Mail Voting Reduces Ballot Roll-Off: Evidence from Washington State

William Marble*

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Abstract

Throughout the United States, an increasing number of states are adopting laws that make it easier for voters to vote by mail (VBM). While research has addressed the potential effects of these changes on turnout, little attention has been paid to other aspects of voter behavior. In this paper, I argue that mail voting can decrease ballot roll-off — the tendency of voters to selectively abstain from voting in some races on the ballot. Roll-off is very common, with roll-off rates often exceeding 10%. Exploiting county-level variation in implementation of mandatory vote-by-mail laws in Washington, I show that VBM is associated with an aggregate decrease in roll-off in a variety of down-ballot races. The results persist even after several mail elections. These findings are consistent with absentee voting causing voters to take more time to educate themselves about their choices.

*Graduate Student in the Political Science Department, Stanford University. Email: wpmarble@stanford.edu. I thank Avi Acharya, Zac Endter, Daniel Gillion, Melissa Michelson, Michael Morse, Rogers Smith, and, especially, John Lapinski and Marc Meredith for helpful discussions and comments. I also thank the Program on Democracy, Citizenship, and Constitutionalism and the Program on Opinion Research and Election Studies, both at the University of Pennsylvania, for research support.

People like voting by mail. Most importantly, a voter spreads the ballot out across the kitchen table and really studies the issues and candidates.

— Sam Reed, former Washington Secretary of State

Observers of United States politics commonly lament low participation rates, with the focus usually being the low rates of voter turnout.[1] But there remains another ubiquitous and more puzzling form of nonparticipation: ballot roll-off, or the "tendency of the electorate to vote for 'prestige' offices but not for lower offices on the same ballot" (Burnham 1965, p. 9).

Roll-off has long been recognized as a widespread phenomenon in American elections. In the sample of statewide elections I present below, aggregate roll-off rates of greater than 10% are common for down-ballot statewide offices. Researchers have also documented that patterns of roll-off systematically differ across demographic groups (Herron & Sekhon 2005, Tomz & Van Houweling 2003, Vanderleeuw & Engstrom 1987). Given that much governing in the United States is done by lower-level state and local officials, roll-off has important consequences for the representativeness of the electorate.

Theoretically, roll-off is puzzling. A sizable literature on turnout emphasizes costs — both material and social — in the decision of whether to vote, and reforms often focus on reducing barriers to turnout.[2] But roll-off cannot be easily explained by this cost-benefit calculus. Why, after paying the fixed costs of voting, would citizens choose to selectively abstain in some races when the marginal cost of voting in additional races is vanishingly small?

[1] In the November 2016 election, 59% of the U.S. voting-eligible population cast a vote for the presidential election; participation by state ranged from 42% in Hawaii to 74% in Minnesota (United States Election Project 2017).

[2] See, for example, Wolfinger & Rosenstone (1980), Aldrich (1993), Riker & Ordeshook (1968), and Gerber, Green & Larimer (2008).

There are two leading explanations. The first emphasizes the lack of information that most voters have about down-ballot races. Feddersen & Pesendorfer (1996) present an influential model of voter abstention that they term the "swing voter's curse." In the model, if a subset of the election has even a small amount of uncertainty over their preferred choice, it can be welfare-enhancing for them to abstain — in effect, delegating the decision to better-informed voters. Because many voters often know little about state and local races, the theory predicts that they should abstain, and empirical evidence supports the claim that well-informed voters are less likely to roll-off (Wattenberg, McAllister & Salvanto 2000). The second explanation focuses on ballot design. Voters who are tasked with making many decisions on a given ballot may become fatigued or confused by the ballot, leading to lower rates of participation (Ansolabehere & Stewart 2005, Nichols & Strizek 1995).

Both explanations suggest that voting technology that gives voters more time to peruse the ballot could help to reduce roll-off. In this paper, I argue that vote-by-mail (VBM) elections — in which voters fill out their ballot at home and mail it back — do just that. VBM could ameliorate the effects of low voter information by giving people the opportunity to seek out information about the issues and candidates on the ballot before making their choice. In fact, advocates of VBM have argued that it leads to a better-informed electorate. For example, the former Washington Secretary of State Office claims that mail voting "increases turnout, simplifies the elections process, and promotes an informed citizenry" (Washington Secretary of State 2007). VBM also allows voters a longer time to examine the ballot before making their choices, potentially mitigating confusion and fatigue — which could result in higher levels of ballot completion.

I test this prediction using data from Washington State, which rolled out mandatory vote-by-mail elections on a county-by-county basis between 1996 and 2012. I exploit this

spatial and temporal variation to estimate the causal effect of mail voting on roll-off.[3] I find that when counties switch to all-mail elections, the roll-off rate significantly decreased. The county-level aggregate roll-off rate decreases by about 0.7 to 1.3 percentage points in races for lieutenant governor, secretary of state, and state auditor — corresponding to a roughly 15% decrease in roll-off compared to the baseline rate. Instrumental variables analysis suggests that the individual-level decrease in roll-off among former polling place voters is around 2.5 to 5 percentage points in those same races. Finally, I find evidence that the decrease in roll-off persists several elections beyond the introduction of mandatory mail voting.

The results highlight a heretofore unexamined participatory consequence of an increasingly common form of election administration (Gronke 2013). Colorado, Oregon, and Washington hold all their elections by mail, and California will hold all of its elections by mail starting in 2018 (National Conference of State Legislators 2017). An additional 19 states conduct some elections entirely by mail. Moreover, even in states that do not hold all-mail elections, many voters opt to vote by mail anyway.[4]

Substantively, my findings bolster the robust literature arguing that election administration can have an impact on voting behavior and suggest a mechanism for increased participation in state and local government. Moreover, they provide another example of the burgeoning literature showing that convenience voting reforms can impact elections (Meredith & Malhotra 2011).

[3]The Washington setting was also employed by Gerber, Huber & Hill (2013) to examine the effect of VBM on turnout.

[4]In 27 states and Washington, D.C., voters do not need an excuse to cast an absentee ballot (National Conference of State Legislatures 2017). In California, for example, 51% of ballots in 2012 and 60% of ballots in 2014 were cast by mail.

Voting Costs, Roll-Off, and Convenience Reforms

Traditionally, only voters who had a documented justification could cast an absentee ballot. However, no-excuse absentee voting has become increasingly common, with 27 states and the District of Columbia now allowing anyone to request an absentee ballot for any reason. Additionally, seven states and D.C. allow voters to register for permanent absentee status. As of 2014, an additional three states — Colorado, Oregon, and Washington — conduct their elections solely by mail, without the option of voting at a traditional polling place (National Conference of State Legislatures 2017). Permanent absentee voters and voters in all-mail jurisdictions are automatically sent a ballot for each election, which they can fill out at home at their convenience.

Turnout

Despite the growing importance of VBM, its consequences beyond turnout are relatively unexamined.[5]

In particular, the question of roll-off is relatively untouched in studies of mail voting. Why would a citizen who has already invested time in voting skip some races? Even if voting is a costly activity, by the time a voter starts filling out her ballot, she has already borne the majority of the cost. There are at least two plausible explanations: the first

[5]There is no consensus on the turnout effect of VBM. Estimates range from a significant increase (Gerber, Huber & Hill 2013, Southwell & Buchett 2000, Richey 2008), to a small or insignificant effect (Berinsky 2005, Berinsky, Burns & Traugott 2001), to a decrease in turnout (Kousser & Mullin 2007, Bergman, Yates & Ginnold 2009). Other research argues that the turnout increases attributable to convenience voting may be concentrated among already over-represented groups, cutting against the argument that lowering voting barriers improves representation (Berinsky 2005, Berinsky, Burns & Traugott 2001).

focusing on voter information and the second focusing on voting technology and ballot design.

A primary explanation is that voters are uncomfortable making decisions in elections that they know little about, so instead of choosing randomly, they selectively abstain. Theoretical work predicts that political information is a determinant of both turnout and roll-off. Feddersen & Pesendorfer (1996) demonstrate that poorly informed voters without strong preferences over their options are strictly better off abstaining than voting, and that this result can lead to high levels of abstention in the population. Wattenberg, McAllister & Salvanto (2000) provide some suggestive empirical evidence in favor of this theory. Using survey data, they argue that several measures of political information predict roll-off. The information theory is also consistent with the observation that roll-off rates are high in relatively obscure races, such as judicial retention elections (Hall & Aspin 1987), and lower in high-profile elections.

Another line of research has pointed to institutional features of voting that could affect roll-off. In general, voting technology can have a substantial impact on the translation of preferences to votes (Ansolabehere & Stewart 2005). The introduction of new voting technology has been associated with higher rates of invalid votes, suggesting that voters who are accustomed to a particular method of voting become confused with new procedures (Nichols & Strizek 1995, Asher, Shussler & Rosenfield 1982). Other researchers have argued that ballot layout can affect the roll-off level as well. In particular, ballots designed with party columns, as opposed to office blocks, encourage straight-ticket voting because it is easier for voters to locate candidates of their own party using this format (Walker 1966, Campbell et al. 1960). This may translate into lower roll-off because voters do not have to undertake the cost of voting in each race separately. Finally, a race's placement on the ballot may impact the roll-off rate. Voters can exhibit a high roll-off rate even in

a high-profile election if the race is placed low on the ballot, an effect that appears to disproportionately impacts black voters (Darcy & Schneider 1989).

Under either explanation — voter information and ballot design — mail voting could decrease roll-off. The information theory would predict that giving voters access to greater information while making their decisions would decrease roll-off. Advocates of mail voting have made the assertion that the extra time afforded to voters when they have an absentee ballot at home prompts them to learn about issues and candidates.[6] Mail voters can use the ballot as a prompt to seek out new information before making their decisions. In Washington, for instance, an Internet search for the candidates would likely turn up the online voter's guide, which has short biographies of each of the candidates.

On the other hand, polling-place voters have a significant time and resource constraint compared to votes filling out an absentee ballot at home. Once voters enter a voting booth, there is no opportunity to update their knowledge about candidates or issues.[7] There is ecological evidence that Google searches for election information are correlated with lower roll-off (Reilly, Richey & Taylor 2012), supporting the idea that voters who seek out information participate at a higher rate.

If the cause of roll-off is confusion about ballot design or fatigue from having to make too many decision on the same ballot, giving voters more time to examine and fill out the ballot may encourage them to vote in races that they otherwise would ignore. Even if the

[6]Sam Reed, the Secretary of State in Washington who oversaw much of the state's transition to vote by mail, has been quoted arguing this point. The quote at the beginning of this paper comes from a report released by his office that described the history of vote-by-mail in Washington up until 2007 and advocated for its continue expansion (Washington Secretary of State 2007).

[7]This argument uses the same logic that justifies mailing sample ballots to voters before Election Day. Not only does the sample ballot serve as a reminder to vote that increases turnout (Wolfinger, Highton & Mullin 2005), it also allows voters to preview the decisions they will have to make in the voting booth.

mental effort required to navigate the ballot does not change, voters can spread that effort out over a long period of time if they vote by mail.

In either case, this reasoning implies that voters who cast absentee ballots will have a lower roll-off rate than polling-place voters. But the extant literature is not conclusive on whether vote-by-mail has an effect on roll-off, and there are significant limitations to the studies examining this question.

Kousser & Mullin (2007) examine a policy in California that forced voters in very small precincts to vote by mail. Using matching, they estimate that in 2000, VBM precincts had a 1.0 to 1.5% lower proportion of votes in down-ballot propositions than similar in-person precincts — though they do not find a similar effect in 2002. This study's small scope — two election years and small precincts — may limit the generalizability of its findings.

A second study examines individual ballot images from Los Angeles County in the 1992 general election, and finds that roll-off in several ballot propositions was indistinguishable between in-person voters and voluntary absentee voters (Dubin & Kaslow 1996). But the observational nature of the study makes it difficult to identify causal effects. The decision to vote by absentee ballot in the election they examined was voluntary, and it is likely that people who opted to vote absentee differ systematically from polling-place voters in ways that are correlated with their down-ballot participation.

Hanmer & Traugott (2004) analyze Oregon's switch to vote-by-mail between the 1996 and 2000 presidential elections by examining ballots from the largest county in Oregon. The authors expected to observe higher roll-off in the 1996 election, in which vote-by-mail was voluntary, than in the 2000 elections, in which the entire election was conducted by mail. However, they find little evidence that vote-by-mail decreases the already-low level of roll-off in Oregon. This study rules out time-invariant effects that could affect roll-off, but the before-after design is sensitive to differences between the elections they examine.

In particular, the 2000 election was more competitive than the 1996 election, so it is likely that more low-information voters participated in 2000. These peripheral voters probably have lower levels of political knowledge, which means they are more likely to roll-off.

In sum, while there are theoretical reasons to expect that VBM would increase ballot completion, the extant evidence is limited in its ability to identify the causal effect of VBM and its generalizability.

Mail Voting in Washington State

Washington State presents a useful case for testing theories about the impact of voting by mail that overcome some limitations in prior research.

Washington has a long history of liberal use of mail and absentee voting. In 1974, the state legislature authorized no-excuse absentee voting — where anyone could choose to vote by mail, for any reason — and in 1993 Washington authorized permanent absentee voting. Vote-by-mail was voluntarily adopted by voters throughout the state rapidly: in 1980, just 12.5% of votes were cast by absentee ballot; by 2000, over half of ballots cast were absentee. Figure 1 shows the growth of absentee voting in Washington from 1980 through 2012.

Until recently, voters chose whether to vote by mail or to vote in person. Over the past two decades, though, Washington has converted to an all-mail election state, making it the second (after Oregon) to do away with in-person polling places. Unlike Oregon, which adopted all-mail elections all at once in 1998, Washington staggered its implementation of all-mail elections over several decades. In 1967, county-level election administrators were given the authority to hold all-mail elections in precincts with fewer than 100 voters, and the cutoff was later increased to 200 voters per precinct. By 2002, five rural counties

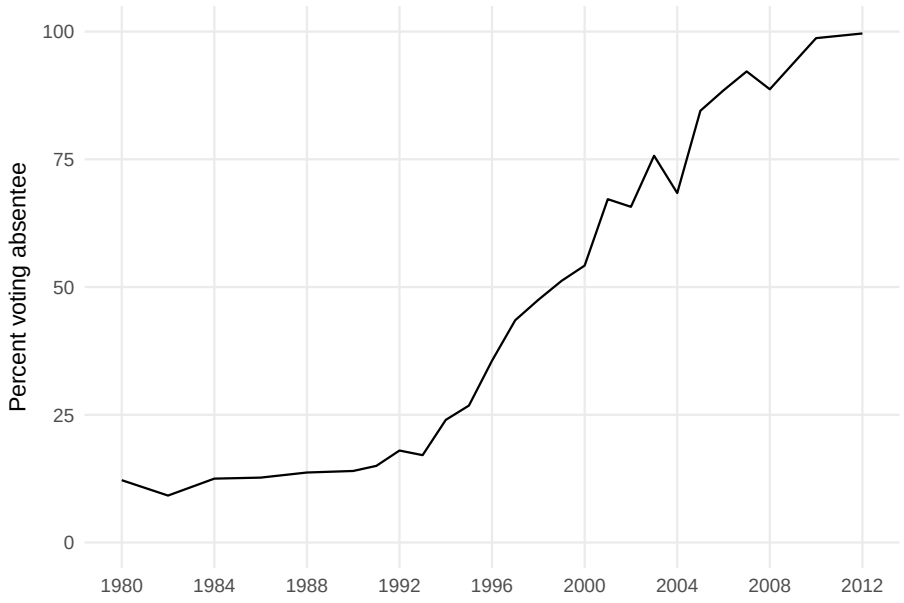

Figure 1: Percentage of votes cast by absentee ballot in Washington. Source: Washington Secretary of State Office.

were holding all-mail elections by drawing all of the precincts below the threshold. That number increased dramatically after 2005, when the legislature passed a law allowing county election administrators to adopt all-mail elections regardless of precinct size. In the 2006 midterm election, there were only five counties that did not hold their elections completely by mail, and by the 2012 presidential election all counties had switched to all-mail elections (Washington Secretary of State 2007, Gerber, Huber & Hill 2013).

Sample ballots for several counties and years are included in Appendix **??** as an example of what Washington voters experience when they vote by mail. Notably, Washington voters are presented with a large number of races — on the order of two to three dozen — on each ballot. This observation gives further credence to the idea that vote-by-mail may decrease roll-off, first because voters have a wide variety of races on which to educate themselves, and second because the text on the ballots is cramped, meaning there may be a high mental

cost associated with navigating the entire ballot. Voters filling out the ballot at home can take the time to figure the ballot out and seek out new information.

Data

I collected county-level vote return data in each even-year election from 1996 through 2012.[8]. I include in the sample races for president, governor, lieutenant governor, secretary of state, state auditor, commissioner of public lands, and the U.S. House of Representatives. With the exception of House races, these races are held on presidential election years and are statewide, meaning voters in each county face the same choices. I exclude statewide offices, such as attorney general, that went uncontested in some years.

The source data report the number of total ballots cast in each county as well as the total number of votes cast in each race on the ballot. This allows me to calculate the roll-off rate for each county-race pair. In particular, the dependent variable — roll-off in a given race-county pair — is the proportion of voters who cast a ballot in the election, but do not cast a valid vote that particular race. For example, if 100 voters cast a ballot, but only 90 valid votes are cast for secretary of state, there is a 10% roll-off rate for that race. In counties that contain multiple congressional districts, I sum the total number of votes for all congressional districts in that county.[9]

[8]All data were downloaded from the Washington Secretary of State's election results website (Washington Secretary of State 2014), except for 2012 results, which were downloaded from the Harvard Data Election Archive (Ansolabehere, Palmer & Lee 2015).

[9]In reality, this operationalization measures both undervotes, or roll-off, where voters do not vote for enough candidates in a given race, and overvotes, where they cast votes for too many candidates. In either case, the voter would be counted in the denominator but not in the numerator. However, nearly all of the residual vote measured in my data set is attributable to roll-off, not overvoting. In 2012, 25 Washington counties explicitly reported the number of undervotes and overvotes. Of nearly 1,000,000 total residual votes, only about 6,700 were overvotes. Further, the vast majority

Notably, because Washington reports the total number of ballots cast, I do not have to rely on the number of votes in the top-of-the-ticket race as a proxy for turnout. This feature of the data is significant for two reasons. First, some voters intentionally do not vote in top-of-the-ticket races (Knack & Kropf 2003). Second, it allows me to use president as a placebo test. There is already a relatively high level of knowledge about presidential candidates, so I do not expect VBM to affect roll-off in presidential races.

The data contain competitive elections in all 39 counties in Washington and a total of 9 election years for House races and 5 election years for all other races, with each observation representing a county-year.[10]

Using aggregate-level election return data makes it difficult to parse out competing explanations for any observed phenomenon. But in this context, there is little alternative. Survey data is likely to underestimate roll-off because voters may not accurately recall which races they voted in and because of social desirability bias (Holbrook & Krosnick 2010). Examining individual ballot images — as opposed to aggregate counts — could be preferable in theory, but it would be difficult to obtain and analyze a large number of ballot images over a significant time span. Moreover, conclusions based on ballot images could be threatened by selection effects if voters opt into absentee voting.

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of those overvotes — more than 5,000 — were cast in the presidential race, an office that is not expected to experience a significant roll-off rate. Thus, in the following analysis I refer to roll-off instead of residual votes in general.

[10]There are several gaps in the data. Observations for Douglas County in 2000 and 2004 were excluded from analysis due to apparent inaccuracies in the source data. Specifically, the reported total number of ballots cast was less than the total number of votes for several races. Therefore, either the total number of ballots cast is under-reported or the number of votes for those offices is over-reported, but there is no way to verify which set of numbers is accurate. An additional two observations, representing King County (Seattle) in 1998 and 2000, are excluded from analyses involving U.S. House races because there was an uncompetitive race in those years. In all, there are 347 observations for House races and 193 observations for presidential-year races.

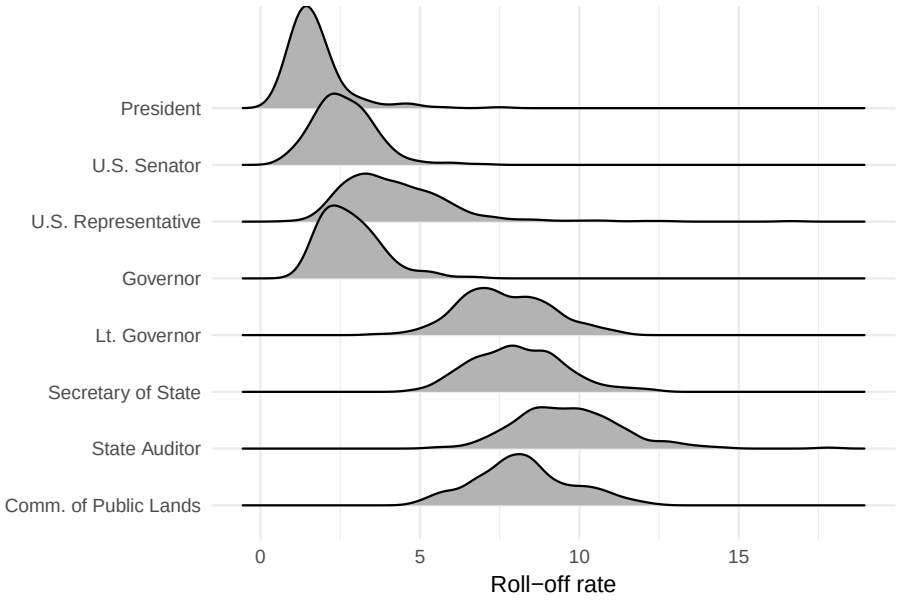

Figure 2: Roll-off rate in each type of race from 1996-2012 at the county-year level.

Figure 2 shows the roll-off pattern in each type of race across all observations in the dataset. As expected, the roll-off rate tends to be lower in races for president, senate, U.S. House, and governor, and greater in lower-salience elections such as secretary of state. For example, the average roll-off rate in gubernatorial races is 2.5%, while it is 9.7% in state auditor races.

Aggregate Effects of Mandatory Vote-by-Mail

The first set of analyses estimate the aggregate effect of mandatory VBM on roll-off. In particular, I seek to estimate the average treatment effect on the treated counties — that is, the decrease in roll-off at the county level that is caused by the policy change.

Identification Strategy

The estimation strategy is a generalized difference-in-differences model that exploits the spatial and temporal variation in the implementation of mandatory VBM across counties. Let Y_{it} be roll-off in county i and year t, and let VBM_{it} be an indicator for whether the county held a mandatory VBM election. I estimate models of the form

$$Y_{it} = \tau VBM_{it} + c_i + \alpha_t + \epsilon_{it} \tag{1}$$

for each office in the dataset. In this specification, c_i is a county fixed effect and α_t is a year fixed effect. The county fixed effect captures time-invariant characteristics of the county that affect roll-off, while the year fixed effect captures year-specific shocks to roll-off that affect all counties equally. The coefficient of interest τ represents the average treatment effect on the treated counties — that is, the average change in roll-off rates that is caused by holding a mandatory VBM election. The ATT is identified in Equation 1 as long as the idiosyncratic error term ϵ_{it} is uncorrelated with VBM_{it} conditional on the fixed effects.

I note briefly that I do not include a control variable for turnout in the main specification, even though turnout may be related to roll-off. In particular, high turnout elections tend to have more low-information voters, which could create a positive correlation between turnout and roll-off. I exclude turnout because it is a post-treatment variable: turnout is affected by whether or not a county holds the election by mail (Gerber, Huber & Hill 2013), so controlling for it could introduce bias into the estimation of τ (Rosenbaum 1984).

A second specification investigates the persistence of the effects. It could be the case, for example, that a switch to vote-by-mail is accompanied by an ambitious voter education project that decreases roll-off, but this effect decays as voting by mail becomes common-

place and the voter education project declines. Without accounting for this "novelty effect" (Gronke & Miller 2012), the impact of vote-by-mail would be biased upward. To account for this possibility, I break up the vote-by-mail variable into three indicator variables: one for the first vote-by-mail election in that county; one for the second vote-by-mail election in that county; and one for the third or greater vote-by-mail election in that county. If there is a decaying effect of vote-by-mail, the magnitude of the coefficient on the variable representing the first vote-by-mail election would be greater than the magnitude of the coefficients on the indicators for the second and third vote-by-mail election. I conduct joint F-tests on the vote-by-mail coefficients to analyze whether the effect changes in subsequent elections after the introduction of vote-by-mail. These models also include county and year fixed effects.

I estimate these models using weighted least squares, where the weights reflect the fact that some counties are more populous than others. This weighting scheme allows counties with more data (and thus lower variance) to have a larger impact than counties with less data.[11] To account for correlations in the error terms, I report robust standard errors clustered at the county level.

Results

The results of the main difference-in-differences specification are given in Table 1, and the treatment effect estimates are plotted in Figure 3. As expected, the coefficient on the vote-by-mail dummy variable (i.e., the ATT) is negative for most races, particularly the races

[11] In particular, let county-specific weight $w_i = \frac{1}{T}\sum_1^T TotalVotes_{it}$ and suppose the (unweighted) model to be estimated is given by $Y_{it} = \beta X_{it} + \epsilon_{it}$. The weighted model is then given by $\sqrt{w_i}Y_{it} = \beta\sqrt{w_i}X_{it} + \sqrt{w_i}\epsilon_{it}$. (For presidential-year races, $T = 5$; for U.S. House races, $T = 9$.) This weighting scheme is appropriate when data contain averages, as is the case with my data (Gould 1999). I implement the weights using the `aweight` specification in Stata.

Office	Pres.	Gov.	Lt. Gov.	Sec. State	Auditor	CPL	US Rep.
All-Mail Election	0.110	-0.310^{*}	-0.784^{***}	-1.320^{***}	-1.238^{***}	-0.804	1.222^{**}
	(0.217)	(0.169)	(0.135)	(0.216)	(0.239)	(0.503)	(0.542)
Observations	193	193	193	193	193	193	347
R-squared	0.399	0.605	0.424	0.627	0.315	0.488	0.344
Counties	39	39	39	39	39	39	39
Years	5	5	5	5	5	5	9
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 1: Dependent variable is roll-off rate, expressed as a percentage. Results from fixed effects regressions, estimated by weighted least squares. Robust standard errors, clustered by county, reported in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

that have generally high levels of roll-off. In state-level races, the magnitude of the effect is smallest in gubernatorial elections, where roll-off decreases by about 0.3 percentage points on average (though this estimate is only marginally significant), and greatest in elections for secretary of state, where roll-off decreases by about 1.3 percentage points. There is also a negative and statistically significant coefficient in the models for lieutenant governor and state auditor.

While the effects are not large in absolute terms, they are substantial relative to the baseline. For example, a decrease in roll-off of 1.3 percentage points for secretary of state corresponds to a 15% reduction of roll-off relative to the baseline, and an additional 30,000 extra votes in the average election.[12]

In contrast, there does not appear to be any effect of VBM on the roll-off rate in presidential elections. This finding is consistent with the hypothesis that roll-off is more prevalent to occur in low-salience elections that voters typically know little about.

The outlier in this pattern of results is the U.S. House, for which vote-by-mail appears to actually increase roll-off. To examine whether this finding is driven by off-year elections

[12]The average roll-off rate in races for secretary of state in counties without mandatory VBM is 8.3% and the average number of votes in each presidential-year election is 2,481,760.

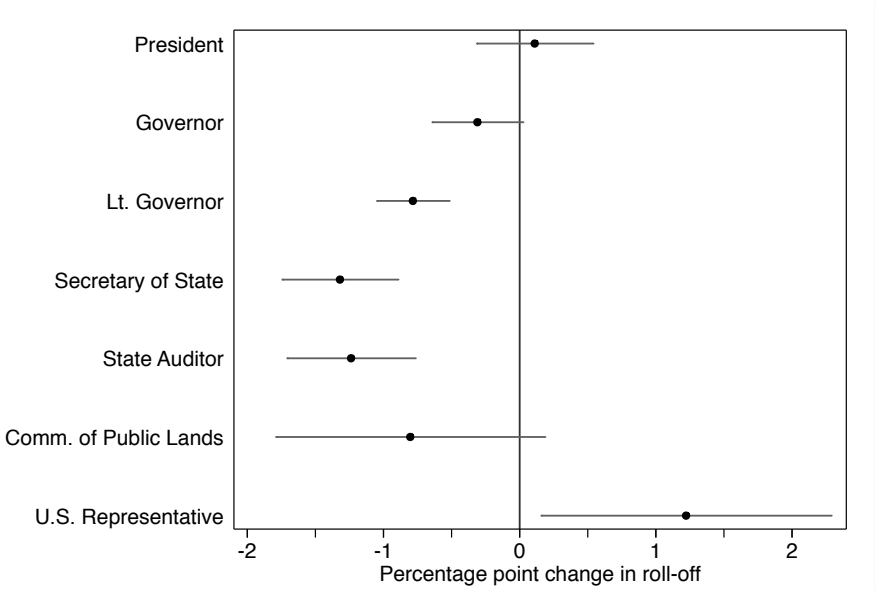

Figure 3: The estimated ATT and 95% confidence intervals of mandatory VBM on roll-off. Full results are reported in Table 1.

that do not include the other races, I omitted off-year elections and re-estimated the model, including only presidential-year elections. Dropping off-year elections introduces greater uncertainty such that the coefficient is not statistically significant at conventional levels, but the sign is still positive ($\beta = 0.494$, S.E. $= 0.315$). This result runs counter to expectations, but House races likely have a weak link to the theory motivating the analysis. House elections are not as prominent as presidential, gubernatorial, and senate elections, but they are still federal elections and demand more attention than elections for other statewide offices.

Next, Table 2 shows the results of the regressions testing the persistence of the effects I find. If the effect is short-lived — perhaps due to increased educational campaigns that accompanied the introduction of mandatory VBM — the magnitude of the effect of all-mail elections should decrease in subsequent elections. But the results run counter to this

Office	Pres.	Gov.	Lt. Gov.	Sec. State	Auditor	CPL	US Rep.
1st VBM elec.	-0.332^{**}	-0.356^{*}	-0.769^{***}	-1.457^{***}	-1.428^{***}	-1.166^{***}	1.834^{***}
	(0.149)	(0.206)	(0.124)	(0.196)	(0.331)	(0.402)	(0.494)
2nd VBM elec.	-2.169^{***}	-0.435	-0.495	-1.727^{***}	-1.908^{*}	-2.424^{***}	-0.834^{*}
	(0.499)	(0.383)	(0.584)	(0.480)	(1.077)	(0.407)	(0.447)
3rd+ VBM elec.	-2.938^{***}	-1.263^{**}	-1.904^{**}	-3.983^{***}	-4.343^{***}	-4.733^{***}	-2.309^{***}
	(0.677)	(0.556)	(0.809)	(0.609)	(1.452)	(0.541)	(0.840)
Observations	193	193	193	193	193	193	347
R-squared	0.603	0.613	0.438	0.646	0.340	0.567	0.563
Counties	39	39	39	39	39	39	39
Years	5	5	5	5	5	5	9
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
F-test p-val.	0.002	0.021	0.001	<.001	0.002	<.001	0.001

Table 2: Dependent variable is roll-off rate, expressed as a percentage. Results from fixed effects regressions, estimated by weighted least squares. Robust standard errors, clustered by county, reported in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

hypothesis: roll-off actually further decreases in subsequent elections after the introduction of mandatory vote-by-mail. The last row of Table 2 shows the results of an F-test that the effect of the first all-mail election is equal to the effect of the second and the effect of the third or more (relative to elections without mandatory VBM). These tests show that for all race types, the difference in the effect from the first all-mail election and the second, third or more all-mail elections is statistically significant. This indicates that as the novelty of all-mail elections wears off, the roll-off rate actually continues to decrease. Even in House races, the effect of all-mail elections on the roll-off rate is negative after the first all-mail election. This finding is consistent with previous research showing that roll-off is lower if voters are more accustomed to the method of voting they are using (Nichols & Strizek 1995, Asher, Shussler & Rosenfield 1982).

Overall, the evidence in this section suggests that the introduction of mandatory VBM in Washington had the effect of decreasing aggregate roll-off at the county level in several down-ballot statewide races. The effect seems to persist several years after the introduction of VBM. The main exception to this pattern of findings is in races for the U.S. House of

Representatives, in which VBM apparently *increased* roll-off. However, this effect diminished after several elections.

In the Appendix, I replicate these results using a randomization-based approach to inference based on Bertrand, Duflo & Mullainathan (2004). The results continue to be significant with p-values generated in that approach.

Individual-Level Effects

The fixed-effects models presented in the last section estimate the aggregate effect of the policy shift on roll-off. However, the policy change most directly affects voters who previously voted in person, which leads to a natural individual-level question: what is the individual-level effect of voting by mail on roll-off? The aggregate data cannot answer this question directly.

Theoretically, there is little reason to believe that the implementation of mandatory VBM should affect the behavior of voters who already voted by mail prior to the policy change. As Figure 1 shows, many voters in Washington voted by absentee ballot even before it was required. In counties that had high levels of absentee voting prior to the switch to mandatory VBM, we therefore should not expect to see a decrease in aggregate roll-off.

To better account for the individual-level effect of mail voting, I estimate a third set of models that use the existence of a mandatory VBM policy as an instrument for the county-level absentee voting rate. This model exploits variation in the absentee voting rate that is attributable to the implementation of mandatory VBM.[13] Spenkuch (2017)

[13]This estimation strategy is similar to estimating the effect of a treatment with one-sided noncompliance, where everyone assigned to the treatment group (i.e., mandatory VBM) receive the

Office	Pres.	Gov.	Lt. Gov.	Sec. State	Auditor	CPL	US Rep.
% absentee	0.384	−1.080*	−2.728***	−4.591***	−4.307***	−2.796	4.245**
	(0.745)	(0.572)	(0.506)	(0.694)	(0.920)	(1.781)	(1.922)
Observations	193	193	193	193	193	193	347
R-squared	0.396	0.606	0.455	0.648	0.344	0.515	0.302
Counties	39	39	39	39	39	39	39
Years	5	5	5	5	5	5	5
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
KP statistic	204.7	204.7	204.7	204.7	204.7	204.7	247

Table 3: The second-stage results of a fixed-effects instrumental variable regression, using the vote-by-mail indicator variable as an instrument for absentee voting. Absentee voting is measured 0 to 1. Thus, the coefficient represents the percentage-point change in roll-off for a county with no absentee voting switching to 100% absentee voting. I report Kleiberg-Paap weak identification statistic; the first-stage regression is identical for all models but U.S. Representative, which also includes off-year elections. Regressions weighted as described in the main text.

proves that the standard IV assumptions — namely, a conditional exogeneity assumption and an exclusion restriction — are sufficient to identify the individual-level treatment effect in ecological data.

In this case, the exogeneity assumption is likely to hold: after accounting for county and year fixed effects, the timing of mandatory VBM is unlikely to be related to the potential outcomes of roll-off. The primary justifications focus on convenience and turnout. The exclusion restriction may be harder to satisfy. As noted above, it's possible that a change in voter administration would be accompanied by higher general awareness of elections. Another violation would occur if VBM changes the composition of the electorate in a way that may be correlated with roll-off rates.

With these caveats in mind, Table 3 presents the results of the instrumental variables regressions. The directions of the coefficients are similar to the fixed effects strategy, and the magnitudes generally increase as expected.

treatment (vote by mail), while some people assigned to the control group (optional VBM) also receive the treatment. For further discussion, see Imbens & Rubin (2015, ch. 23).

The estimates suggest that absentee voting causes a 2.7 percentage point decrease in roll-off in the lieutenant governor's race and around 4.5 percentage points for secretary of state and state auditor. Again, the point estimate for U.S. House is positive and quite large at 4.2 percentage points.

Discussion and Conclusion

Overall, my findings suggest that beyond simply affecting turnout, vote-by-mail can induce citizens to vote in down-ballot races that they might ignore with traditional polling-place voting. The magnitude of this decrease in roll-off is politically meaningful — it translates into tens of thousands of votes in a typical statewide election. These results have important implications for representation in state government: Roll-off is more common among already-underrepresented groups, so finding ways to decrease roll-off plays an important part in increasing representation.

The results I present are consistent with the theoretical prediction that more knowledgeable voters are less likely to roll off.[14] They suggest, however, that institutional choices may have the potential to overcome the presumed negative effects of low voter knowledge.

The results in this paper also have implications for the normative literature on convenience voting. Critics of convenience voting argue that it may erode civic engagement (Thompson 2004, Thompson 2008). According to the argument, when a voter receives her ballot in the mail at home, fills it out at home, and returns it in solitude, she forgoes the civic experience of coming together in public with her fellow citizens to elect the leaders of their society. Indeed, Funk (2010) argues that the changes in turnout following the in-

[14] I have not come up with a more direct test of the mechanism here. Google Trends data would be the most straightforward, but data is not available at small enough geographic units.

troduction of mail voting in Switzerland are consistent with VBM undermining the social pressure to vote. However, if mail voting can promote participation beyond its effects on turnout, it might help to mitigate this critique.

In this paper, I show that mail voting decreases ballot roll-off in statewide races. When counties instituted mandatory vote by mail elections, aggregate roll-off tends to decrease by roughly 0.7 to 1.3 percentage points across several statewide races. These effects persist at least several elections after the introduction of VBM. An instrumental variables approach suggests that mail voting is associated with a larger individual-level effect on roll-off of around 2 to 4 percentage points.

I suggest that this effect is due in part to mail voting overcoming the swing voter's curse: when people have an opportunity to educate themselves about the races on the ballot, they are more likely to vote. However, further research is needed to demonstrate this mechanism.

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Appendices

Placebo Laws

Errors in diff-in-diff models tend to be spatially and serially correlated, leading to anti-conservative standard error estimates (Bertrand, Duflo & Mullainathan 2004). To probe the robustness of my results, I performed a simulation-based placebo test to generate a null distribution of the treatment effect estimates. The idea is to randomly generate the year each county switches to all-mail elections, estimate the model, and then repeat. Because the treatment date is randomly assigned for each county, we can be confident that it is uncorrelated with the regression error for the year in which it was assigned, as well as the regression errors in other counties. The empirical distribution of point estimates is an approximation of the sampling distribution under the null. The procedure I employ is as follows:

1. For each county, randomly generate a placebo "VBM adoption year" between 1996 and 2012, inclusive (the years in my sample).

2. Set $\text{VBM}_{it} = 1$ if Year_{it} is after the placebo VBM adoption year.

3. Using the placebo data, estimate the main equation given in Section **??** and store the coefficient on VBM_{it} (i.e., the average treatment effect on the treated).

4. Repeat steps 1-3 a suitably large number of times (I performed 10,000 simulations).

5. Derive an empirical sampling distribution of the ATT under the null using a density estimator on the stored coefficients.

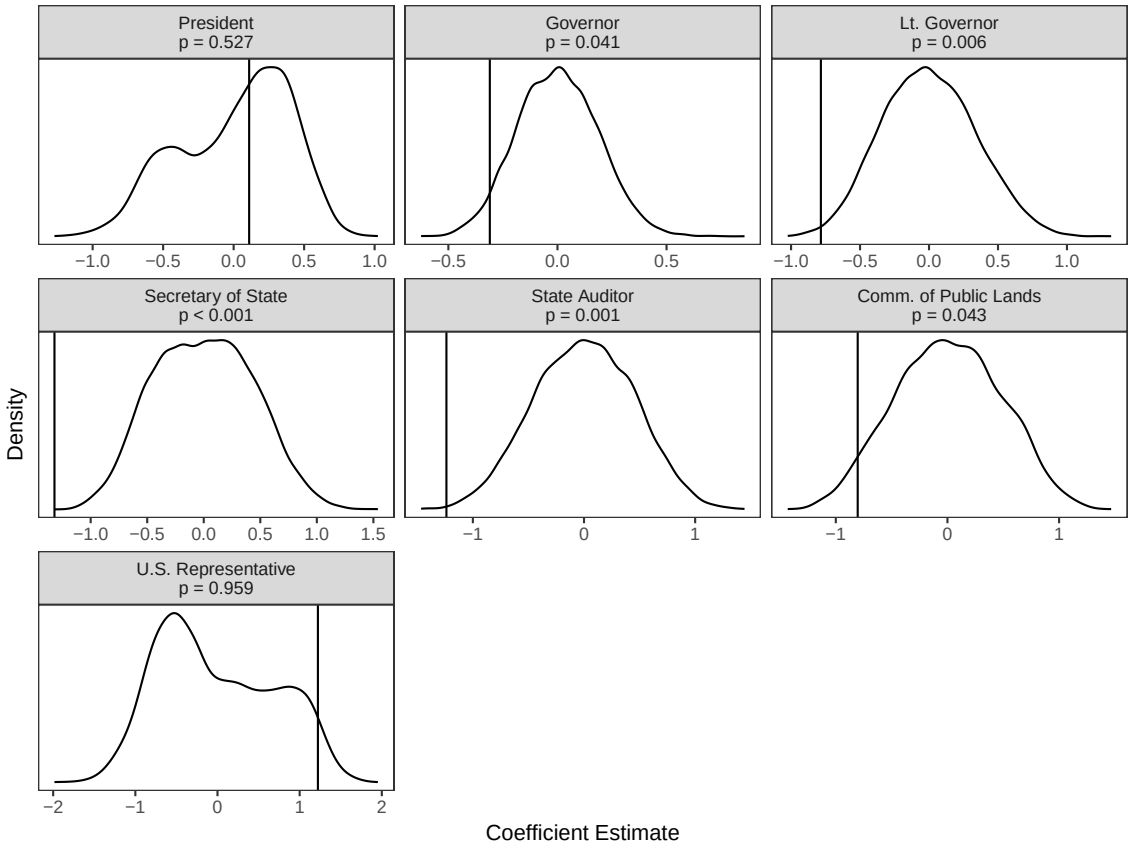

Figure 4: Kernel density plots of 10,000 draws from the simulated null distribution of difference-in-difference estimates based on placebo laws. The vertical line shows the actual estimate from Table 1 in the main text. One-sided p-values are reported.

Because VBM_{it} is randomly assigned, the estimated ATT should have mean 0 by construction. I then compare the estimate derived from the actual data to the placebo estimates to compute a p-value, where $p = \frac{1}{M}\sum_{i=1}^{M} I(\tilde{\tau}_i \leq \hat{\tau})$, where M is the number of simulations, $\tilde{\tau}_i$ is the estimated ATT from the ith simulation, $\hat{\tau}$ is the actual estimate using the real data, and $I(\cdot)$ is the indicator function.

Figure 4 plots the distribution of the estimated average treatment effect on the treated (ATT) using the primary specification reported in the text, along with the actual results

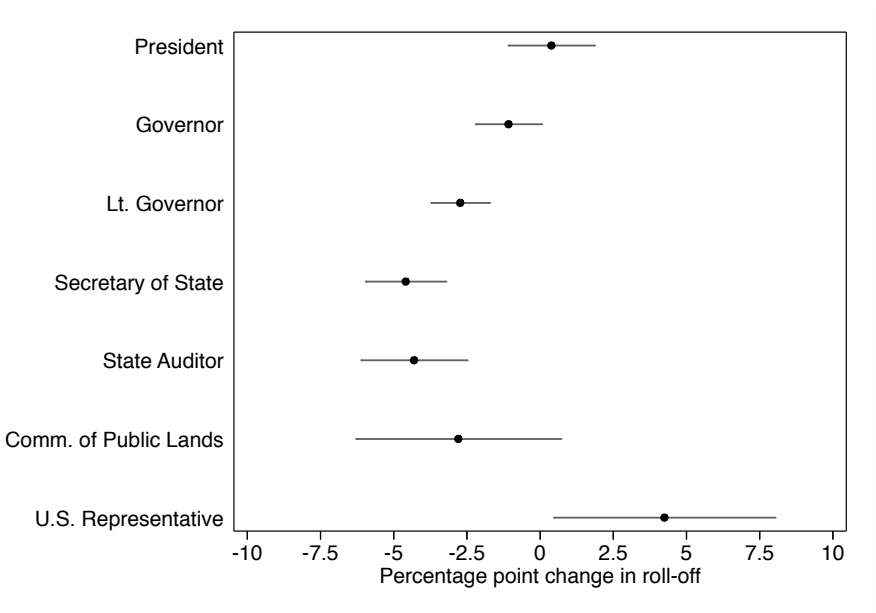

Figure 5: Point estimates and 95% confidence intervals for the instrumental variables estimates of the effect of absentee voting on roll-off, with all-mail voting as the excluded instrument.

from Table 1.

Cross-Sectional Regressions

The main text used fixed-effects models to investigate the impact of all-mail voting on roll-off. This method is preferable for analyzing panel data because it leverages within-unit variation to rule out some types of spurious correlations. Even though some demographic characteristics are known to be associated with roll-off, they are not suitable for inclusion in the fixed effect model because demographics are stable from year to year.

As a robustness test, I estimated cross-section regressions that include county-level demographic covariates and omit county fixed effects. In contrast to the fixed effects models, cross-section regression exploits variation across counties. The models I estimate are given by:

$$\text{Y}_i = \alpha + \delta_1 \text{ VBM}_i + \beta_1 \text{Turnout}_i + \theta \text{Covariates}_i + \gamma \text{Year}_i + \epsilon_i \quad (2)$$

$$\text{Y}_i = \alpha + \delta_1 \text{ VBM}_i + \delta_2 \text{VBM x Prior Abs}_i + \beta_1 \text{Turnout}_i + \theta \text{Covariates}_i + \gamma_t + \epsilon_{it} \quad (3)$$

In these models, θ represents a vector of coefficients corresponding to the demographic covariates. The demographic variables include percentage of the voting age population (VAP) that is white, percentage of VAP that is black, percentage of VAP that is Hispanic, and percentage of VAP that is 65 or old. I obtained these estimates from the Census Bureau.[15]

If the effect I report in the main text is robust to demographic covariates, the coefficients on the all-mail election indicator should have the same sign as the fixed-effects models

[15] Available online at `http://www.census.gov/popest/data/historical/`. Last accessed on July 11, 2014. I employ a noisy measure of voting age population; age categories available in Census-produced estimates include 15 to 19 years old and 20 to 24 years old. Thus, 18- and 19-year olds are excluded from counts of VAP.

reported in Tables 1 and **??**.

Table 4 shows the results of the cross-sectional models. Indeed, the coefficients run in the same direction, and on inspection, have relatively similar magnitudes to their fixed-effects counterparts.

A note of caution in directly interpreting the coefficients on the demographic variables. First, a full battery of controls, such as urban/rural, education levels, and mobility, are not included. Because these and other factors may be correlated with the observed roll-off rate and with the included covariates, the coefficients on the demographic variables may be confounded. Second, Washington is a relatively homogenous state, meaning that there is not a large amount of variation in these variables. But, overall, these models show that my main results are robust to alternate specifications.

Cross Section Regressions Including Demographic Covariates

Office:	(1) President	(2) Governor	(3) Lt. Gov.	(4) Secretary of State	(5) State Auditor	(6) Comm. of Public Lands	(7) U.S. Rep.
All-Mail Election	0.205	−0.346	−1.085	−1.409	−1.336	−1.135	1.165
	[−0.173 0.584]	[−0.688 −0.003]	[−1.543 −0.626]	[−1.941 −0.878]	[−2.169 −0.502]	[−1.957 −0.313]	[−0.041 2.370]
Turnout	0.015	0.006	0.022	−0.021	−0.022	0.057	−0.002
	[−0.043 0.072]	[−0.035 0.046]	[−0.077 0.121]	[−0.097 0.055]	[−0.172 0.128]	[−0.055 0.168]	[−0.071 0.067]
% white	−8.355	−0.153	−14.644	−14.127	−28.410	−15.994	−14.929
	[−15.843 −0.867]	[−4.609 4.303]	[−30.044 0.756]	[−27.681 −0.573]	[−51.143 −5.678]	[−33.511 1.522]	[−24.610 −5.247]
% black	−18.523	−7.813	−13.321	−14.739	−58.295	−25.139	−0.021
	[−38.824 1.778]	[−20.040 4.413]	[−71.271 44.628]	[−61.781 32.302]	[−145.487 28.897]	[−85.641 35.363]	[−24.697 24.654]
% Hispanic	4.162	−0.499	−2.856	−3.783	−5.544	−3.213	−2.569
	[2.123 6.202]	[−2.750 1.752]	[−7.132 1.420]	[−8.205 0.640]	[−10.794 −0.294]	[−6.116 −0.310]	[−6.444 1.307]
% age 65+	5.796	1.311	2.388	6.746	7.966	1.880	6.652
	[1.468 10.125]	[−8.139 10.761]	[−9.529 14.306]	[−4.739 18.231]	[−8.534 24.465]	[−13.642 17.401]	[−0.271 13.574]
Constant	7.301	2.219	18.268	20.172	35.964	18.339	17.311
	[1.148 13.453]	[−2.026 6.464]	[2.826 33.709]	[6.671 33.673]	[12.381 59.548]	[3.072 33.606]	[5.960 28.662]
Observations	193	193	193	193	193	193	347
R-squared	0.502	0.433	0.422	0.511	0.363	0.443	0.416
Number of years	5	5	5	5	5	5	9
County fixed effects	No	No	No	No	No	No	No
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 4: Weighted least squares regression estimates reported. 95% confidence intervals, clustered by county, reported in brackets.