen, according to nearly all of the models, in the years after the switch to the institution.

Table B.4: The Effects of Vote-by-Mail on Municipal Revenue and Expenditures

|                           | Total Revenue       | Total Expenditures  | Property Tax Levy  | Property Tax Revenue |
|---------------------------|---------------------|---------------------|--------------------|----------------------|
|                           | (1)                 | (2)                 | (3)                | (4)                  |
| <i>Full Sample:</i>       |                     |                     |                    |                      |
| <i>VBM</i>                | −0.025<br>(0.026)   | −0.020<br>(0.029)   | −0.017<br>(0.016)  | −0.008<br>(0.011)    |
| <i>VBM<sub>t−1</sub></i>  | −0.048*<br>(0.025)  | −0.044*<br>(0.027)  | −0.036*<br>(0.021) | −0.015<br>(0.013)    |
| <i>VBM<sub>t−2</sub></i>  | −0.036<br>(0.044)   | −0.037<br>(0.044)   | −0.038*<br>(0.021) | −0.045**<br>(0.019)  |
| <i>VBM<sub>t−3</sub></i>  | −0.006<br>(0.050)   | −0.015<br>(0.049)   | −0.026<br>(0.023)  | −0.029<br>(0.018)    |
| <i>VBM<sub>t−4</sub></i>  | −0.021<br>(0.039)   | −0.026<br>(0.041)   | −0.019<br>(0.019)  | −0.018<br>(0.014)    |
| Observations              | 2,565               | 2,565               | 468                | 2,564                |
| Adjusted R <sup>2</sup>   | 0.702               | 0.695               | 0.984              | 0.935                |
| <i>Restricted Sample:</i> |                     |                     |                    |                      |
| <i>VBM</i>                | −0.032<br>(0.021)   | −0.027<br>(0.026)   | −0.009<br>(0.016)  | −0.017*<br>(0.010)   |
| <i>VBM<sub>t−1</sub></i>  | −0.063**<br>(0.025) | −0.065**<br>(0.028) | −0.022<br>(0.026)  | −0.020<br>(0.017)    |
| <i>VBM<sub>t−2</sub></i>  | −0.054<br>(0.044)   | −0.060<br>(0.045)   | −0.043<br>(0.036)  | −0.057**<br>(0.022)  |
| <i>VBM<sub>t−3</sub></i>  | 0.003<br>(0.039)    | −0.017<br>(0.038)   | −0.036<br>(0.045)  | −0.046**<br>(0.020)  |
| <i>VBM<sub>t−4</sub></i>  | 0.002<br>(0.035)    | −0.007<br>(0.038)   | −0.024<br>(0.038)  | −0.024<br>(0.015)    |
| Observations              | 2,359               | 2,359               | 408                | 2,358                |
| Adjusted R <sup>2</sup>   | 0.702               | 0.694               | 0.981              | 0.932                |
| Municipality FE           | Yes                 | Yes                 | No                 | Yes                  |
| County FE                 | No                  | No                  | Yes                | No                   |
| Year FE                   | Yes                 | Yes                 | Yes                | Yes                  |
| Controls                  | Yes                 | Yes                 | Yes                | Yes                  |

*Note:* Robust standard errors clustered by county in parentheses. \*p<0.1; \*\*p<0.05; \*\*\*p<0.01. Control variables include the following demographic controls at the municipality level: percent Black, percent Latino, median income, percent with high school degree or higher, percent urban and total population.

## C Catalist Results

### C.1 Main Results

As discussed in the body of the paper, I use data from the Catalist voter file to address a competing explanation for the municipal finance results. In particular, one may be concerned that the effects of vote-by-mail are driven by changes in the composition of the electorate. Therefore, in this section I estimate the effects of vote-by-mail on who votes in Washington's municipal elections. There are a number of potential drawbacks to using Catalist data for this application. Notably, the data is a cross-sectional extract of the Catalist voter file at the time I downloaded it,<sup>8</sup> which means that registered voters who moved into (out of) a municipality after 2011 will be erroneously included in (excluded from) my measures of the composition of the electorate. Therefore, I first complete an exercise to validate the accuracy of the Catalist data. In particular, I show that the measure of the partisan composition of the electorate created with the Catalist data is highly correlated with official presidential election outcomes in municipalities, which indicates that this data is of sufficient quality for my purposes.

I follow the procedure located in Appendix D in Kogan, Lavertu and Peskowitz (2018) to validate the Catalist data. In particular, I compare presidential election results at the municipality level to the predicted partisanship of the electorate calculated with the Catalist data. I use the data from Einstein and Kogan (2016) to identify the 2008 presidential vote share in Washington's municipalities. Because municipal elections in Washington take place off-cycle, I cannot directly compare the partisan composition of the electorate in municipal elections to the presidential election vote share in the same year. Therefore, I examine the correlation between Obama's vote share in 2008 to the predicted partisanship of the electorate from Catalist in 2007 and 2009. These results are displayed in Figure C.1. The correlation between the 2008 democratic presidential vote share and the 2007 partisan composition of the electorate is 0.83 and in 2009 it is 0.79. Despite the fact that I am not comparing elections in the same year, these correlation coefficients are similar to what is found by Kogan, Lavertu and Peskowitz (2018). Further, the high level of correlation between the actual results of a presidential election and the predicted partisanship of the electorate in municipalities from Catalist bolsters the confidence in the Catalist data and indicates that this data is sufficient for my purposes.

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<sup>8</sup>The data was downloaded in June of 2017.

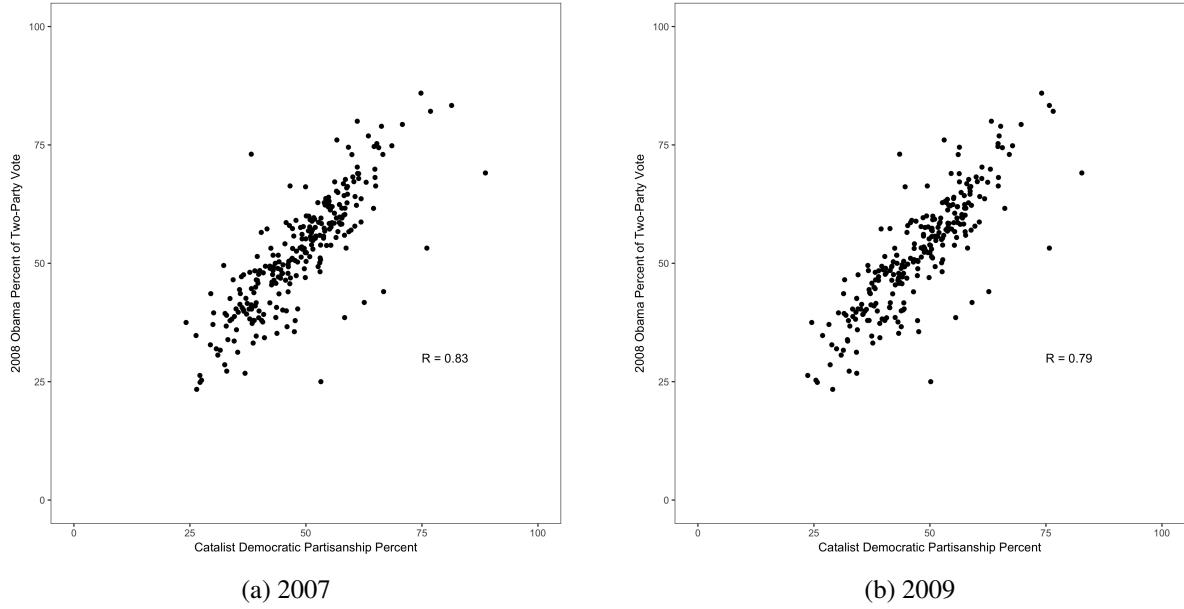


Figure C.1: Catalist Partisanship Predictions in 2007 and 2009 Compared to 2008 Presidential Election Results in Washington's Municipalities

Nevertheless, the potential measurement error in the Catalist dependent variables could decrease the precision of the estimated regression coefficients and potentially result in incorrectly failing to reject the null hypothesis that vote-by-mail does not have an effect on the composition of the electorate. I address this potential issue by following the suggestions of Rainey (2014) for arguing that a variable has a substantively negligible effect. In particular, I present 90% confidence intervals with the point estimates on the regression coefficients, and if the bounds of the confidence intervals are sufficiently small I can fail to reject the null hypothesis that vote-by-mail has a meaningful effect on the composition of the electorate.

Table C.1 displays estimates from models in the form of Equation 1 in the body of the paper with dependent variables measuring the ideology and partisan composition of the electorate<sup>9</sup> along with dependent variables measuring the size of certain demographic groups in the electorate that are likely to prefer lower taxes. The dependent variable in each column of the table is the percentage of voters that are members of the particular group. Notably, across all of the models only one coefficient on the vote-by-mail variable achieves even a marginal level of statistical significance and this estimate indicates that vote-by-mail causes a slight increase in the percentage of liberals in the electorate. The main quantity of interest here, however, is the bound of the 90% confidence interval that is in the direction that would be consistent with the electorate

<sup>9</sup>I estimate the partisan and ideological composition of the electorate with the procedure used by Kogan, Lavertu and Peskowitz (2018).

Table C.1: The Effects of Vote-by-Mail on the Composition of the Electorate

|                           | Liberal                  | Democrat                 | Homeowner                 | Income > \$100,000        | 65 and Older              |
|---------------------------|--------------------------|--------------------------|---------------------------|---------------------------|---------------------------|
|                           | (1)                      | (2)                      | (3)                       | (4)                       | (5)                       |
| <i>Full Sample:</i>       |                          |                          |                           |                           |                           |
| Vote-by-Mail              | 0.402*<br>(0.060, 0.744) | 0.270<br>(-0.025, 0.565) | -1.901<br>(-4.582, 0.781) | -1.548<br>(-3.207, 0.112) | -1.292<br>(-2.896, 0.311) |
| Observations              | 1,649                    | 1,649                    | 1,649                     | 1,649                     | 1,649                     |
| Adjusted R <sup>2</sup>   | 0.948                    | 0.943                    | 0.974                     | 0.981                     | 0.789                     |
| <i>Restricted Sample:</i> |                          |                          |                           |                           |                           |
| Vote-by-Mail              | 0.336<br>(-0.028, 0.700) | 0.208<br>(-0.050, 0.466) | -2.076<br>(-5.106, 0.954) | -1.716<br>(-3.550, 0.118) | -0.980<br>(-2.600, 0.641) |
| Observations              | 1,511                    | 1,511                    | 1,511                     | 1,511                     | 1,511                     |
| Adjusted R <sup>2</sup>   | 0.950                    | 0.944                    | 0.972                     | 0.981                     | 0.792                     |
| Municipality FE           | Yes                      | Yes                      | Yes                       | Yes                       | Yes                       |
| Year FE                   | Yes                      | Yes                      | Yes                       | Yes                       | Yes                       |
| Controls                  | No                       | No                       | No                        | No                        | No                        |

*Note:* Robust standard errors clustered by county in parentheses. \*p<0.1; \*\*p<0.05; \*\*\*p<0.01.

becoming more conservative. For the ideological and partisan composition dependent variables, the bounds indicate that vote-by-mail causes, at most, a 0.05% decrease in the share of liberals or Democrats in the electorate. This is an incredibly small effect and can certainly not explain the somewhat large changes in municipal spending patterns after the switch to all-mail elections. Further, for the models that estimate the effects of vote-by-mail on the percentage of homeowners, voters with a family income greater than \$100,000 per year, and voters 65 or older in the electorate, the upper bound of the 90% confidence intervals never exceeds 0.954. This indicates that vote-by-mail causes no larger than a 1% increase in the percentage of the electorate held by these demographic groups that are more likely to support lower taxing and spending. Encouragingly, this is consistent with the individual-level analysis of the effects of vote-by-mail in Washington, which finds that the institution causes an increase in turnout among young voters and individuals with little prior vote history (Gerber, Huber and Hill 2013). In all, these results do not provide any support for the alternative explanation that the effects of vote-by-mail on taxing and spending are caused by changes in the composition of the electorate after the reform is implemented.

## C.2 Robustness Checks

Table C.2 displays the results of models with the disaggregated housing measures available in the Catalist data with the full and restricted samples of municipalities, respectively. Specifically, rather than combining



“likely homeowners” and “homeowners” as well as “likely renters” and “renters” into the same category. I run models with each individual category as the dependent variable. The results are similar to those presented in Table C.1. Vote-by-mail does not have a statistically significant effect on any of the housing dependent variables, and, further, the bounds 90% confidence intervals presented indicate substantively negligible effects (Rainey 2014).

Table C.2: The Effects of Vote-by-Mail on the Composition of the Electorate

|                           | Homeowner                | Likely Homeowner          | Renter                    | Likely Renter            |
|---------------------------|--------------------------|---------------------------|---------------------------|--------------------------|
|                           | (1)                      | (2)                       | (3)                       | (4)                      |
| <i>Full Sample:</i>       |                          |                           |                           |                          |
| Vote-by-Mail              | 0.976<br>(−0.483, 2.434) | −2.120<br>(−5.453, 1.213) | −0.003<br>(−0.011, 0.005) | 0.979<br>(−0.486, 2.443) |
| Observations              | 1,649                    | 1,649                     | 1,649                     | 1,649                    |
| Adjusted R <sup>2</sup>   | 0.780                    | 0.969                     | 0.684                     | 0.777                    |
| <i>Restricted Sample:</i> |                          |                           |                           |                          |
| Vote-by-Mail              | 1.047<br>(−0.595, 2.688) | −2.547<br>(−6.225, 1.131) | −0.003<br>(−0.011, 0.005) | 1.051<br>(−0.597, 2.699) |
| Observations              | 1,511                    | 1,511                     | 1,649                     | 1,511                    |
| Adjusted R <sup>2</sup>   | 0.776                    | 0.968                     | 0.684                     | 0.774                    |
| Municipality FE           | Yes                      | Yes                       | Yes                       | Yes                      |
| Year FE                   | Yes                      | Yes                       | Yes                       | Yes                      |
| Controls                  | No                       | No                        | No                        | No                       |

Note: Robust standard errors clustered by county in parentheses. \*p<0.1; \*\*p<0.05; \*\*\*p<0.01.

## D Robustness Checks: Audit Reports

The audit report data present a number of empirical challenges because audits do not take place at universal intervals across all municipalities and there are sometimes multiple audits happening simultaneously in a municipality. For example, the period of time that the auditor is examining can range from one year to four years and a municipality could have its financial records audited while a three year accountability audit is also taking place. This presents a problem because the data are not in the standard panel format where each observation would be a municipality-year. To address issues that could potentially arise as a result of this unbalanced panel, I calculate non-parametric difference-in-differences estimates that compare changes in treated municipalities after a switch to vote-by-mail to changes in those municipalities which have not

yet been treated. Specifically, I calculate the sample analogs of the population quantities in the following equation:

$$\hat{\beta} = (E[AvgFindings_{m,t} | m = Treated, t = 1] - E[AvgFindings_{m,t} | m = Treated, t = 0]) - (E[AvgFindings_{m,t} | m = Control, t = 1] - E[AvgFindings_{m,t} | m = Control, t = 0]) \quad (5)$$

In this framework,  $t = 0$  indicates the time period before a switch to vote-by-mail and  $t = 1$  indicates the time period after a switch to vote-by-mail. The municipalities in the group  $m = Treated$  are those that switched to vote-by-mail in a particular year and the municipalities in the group  $m = Control$  are the municipalities that have not yet been treated and are not treated in the year in which the effect is being estimated.

I restrict my analysis of this data to the municipalities that switched to vote-by-mail in 2005 and 2006, because as we move away from the year in which most counties decided to switch to vote-by-mail (2005) the number of counties left to be treated or to be a part of the control group drops dramatically. In fact, after 2006 there are only five counties that have not yet been treated with vote-by-mail and, as a result, there are an insufficient number of clusters for valid inference (Cameron, Gelbach and Miller 2008).

I calculate the sample averages and estimate Equation 5 by using data on audit reports that began and ended in the year prior to vote-by-mail ( $t = 0$ ) and began and ended by the year after the switch to vote-by-mail ( $t = 1$ ). For example, for the municipalities that switched in 2005, I compare the average number of findings for reports that took place in 2004 to those that took place in 2006. Then, I estimate the same model but expand this bandwidth to include reports that began and ended within two years of the switch to vote-by-mail. I only include municipalities in my sample that have audit reports in both the time periods before and after treatment to account for the fact that some municipalities may not be audited every year. The standard error of the difference-in-differences estimates is calculated as the standard deviation of 500 block bootstraps with each county being its own cluster. This is then used to construct 95% confidence intervals around the estimates.

The quantities estimated from Equation 5 are displayed in Figure D.1 with 95% confidence intervals for municipalities that switched to vote-by-mail in 2005 and those that switched in 2006. The estimated effect of vote-by-mail on the average number of audit findings using a bandwidth of one year around the switch

to vote-by-mail is shown with the solid line and the estimates using a bandwidth of two years around the switch is shown with the dashed line. For 2005, the difference-in-differences estimates are in the expected direction but are not statistically significant. However, for the municipalities that switched in 2006 the point estimates are negative and statistically distinguishable from 0 for both the one year (-0.261) and two year (-0.129) bandwidths. This indicates that among the municipalities that switched to vote-by-mail in 2006 that the institution caused a decrease in the average number of findings on accountability audit reports. These results are substantively larger than the results presented in Table 3 in the main paper.

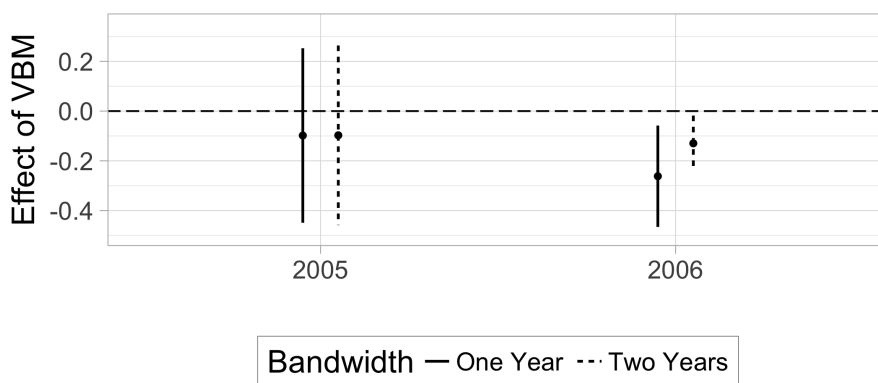


Figure D.1: The effects of vote-by-mail on the average number of municipal audit findings. One-year and two-year bandwidth estimates are displayed with county-level block-bootstrap 95 % confidence intervals.

Placebo tests for the analyses examining the effects of vote-by-mail on the average number of findings are displayed in Figure D.2. The point estimates displayed in the figure are the effect of vote-by-mail in treated municipalities in time period  $t - 1$  (the year before switching) on the average number of findings. The results indicate no pretreatment differences between treatment and control groups for municipalities that switched in both 2005 and 2006.

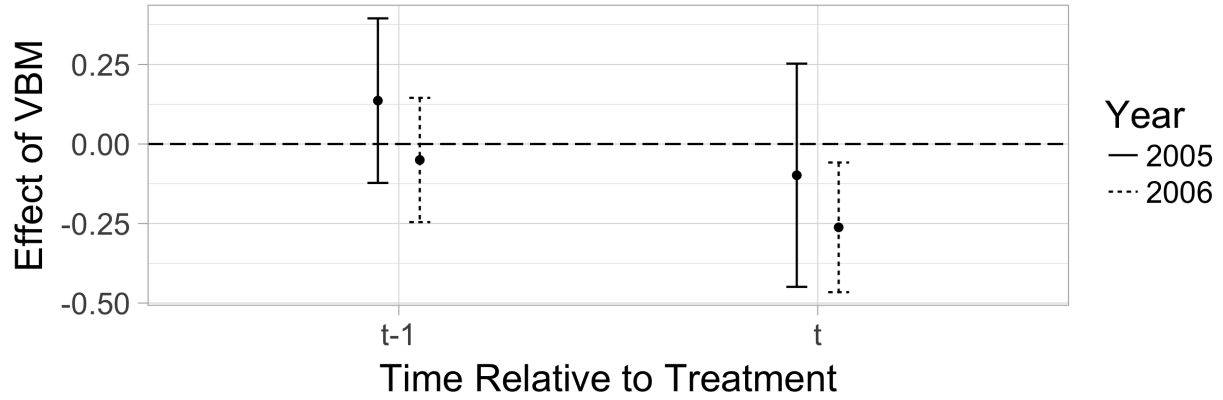


Figure D.2: Placebo test of the effect of vote-by-mail on average number of audit findings for municipalities treated in 2005 and 2006 with one-year bandwidth around treatment year. Point estimates as well as block-bootstrapped (county-level blocks) 95 % confidence intervals are displayed. Estimates at time  $t$  are the same as in the body of the paper. Estimates at time  $t - 1$  represent the effect of switching to vote-by-mail one year in the future.

## E Robustness Checks and Additional Results: Voter Behavior

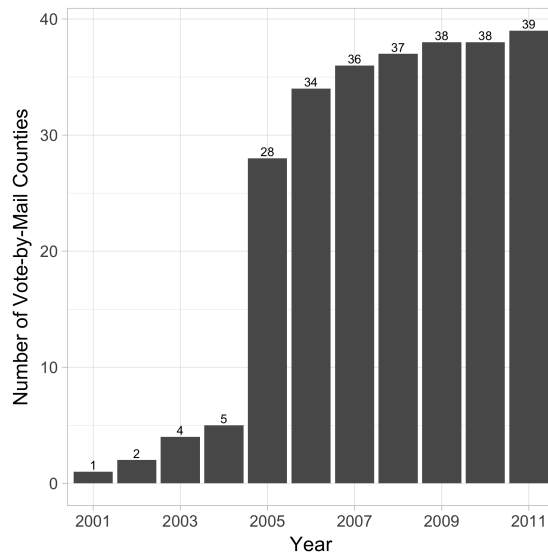
To assess the parallel trends assumption Table E.1 displays the results of models in the form of Equation 2 with municipal turnout and countywide turnout as the dependent variables. Across all of the specifications of the model, the leads of the vote-by-mail indicator variable are not statistically significant.

Table E.1: The Effects of Vote-by-Mail on Voter Turnout

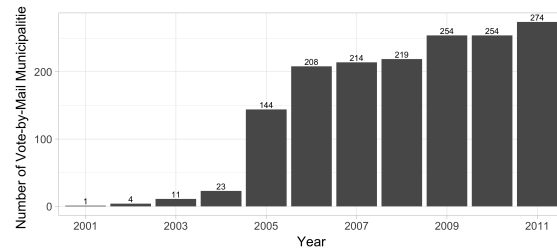
|                           | Municipal Turnout  |                  |                  | Countywide Turnout  |                    |                   |
|---------------------------|--------------------|------------------|------------------|---------------------|--------------------|-------------------|
|                           | (1)                | (2)              | (3)              | (4)                 | (5)                | (6)               |
| <i>Full Sample:</i>       |                    |                  |                  |                     |                    |                   |
| $VBM_{t+3}$               |                    |                  | 0.828<br>(1.404) |                     |                    | -1.605<br>(1.413) |
| $VBM_{t+2}$               |                    | 1.130<br>(2.483) | 1.710<br>(3.279) |                     | 1.006<br>(1.375)   | -0.566<br>(1.534) |
| $VBM_{t+1}$               | 0.783<br>(1.564)   | 1.749<br>(3.576) | 2.436<br>(4.521) | 0.757<br>(0.630)    | 1.785<br>(1.590)   | 0.031<br>(1.998)  |
| $VBM$                     | 4.956**<br>(2.009) | 6.265<br>(4.738) | 7.057<br>(5.783) | 3.805***<br>(0.770) | 5.024**<br>(1.963) | 3.116<br>(2.280)  |
| Observations              | 1,546              | 1,546            | 1,546            | 234                 | 234                | 234               |
| Adjusted R <sup>2</sup>   | 0.693              | 0.693            | 0.693            | 0.916               | 0.916              | 0.916             |
| <i>Restricted Sample:</i> |                    |                  |                  |                     |                    |                   |
| $VBM_{t+3}$               |                    |                  | 0.703<br>(1.425) |                     |                    | -1.737<br>(1.557) |
| $VBM_{t+2}$               |                    | 1.030<br>(2.539) | 1.537<br>(3.354) |                     | 0.928<br>(1.404)   | -0.809<br>(1.760) |
| $VBM_{t+1}$               | 0.548<br>(1.514)   | 1.456<br>(3.687) | 2.069<br>(4.646) | 0.726<br>(0.807)    | 1.739<br>(1.784)   | -0.245<br>(2.454) |
| $VBM$                     | 4.873**<br>(1.944) | 6.126<br>(4.909) | 6.838<br>(5.985) | 4.100***<br>(0.986) | 5.344**<br>(2.262) | 3.144<br>(2.856)  |
| Observations              | 1,422              | 1,422            | 1,422            | 204                 | 204                | 204               |
| Adjusted R <sup>2</sup>   | 0.706              | 0.706            | 0.706            | 0.920               | 0.920              | 0.920             |
| Municipality FE           | Yes                | Yes              | Yes              | No                  | No                 | No                |
| County FE                 | No                 | No               | No               | Yes                 | Yes                | Yes               |
| Year FE                   | Yes                | Yes              | Yes              | Yes                 | Yes                | Yes               |
| Controls                  | Yes                | Yes              | Yes              | Yes                 | Yes                | Yes               |

*Note:* Robust standard errors clustered by county in parentheses. \*p<0.1; \*\*p<0.05; \*\*\*p<0.01. Control variables include an indicator for if there was a mayoral race (Columns 1-4) as well as the following demographic controls at the municipality (Columns 1-4) or county (Columns 5-8) level: percent Black, percent Latino, median income, percent with high school degree or higher, percent urban and total population.

## F Count of Treated Units By Year



(a) Number of Counties



(b) Number of Municipalities

Figure F.1: Number of counties (Panel A) and municipalities (Panel B) holding vote-by-mail elections in Washington 2001 - 2011. Note that the 6 municipalities whose borders cross county boundaries are removed from the sample.