

How Electoral Institutions Affect Political Accountability: Evidence from Vote-by-Mail in Washington*

James Szewczyk
Department of Political Science
Emory University
james.szewczyk@emory.edu

July 26, 2017

Abstract

In this paper, I develop a theory of how vote-by-mail increases the propensity of voters to obtain political information before voting, which induces incumbents behave competently and more in line with the interests of their constituents. Exploiting the staggered implementation of vote-by-mail in Washington and a number of sources of data, I utilize a difference-in-differences estimation strategy to test this theory on the behavior of voters and municipal officials in the state. I find that vote-by-mail causes a decrease in ballot roll-off on statewide ballot measures in presidential election years, which is consistent with the theory that voters gather more information when voting in all-mail elections. Further, switching from traditional polling place elections to vote-by-mail results in a decrease in negative findings on accountability audit reports and a decrease in taxing and spending in Washington's municipalities. Using data from the Catalist voter file I show that these results cannot be explained by changes in the composition of the electorate caused by vote-by-mail.

*I am grateful to Zac Peskowitz, Tom Clark, Miguel Rueda, Jeff Staton, Pablo Montagnes, Braden Dauzat, Dani Villa, and Kirsten Widner for helpful feedback on this project. I also thank Vladimir Kogan, Stephane Lavertu, and Zac Peskowitz for generously sharing data.

Introduction

In a democracy, elected officials are ideally held accountable through regular elections. Elections give voters the opportunity to evaluate the performance of politicians and decide whether or not to retain incumbents. This generates a set of reelection incentives that shape the behavior of politicians, and when incumbents will face an election in the future they change their behavior in order to behave more in line with the interests of their constituents (e.g., Alt, De Mesquita and Rose 2011, Besley and Case 1995*b*, Ferraz and Finan 2011). Further, the specific rules that govern who gets to vote and how ballots are cast affect this strategic interaction between voters and politicians. For example, electoral institutions such as the timing of elections (Anzia 2011), the type of ballot used (Fujiwara 2015), and compulsory voting (Bechtel, Hangartner and Schmid 2016, Fowler 2013) have been shown to affect policy outcomes. Thus, it is clear that sometimes seemingly small manipulations of electoral rules can dramatically change the incentives of elected officials and have profound implications for the nature of democratic governance.

In this paper, I build on this previous work that examines the consequences of electoral institutions by developing a theory of how vote-by-mail elections affect the behavior of elected officials and policy outcomes. Vote-by-mail is an increasingly popular electoral institution in which all voters cast their ballot through the mail rather than going to a traditional polling place. I posit that vote-by-mail elections result in voters obtaining more information about their elected officials. A voter is able to gain relevant political information when 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 little about the candidates when voting at a polling place, he is unable to obtain additional information before casting his vote. Drawing on the theoretical (e.g., Ashworth 2012) and empirical (e.g., Snyder and Strömberg 2010) literature on how political information affects electoral accountability, I propose that this increase in information among voters strengthens the electorate's ability to hold politicians accountable through elections. That is, vote-by-mail helps voters overcome the information asymmetries between themselves and elected officials, which induces elected officials to act more in line with voters' interests.

I test the theory in the context of elections and governance in the state of Washington and utilize a difference-in-differences design that exploits the staggered implementation of vote-by-mail in the state. I find evidence that voters seek out more information when using vote-by-mail and that the institution also

affects the behavior of elected officials and policy outcomes. With respect to voters, I show that switching to vote-by-mail causes a decrease in ballot roll-off for statewide ballot measures in presidential election years, which is consistent with the logic that vote-by-mail results in voters seeking out more political information. I then examine how vote-by-mail affects two dimensions of the behavior of politicians in Washington's municipalities. I find that when a municipality is assigned to switch to vote-by-mail elections that the average number of violations documented in accountability audits completed by the state government decreases. Further, consistent with models of political economy (Persson and Tabellini 2000) and previous work that suggests voters punish incumbents for high levels of spending (Besley and Case 1995*b*, Peltzman 1992), I also find that switching to vote-by-mail results in a decrease in revenue, especially from property taxes, and a decrease in total expenditures in municipalities in Washington. These results are conditioned by the presence of a mayor-council form of government and the level of electoral competition in a municipality. Finally, using data from Catalist on the characteristics of voters in Washington's municipal elections I show that these results regarding municipal fiscal policy cannot be explained by changes in the composition of the electorate caused by vote-by-mail.

In all, the findings that I present in this paper are important for a number of reasons. First, as Trounstein (2010) notes there is a lack of research on responsiveness and accountability at the local level. Thus, this paper builds on more recent work that demonstrates how fiscal policy outcomes in cities are affected by the ideology of citizens (e.g. Einstein and Kogan 2016, Tausanovitch and Warshaw 2014) by showing how electoral institutions also shape these policies. Further, although there is a large literature on the effects of vote-by-mail in the United States (e.g., Bergman and Yates 2011, Berinsky, Burns and Traugott 2001, Gerber, Huber and Hill 2013, Gronke and Miller 2012, Kousser and Mullin 2007, Magleby 1987, Richey 2008), it focuses almost exclusively on how the institution affects voter turnout (Gronke et al. 2008). Therefore, I expand our understanding of vote-by-mail in at least two ways. First, I propose a new theory of how the institution affects the behavior of voter: vote-by-mail leads voters to gather more information about elected officials. Second, I move beyond previous work on vote-by-mail by examining how these changes in voter behavior shape the incentives of elected officials and affect policy outcomes. More broadly, these findings show that an electoral institution that has been given little attention in the literature on the policy consequences of electoral reforms has profound effects on electoral accountability and public policy.

This paper proceeds as follows. First, I develop a theory of how vote-by-mail affects the acquisition of political information and electoral accountability and then I also derive a number of testable implications

of this theory. Next, I discuss the institutional context of Washington, where I test this theory, and present my difference-in-differences estimation strategy. Finally, I trace out the effects of vote-by-mail on the behavior of voters and elected officials before concluding.

How Vote-by-Mail Affects Political Information

The central theoretical argument in this paper is that holding elections by mail results in voters gathering more political information than they would have in a polling place election. The mechanism that increases the probability that voters in all-mail elections will obtain relevant information about elected officials is simple: voters have the ability and time to more gather 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 traditional polling place.

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 during through elections (Berry and Gersen 2009). However, when casting a ballot most citizens do not have sufficient information or interest in a particular office to evaluate all of these elected officials. That is, 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 he is running for reelection. Moreover, when casting a ballot at a polling place if an individual comes across a race that he has very little information on or knows nothing about, he has no time or ability to obtain additional information about these races. , However, in elections that take place entirely by mail 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 particularly 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 an election that takes place entirely by mail. 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). Indeed, voters do seem to use the additional time that they have with their ballot when they receive it through the mail to gather additional information about candidates and policy issues.

A common critique of vote-by-mail is that it takes away from the social and communal nature of voting in a polling place with other citizens on Election Day. But, others have argued that being able to take additional time examining the ballot is a sufficient replacement for this experience. For example, an opinion piece in support of vote-by-mail in the Vancouver, Washington newspaper *The Columbian* argues, “For critics who complain about the demise of the patriotic ‘Election Day’ experience...the quick response is that it’s been replaced by a new family experience. 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 research about the election under a vote-by-mail system but they are also building additional discussion about the issues into their voting experience. Further, this shows that the informational mechanism that I posit in this paper has been used in the popular press in support of vote-by-mail, which suggests that the public, and especially elected officials, are aware of the potential for the institution to increase the level of information about politics that voters obtain.

In sum, I argue that vote-by-mail elections increase the propensity of voters to seek out information about elected officials because when casting their vote through the mail voters have additional time with the ballot, which gives them the ability to gather additional information about the election. Next, I turn to a discussion of how I expect elected officials to respond to this change in the information environment that results from all-mail elections.

Political Information and Electoral Accountability

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 elections can be used to induce accountability among elected representatives (Ashworth 2012). For example, Besley (2006) presents a two-period model where voters evaluate the incumbent politician 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 policy makers 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. That is, 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 are unable or unwilling to observe what policies

are implemented and the consequences of those policy choices, which also contributes to this informational asymmetry.

That being said, this literature suggests that increasing voters' information about incumbents will lead them to work harder for their constituents. Specifically, Besley (2006) argues that increasing policy relevant information about the incumbent increases voter welfare. This prediction is consistent with other models in the political agency literature and as Ashworth (2012) notes, "Most models considered in the literature have similar, and very sharp, predictions for ... informational changes. The voter observes a noisy measure of the incumbent's action. And the likelihood of responsiveness is decreasing in the amount of noise" (191). Further, there is also empirical evidence that is consistent with these theoretical predictions (e.g., Snyder and Strömberg 2010). As I have argued, the institution of vote-by-mail increases the probability that voters seek out information about elected officials and, thus, decreases the noise in voters' knowledge of the actions of incumbents. As a result, I expect that in political jurisdictions where vote-by-mail elections are held, elected officials will be more competent and more responsive to their constituents.

In sum, based on the theoretical work on electoral accountability I expect that when institutions are put in place that help voters obtain relevant political information, politicians should work harder to implement policies that are more in line with the preferences of the electorate and behave more competently. Moreover, I propose that holding elections entirely by mail helps voters overcome these informational asymmetries. Specifically, vote-by-mail increases the probability that voters acquire relevant information about the incumbent's performance in office, which increases the accountability of politicians.

Observable Implications

This theory generates a number of observable implications about the behavior of both voters and elected officials in jurisdictions where vote-by-mail elections are held. I now turn to a discussion of the specific predictions that I test in this paper in the context of Washington.

Voter Behavior. Because the process of gathering information should lead to increased political knowledge, ideally a researcher would examine measures of voters' knowledge of politics before and after ballots are sent out in order to test the theory. However, to my knowledge no such survey data is available in Washington, which is the context used for my empirical analyses, during their transition to vote-by-mail.

In order to overcome this issue and get at how vote-by-mail shapes the information environment of elections, I examine the effect of switching to vote-by-mail on ballot roll-off in Washington. Ballot roll-off is a concept that captures the proportion of individuals that cast a ballot in an election but do not vote in a given race. For example, ballot roll-off could measure the proportion of individuals who vote for, say, president but not their congressman. Previous research has found that one of the main contributors to ballot roll-off is the low levels of information about certain races (Bullock and Dunn 1996, Wattenberg, McAllister and Salvanto 2000). In fact, Wattenberg, McAllister and Salvanto (2000) even compare voting to taking an SAT test and argue that voters choose to only participate in races about which they are informed. Further, others have found that processes that increase information about lower salience elections, such as campaign spending (Hall and Bonneau 2008; 2012) and partisan cues (Schaffner, Streb and Wright 2001), decrease ballot roll-off. Thus, if vote-by-mail affects how voters gather information about elections in the way that I have theorized, I expect that switching to vote-by-mail will decrease ballot roll-off.

Elected Official Behavior. I examine two different dimensions of the behavior of elected officials. First, if voters are obtaining more information about incumbents, then elected officials should behave in a more competent manner. That is, under vote-by-mail elected officials anticipate that voters will have more information about their performance so they will avoid misappropriating resources or breaking laws as they know these actions are more likely to be observed, which will hurt their reelection chances. This is consistent with previous work that examines how electoral incentives affect corrupt behavior among politicians. For example, Ferraz and Finan (2011) find that mayors in Brazilian municipalities that are up for reelection engage in less corruption than mayors serving in their final term. Further, even if elected officials are not directly engaging in misconduct themselves, I still expect that if they are managing the government well that there will be fewer violations of protocol among civil servants. Thus, in political jurisdictions with vote-by-mail elections I expect that there will be more safeguards of public resources and fewer breaches of good governance policies.

Not only should increased political information affect the general levels of competence among elected officials, but it should also lead to higher levels of responsiveness to the policy preferences of voters. Although individual voters may differ with regard to which policy they think is optimal among a host of different policy choices, most voters prefer that government services are provided to the public efficiently and at a low cost. Indeed, previous research suggests that voters punish incumbents for high levels of

taxation and spending (Besley and Case 1995*b*, Peltzman 1992), and 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). Further, empirical evidence indicates that when government officials have to face reelection that taxing and spending decreases. For example, when governors are in their final term of office, and consequently do not have reelection incentives, there are higher levels of expenditures relative to those that may seek reelection (Alt, De Mesquita and Rose 2011, Besley and Case 1995*a*), and when officials charged with assessing property taxes are elected, rather than appointed, assessments occur less frequently and revenue generated from property taxes decreases (Sances 2016). This is also consistent with work that finds that when voters are faced with ballot measures, and thus policy outputs are more likely to reflect the preference of the median voter, that the level of government spending and tax rates decrease (Matusaka 2004). Based on this previous work, I hypothesize that vote-by-mail elections will lead politicians to decrease taxing and spending as a result of increased responsiveness.

Estimating the Effects of Vote-by-Mail

Analyzing the causal effects of an institutional change can often prove challenging due to the difficulties of identifying the appropriate group to compare to the unit that has the institutional arrangement of interest. For my purposes, however, the state of Washington provides a good case to study the effects of vote-by-mail for a number of reasons. First, I am able to test the implications of the theory on the behavior of elected officials in the context of elections and governance in the 281 incorporated municipalities in the state. Second, because of the nature of the switch to vote-by-mail in Washington I am able to use a difference-in-differences estimation strategy to credibly estimate the causal effects of this institution.

Municipal Elections in Washington

Elections for municipal office in Washington are low-information environments, and, like many other local races, municipal elections in the state 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 these types of local elections are indicative of an electoral environment in which voters have little

access to political information and in which incumbents are not held accountable. 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 relevant policy.

Further, Washington's municipal elections in particular lack a number of features that would 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. It is widely known that party ID serves as a strong heuristic for voters and the absence of this shortcut greatly 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 even know the name of the incumbent will not be able to perform a simple retrospective evaluation of his 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, and 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). Further, in 1993 the state passed a law that allowed voters to apply for permanent absentee status (Reed 2007). This allowed voters to receive their ballot in the mail without an excuse for every election. Many voters in Washington chose to take advantage of this feature of their electoral system.

There were also a number of reforms in Washington that gradually forced certain citizens to 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 vote-by-mail elections (Reed 2007). They

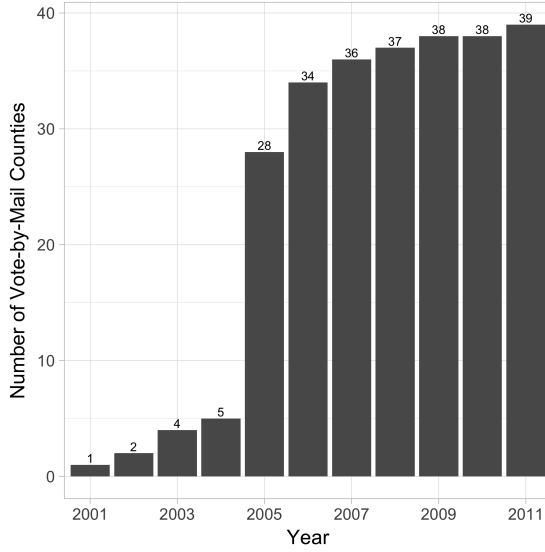
were known as mail ballot precincts. During this time, many rural counties with small populations redrew their precincts so that they were sufficiently small to allow the entire county to hold vote-by-mail elections. Second, in 2005 the state legislature passed a law, HB 1754, that included a number of electoral reforms including a statute that allowed individual counties to decide to hold all of their elections entirely by mail, regardless of the size of their precincts. Individual counties, under the direction of their county councils, gradually switched to vote-by-mail elections in the years following HB 1754. Finally, in 2011, when only one county had yet to switch to vote-by-mail, the state legislature enacted another law that switched all elections in the state to vote-by-mail.

Figure 1 displays the number of counties and municipalities that held vote-by-mail elections in each year from 2001 - 2011. I show the counts for both of these political jurisdictions, because although the transition to vote-by-mail in Washington took place at the county level, I examine outcomes at both the county and municipality level in this study. Panel A displays the number of counties in each year. As can be seen by looking at the number of counties who held vote-by-mail 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 decide to switch to vote-by-mail in 2005, regardless of precinct sizes, many counties decided to immediately discontinue traditional 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. Panel B, which displays the number of municipalities holding vote-by-mail elections in each year, shows a similar pattern to Panel A.

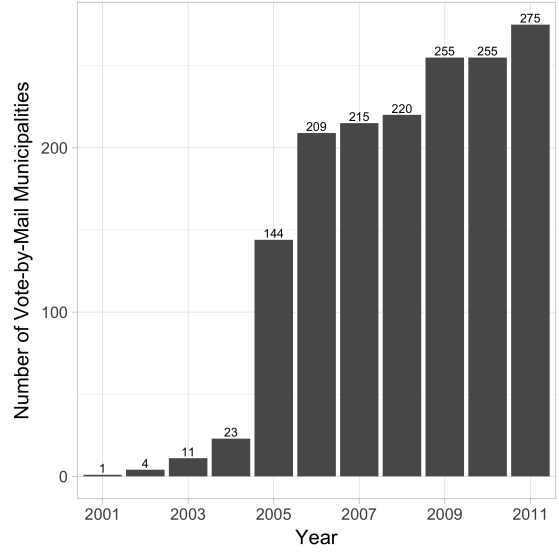
Identification Strategy

In the analyses that I present in this paper, I utilize a difference-in-differences estimation strategy, which allows me to exploit the staggered implementation of vote-by-mail in Washington to estimate the effects of holding all-mail elections. Using difference-in-differences 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 it¹ as being in treatment for all of the years in which the the county held vote-by-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. This setting differs from classic

¹I omit the 6 municipalities in Washington that cross county borders from my analyses.



(a) Number of Counties



(b) Number of Municipalities

Figure 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.

difference-in-differences applications because the treatment of vote-by-mail is applied in different years depending on the county and the composition of the treatment and control groups changes over time, but difference-in-differences can be easily extended to multiple time periods within a regression framework (Angrist and Pischke 2009). This allows me to use the full sample of municipalities in Washington over an extended period in most of my analyses.

Finally, because it is reasonable to assume that counties, and the municipalities nested within them, where the local election officials intentionally redrew precincts to force vote-by-mail elections prior to 2005 may be quite different than other counties and municipalities in the state, I run all of my 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 Impact of Vote-by-Mail on Voters

My first set of analyses considers the effects of vote-by-mail on the behavior of voters. Specifically, in relation to the theory presented above, I am interested in examining if switching to vote-by-mail resulted in voters gathering more information about their elected officials and the races on the ballot than they would

have otherwise. As discussed, I argue that vote-by-mail elections increase the amount of information that voters obtain about the races on the ballot. Conditional on a voter casting a ballot, this should, in turn, result in voters being more likely to vote in lower salience races under a vote-by-mail electoral system, which will result in decreased ballot roll-off.

I test this hypothesis using ballot roll-off rates for statewide ballot measures. In Washington there is a robust system of direct democracy, and individuals can submit initiatives to appear on the ballot that are voted on to become law as well as referenda on recently passed laws by the legislature. A number of these measures appear on the ballot every year. It is appropriate to use voting rates on ballot measures to examine ballot roll-off because they appear on the ballot for everyone across the entire state, which aids in the comparison of polling place and vote-by-mail voters. On the other hand, if I were to examine ballot roll-off in sub-state races there would be a concern that there may be some unobservable factor that, say, affects the level of participation in certain local elections in a particular year but not races in other jurisdictions, which would violate the parallel trends assumption necessary for unbiased difference-in-differences estimates. Further, it is not possible to construct a measure of ballot roll-off across all municipalities in Washington, because 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. Finally, examining ballot roll-off on statewide ballot measures is also desirable because it is a hard test of the theory due to the fact that ballot measures in Washington can sometimes be rather high profile because citizens are voting on important policy issues. I now discuss the data used in this set of analyses. Summary statistics of all data used in the paper are located in Appendix A.

Data

Statewide Ballot Measures. In order to create the dependent variable measuring ballot roll-off, I collect the results of all statewide ballot measures in Washington broken down by county as well as county-level voter turnout. This information is gathered for the 54 statewide ballot measures that Washingtonians voted on in November general elections from 2000 - 2010 and is obtained from the Washington Secretary of State's website as well as individual county election offices. This data is used to generate the variable $BallotRollOff_{c,t}$, which is the proportion of individuals in a county that cast a ballot but did not vote for a given ballot measure. The variable is created by taking the number of individuals in a county who voted for a given ballot measure and then dividing it by total ballots cast in that county. Then, this quantity

is subtracted from 1 and averaged across all ballot measures being voted on in every county in a given year. The variable ranges from 0 to 1 with 1 indicating that no voters in a county voted for a given ballot measure and 0 indicating that there was no ballot roll-off.

Vote-By-Mail. The main independent variable of interest in my analyses, $VBM_{c,t}$, is created using a document released from the Washington Secretary of State’s Office that contains information about when each county switched to vote-by-mail. It is a dummy variable that is equal 1 in all years in which a county holds its elections entirely by mail and 0 otherwise.

Results

I now present the results of my models examining the effect of vote-by-mail on ballot roll-off. Within the difference-in-differences framework, I estimate the following equation,

$$BallotRollOff_{c,t} = \beta_1 VBM_{c,t} + \lambda_c + \tau_t + \epsilon_{c,t} \quad (1)$$

where the dependent variable, $BallotRollOff_{c,t}$ is, as discussed, the proportion of individuals who did not cast a vote for a given measure out of the total number of ballots returned. The main independent variable in the model is $VBM_{c,t}$, which is a dummy variable equal to one in the years in which a county held vote-by-mail elections and zero for the years before the electoral reform in the county. The estimated value of the coefficient β_1 is the causal effect of interest. Further, λ_c represents a set of county dummy variables in order to control for time invariant unobserved differences between counties that may affect the timing of the switch to vote-by-mail and ballot roll-off, and τ_t are year fixed effects to control for year-specific factors. Finally, $\epsilon_{c,t}$ is the error term. Because treatment is applied at the county level, I report robust standard errors clustered by county. I run these models with various subsets of the data based on the timing of elections, because I expect that the effect of vote-by-mail on ballot roll-off should vary based on the salience of statewide ballot measures relative to other races on the ballot in that election.

The results of the models of this form are displayed in Table 1. Column 1 and Column 2 display the relationship between vote-by-mail and ballot roll-off in all November general elections in Washington from 2000 - 2010 with the full and restricted sample of counties, respectively. Recall, the restricted sample

consists of counties that switched to vote-by-mail in 2005 or later, which is after the law was passed that allowed counties to decide whether or not they wanted to switch to vote-by-mail. The coefficient on the vote-by-mail indicator in these models is in the expected direction. That being said, it is quite small and not statistically significant for the full sample though it is marginally significant with the restricted sample. This seems to indicate that vote-by-mail has, if anything, a very substantively small effect on ballot roll-off in voting on statewide ballot measures. However, these results may be due to the fact that the data used in the model displayed in Column 1 and Column 2 includes off-cycle elections that take place in odd years. In these years, as discussed, elections for local offices in Washington are held. Consequently, there may not be much roll-off for statewide races in these years, like ballot measures, because these are some of the most salient races of those election cycles.

Table 1: The Effects of Vote-by-Mail on Ballot Roll-Off

	<i>Dependent variable:</i>					
	All Elections		Ballot Roll-Off		Presidential Elections	
	(1)	(2)	(3)	(4)	(5)	(6)
Vote-by-Mail	−0.004 (0.003)	−0.004* (0.002)	−0.002 (0.004)	−0.003 (0.004)	−0.015** (0.006)	−0.012* (0.007)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Municipal FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	390	340	195	170	78	68
Adjusted R ²	0.740	0.755	0.736	0.741	0.658	0.634

Note: Results with full data are displayed in Columns 1, 3, and 5. Results with restricted data including only counties treated in 2005 or later are displayed in Columns 2, 4, and 6. Robust standard errors clustered by county in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Thus, I next examine the effect of vote-by-mail on roll-off but subset the data to only include even-year elections and then just presidential election years, because in these years there are races about which voters are much more informed.² The results of the models with only even-year elections are displayed in Column 3 (full sample) and Column 4 (restricted sample) of Table 1. In even-years, which includes presidential and midterm elections, vote-by-mail still does not have a significant effect on ballot roll-off. However, when examining only presidential elections in Column 5 (full sample) and Column 6 (restricted sample) vote-by-mail significantly decreases ballot roll-off for statewide ballot measures in Washington.

²There were no statewide ballot measures in 2004, so the analyses with even-year elections includes the years 2000, 2002, 2006, 2008, and 2010 while the analyses with presidential election years includes 2000 and 2008.

The estimated effect is -0.015 when using the full sample of data and -0.012 when using the restricted sample of data. These coefficients on the vote-by-mail variable are statistically significant and indicate that vote-by-mail causes fewer voters to cast their ballot without voting for statewide ballot measures.

This is indeed a substantively large effect. Over the time period of interest, 2,824,466 individuals cast their ballot in presidential elections in Washington on average.³ The difference-in-differences estimate using the full sample of counties in Column 5 of Table 1 indicates that holding vote-by-mail elections causes a 1.5% increase in the number of individuals that vote for statewide ballot measures, which is equivalent to 43,179 more individuals voting for statewide ballot measures. This is an especially significant finding considering the fact that previous studies have shown that switching to vote-by-mail in Washington resulted in an increase in voter turnout in presidential election years. Specifically, Gerber, Huber and Hill (2013) find that turnout in Washington increased by 2.8% in presidential election years after switching to vote-by-mail and that this effect is driven by registrants that participated less frequently before vote-by-mail. It is likely that these voters have relatively low levels of political information because they did not regularly participate in politics prior to vote-by-mail. Thus, despite that fact that vote-by-mail is driving voters with lower levels of political information to cast their ballot in these elections, ballot roll-off still decreases.

In all, these findings are consistent with the theory presented in this paper. Holding vote-by-mail elections results in voters gathering more information about lower salience races on the ballot, which, in turn, results in individuals voting in these races and not only voting, say, for President. Although the results presented thus far pertain to ballot roll-off in voting for statewide ballot measures, I argue that this finding should also hold for other low-salience races and that vote-by-mail also increases the amount of information that voters gather in these elections. Specifically, I expect that this mechanism also operates for municipal elections, which, as discussed, are particularly low information environments. Thus, in the next section of this paper I analyze the effects of vote-by-mail on the behavior of elected officials in Washington's municipalities.

The Impact of Vote-by-Mail on Elected Officials

I now turn to an analysis of how vote-by-mail affects the behavior of politicians and policy outcomes. As discussed, if vote-by-mail leads voters to gather more information about those running for office this should

³This figure is the average turnout across the 2000, 2004, and 2008 presidential elections. The data was obtained from the Washington Secretary of State's website: <https://www.sos.wa.gov/elections/voter-participation.aspx>

induce elected officials to work harder and to behave more in line with the interests of their constituents. Specifically, I analyze the effect of vote-by-mail on the likelihood that municipal governments receive negative accountability audit findings as well as on the finances of municipalities. I next discuss the data used in these analyses before presenting my results.

Data

Audit Reports. First, I gather information from audit reports that the Washington State Auditor’s Office releases for every governmental entity in the state, including municipalities. Municipalities are regularly subject to “accountability audits” and “financial audits” that are completed by representatives of the State Auditor’s Office. Accountability audits are intended to ensure that municipalities are safeguarding public resources and following necessary policies and laws in day-to-day governance, while financial audits assess whether or not a municipality’s financial statements are being completed accurately. 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 specified 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 and if a municipality has zero findings for a given audit period this indicates that no violations were found. These reports include the misconduct of both career government employees as well as elected officials. But, elected officials - either the mayor or a city manager that is directly accountable to the city council - are directly 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 examples of some of the activities for which a municipality would receive a finding.

I obtain the audit reports by scraping the Washington State Auditor’s website where a PDF of each report from January 2005 to present is publicly available.⁴ The audit periods covered in my data, however, begin prior to 2005 because the audit reports are typically released a few years after the audit is completed. I obtain from these reports the type of audit that was being completed, the exact time period for which the government’s activities were being audited, and the number of “findings” that the auditor discovered. This information is used to create the dependent variable, $NumFindings_{m,t}$, which is the number of findings

⁴These reports were obtained from the following website in January of 2017: <http://portal.sao.wa.gov/reportsearch>. The number of findings in each report and the time period of each audit were extracted from these documents.

Schedule of Audit Findings

City of Spokane
Spokane County
November 15, 2006

2. The City of Spokane did not follow competitive bid law.

Description of Condition

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.

(a) Example of Audit Finding in City of Spokane

Schedule of Audit Findings and Responses

City of Arlington
Snohomish County
August 5, 2010

1. The City of Arlington does not have adequate support for its allocation of shared costs to utility funds.

Background

Cities incur costs from their central services, such as general administration, human resources, payroll and purchasing, that are shared among all departments that benefit from the services. The City has a responsibility to adopt a fair and equitable method of distributing these shared costs among departments. Additionally, state law prohibits resources restricted for certain uses, such as utilities that collect usage-based fees from citizens, to benefit other funds.

(b) Example of Audit Finding in City of Arlington

Figure 2: Examples of audit findings in two municipalities in Washington.

in audit reports for a municipality in a given time period. This is a measure of the frequency in which laws and regulations were broken by the municipality or public resources were misappropriated over a specified period of time.

Municipal Revenue and Expenditures. To test my hypotheses on the effect of vote-by-mail on municipal revenue and expenditures, I collect data from the Washington State Auditor's Office on the finances of Washington municipalities from 2001 - 2010.⁵ The Auditor's Office systematically collects detailed revenue and expenditure data from each municipality in the state at the end of every calendar year and the law requires that each municipality report this information. This allows me to create a panel of municipal finances in Washington over the time period of interest.

I use this data to examine the taxing and spending behavior of municipalities. First, I create variables measuring the total revenue and the sources of this revenue in Washington's municipalities. Specifically, I generate the following variables: $\text{LogRevenuePerCapita}_{m,t}$, $\text{LogTaxRevenuePerCapita}_{m,t}$, and

⁵Spreadsheets containing detailed financial data on all of Washington's political entities are available on the State Auditor's website (<http://portal.sao.wa.gov/LGCS/Reports/ViewExportedData.aspx>) The variables that were constructed from this data were generated after correspondence with the State Auditor's Office to ensure the proper calculation of municipal revenue and expenditures.

LogPropertyTaxRevenuePerCapita_{m,t}. These are measures in each year from 2001-2010 of each municipality's total revenue as well as total tax revenue and, specifically, property tax revenue. Second, I create a dependent variable measuring total spending municipalities, *LogExpendituresPerCapita_{m,t}*. All of these 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 and log this value in order to adjust for skew in the data.

Municipal Election Results. I collect the results of city council and mayoral elections for all municipalities in Washington from 2001 to 2010. The results were collected for all odd-year November general elections from individual county election offices and then were coded in order to create a dataset of all municipal elections over the time period of interest.⁶ This dataset was used to create a variable to measure the *Competitiveness_{m,t}* of the elections in a given municipality in each election year. In order to create this variable I simply divide the number of votes for the second highest vote getter in a give 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 measure of competitiveness is equal to 0 when all races in a municipality are uncontested and 1 if the the top two candidates tied in all races. Due to the timing of local elections in Washington, this variable is only observed every other year.

United States Census. Finally, in some of my analyses I control for the demographic characteristics of Washington's municipalities using data from the 2000 Decennial U.S. Census and the 2009 and 2010 American Community Survey's 3 Year Estimates. I interpolate the Census data for each missing year of the panel (2001 - 2008) using the interpolation method recommended by the U.S. census for intercensal estimates.⁷ I control for the racial characteristics of municipalities by creating the variables *PercentAfricanAmerican_{m,t}* and *PercentLatino_{m,t}*. Further, I include the variables *PercentHighSchoolDegree_{m,t}* and *MedianIncome_{m,t}*, which measure the socioeconomic characteristics of municipalities. I also control for the proportion of elderly in a municipality, *Percent65Older_{m,t}*, how urban a municipality is, *PercentUrban_{m,t}*, and each municipality's population, *TotalPopulation_{m,t}*. These are demographic characteristics that may have influ-

⁶The results were obtained from individual counties because the Washington Secretary of State's Office does not maintain local election results. The majority of documents containing election results were not machine readable the election results for most municipalities were coded by hand in order to create the measure of competitiveness.

⁷This method for linear interpolation of census data for years without census estimates can be found at the following link: https://www.census.gov/popest/methodology/2000-2010_Intercensal_Estimates_Methodology.pdf

enced the timing of the switch to vote-by-mail in Washington⁸ and may also affect the level of revenue and expenditures in municipalities.

Results: Accountability Audits

The first dimension of politician behavior that I examine is the competence and quality of elected officials and the governments that they are tasked with overseeing. Specifically, I expect that in municipalities with vote-by-mail elections, which results in incumbents anticipating that voters will gather more information about them, that elected officials will be less likely to break laws or misappropriate public resources. To examine the relationship between vote-by-mail and accountability in the context of Washington, I analyze data that I collected from audit reports on Washington's municipalities.

The audit report data presents a number of challenges because audits do not take place at universal intervals across all municipalities and there are sometimes multiple types of audits happening simultaneously in a given municipality. For example, the period of time that the auditor is examining can be greater than 1 year (in the data, the length of the audit period ranges from 1 year to 4 years) and a municipality could be having its financial records being audited while, say, a three year accountability audit is taking place. This presents a problem because the data are not in the standard panel format where each observation would be a municipality-year. Thus, I can not use the same approach as in the previous section where I adapted the difference-in-differences framework over multiple periods to a single regression.

In order to examine the effect of vote-by-mail on the average number of findings that a municipality receives in a given year, I calculate a series of non-parametric difference-in-differences estimates where I compare changes in treated municipalities after a switch to vote-by-mail to changes in those municipalities which have not yet been treated. Specifically, in order to estimate the difference-in-differences estimator of the effect of vote-by-mail on the number of findings I calculate the sample analogs of the population quantities in the following equation.

$$\hat{\beta} = E[\text{NumFindings}_{m,t} | m = \text{Treated}, t = 1] - E[\text{NumFindings}_{m,t} | m = \text{Treated}, t = 0] - \\ E[\text{NumFindings}_{m,t} | m = \text{Control}, t = 1] - E[\text{NumFindings}_{m,t} | m = \text{Control}, t = 0] \quad (2)$$

⁸For example, in the Appendix of Gerber, Huber and Hill (2013), the authors show that the proportion of a county that is rural influenced the timing of the switch to vote-by-mail.

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 given 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. As a results of this setup, 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 5 counties that have not yet been treated with vote-by-mail. This creates issues because there are few municipalities with which to estimate the effect and, further, because treatment is applied at the county level there are an insufficient number of clusters for valid inference (Cameron, Gelbach and Miller 2008).⁹

Therefore I restrict my analyses of these data to examining the effect of switching to vote-by-mail on the municipalities that switched to the institution in 2005 and 2006.¹⁰ Specifically, I estimate the sample averages and plug them into Equation 2, first, 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 those municipalities that switched in 2005, I compare the average number of findings for reports completed in 2004 to those completed in 2005 or 2006. Then, I estimate the same model but expand this bandwidth to include reports that ended within two years and then within three years of the switch to vote-by-mail. In order to calculate the standard error for these estimates I employ the block bootstrap with each county being its own block. The standard error is calculated as the standard deviation of 500 block bootstraps of the difference-in-differences estimator, which is then used to construct confidence intervals around the estimates.

The estimates are displayed in Figure 3. Panel A shows 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. The difference-in-differences estimates indicate that, as expected, switching to vote-by-mail has a negative effects on audit report findings. For 2005, the point estimate (-0.104) is distinguishable from zero with 90% confidence but the 95% confidence interval crosses zero. Further, the point estimate for 2006 (-0.286) is substantively larger and statistically significant. As I expand the bandwidth to two years before and after (Figure 3 Panel B) as well as three years before and after (Figure 3 Panel C) the switch to vote-by-mail

⁹Specificially, in 2007, 2008, and 2009 there are only 5, 3, and then 2 counties left in the sample, respectively.

¹⁰I do not analyze years prior to 2005, because the earliest observations in the accountability report data are in 2002.

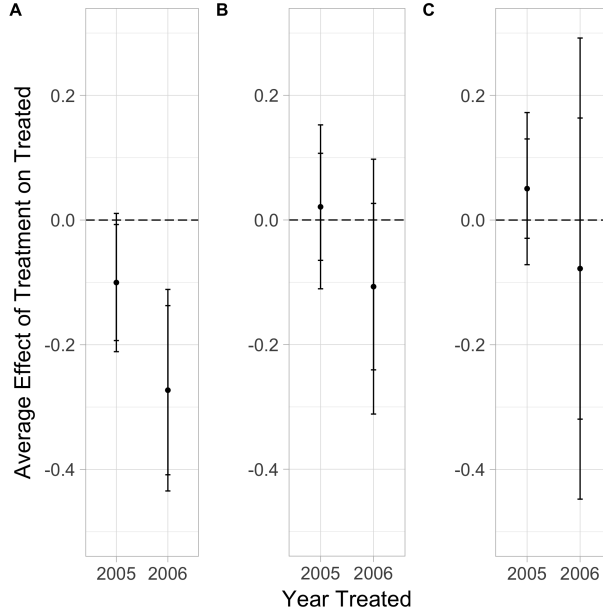


Figure 3: The effect of vote-by-mail on the average number of municipal audit findings. One-year, two-year, and three-year bandwidth estimates are displayed in Panel A, B, and C, respectively. Panels display point estimates as well as block-bootstrap (county level blocks) 90 % and 95 % confidence intervals.

the estimated effect of vote-by-mail loses its significance. This could be the result of a process where as time moves further away from when the switch to vote-by-mail took place, the effect of the institution on the behavior of elected officials diminishes. Or, it could also be the case that as I expand the bandwidth around the switch to vote-by-mail that I introduce noise, which increases the size of the standard errors. In Appendix B, I present a series of placebo tests to examine if there are pretreatment differences between the group of municipalities that switched to vote-by-mail and the group that did not. Encouragingly, the results of the placebo tests indicate that future switches to vote-by-mail have no effect on the average number of findings.

In all, these findings indicate that there is suggestive evidence that in the year directly after the switch to vote-by-mail that the institution had a negative effect on the number of findings documented in audit reports. This is a substantively important finding. As discussed, an audit report finding represents a substantial breach of law so a decrease in these types of violations indicates that government officials are behaving more competently and as more faithful agents of their constituents. This provides evidence in line with the theory that elected officials strategically respond to their expectation that voters will gather more information about incumbents when voting in an all-mail election. Further, these results cannot be explained by any potential changes in the composition of the electorate that are a result of vote-by-mail,

which provides additional evidence that the informational mechanism that I have proposed is indeed operating. That being said, this set of analyses has its drawbacks because I am only able to examine the effects of vote-by-mail in a small subset of municipalities in the state of Washington. Thus, I next turn an analyses of the finances of municipalities in Washington.

Results: Municipal Fiscal Policy

To estimate the effects of vote-by-mail on municipal revenue and expenditures, I again estimate models of the following form,

$$Y_{m,t} = \beta_1 VBM_{m,t} + \delta \mathbf{X}_{m,t} + \lambda_m + \tau_t + \epsilon_{m,t} \quad (3)$$

where $Y_{m,t}$ are the dependent variables pertaining to the revenue and expenditures of municipalities. I specifically on estimate the effect of vote-by-mail on $\text{LogRevenuePerCapita}_{m,t}$, $\text{LogTaxRevenuePerCapita}_{m,t}$, $\text{LogPropertyTaxRevenuePerCapita}_{m,t}$, and $\text{LogExpendituresPerCapita}_{m,t}$. The estimate of β_1 represents the causal effect of vote-by-mail on these outcomes. $\mathbf{X}_{m,t}$ is a vector of demographic control variables obtained from the U.S. Census that are included in some specifications of the model. As before, λ_m and τ_t are municipal and year fixed effects, and I report cluster-robust standard errors with clusters at the county level.

Recall that under the assumption that voters are fiscal conservatives I expect that when a political jurisdiction switches to vote-by-mail, elected officials will be more responsive to their constituents and, consequently, that taxing and spending will decrease. Table 2 displays the results of the models with total revenue (Columns 1-4) and total expenditures (Columns 5-8) as the dependent variable across a variety of specifications. Across all of the models there is a consistent theme: when a municipality switches to vote-by-mail, total revenue and expenditures decrease. The results maintain standard levels of statistical significance for both the full and restricted sample as well as with or without the vector of control variables. For example, in the model with the full sample of municipalities and controls for demographic characteristics, switching to vote-by-mail leads to a statistically significant 0.059 decrease in log revenue per capita (Column 2). Taking into account the log transformation of the dependent variable, this is equivalent to a 5.9% decrease in total revenue per capita. In the analogous model with the log expenditures per capita de-

pendent variable the coefficient on the vote-by-mail variable is statistically significant and equal to -0.071, which indicates that vote-by-mail elections cause a 7.1% decrease in total expenditures per capita. This provides convincing evidence that vote-by-mail causes elected officials to implement more conservative fiscal policies Washington's municipalities.

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

	<i>Dependent variable:</i>							
	Log Revenue Per Capita				Log Expenditures Per Capita			
	Full Sample		Restricted Sample		Full Sample		Restricted Sample	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Vote-by-Mail	-0.051*	-0.059*	-0.055**	-0.066**	-0.064**	-0.071**	-0.071***	-0.080***
	(0.029)	(0.031)	(0.026)	(0.028)	(0.026)	(0.029)	(0.022)	(0.025)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipal FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,598	2,589	2,387	2,378	2,598	2,589	2,387	2,378
Adjusted R ²	0.704	0.707	0.707	0.709	0.695	0.700	0.697	0.701

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

I next turn to an analysis of the effects of vote-by-mail on revenue obtained from taxes and, specifically, property taxes. If elected officials are really responding to the electoral incentives created by vote-by-mail then we should expect that there is a decrease in revenue sources, namely taxes, that particularly impact the pocketbooks of voters. The results of these models are displayed in Table 3. First, Columns 1-4 display the effect of vote-by-mail on log total tax revenue per capita. Although, the sign of the coefficient on the vote-by-mail variable is negative across the different specifications of the model it is never statistically significant. However, Columns 5-8, which display the models with log property tax revenue per capita as the dependent variable, indicate that the decrease in total revenue documented in Table 2 seems to be largely driven by a decrease in property taxes. Depending on the specification of the model the results show that vote-by-mail caused a 3.6% (Column 6) to 4.0% (Column 7) decrease in revenue from property taxes in Washington's municipalities.¹¹ Thus, not only does vote-by-mail cause lower levels of revenue and

¹¹In Appendix C, I present a series of robustness checks of these results regarding municipal finances. First, I estimate models with a series of leads and lags of the vote-by-mail independent variable to assess if the parallel trends assumption necessary for a causal interpretation of difference-in-differences estimates holds. The results of these models indicate that for the dependent variables that vote-by-mail has a robust and negative effect on - total revenue, expenditures, and property tax revenue - there are no pretreatment differences between municipalities that switched to vote-by-mail at different times. However, in one specification of the leads and lags model with total tax revenue as the dependent variable a lead variable is negative and statistically significant. Second, I estimate models that include a municipality specific time trend. For the models with property tax revenue as the

expenditures, but also these decreases in total revenue are driven by a decrease in property taxes, which is a revenue source that is most likely to directly effect many voters.

Table 3: The Effects of Vote-by-Mail on Municipal Tax Revenue

	<i>Dependent variable:</i>							
	Log Tax Revenue Per Capita				Log Property Tax Revenue Per Capita			
	Full Sample	Restricted Sample			Full Sample	Restricted Sample		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Vote-by-Mail	−0.019 (0.014)	−0.022 (0.015)	−0.020 (0.014)	−0.025 (0.016)	−0.038*** (0.010)	−0.036*** (0.009)	−0.040*** (0.011)	−0.037*** (0.010)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipal FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,598	2,589	2,387	2,378	2,597	2,588	2,386	2,377
Adjusted R ²	0.957	0.958	0.956	0.957	0.934	0.935	0.932	0.933

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

In sum, the results presented thus far are consistent with the theory of I have put forth in this paper. Specifically, vote-by-mail causes voters in Washington to obtain more information about politics than they would when voting at a traditional polling place. In response, elected officials behave more competently and shift public policy to be more reflective of the preference of their constituents, which results in lower levels of taxing and spending in Washington’s municipalities.

Vote-by-Mail, Municipal Institutions, and Electoral Competition

In this section, I examine if the relationship between vote-by-mail and municipal revenue and expenditures varies across municipalities with different institutional arrangements and levels of electoral competition. First, I examine if the expected municipal finance outcomes vary across cities with mayor-council governments as opposed to those with council-manager governments. Scholars generally argue that in cities with strong mayors that 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. Specifically, I estimate the following equation,

dependent variable the results are consistent with those presented in this section and the effect of vote-by-mail on total tax revenue becomes statistically significant when the time trend is included. Further, although in some specifications of the models with total revenue and total expenditures as the dependent variables the coefficient on the vote-by-mail variable is not statistically significant, it is always negative and substantively similar to the results presented in the body of the paper.

$$\begin{aligned}
Y_{m,t} = & \beta_1 VBM_{m,t} + \beta_2 MayorCouncil_m + \\
& \beta_3 VBM_{m,t} \times MayorCouncil_m + \\
& \delta \mathbf{X}_{m,t} + \tau_t + \epsilon_{m,t}
\end{aligned} \tag{4}$$

where $MayorCouncil_m$ is a dummy variable equal to 1 when 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, these estimates from these analyses do 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 (expenditures), 3 (revenue), 5 (total taxes), and 7 (property taxes) of Table 4. The results show that the coefficient on the vote-by-mail indicator is consistently positive but only statistically significant for the total taxes dependent variable, but 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. 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,

$$\begin{aligned}
Y_{m,t} = & \beta_1 VBM_{m,t} + \beta_2 Competitiveness_{m,t-1|t-2} + \\
& \beta_3 VBM_{m,t} \times Competitiveness_{m,t-1|t-2} + \\
& \delta \mathbf{X}_{m,t} + \lambda_m + \tau_t + \epsilon_{m,t}
\end{aligned} \tag{5}$$

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. The results, displayed

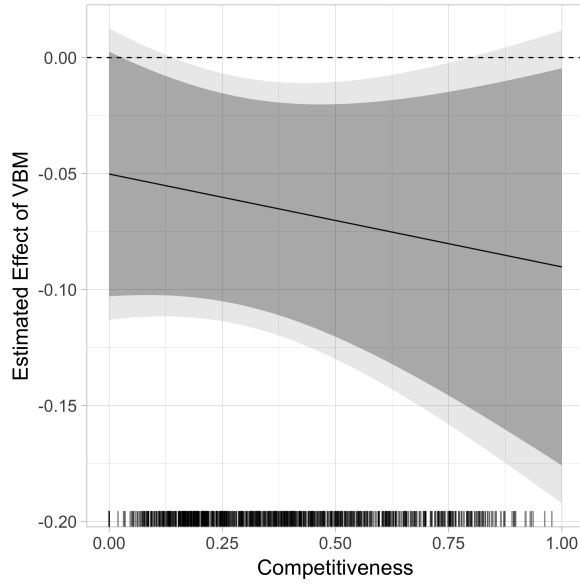
in Table 4, show that for the expenditures (Column 2), revenue (Column 4), and total taxes (Column 6) dependent variables that the interaction between the vote-by-mail dummy and the competitiveness variable is negative. This indicates that the impact of vote-by-mail becomes more negative as electoral competition increases. However, the interaction term in the model with property tax revenue as the dependent variable (Column 8) is positive, which is opposite of what is expected.

Table 4: The Effects of Vote-by-Mail on Municipal Revenue and Expenditures (Full Data)

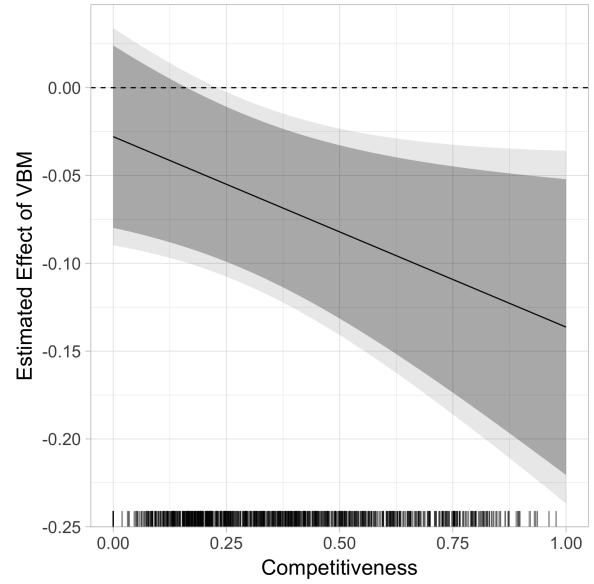
	<i>Dependent variable (log per capita):</i>							
	Expenditures		Revenue		Total Taxes		Property Taxes	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Vote-by-Mail	0.108 (0.087)	−0.050 (0.038)	0.119 (0.076)	−0.028 (0.040)	0.077** (0.039)	−0.001 (0.019)	0.015 (0.042)	−0.031*** (0.011)
Mayor-Council	0.131 (0.123)		0.132 (0.113)		0.023 (0.100)		0.115* (0.067)	
Vote-by-Mail*Mayor	−0.219*** (0.076)		−0.222*** (0.064)		−0.078* (0.047)		−0.084* (0.046)	
Competitiveness		−0.015 (0.048)		0.005 (0.052)		0.040** (0.018)		−0.025 (0.021)
Vote-by-Mail*Comp		−0.040 (0.068)		−0.108 (0.074)		−0.077*** (0.025)		0.035 (0.031)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipal FE	No	Yes	No	Yes	No	Yes	No	Yes
Observations	2,589	2,162	2,589	2,162	2,589	2,162	2,588	2,161
Adjusted R ²	0.191	0.729	0.179	0.730	0.436	0.964	0.461	0.945

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

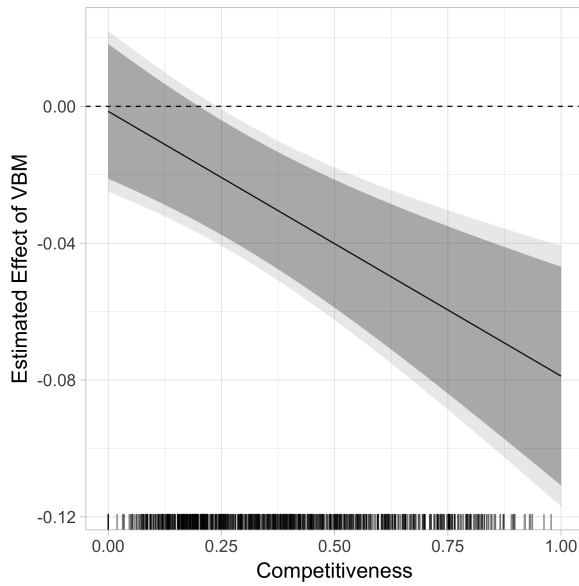
In order to aid in the interpretation of the interaction variables, Figure 4 display the marginal effect of vote-by-mail on the dependent variables of interest across different levels of competitiveness using the results from the models with the full sample of municipalities. The solid black line in the figures represents the estimated marginal effect of switching to vote-by-mail at a given value of competitiveness. 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 effect of vote-by-mail on total expenditures (Panel A), total revenue (Panel B), and total tax revenue (Panel C) becomes more negative. This relationship appears to be strongest for the total revenue and total tax



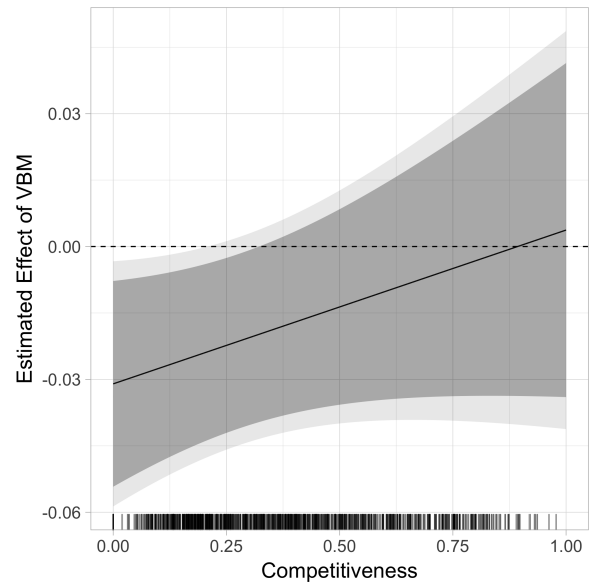
(a) Log Total Expenditures Per Capita



(b) Log Total Revenue Per Capita



(c) Log Total Tax Revenue Per Capita



(d) Log Property Tax Revenue Per Capita

Figure 4: 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.

dependent variables. Somewhat surprisingly Panel D of Figure 4 shows that the relationship between electoral competitiveness and revenue from 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 generally 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, the municipal fiscal policy outcomes, for the most part, are more responsive to the constituents' 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.

Alternative Explanation: Composition of the Electorate

I have argued in this paper that the public policy consequences of vote-by-mail are a result of the informational effects of the institution. However, a potential alternative explanation for these results is that vote-by-mail changes the *composition* of the electorate and not only the information environment of elections. Specifically, there are two potential effects that vote-by-mail may have on the composition of the electorate. First, vote-by-mail may *decrease* the socioeconomic bias in the electorate, because mailing a ballot to potential voters substantially decreases the costs of voting. This may encourage less affluent citizens to participate. Research shows that vote-by-mail does in fact increase political participation (e.g., Gerber, Huber and Hill 2013, Magleby 1987, Richey 2008)¹² and that this increase is especially apparent among infrequent voters (Gerber, Huber and Hill 2013). On the other hand, vote-by-mail could *increase* the socioeconomic bias in the electorate by increasing the political power of homeowners and those with more permanent living situations. Indeed, scholars have argued that many "convenience" electoral reforms in the United States may actually exacerbate socioeconomic bias in the electorate (Berinsky 2005), and because in vote-by-mail elections the ballot is mailed to individuals who are registered to vote where they live, the institution may especially advantage those with more permanent living situations who are more likely to be registered to vote (Ansolabehere et al. 2012). If this latter hypothesis were to be confirmed then it could possibly explain the effect of vote-by-mail on municipal finances that I have documented in this

¹²But, other scholars have found that vote-by-mail causes a decrease in participation in at least some types of elections (e.g., Bergman and Yates 2011, Kousser and Mullin 2007, Southwell and Burchett 2000)

paper. However, if it is not the case that vote-by-mail increases the socioeconomic bias in the electorate, then we can have more confidence that these findings are the result of voters gathering more information about their elected officials and not changes in the composition of the electorate.

Thus, in order to assess this alternative explanation, I examine the effect of vote-by-mail on the composition of the electorate in Washington’s municipal elections using data from the Catalist voter file. Catalist merges voter files with a variety of commercial and 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 that have into a variety of demographic characteristics. Particularly, I am able to examine the housing choices by measuring the $ProportionHomeowner_{m,t}$ and $ProportionRenter_{m,t}$ in Washington’s electorate. Further, I examine the effect of vote-by-mail on the distribution of family income in the electorate by creating the variables $ProportionIncome<\$40,000_{m,t}$ and $ProportionIncome>\$100,000_{m,t}$ as well as the effect of the institution on the distribution of ages in the electorate using the variables $ProportionUnder30_{m,t}$ and $ProportionOver65_{m,t}$. I examine the effects of vote-by-mail on the composition of the electorate by estimating models in the form of Equation 3.

Table 5 shows these results using the full sample of municipalities.¹³ First, the results indicate that switching to vote-by-mail does not have a statistically significant effect on the proportion of homeowners in the electorate (Column 1).¹⁴ Further, it also appears that vote-by-mail elections do not have an effect on the proportion of renters in the electorate (Column 2). But, it could be the case that the null results reported here are simply the a consequence of a lack of statistical power or measurement error in the dependent variable. Therefore, I present 90% confidence intervals with the coefficient estimates as Rainey (2014) argues that if the values within a 90% confidence interval are substantively small a researcher can argue for a negligible effect. The upper bound on the confidence interval for the proportion of homeowners dependent variable is 0.009, which is indeed substantively negligible. This indicates that, at best, vote-by-mail results in .9 % increase in the number of homeowners in the electorate in municipal elections in Washington. This is substantively very small and unlikely to induce the changes in revenue and expenditures reported in this paper. Therefore, it is evident that vote-by-mail does not result in an increase in the electoral power of homeowners, which in turn causes changes in municipal fiscal policy.

¹³The results with the restricted sample are consistent with those presented here and are displayed in Appendix D.

¹⁴In the Appendix I present results with an alternative coding of the homeownership variable. The Catalist data codes voters as being either “Homeowners” or “Likely Homeowners” and the results presented in Table 5 use the proportion of voters in either of those categories combined. In the Appendix I present results with the disaggregated version of this variable, which are consistent with those presented in Table 5.

Table 5: The Effects of Vote-by-Mail on the Composition of the Electorate (Full Data)

	<i>Dependent variable:</i>					
	Homeowner	Renter	< \$40,000	> \$100,000	Under 30	65 and Older
	(1)	(2)	(3)	(4)	(5)	(6)
Vote-by-Mail	−0.028 (−0.066, 0.009)	0.015 (−0.006, 0.035)	0.012* (0.001, 0.024)	−0.020 (−0.046, 0.005)	0.002*** (0.001, 0.003)	−0.018** (−0.030, −0.006)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Municipal FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,374	1,374	1,374	1,374	1,374	1,374
Adjusted R ²	0.970	0.773	0.946	0.979	0.713	0.775

Note: Results with full data. 90 % confidence intervals in parentheses. *p<0.1; **p<0.05; ***p<0.01.

In Table 5, I present additional evidence against the hypothesis that vote-by-mail increases socioeconomic bias in the electorate. Column 3 shows that vote-by-mail increases the proportion of individuals in the electorate with an annual household income less than \$40,000 and Column 4 indicates that there is no significant effect of vote-by-mail on the proportion of the electorate with a household income greater than \$100,000 per year. Further, Columns 5 and 6 indicate that switching to vote-by-mail causes an increase in the proportion of voters younger than 30 and a decrease in the proportion of voters older than 65. These results indicate that vote-by-mail increases the proportion of lower income and younger voters who participate and, thus, diminishes the socioeconomic bias of the electorate in Washington's municipal elections. These are the types of individuals that we would least expect to be vocal and organized proponents of decreases in taxing and spending.

In all, this section provides 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. Rather, it appears that vote-by-mail slightly increase the proportion of less affluent voters who participate in Washington's municipal elections. These results regarding the composition of the electorate, however, should be extrapolated to other contexts with caution. The sample analyzed here only includes the composition of the electorate in incorporated municipalities in a single state that hold their elections off-cycle in odd-numbered years. Off-cycle elections are low-turnout and low-salience events, so it is possible that this is the context where we would be most likely to find a compositional effect of vote-by-mail. This is especially the case because the estimated magnitude of the compositional changes are quite small.

Thus, future research should examine how the compositional effects of vote-by-mail vary across levels of salience and timing of elections.

Conclusion

In this paper, I examine the effects of vote-by-mail on the behavior of voters and elected officials. I first develop a theory of how vote-by-mail increases the likelihood that voters seek out relevant political information. Then, drawing on theoretical work on political accountability, I propose that in response to this change in voter behavior incumbents will work harder and engage in behavior more in line with the preferences of their constituents. I utilize a difference-in-differences design that exploits the staggered implementation of vote-by-mail in Washington to test this theory. Specifically, I trace out how the switch from traditional polling place elections to vote-by-mail affects the behavior of voters and elected officials as well as policy outcomes in Washington's municipalities. Consistent with the theory, I show that vote-by-mail leads to a decrease in ballot roll-off on statewide ballot measures in presidential election years. Further, I find that this electoral institution causes a decrease in the number of violations documented in audit reports of municipal governments as well as a decrease in taxation and spending. Finally, I present evidence that these policy implications of vote-by-mail cannot be explained by changes in the composition of the electorate. Taken together, these findings provide compelling evidence that vote-by-mail causes voters to gather more information about their elected officials, which induces elected officials to be more responsive to their constituents.

The results presented in this paper have a number of broad implications. First, this paper contributes to our understanding of how the increasingly popular electoral institution of vote-by-mail affects representation and electoral accountability. Since Oregon switched to vote-by-mail in 1998 there are now three states (Oregon, Washington, and Colorado) in the United States that hold exclusively vote-by-mail elections and some counties in California will begin to switch to all-mail elections in 2018. Further, at least one other state (Hawaii) has legislation pending that would switch the electoral system to vote-by-mail and there are also rules in states that require certain sub-state jurisdictions to hold their elections by mail or give them the option to do so if they so choose. Moreover, entire countries, including Switzerland, hold all-mail elections. Thus, it is clear that we should have solid understanding of how this institution shapes the incentives and behaviors of elected officials. There has been much debate about the effects of

vote-by-mail on voter turnout (e.g., Bergman and Yates 2011, Berinsky, Burns and Traugott 2001, Gerber, Huber and Hill 2013, Gronke and Miller 2012, Karp and Banducci 2000, Kousser and Mullin 2007, Magleby 1987, Richey 2008), but scholars have generally ignored other potential effects of the institution (Gronke et al. 2008). This paper, consequently, contributes to and broadens this literature by examining how vote-by-mail affects the information environment of elections and shapes the incentives of elected officials as well as policy outcomes.

Second, these results speak more broadly 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 (e.g. Schlozman, Verba and Brady 2012) and that public policy favors the affluent (e.g. Bartels 2016). Other work finds that institutions that provoke accountability among local officials actually further bias 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 greater accountability among elected officials, this results in policy outcomes - lower property taxes and lower levels of spending - that actually further bias policy outcomes in favor of high socioeconomic status individuals.

Finally, I contribute to other research that shows that choices about electoral rules - ranging from the timing of elections (Anzia 2011) to compulsory voting (Bechtel, Hangartner and Schmid 2016, Fowler 2013) - can have large implications for the behavior of politicians and policy outcomes. In all, this paper shows that if scholars wish to understand the nature of democratic governance and policymaking, an increased amount of attention should be paid to how electoral institutions, which condition the strategic interactions between voters and elected officials, affect how politicians behave while in office and, ultimately, public policy.

References

- Alt, James, Ethan Bueno De Mesquita and Shanna Rose. 2011. "Disentangling Accountability and Competence in Elections: Evidence From US Term Limits." *The Journal of Politics* 73(1):171–186.
- Angrist, Joshua David and Jörn-Steffen Pischke. 2009. *Mostly Harmless Econometrics: An Empiricist's Companion*. Princeton University Press.
- Ansolabehere, Stephen and Eitan Hersh. 2012. "Validation: What big data reveal about survey misreporting and the real electorate." *Political Analysis* p. mps023.
- Ansolabehere, Stephen, Eitan Hersh, Kenneth Shepsle et al. 2012. "Movers, Stayers, and Registration: Why Age is Correlated With Registration in the US." *Quarterly Journal of Political Science* 7(4):333–363.
- Anzia, Sarah F. 2011. "Election Timing and the Electoral Influence of Interest Groups." *The Journal of Politics* 73(02):412–427.
- Ashworth, Scott. 2012. "Electoral Accountability: Recent Theoretical and Empirical Work." *Annual Review of Political Science* 15:183–201.
- Bartels, Larry M. 2016. *Unequal Democracy: The Political Economy of the New Gilded Age*. Princeton University Press.
- Bechtel, Michael M, Dominik Hangartner and Lukas Schmid. 2016. "Does Compulsory Voting Increase Support for Leftist Policy?" *American Journal of Political Science* 60(3):752–767.
- Bergman, Elizabeth and Philip A. Yates. 2011. "Changing Election Methods: How Does Mandated Vote-By-Mail Affect Individual Registrants?" *Election Law Journal* 10(2):115–127.
- Berinsky, Adam J. 2005. "The Perverse Consequences of Electoral Reform in the United States." *American Politics Research* 33(4):471–491.
- Berinsky, Adam J., Nancy Burns and Michael W. Traugott. 2001. "Who Votes by Mail?: A Dynamic Model of the Individual-Level Consequences of Voting-By-Mail Systems." *Public Opinion Quarterly* 65(2):178–197.
- Berry, Christopher R. and Jacob E. Gersen. 2009. "Fiscal Consequences of Electoral Institutions." *The Journal of Law and Economics* 52(3):469–495.
- Berry, Christopher R. and Jacob E. Gersen. 2010. "The Timing of Elections." *The University of Chicago Law Review* pp. 37–64.
- Besley, Timothy. 2006. *Principled Agents?: The Political Economy of Good Government*. Oxford University Press.
- Besley, Timothy and Anne Case. 1995a. "Does electoral accountability affect economic policy choices? Evidence from gubernatorial term limits." *Quarterly Journal of Economics* 110(3):769–98.
- Besley, Timothy and Anne Case. 1995b. "Incumbent Behavior: Vote Seeking, Tax Setting and Yardstick Competition." *American Economic Review* 85(1):25–45.

- Bullock, Charles S. and Richard E. Dunn. 1996. "Election Roll-Off a Test of Three Explanations." *Urban Affairs Review* 32(1):71–86.
- Cameron, A Colin, Jonah B Gelbach and Douglas L Miller. 2008. "Bootstrap-based improvements for inference with clustered errors." *The Review of Economics and Statistics* 90(3):414–427.
- Einstein, Katherine Levine and Vladimir Kogan. 2016. "Pushing the City Limits: Policy Responsiveness in Municipal Government." *Urban Affairs Review* 52(1):3–32.
- Ferraz, Claudio and Frederico Finan. 2011. "Electoral Accountability and Corruption: Evidence From the Audits of Local Governments." *The American Economic Review* 101(4):1274–1311.
- Fowler, Anthony. 2013. "Electoral and Policy Consequences of Voter Turnout: Evidence From Compulsory Voting in Australia." *Quarterly Journal of Political Science* 8(2):159–182.
- Fujiwara, Thomas. 2015. "Voting Technology, Political Responsiveness, and Infant Health: Evidence from Brazil." *Econometrica* 83(2):423–464.
- Gerber, Alan S., Gregory A. Huber and Seth J. Hill. 2013. "Identifying the Effect of All-Mail Elections on Turnout: Staggered Reform in the Evergreen State." *Political Science Research and Methods* 1(01):91–116.
- Gronke, Paul, Eva Galanes-Rosenbaum, Peter A Miller and Daniel Toffey. 2008. "Convenience voting." *Annu. Rev. Polit. Sci.* 11:437–455.
- Gronke, Paul and Peter Miller. 2012. "Voting by Mail and Turnout in Oregon: Revisiting Southwell and Burchett." *American Politics Research* 40(6):976–997.
- Hajnal, Zoltan L and Paul G Lewis. 2003. "Municipal Institutions and Voter Turnout in Local Elections." *Urban Affairs Review* 38(5):645–668.
- Hall, Melinda Gann and Chris W. Bonneau. 2008. "Mobilizing Interest: The Effects of Money on Citizen Participation in State supreme Court Elections." *American Journal of Political Science* 52(3):457–470.
- Hall, Melinda Gann and Chris W. Bonneau. 2012. "Attack Advertising, the White Decision, and Voter Participation in State Supreme Court Elections." *Political Research Quarterly* 66(1):115–126.
- In Our View: A Good Way to Vote.* 2010. *The Columbian* October 22.
- Karp, Jeffrey A. and Susan A. Banducci. 2000. "Going Postal: How All-Mail Elections Influence Turnout." *Political Behavior* 22(3):223–239.
- Kogan, Vladimir, Stéphane Lavertu and Zachary Peskowitz. 2016. "Do School Report Cards Produce Accountability Through the Ballot Box?" *Journal of Policy Analysis and Management* 35(3):639–661.
- Kousser, Thad and Megan Mullin. 2007. "Does Voting by Mail Increase Participation? Using Matching to Analyze a Natural Experiment." *Political Analysis* 15(4):428–445.
- Magleby, David B. 1987. "Participation in Mail Ballot Elections." *The Western Political Quarterly* 40(1):79–91.
- Matsusaka, John G. 2004. *For the Many or the Few: How the Initiative Process Changes American Government.* University of Chicago Press, Chicago.

- Peltzman, Sam. 1992. "Voters as Fiscal Conservatives." *The Quarterly Journal of Economics* 107(2):327–361.
- Persson, Torsten and Guido Enrico Tabellini. 2000. *Political Economics: Explaining Economic Policy*. MIT Press.
- Rainey, Carlisle. 2014. "Arguing for a negligible effect." *American Journal of Political Science* 58(4):1083–1091.
- Reed, Sam. 2007. "Washington States Vote-by-Mail Experience 2007." *Office of the Secretary of State*.
- Richey, Sean. 2008. "Voting by Mail: Turnout and Institutional Reform in Oregon." *Social Science Quarterly* 89(4):902–915.
- Sances, Michael W. 2016. "The Distributional Impact of Greater Responsiveness: Evidence from New York Towns." *The Journal of Politics* 78(1):105–119.
- Schaffner, Brian F., Matthew Streb and Gerald Wright. 2001. "Teams Without Uniforms: The Nonpartisan Ballot in State and Local Elections." *Political Research Quarterly* 54(1):7–30.
- Schlozman, Kay Lehman, Sidney Verba and Henry E. Brady. 2012. *The Unheavenly Chorus: Unequal Political Voice and the Broken Promise of American Democracy*. Princeton University Press.
- Sharp, Elaine B. 1997. "A comparative anatomy of urban social conflict." *Political Research Quarterly* 50(2):261–280.
- Snyder, James M. and David Strömberg. 2010. "Press Coverage and Political Accountability." *Journal of Political Economy* 118(2):355–408.
- Southwell, Priscilla L. and Justin I. Burchett. 2000. "The effect of all-mail elections on voter turnout." *American Politics Research* 28(1):72–79.
- Tausanovitch, Chris and Christopher Warshaw. 2014. "Representation in Municipal Government." *American Political Science Review* 108(3):605–641.
- Trounstine, Jessica. 2010. "Representation and Accountability in Cities." *Annual Review of Political Science* 13:407–423.
- Wattenberg, Martin P, Ian McAllister and Anthony Salvanto. 2000. "How Voting is Like Taking an Sat Test An Analysis of American Voter Rolloff." *American Politics Research* 28(2):234–250.

A Summary Statistics

Summary statistics for all data used in the paper are in Table A.1 (ballot measures), Table A.2 (audit reports), Table A.3 (vote-by-mail indicator and competitiveness), Table A.4 (municipal revenue and expenditures and U.S. Census data), and Table A.5 (Catalist data).

Table A.1: Summary Statistics of Statewide Ballot Measure Data

Statistic: Ballot Roll-Off	N	Mean	St. Dev.	Min	Max
<i>Full Sample</i>					
All Elections	390	0.052	0.019	0.014	0.133
Even-Year Elections	195	0.055	0.017	0.028	0.115
Presidential Elections	78	0.059	0.018	0.032	0.115
<i>Restricted Sample</i>					
All Elections	340	0.051	0.019	0.014	0.133
Even-Year Elections	170	0.054	0.017	0.028	0.115
Presidential Elections	68	0.058	0.018	0.032	0.115

Table A.2: Summary Statistics of Audit Report Data

Statistic: Number of Findings	N	Mean	St. Dev.	Min	Max
Full Data	4,036	0.182	0.537	0	8
<i>2005 Sample</i>					
One-Year Bandwith	403	0.176	0.556	0	4
Two-Year Bandwith	582	0.172	0.529	0	4
Three-Year Bandwith	713	0.171	0.562	0	7
<i>2006 Sample</i>					
One-Year Bandwith	319	0.172	0.536	0	4
Two-Year Bandwith	587	0.169	0.582	0	7
Three-Year Bandwith	699	0.166	0.560	0	7

Table A.3: Summary Statistics of Vote-by-Mail Indicator and Electoral Competitiveness

Statistic	N	Mean	St. Dev.	Min	Max
<i>Full Sample</i>					
Vote-By-Mail	2,602	0.488	0.500	0	1
Competitiveness	2,225	0.293	0.260	0.000	0.978
<i>Restricted Sample</i>					
Vote-By-Mail	2,391	0.461	0.499	0	1
Competitiveness	2,004	0.298	0.259	0.000	0.933

Table A.4: Summary Statistics of Full Sample of Municipal Revenue, Expenditure, and Demographic Data

Statistic	N	Mean	St. Dev.	Min	Max
<i>Full Sample</i>					
Log Revenue Per Capita	2,598	7.431	0.606	4.834	10.583
Log Total Tax Revenue Per Capita	2,598	6.164	0.642	3.908	8.042
Log Property Tax Revenue Per Capita	2,597	5.140	0.591	3.134	6.989
Log Expenditures Per Capita	2,598	7.401	0.616	4.256	10.441
Percent African American	2,598	1.440	2.527	0.000	17.566
Percent 65 or Older	2,598	14.176	6.840	1.500	79.200
Percent Latino	2,598	11.857	18.241	0.000	98.536
Median Income	2,589	49,206.670	23,577.640	4,750.000	204,375.000
Percent High School Degree	2,598	83.609	12.597	20.011	100.000
Percent Urban	2,598	0.611	0.468	0.000	1.000
Total Population	2,598	13,864.590	43,458.670	24.000	595,240.000
<i>Restricted Sample</i>					
Log Revenue Per Capita	2,387	7.430	0.602	4.834	10.583
Log Total Tax Revenue Per Capita	2,387	6.181	0.628	3.908	7.952
Log Property Tax Revenue Per Capita	2,386	5.164	0.585	3.134	6.989
Log Expenditures Per Capita	2,387	7.400	0.614	4.256	10.441
Percent African American	2,387	1.534	2.611	0.000	17.566
Percent 65 or Older	2,387	13.822	6.669	1.500	79.200
Percent Latino	2,387	11.999	18.466	0.000	98.536
Median Income	2,378	50,679.110	23,955.590	4,750.000	204,375.000
Percent High School Degree	2,387	83.879	12.844	20.011	100.000
Percent Urban	2,387	0.642	0.458	0.000	1.000
Total Population	2,387	14,895.390	45,181.160	24.000	595,240.000

Table A.5: Summary Statistics of Catalist Data

Statistic	N	Mean	St. Dev.	Min	Max
<i>Full Sample</i>					
Proportion Homeowner	1,404	0.387	0.267	0.000	1.000
Proportion Likely Homeowner	1,404	0.144	0.103	0.000	0.750
Proportion Homeowner or Likely Homeowner	1,404	0.531	0.292	0.000	1.000
Proportion Renter	1,404	0.0004	0.001	0.000	0.019
Proportion Likely Renter	1,404	0.036	0.045	0.000	0.348
Proportion Renter or Likely Renter	1,404	0.036	0.045	0.000	0.348
Proportion Income < 40,000	1,404	0.335	0.182	0.000	1.000
Proportion Income > 100,000	1,404	0.167	0.210	0.000	1.000
Proportion Under 30	1,404	0.009	0.016	0.000	0.111
Proportion Over 65	1,404	0.485	0.107	0.000	1.000
<i>Restricted Sample</i>					
Proportion Homeowner	1,289	0.409	0.266	0.000	1.000
Proportion Likely Homeowner	1,289	0.146	0.103	0.000	0.750
Proportion Homeowner or Likely Homeowner	1,289	0.555	0.287	0.000	1.000
Proportion Renter	1,289	0.0004	0.001	0.000	0.019
Proportion Likely Renter	1,289	0.037	0.046	0.000	0.348
Proportion Renter or Likely Renter	1,289	0.037	0.046	0.000	0.348
Proportion Income < 40,000	1,289	0.329	0.184	0.000	1.000
Proportion Income > 100,000	1,289	0.178	0.216	0.000	1.000
Proportion Under 30	1,289	0.009	0.016	0.000	0.111
Proportion Over 65	1,289	0.481	0.107	0.000	1.000

B Robustness Checks: Audit Report Results

Placebo tests for the analyses examining the effects of vote-by-mail on the average number of findings are displayed in Figure B.1. The point estimates displayed in the figure are the effect of vote-by-mail in treated municipalities (calculated in the same way as Equation 2 in the body of the paper) in time period $t - 1$ (the year before switching) and $t - 2$ (two years 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. Due to data availability I am only able to analyze $t - 1$ for municipalities that switched to vote-by-mail in 2005.

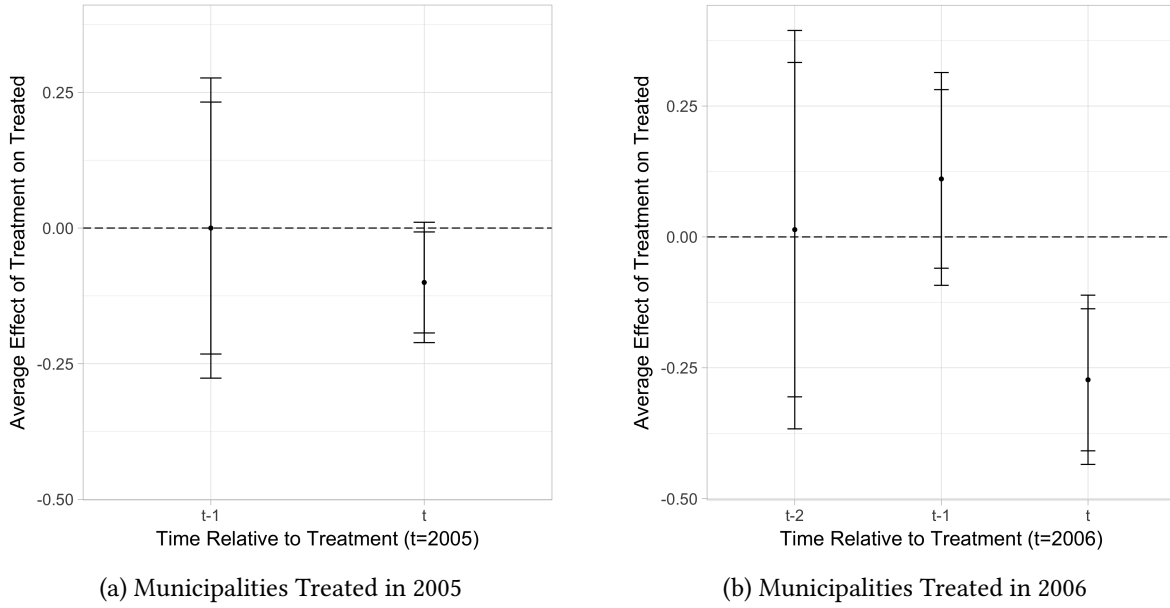


Figure B.1: Placebo test of the effect of vote-by-mail on average number of audit findings for municipalities treated in 2005 (Panel A) and 2006 (Panel B) with one-year bandwidth around treatment year. Point estimates as well as block-bootstrapped (county-level blocks) 90 % and 95 % confidence intervals are displayed. Estimates at time t are the same as in the body of the paper. Estimates at time $t - 1$ and $t - 2$ represent the effect of switching to vote-by-mail either one year or two years in the future.

C Robustness Checks: Municipal Fiscal Policy Results

C.1 Leads and Lags

I estimate a series of models 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,

$$Y_{m,t} = \sum_{\tau=0}^n \beta_{-\tau} VBM_{m,t-\tau} + \sum_{\tau=1}^q \beta_{\tau} VBM_{m,t+\tau} + \delta \mathbf{X}_{m,t} + \lambda_m + \tau_t + \epsilon_{m,t} \quad (6)$$

where the variable $VBM_{m,t}$ now only takes on a value of one in the year in which a municipality switched to vote-by-mail and, thus, $VBM_{m,t-1}$ is one only in the year before a municipality switched to vote-by-mail. As before, $Y_{m,t}$ are the municipal fiscal policy dependent variables, $\mathbf{X}_{m,t}$ is a vector of demographic controls, and λ_m and τ_t are municipal and year fixed effects. The sums in the empirical model allow for lead and lag variables. Specifically, I vary the value of n to allow for up to 3 lagged variables, or potential pretreatment effects, and the value of q to allow for a lead variable, or potential posttreatment effects. The expectation is that the lead variables should not be statistically significant, because if they were that would indicate pretreatment differences between treatment and control groups.

The results of these models for the log revenue per capita and log expenditures per capita are displayed in Table C.1 and Table C.2, respectively. All of the lead variables are statistically insignificant, which provides evidence that there are not pretreatment differences between the two groups and support that the parallel trends assumption holds. Further, the indicator for the year of the switch to vote-by-mail and the lagged variable are negatively across all specifications of the model and statistically significant in most.

Moving next to Table C.3, I run the leads and lags models with log total tax revenue per capita as the dependent variable. In the models that include one lead and one lag variable, the lead variable is statistically significant with both the full (Column 1) and restricted (Column 4) samples. This indicates that it is possible municipalities with lower levels of tax revenue selected into vote-by-mail before other municipalities. However, I argue that this is not concerning for a number of reasons. First, this is not a very robust finding and as additional lead variables are included, the effect disappears. Second, as the results in the body of the paper showed the effect of vote-by-mail had the least robust effect on total tax revenue.

Finally, the leads and lags models with the other dependent variables do not indicate that there are similar selection issues.

In Table C.4 I present that models with log property tax revenue per capita as the dependent variable. Across all of the specifications of the model there is no evidence that municipalities with lower property taxes switched to vote-by-mail earlier. In fact, if anything it appears that the coefficients on the lead variables are positive and in one specification (Column 2) marginally significant.

Table C.1: The Effects of Vote-by-Mail on Municipal Revenue

	<i>Dependent variable:</i>					
	Log Revenue Per Capita					
	Full Sample		Restricted Sample			
	(1)	(2)	(3)	(4)	(5)	(6)
VBM_{t-3}			-0.020 (0.048)			-0.037 (0.046)
VBM_{t-2}		-0.018 (0.030)	-0.028 (0.043)		-0.026 (0.028)	-0.046 (0.041)
VBM_{t-1}	-0.013 (0.032)	-0.018 (0.038)	-0.038 (0.049)	-0.008 (0.031)	-0.017 (0.037)	-0.050 (0.051)
VBM_t	-0.038 (0.027)	-0.035 (0.032)	-0.045 (0.040)	-0.052** (0.023)	-0.057* (0.031)	-0.084** (0.036)
VBM_{t+1}	-0.053* (0.031)	-0.048 (0.032)	-0.047 (0.033)	-0.079** (0.033)	-0.077** (0.036)	-0.089*** (0.035)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Municipal FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,314	2,041	1,769	2,126	1,876	1,627
Adjusted R ²	0.718	0.729	0.733	0.718	0.728	0.735

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

Table C.2: The Effects of Vote-by-Mail on Municipal Expenditures

	<i>Dependent variable:</i>					
	Log Expenditures Per Capita					
	Full Sample			Restricted Sample		
	(1)	(2)	(3)	(4)	(5)	(6)
VBM_{t-3}			-0.024 (0.050)			-0.047 (0.047)
VBM_{t-2}		-0.010 (0.036)	-0.024 (0.054)		-0.012 (0.034)	-0.040 (0.053)
VBM_{t-1}	-0.024 (0.029)	-0.031 (0.033)	-0.050 (0.046)	-0.022 (0.027)	-0.032 (0.031)	-0.066 (0.046)
VBM_t	-0.037 (0.024)	-0.047 (0.030)	-0.059 (0.042)	-0.046** (0.021)	-0.066** (0.027)	-0.102*** (0.039)
VBM_{t+1}	-0.061** (0.031)	-0.060* (0.032)	-0.066* (0.037)	-0.090*** (0.033)	-0.096*** (0.035)	-0.121*** (0.036)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Municipal FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,314	2,041	1,769	2,126	1,876	1,627
Adjusted R ²	0.709	0.714	0.719	0.709	0.712	0.720

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

Table C.3: The Effects of Vote-by-Mail on Total Municipal Tax Revenue

	<i>Dependent variable:</i>					
	Log Tax Revenue Per Capita					
	Full Sample			Restricted Sample		
	(1)	(2)	(3)	(4)	(5)	(6)
VBM_{t-3}			0.009 (0.011)			0.011 (0.010)
VBM_{t-2}		0.002 (0.015)	0.003 (0.018)		0.005 (0.010)	0.009 (0.013)
VBM_{t-1}	-0.022* (0.011)	-0.020 (0.015)	-0.018 (0.022)	-0.020** (0.008)	-0.015 (0.011)	-0.007 (0.015)
VBM_t	-0.035** (0.015)	-0.025 (0.018)	-0.028 (0.021)	-0.037** (0.016)	-0.021 (0.018)	-0.021 (0.017)
VBM_{t+1}	-0.021 (0.014)	-0.019 (0.013)	-0.017 (0.014)	-0.021 (0.017)	-0.017 (0.016)	-0.010 (0.016)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Municipal FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,314	2,041	1,769	2,126	1,876	1,627
Adjusted R ²	0.964	0.968	0.971	0.964	0.969	0.972

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

Table C.4: The Effects of Vote-by-Mail on Municipal Property Tax Revenue

	<i>Dependent variable:</i>					
	Log Property Tax Revenue Per Capita					
	Full Sample			Restricted Sample		
	(1)	(2)	(3)	(4)	(5)	(6)
$VB M_{t-3}$			0.016 (0.010)			0.011 (0.011)
$VB M_{t-2}$		0.023* (0.012)	0.021 (0.016)		0.013 (0.009)	0.007 (0.013)
$VB M_{t-1}$	0.010 (0.010)	0.014 (0.012)	0.003 (0.017)	0.006 (0.011)	0.007 (0.012)	-0.012 (0.017)
$VB M_t$	0.003 (0.010)	-0.001 (0.013)	-0.011 (0.018)	-0.002 (0.009)	-0.009 (0.012)	-0.029* (0.017)
$VB M_{t+1}$	-0.003 (0.010)	-0.003 (0.012)	-0.015 (0.016)	-0.003 (0.010)	-0.007 (0.012)	-0.028* (0.017)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Municipal FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,314	2,041	1,769	2,126	1,876	1,627
Adjusted R ²	0.941	0.946	0.954	0.939	0.945	0.954

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

C.2 Time Trends

I further probe the municipal fiscal policy results by running models with municipal specific time trends.

To do this I estimate models of the following form,

$$Y_{m,t} = \beta_1 VB M_{m,t} + \delta \mathbf{X}_{m,t} + \lambda_m + \lambda_m * t + \tau_t + \epsilon_{m,t} \quad (7)$$

which is the same as the baseline specification except for the fact that I now include a time trend, t , multiplied by municipality dummies, which allows each municipality to follow a different trend. The results for with the models in which log expenditures per capita and log total revenue per capita are the

dependent variables are displayed in Table C.5. The models with the tax revenue dependent variables are displayed in Table C.6.

Table C.5: The Effects of Vote-by-Mail on Municipal Revenue and Expenditures

	<i>Dependent variable:</i>							
	Log Revenue Per Capita				Log Expenditures Per Capita			
	Full Sample		Restricted Sample		Full Sample		Restricted Sample	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Vote-by-Mail	−0.039 (0.034)	−0.039 (0.034)	−0.051* (0.028)	−0.051* (0.028)	−0.032 (0.032)	−0.033 (0.032)	−0.042 (0.027)	−0.042 (0.027)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipal FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipal FExYr Trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,598	2,589	2,387	2,378	2,598	2,589	2,387	2,378
Adjusted R ²	0.744	0.747	0.747	0.749	0.739	0.742	0.742	0.745

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

The results of these models are largely consistent with those presented in the body of the paper and, notably, across all of the specifications the coefficient on the vote-by-mail dependent variables stays negative and substantively similar to the models without the time trends. That being said, in some of the models with log revenue per capita and log expenditures per capita the coefficient on the vote-by-mail dependent variable does not maintain standard levels of statistical significance.

Table C.6: The Effects of Vote-by-Mail on Municipal Tax Revenue

	<i>Dependent variable:</i>							
	Log Tax Revenue Per Capita				Log Property Tax Revenue Per Capita			
	Full Sample		Restricted Sample		Full Sample		Restricted Sample	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Vote-by-Mail	−0.034** (0.016)	−0.033** (0.015)	−0.043*** (0.013)	−0.041*** (0.013)	−0.030** (0.012)	−0.030** (0.012)	−0.028* (0.014)	−0.027* (0.015)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipal FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipal FExYr Trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,598	2,589	2,387	2,378	2,597	2,588	2,386	2,377
Adjusted R ²	0.975	0.975	0.975	0.975	0.958	0.959	0.957	0.957

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

D Robustness Checks: Composition of Electorate Results

D.1 Restricted Data

Table D.1 displays the estimated effect of vote-by-mail on the composition of the electorate in Washington’s municipal elections with the restricted data. The results are largely consistent with those displayed in the body of the paper.

Table D.1: The Effects of Vote-by-Mail on the Composition of the Electorate (Restricted Data)

	<i>Dependent variable:</i>					
	Homeowner	Renter	< 40,000	> 100,000	Under 30	65 and Older
	(1)	(2)	(3)	(4)	(5)	(6)
Vote-by-Mail	−0.031 (−0.073, 0.010)	0.016 (−0.007, 0.039)	0.009 (−0.004, 0.023)	−0.023 (−0.050, 0.005)	0.002* (0.0001, 0.003)	−0.016** (−0.029, −0.003)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Municipal FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,259	1,259	1,259	1,259	1,259	1,259
Adjusted R ²	0.968	0.770	0.956	0.979	0.721	0.778

Note: 90 % confidence intervals in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Table D.2 and Table D.3 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 the text of the paper. 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 D.2: The Effects of Vote-by-Mail on the Composition of the Electorate (Full Data)

	<i>Dependent variable:</i>			
	Homeowner (1)	Likely Homeowner (2)	Renter (3)	Likely Renter (4)
Vote-by-Mail	−0.032 (−0.079, 0.015)	0.004 (−0.007, 0.014)	−0.00005 (−0.0002, 0.0001)	0.015 (−0.006, 0.036)
Year FE	Yes	Yes	Yes	Yes
Municipal FE	Yes	Yes	Yes	Yes
Observations	1,374	1,374	1,374	1,374
Adjusted R ²	0.966	0.878	0.673	0.770

Note: 90 % confidence intervals in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Table D.3: The Effects of Vote-by-Mail on the Composition of the Electorate (Restricted Data)

	<i>Dependent variable:</i>			
	Homeowner (1)	Likely Homeowner (2)	Renter (3)	Likely Renter (4)
Vote-by-Mail	−0.038 (−0.088, 0.013)	0.006 (−0.003, 0.016)	−0.0001 (−0.0002, 0.0001)	0.016 (−0.007, 0.039)
Year FE	Yes	Yes	Yes	Yes
Municipal FE	Yes	Yes	Yes	Yes
Observations	1,259	1,259	1,259	1,259
R ²	0.972	0.902	0.743	0.815

Note: 90 % confidence intervals in parentheses. *p<0.1; **p<0.05; ***p<0.01.