Do Newer Methods Deliver? Re-evaluating the Impact of Universal Vote by Mail

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Abstract

Universal vote by mail (VBM) systems offer a theoretically large decrease in voting costs: ballots are mailed to all registered voters – no request needed – who may vote from home. However, previous studies find VBM leads to quite modest increases in turnout, contrasting the intuition from canonical costly voting models. In this paper, I use recently-developed estimation techniques appropriate for settings with variation in treatment timing and find that VBM leads to a moderate increase in turnout. Having shown that these results are not simply an artifact of the chosen empirical specification, I turn to heterogeneity analyses to understand where VBM is most effective in boosting turnout. I find that VBM has its largest turnout effects in counties with higher baseline voter registration rates and higher baseline Democrat vote shares. Additionally, I find that turnout effects are largest in rural counties and in counties with older voters.

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Universal vote by mail (VBM) systems require election officials to mail ballots to every registered voter in the jurisdiction. This allows individuals to cast their ballot from home and, unlike absentee voting, does not require a request from the voter, lowering voting "costs." Further, receiving a ballot in the mail may create a "reminder" effect for voters and lower costs this way as well. As such, canonical costly voting models of political economy (Downs [1957] and Riker and Ordeshook [1968]) predict VBM should lead to an increase in voter turnout. In this paper, I exploit the staggered adoption of VBM by counties in Washington and Utah and use recently-developed estimation techniques from Callaway and Sant'Anna [2021] and Deshpande and Li [2019] to test this prediction. The results are somewhat surprising: VBM leads to an increase in voter turnout of between 1.15 percentage points (in Washington) and 3.1 percentage points (in Utah), which I argue are relatively small effects given the theoretically large impact VBM has on voting costs.

These estimates are in line with recent studies of VBM using the same Washington-Utah variation but different methodologies: 1.8-2.9 p.p. from Barber and Holbein [2020] and 2.1 p.p. from Thompson et al. [2020]. However, when compared to the established effects of other changes to voting costs – perhaps, a priori, less impactful ones than VBM – they appear small. Cantoni [2020] finds that a one standard deviation increase in distance to polling place (0.245 miles) yields a decrease in turnout of around 1-3 percentage points. Kaplan and Yuan [2020] show that just one additional day of early in-person voting leads to a 0.22 percentage point increase in turnout (some states have early voting periods lasting up to 46 days [National Conference of State Legislatures]).[1] The modest impact of VBM on voter turnout calls into question the positive implications of costly voting models. Theoretically,

[1] Both Cantoni [2020] and Kaplan and Yuan [2020] draw on data sets of registered voters and, in the results reported above, calculate turnout as a proportion of registered voters. The VBM results presented (my own, along with those in Thompson et al. [2020] and Barber and Holbein [2020]) calculate turnout as a proportion of voting-age population. Therefore, the former estimates may seem large in comparison simply due to the smaller denominator. However, in a second set of results which measures turnout as a proportion of voting-age population, Cantoni [2020] finds that a 1 mile increase in distance to polling place leads to a decrease in turnout of between 8.6-14.5 percentage points in the 2012 presidential race and 4.7-7.9 percentage points in the 2014 midterm.

VBM seems to create a large decrease in voting costs, so it is surprising that we do not observe a large turnout increase in response.

I turn to heterogeneity analyses to further understand the impacts of VBM on turnout. Recent work suggests that the response to changes in voting costs depends on context and the characteristics of the voting population [Cantoni and Pons, 2022, Bonica et al., 2021, Cantoni, 2020, Kaplan and Yuan, 2020]. This heterogeneity may reflect different baseline voting costs across sub-populations (Chen et al. [2020] provides evidence of this) or disparate attitudes amongst individual voters. In studying the impact of VBM on turnout across different sub-populations in Utah and Washington, this paper furthers our understanding of when and where VBM can be a useful tool in increasing voter participation.

In particular, I estimate "turnout effects" for each county using stacked difference-in-differences regressions (in the style of Deshpande and Li [2019]), where treated counties (those who have adopted VBM) are compared to counties within the same state which have not yet adopted VBM in the first two elections the treated county uses the system. Rather than aggregating the county-level regression coefficients, I graph the relationship between these coefficients and various county-level characteristics measured in the election just *prior* to treatment. These characteristics include voter registration rates, Democrat vote share, (logged) population, and median age, with the idea that the effectiveness of VBM could depend on each of these qualities. Then, I aggregate the stacked regression coefficients separately for treated counties which are above and below the medians of each of these baseline characteristics (i.e., separate regressions for counties above and below the median Democrat vote share, etc.).

It is important to recall that VBM is inherently tied to voter registration. Ballots are mailed to every *registered* voter; of course, low-participation voters (the presumed target population of policies aimed at increasing turnout) are less likely to be registered. If individuals who are most likely to be converted from non-voters to voters by VBM are not registered, then the policy change has no chance to reach them. Notably, Bonica et al. [2021] show that VBM appears much more effective in boosting turnout when analysis focuses on registered voters only: they find that VBM led to an increase in turnout of between 5.8-8.2 percentage points among registered voters in Colorado. Indeed, I confirm that the size of the turnout effect in Utah and Washington is positively correlated with a county's "registration rate" (number of registered voters as a proportion of voting-age population) in the last election prior to VBM. This finding suggests that policy-makers implementing VBM with the intent to increase voter turnout should consider passing policies which boost voter registration in tandem.[2]

Recent discourse often centers around a partisan angle to VBM: many Republicans seem to distrust VBM and believe it benefits Democrats (Phillips [2020]), even though recent studies have shown that it has no impact on partisan vote shares (see Barber and Holbein [2020] and Thompson et al. [2020]). For this reason, it may take Republican voters longer to "trust" VBM, delaying any turnout response. Therefore, one might expect to see a larger turnout response from left-leaning counties (at least initially). Using vote share data from Thompson et al. [2020], the Washington Secretary of State, the Washington State Archives, and the Utah Lieutenant Governor, I show that the treatment effect of VBM on turnout is positively correlated with the Democrat vote share in the election just before VBM implementation.[3] This finding is consistent with results from Kaplan and Yuan [2020], which finds that early in-person voting has a larger impact on turnout of Democrats, and could suggest that left-leaning counties are more responsive to the introduction of convenience voting methods.

Heterogeneity in the turnout response could suggest variation in baseline voting costs

[2] Bhatt et al. [2020] find that decreasing voter registration costs via outreach programs and mailings increases both registration and voter turnout. Braconnier et al. [2017] show that decreasing information costs and administrative burdens increases voter registration and obtain results which suggest that decreasing registration costs could increase voter participation.

[3] To avoid any peculiarities of one race, I determine the vote share by averaging the sum of votes for Democrats in any federal or gubernatorial election and divide by the total number of votes cast in those elections.

across subgroups: we would expect VBM to lead to larger turnout increases in counties that had higher voting costs pre-VBM. In the absence of a good measure for voting cost within my data, I use data from the Survey of Epidemiology and End Results (SEER) population database and the Census Bureau on county-level population and median age to test for such relationships. There is a negative relationship between turnout effect and (logged) population, suggesting that VBM may be most impactful in rural communities where there is greater distance between voters and their polling places (consistent with the finding that distance to polling place serves as a barrier to voter turnout [Cantoni, 2020]). Additionally, there is a strong positive correlation between turnout effect and median age, suggesting that older voters are most responsive to VBM. This is consistent with other work showing that older voters are most likely to vote by mail or via absentee ballot (Morris [2020] and Gronke et al. [2020]), perhaps due to higher costs of voting (transportation issues, health, etc.).

In this paper, I use recently-developed econometric techniques from Callaway and Sant'Anna [2021] and Deshpande and Li [2019] to show that VBM increases voter turnout by 1.15-3.1 percentage points, supporting the recent body of literature on VBM's modest impact on voter turnout (Barber and Holbein [2020] and Thompson et al. [2020]). Having shown that the moderate turnout increase is not just an artifact of the chosen empirical specification, I focus on understanding where VBM is most successful in increasing voter turnout. The impact of VBM is heterogeneous according to a number of county characteristics (as measured in the election prior to implementation of VBM): registration rate, Democrat vote share, population, and median age. The results suggest that voters in left-leaning counties, older voters, and voters in less populous (presumably more rural) counties may face higher voting costs in the absence of VBM. In addition, they have important implications for policymakers wishing to increase turnout through the use of VBM: high voter registration is a key component to the "effectiveness" of VBM.

1 Institutional Background

Prior to 2020, five states conducted elections entirely by mail: Colorado, Hawaii, Oregon, Utah, and Washington.[4] In the wake of COVID-19, several states loosened their restrictions on mail-in and absentee voting in 2020, which was met by claims that the Democratic party would unfairly benefit from the changes (Rutenberg et al. [2020]). Several "no-excuse" absentee ballot states, where normally a voter has to provide an approved reason to receive an absentee ballot, allowed voters to request an absentee ballot without a reason. A few states used VBM and mailed ballots to all registered voters. As of 2024, eight states allow all elections to be conducted entirely by mail, with Nevada and Vermont first implementing VBM in 2022. Additionally, fifteen other states permit certain counties or jurisdictions to hold some elections by mail.

Washington and Utah passed VBM laws in 2005 and 2012, respectively.[5] Both states' laws allowed counties to make the choice to conduct elections with VBM.[6] Implementation decisions were made by county commissions and county clerks or auditors, who were in charge of local election administration. An oft-cited reason for moving to VBM according to county legislative meeting minutes was to reduce election administration costs (for several Washington counties, the aim more specifically was to avoid purchasing costly equipment mandated by the Help America Vote Act of 2002). These laws resulted in staggered adoption in each state: see Figure 1 for a map of VBM take-up over time and Figure 2 for the share of counties in each state implementing VBM over the implementation period.

Due to the staggered nature of adoption, Washington and Utah may serve as better settings to analyze the impacts of VBM than Colorado, Hawaii, Nevada, Oregon, and Vermont,

[4] California also began rolling out VBM in 2018 after the passage of the Voter's Choice Act.

[5] Prior to 2005, some counties in WA implemented VBM by making use of a "small precincts law" which allowed precincts with under 200 voters to switch to VBM.

[6] An exception is Pierce County, WA, which was required to switch to VBM by the Washington State Legislature in 2011.

where take-up occurred simultaneously statewide. In particular, comparing counties who have adopted VBM to counties who have not yet adopted VBM *within the same state* should difference out any peculiarities of certain statewide elections or other changes in state election laws that might have occurred concurrently with VBM take-up in a way that cross-state comparisons using Colorado, Hawaii, Nevada, Oregon, and Vermont cannot. I follow Barber and Holbein [2020], Thompson et al. [2020], and Gerber et al. [2013] in utilizing this staggered adoption in the empirical design described below.

2 Data

I begin with the county-level 1996-2018 data set provided by Thompson et al. [2020], which includes county-level VBM adoption dates, counts of registered voters, total ballots cast, and ballots cast for Democratic and Republican candidates in gubernatorial and federal races. I augment this data by collecting these same county-level outcomes from 1986-1994 in order to include outcomes up to ten years prior to the first election with VBM so that I may evaluate pre-trends in event-study style specifications.[7] I obtained these outcomes from the Washington Secretary of State, Washington State Archives, and the Utah Lieutenant Governor's websites. Since all federal and gubernatorial races take place in even-yeared elections in Washington and Utah, my data is only comprised of observations in even years (1986, 1988, etc.), and counties which adopt VBM in odd years are coded as first-treated in the subsequent even year.

I collect data on the voting-age population in each county (counts of all residents aged 18 and up) from the Survey of Epidemiology and End Results (SEER) population database, which allows me to calculate turnout and registration rates by dividing ballots cast and registered voters by voting-age population. I use data from SEER and the Census Bureau

[7] I do not add data from 2020 or beyond because the last of the counties in my sample adopted VBM in 2020, meaning there would be no control counties left to make comparisons with in 2020 or 2022.

on county-level population and median age.

3 The Turnout Effect

Costly voting models (Downs [1957] and Riker and Ordeshook [1968]) predict that a decrease in the cost of voting should lead to an increase in voter turnout. Arguably, VBM represents a large decrease in voting costs: every registered voter is mailed a ballot (without having to request one, as one does in a system of absentee voting). Voters in jurisdictions using VBM, therefore, face a lower voting cost than voters in jurisdictions with Election Day-only voting (no need to travel to the polling place) and voters in jurisdictions with absentee voting (no need to request a ballot). In addition, receiving a ballot in the mail may provide a kind of "reminder" to voters that they would not have gotten otherwise, also theoretically increasing their propensity to vote.

Surprisingly, recent work has found only modest increases in turnout after the implementation of VBM: Barber and Holbein [2020] and Thompson et al. [2020] find increases of 1.8-2.9 and 2.1 percentage points, respectively. These turnout effects are relatively moderate given the dampening effect that costs of voting (which are mitigated by VBM) have been shown to have on turnout (Cantoni [2020]). In the following section, I estimate the impact of VBM on voter turnout using recently-developed estimation strategies from Callaway and Sant'Anna [2021] and Deshpande and Li [2019] and also find modest turnout effects.

3.1 Methodology

A two-way fixed effects design may involve making comparisons between later-treated counties (as treatment) and earlier-treated counties (as controls) – see Goodman-Bacon [2021]. Indeed, conducting a Bacon decomposition (which decomposes the design into its different sources of variation, i.e., what groups are being compared) shows that comparisons between later-treated counties (as treatment) and already-treated counties (as controls) account for 18.2% of the coefficient in the following traditional two-way fixed effects specification, written below:

$$y_{ct} = \alpha_0 + \beta D_{ct} + \alpha_c + \alpha_t + \epsilon_{ct}, \tag{1}$$

where y_{ct} is the turnout rate in county c and election year t, D_{ct} is a dummy variable which equals one after a county adopts VBM, and α_c and α_t are county and election fixed effects, respectively.

See Figure 3 for the decomposition. Using a two-way fixed effects specification can be problematic if we expect treatment effects to be dynamic: we may be using earlier-treated counties as controls while they are still experiencing changes in outcomes caused by their own implementation of VBM. There are a priori reasons to expect the effects of VBM to be dynamic: it may take a while for voters to adjust to (or have confidence in) the new system. Voter organization and mobilization groups, as well as candidates, need to adjust their "get out the vote" efforts accordingly, perhaps focusing more on the registration of new voters (as ballots are only mailed to registered voters).

Recent advances in econometrics have offered a few potential alternatives to two-way fixed effects estimation. In this paper, I use both the "stacked regression" method (in the style of Deshpande and Li [2019]) and "group-time average treatment effects" from Callaway and Sant'Anna [2021]. Both of these methods rely on making comparisons between treated units and not-yet-treated units, bypassing the issue of using already-treated units as controls. For reasons explained below, my preferred specification in this paper is that from Callaway and Sant'Anna [2021].

Determining the impact of VBM over the course of several elections requires analyzing Washington and Utah separately. In 2007, Washington passed a law allowing online voter registration, which went into effect in 2008; in 2010, Utah also implemented online voter registration. Since voter registration is so closely tied to VBM (only registered voters are mailed ballots) and the largest adoption group (providing much of my variation) is a group of Washington counties which took up VBM in 2006, I would be unable to distinguish between the effects of VBM and online voter registration if I were to compare counties in Washington to counties in Utah post-2006. For this reason, I separate the analysis by state in all specifications. Through directly comparing counties in Washington (Utah) to other counties in Washington (Utah), any impact of the online voter registration law should be differenced out (as it "turns on" at the same time for both treatment and control counties).

3.1.1 Stacked Method

First, I reorganize my data into "stacks" (as in Deshpande and Li [2019]), where each stack s is defined by some treated county c. The stack contains county c and all other counties from the same state which adopt VBM at least 3 years after county c, so that for the first two elections where county c has VBM, these counties are not-yet-treated and therefore an ideal control group. This comparison is given by the following specification:

$$y_{cst} = \alpha_0 + \beta VBM_{cst} + \alpha_{cs} + \alpha_{st} + \epsilon_{cst} \tag{2}$$

for county c, stack s, and election year t, where stacks are composed of the "treated" unit (county) of interest and comparison counties *from the same state* which are not-yet-treated. County-stack fixed effects, α_{cs}, act as county fixed effects, and stack-election year fixed effects, α_{st}, act as election year fixed effects.

Note that if county A adopts VBM in 2004, and counties B, C, and D adopt in 2006, 2006, and 2008, respectively, only county D is used as a comparison county according the way I have defined my stacks. To allow for a more flexible control group, where counties B and C can be used as controls for county A in 2004 and then removed from A's control group thereafter, I turn to a separate estimation method proposed by Callaway and Sant'Anna [2021].

3.1.2 Group-Time Average Treatment Effects

I estimate "group-time average treatment effects" (Callaway and Sant'Anna [2021]), where I limit to comparisons between treated counties and not-yet-treated counties. These group-time average treatment effects are given by

$$ATT(g,t) = \mathbb{E}[Y_t(g) - Y_t(0)|G_g = 1], \tag{3}$$

where Y_t is an outcome of interest in period t (here, periods correspond to election years) and G_g is a dummy variable equal to one if a county first implements VBM in period g. All counties which first implement VBM in period g belong to "group" g, so that $ATT(g,t)$ is the average treatment effect of VBM for counties in group g in election t. I calculate these group-time average treatment effects for several elections relative to treatment for each group. As mentioned above, I separate the analyses of Utah and Washington; that is, I only make comparisons between treated counties in Utah (Washington) and not-yet-treated counties in Utah (Washington). Callaway and Sant'Anna [2021] provide a few different ways to aggregate the estimated $ATT(g,t)$'s. I present the different aggregations and discuss their pros and cons below.

3.2 Turnout Results

In Table 1 (2), I report three separate aggregations of group-time average treatment effects from Washington (Utah). In the simple aggregation (column (1)), I average all group-time average treatment effects together; one drawback of this approach is that it puts more weight on cohorts that have implemented VBM for longer. Next, I present the dynamic aggregation (column (2)), where I average group-time average treatment effects across all elections relative to treatment (i.e., first implementation of VBM). That is, I average all of the "first election effects," the "second election effects," etc. together. This is subject to similar composition issues as the simple aggregation: naturally, counties which implemented VBM the earliest will see their effects in more of these averages (i.e., a county which adopted VBM in 1996 has an "eighth election effect" present in the data, but a county which adopted VBM in 2012 does not).

To address the concerns of the first two aggregation methods, I report the group aggregation of group-time average treatment effects in column (3). This measure takes the average effect (across all elections post-treatment) for each adoption group (defined by the year a county first took up VBM) and then averages those averages across groups. It is, as noted in Callaway and Sant'Anna [2021], the "average effect of participating in the treatment experienced by all units that ever participated in the treatment... its interpretation is the same as the [average treatment effect on the treated] in the canonical [difference-in-differences] setup with two periods and two groups." The group aggregation (column (3) in Tables 1 and 2) is my preferred aggregation method since it is not subject to the same compositional issues as the first two methods.

Table 3 reports the group aggregations of group-time average treatment effects from Washington and Utah, as well as results from the two-way fixed effects and stacked specifications from equations (1) and (2), respectively.[8] Ultimately, the results do not differ too much across specifications and are quite similar to findings from Barber and Holbein [2020] and Thompson et al. [2020]: using my preferred method, I find that VBM led to an increase in turnout rate of between 1.15 and 3.1 percentage points. As noted above, this is quite a modest reaction from voters given their established responsiveness to costs of voting [Cantoni, 2020, Kaplan and Yuan, 2020].

In Washington, VBM led to an increase in turnout rate of 1.15 percentage points. In Utah, it led to an increase in turnout rate of 3.1 percentage points. Figures 1 and 2 display estimates of group-time average treatment effects relative to the first election of implementation. These figures show the effect of VBM in the first three elections of implementation (indexed by 0, 1, and 2 on the x-axis). Note that the turnout effect increases over time in Washington, whereas it *decreases* over time in Utah. Further analysis is needed to understand the different dynamic effects across settings, but these results suggest the importance of context in studying the impact of VBM on turnout. Indeed, there are several reasons to expect that the effectiveness of VBM may differ based on the implementing county. In the following section, I explore heterogeneity in the turnout effect according to several pre-VBM county characteristics.

4 Heterogeneity in the Turnout Effect

As with any policy, the success of VBM is likely context-dependent: its ability to increase voter turnout relies on underlying qualities of the county taking up the treatment. For example, consider a county's registration rate (the number of registered voters divided by the voting-age population). Ballots are only mailed out to all *registered* voters under VBM; therefore, counties with higher registration rates should theoretically see larger increases in turnout as a result of the policy. However, voter registration may itself be affected by VBM:

[8] I amend the two-way fixed effects specification from (1) by including state-by-election year fixed effects in order to limit to within-state comparisons, for reasons mentioned above.

campaigners and organizers may increase efforts to register new voters knowing that voting is now easier. For this reason, I study heterogeneity in the turnout effect according to *baseline* characteristics measured in the election just *prior* to a treated county's first election with VBM.

A county's registration rate is not the only attribute VBM's success may depend upon. There may be cross-county variation in baseline voting costs due to a variety of factors, due to qualities of a county itself and to individual characteristics of the county's voters. Therefore, turnout effects might differ according to baseline demographic characteristics of counties: I focus on (logged) population and median age. Finally, since many Republicans have been shown to be skeptical of VBM (Phillips [2020]), turnout effects might look different in left and right-leaning counties. I examine heterogeneity by baseline Democrat vote share, averaging the sum of votes for Democrats in all federal and gubernatorial races and dividing by the total number of votes cast in those races in the baseline election. In the following section, I describe the methods I use for my heterogeneity analyses.

4.1 Methodology

I create binned scatterplots to visualize the potentially heterogeneous effects of VBM. In particular, I plot the relationships between the effect of VBM on turnout (as measured by the coefficient from a stacked regression for each treated county) and baseline variables of interest, all measured in the election just *before* a treated county implemented VBM: registration rate, Democrat vote share (averaged over all federal and gubernatorial races), median age, and (logged) population. Table 4 gives the summary statistics of each of these baseline characteristics.

Next, I re-estimate the stacked regression specified in equation (2) separately for stacks defined by treated counties which are above and below the sample medians of each of the baseline characteristics mentioned above. For example, I estimate equation (2) where stacks are composed of the "treated" county of interest and comparison counties from the same state which are not-yet-treated, where the treated county's baseline registration rate is *above* the sample median baseline registration rate; then, I do the same where all treated counties of interest have baseline registration rates *below* the sample median.

4.2 Heterogeneity Results

Figures 6-9 display the binned scatterplots. I find that VBM sees its largest turnout effects in counties that had the highest registration rates pre-VBM. This makes sense (and has potential implications for policy-makers moving forward) – since in VBM, ballots are automatically mailed to all *registered* voters, high voter registration is key to the policy's success in increasing turnout. Therefore, policy-makers interested in increasing voter turnout via VBM should consider passing legislation which increases voter registration in addition.

There is a positive relationship between VBM turnout effect and Democratic vote share pre-VBM. This could be due to initial distrust of the new system by Republicans, as recent discourse would suggest exists. However, this could also be explained by differences in pre-VBM costliness of voting which are correlated with demographic characteristics. To the extent that demographic characteristics are correlated with Democrat vote share, this graph may be showing that voters previously facing disproportionately high costs of voting are benefiting more from VBM.

Perhaps more informative are the results for baseline population and median age. I find a negative correlation between turnout effect and (logged) population. This makes sense: voters in rural communities likely live further away from their polling places, meaning VBM introduces a greater cost differential from Election Day voting as compared to voters in more populous counties. Finally, I find a strong positive correlation between turnout effect and baseline median age. In fact, as shown in Table 5, the coefficients for above and below-median specifications are statistically distinct from one another. Other work has established that older voters are more likely to vote with mailed ballots than other age groups (Morris [2020] and Gronke et al. [2020]), so it makes sense that older counties see greater turnout effects. These two findings are suggestive that VBM is most effective in increasing turnout for groups facing higher voting costs: rural voters (due to proximity to polling places) and older voters (due to potential transportation issues, health, etc.).

5 Discussion & Conclusion

Universal vote by mail has a modest impact on turnout: in Washington, its implementation increased the turnout rate by 1.15 percentage points; in Utah, turnout rate raised by 3.1 percentage points. These small increases, also found by other researchers (Barber and Holbein [2020] and Thompson et al. [2020]), contradict the intuition from costly voting models (Downs [1957] and Riker and Ordeshook [1968]). Given that VBM – at least in theory – greatly reduces the cost of voting, why is the turnout response from voters so muted? In this paper, I argue that context matters: baseline characteristics of the counties taking up VBM affect how much VBM changes turnout.

I find a negative correlation between turnout effect and (logged) population and a positive correlation between median age and turnout effect, suggesting that VBM is most impactful for rural and older voters. These findings make correspond with the established dampening effect distance to polling place has on turnout [Cantoni, 2020] and the higher propensity older voters have to vote with mailed ballots [Morris, 2020, Gronke et al., 2020]. I also find a positive relationship between Democrat vote share and turnout effect, consistent with other work which has found that Democrats are more responsive to the introduction of convenience voting [Kaplan and Yuan, 2020]. Further research is needed to understand if this finding comes from a lack of trust of VBM from Republicans, a correlation between underlying voting costs and political preferences, or something else.

There is a strong positive relationship between turnout effect and baseline registration rates. This finding holds an important policy implication: policy-makers enacting VBM as a means to increase turnout should seriously consider passing policies which will increase voter registration alongside it. After all, under VBM, ballots are only mailed to *registered* voters. Boosting voter registration is a necessary first step to increasing voter turnout: without increased registration rates, VBM cannot be effective in easing the burden of high-cost voters.

The results in this paper suggest that universal vote by mail may not be a "one size fits all" method of increasing turnout: context *matters* with the implementation of this policy. Although I find the turnout effect was largest for older voters and rural voters in Utah and Washington, previous research has shown VBM was most impactful for different groups in Colorado.[9] Ultimately, VBM's impact relies on both the prior voting costs and attitudes of individual voters, which may vary widely across settings. However, the findings in this paper suggest that policy-makers and organizers do have a tool at their disposal to ensure VBM's efficacy: increasing voter registration.

[9] Bonica et al. [2021] find the turnout effect was largest for "young people, blue-collar workers, voters with less educational attainment, and voters of color" in Colorado.

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A Appendix

A.1 Tables and Figures

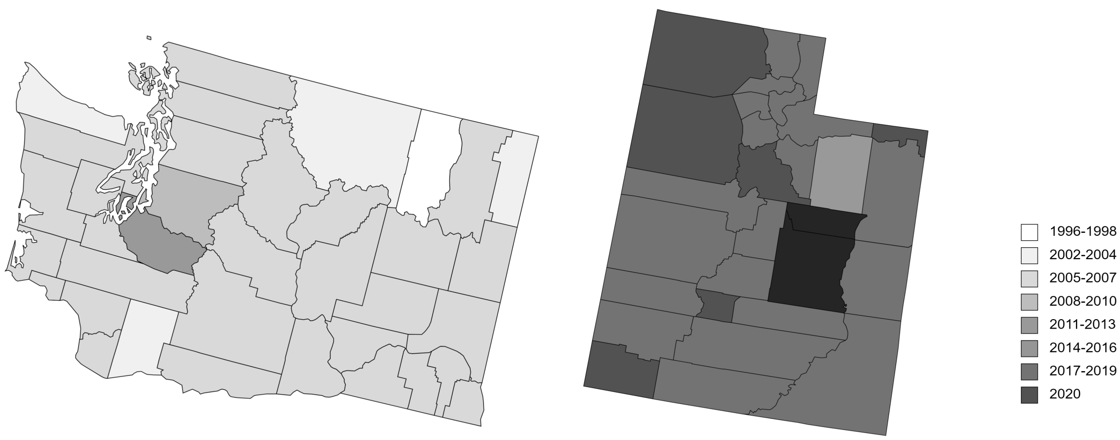

Figure 1: This map shows county-level adoption of VBM in Washington and Utah between 1996 and 2020.

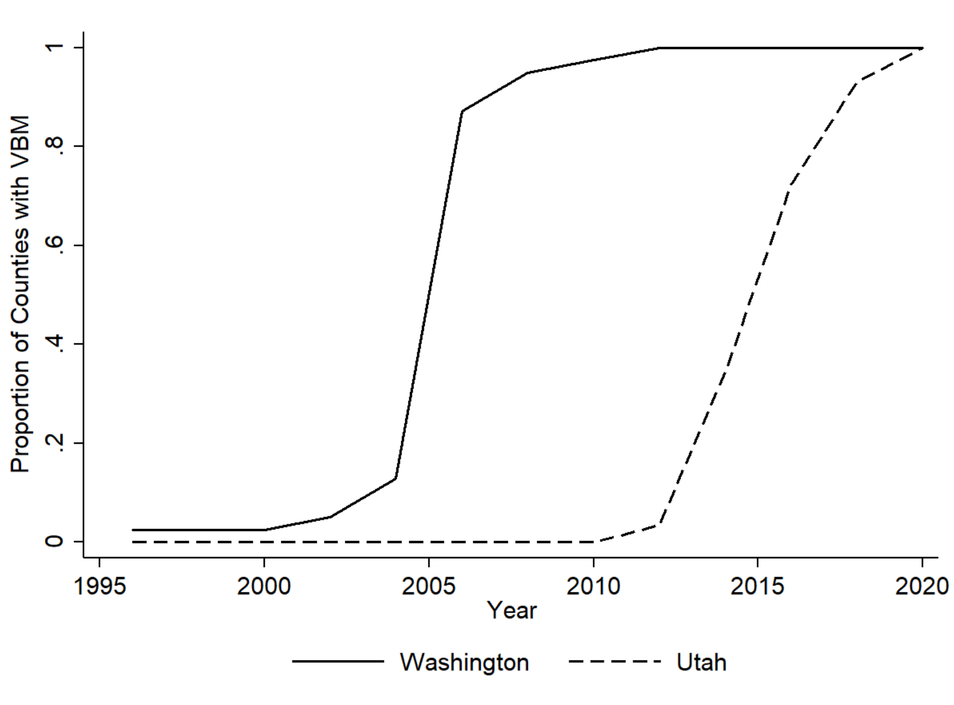

Figure 2: This graph displays the proportion of counties implementing VBM in Washington and Utah between 1996 and 2020.

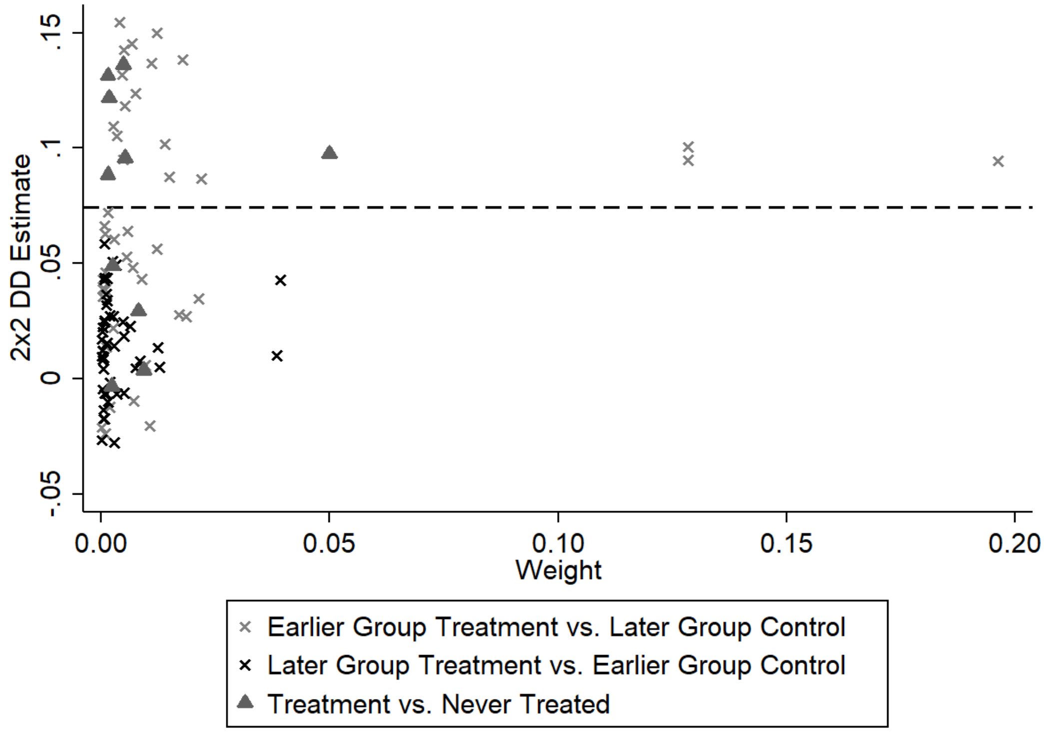

Figure 3: This graph displays the Bacon decomposition of the two-way fixed effects specification estimating the effect of VBM on voter turnout.

Notes: This graph displays all 2x2 DD estimators and their associated weights; the dashed line represents the estimated coefficient from $y_{ct} = \alpha_0 + \beta D_{ct} + \alpha_c + \alpha_t + \epsilon_{ct}$, where y_{ct} is the turnout rate in county c and election year t, D_{ct} is a dummy variable which equals one after a county adopts VBM, and α_c and α_t are county and election year fixed effects, respectively. Notably, the estimated coefficient reported here (0.074) represents an overestimate of the turnout effect, because the decomposition does not allow me to include state by year fixed effects.

Table 1: Estimates of the turnout effect in Washington using Callaway and Sant'Anna [2021].

	(1) Simple Agg.	(2) Dyn. Agg.	(3) Group Agg.
VBM	0.008	0.0139**	0.0115*
	(0.0083)	(0.0064)	(0.0067)
N	780	780	780

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: Bootstrapped standard errors are listed in parentheses. The above table displays the simple, dynamic, and group aggregations of group-time average treatment effects of VBM in Washington state.

Table 2: Estimates of the turnout effect in Utah using Callaway and Sant'Anna [2021].

	(1) Simple Agg.	(2) Dyn. Agg.	(3) Group Agg.
VBM	0.0282**	0.0222*	0.031**
	(0.0136)	(0.013)	(0.0148)
N	580	580	580

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: Bootstrapped standard errors are listed in parentheses. The above table displays the simple, dynamic, and group aggregations of group-time average treatment effects of VBM in Utah.

Table 3: Comparing methods: Universal VBM led to an increase in turnout rate of between 1.15 and 3.1 percentage points.

	Group Agg. (WA)	Group Agg. (UT)	Stacked	Two-Way Fixed Effects
VBM	0.0115*	0.031**	0.0117**	0.0176**
	(0.0068)	(0.0158)	(0.005)	(0.009)
N	780	580	2,002	1,156

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: Standard errors are listed in parentheses. The table above compares the effect of Universal VBM across different specifications. The outcome of interest is turnout rate, calculated by total ballots cast divided by the voting-age population (18+ population taken from SEER). In each of these specifications, I limit to within-state comparisons to ensure the results are not driven by other state-level changes.

In the first two columns, I present the average of group-time treatment effects (Callaway and Sant'Anna [2021]), estimated by $\sum_{g \in \mathcal{G}} \theta_{sel}(g) \mathbb{P}(G = g | G \leq \tau)$, where $\theta_{sel}(\tilde{g}) = \frac{1}{\tau - \tilde{g} + 1} \sum_{t=\tilde{g}}^{\tau} ATT(\tilde{g}, t)$. I calculate these separately for Washington (column 1) and Utah (column 2); the standard errors are bootstrapped.

In the third column, I present the result from a "stacked" specification in the style of Deshpande and Li [2019]. In particular, I estimate $y_{cst} = \alpha_0 + \beta VBM_{cst} + \alpha_{cs} + \alpha_{st} + \epsilon_{cst}$ for county c, stack s, and election year t, where stacks are composed of the "treated" unit (county) of interest and comparison counties *from the same state* which are not-yet-treated. County-stack fixed effects, α_{cs}, act as county fixed effects, and stack-election year fixed effects, α_{st}, act as election year fixed effects. Standard errors are clustered at the county level.

Finally, in the fourth column, I present the result from a two-way fixed effects specification with state-by-election year fixed effects. I estimate $y_{clt} = \alpha_0 + \beta VBM_{clt} + \alpha_{cl} + \alpha_{lt} + \epsilon_{clt}$ for county c, state l, and election year t. Standard errors are clustered at the county level.

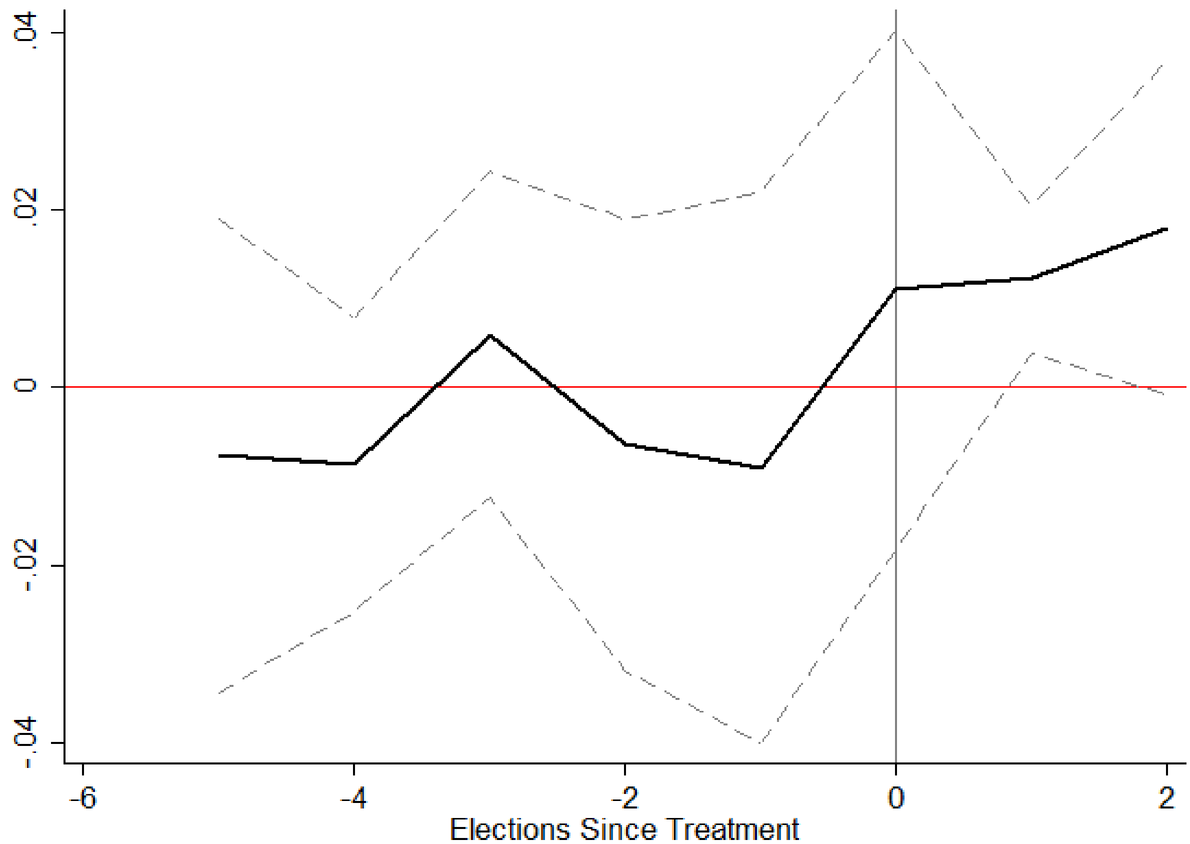

Figure 4: The average effect of Universal VBM over the first three elections of implementation was 1.39 percentage points in Washington.

Notes: This graph displays the relationship between VBM and turnout rate (ballots cast as a proportion of 18+ population) in Washington by presenting estimations of $\hat{\theta}_D(t)$ for each election t relative to treatment, where $\hat{\theta}_D(t) = \sum_{g=1996/2}^{2012/2} \sum_{e=1986/2}^{2012/2} \mathbf{1}\{e - g = t\} \widehat{ATT(g,e)} \mathbb{P}(G = g | e - g = t)$. g indexes timing groups and e indexes even-yeared, general elections. All years are divided by two so that the data is a county-election panel, with observations every election. $\widehat{ATT(g,e)}$ is the difference between changes in turnout rate in counties in group g in elections e and $g - 1$ and changes in turnout rate in counties not-yet-treated by election e in elections e and $g - 1$.

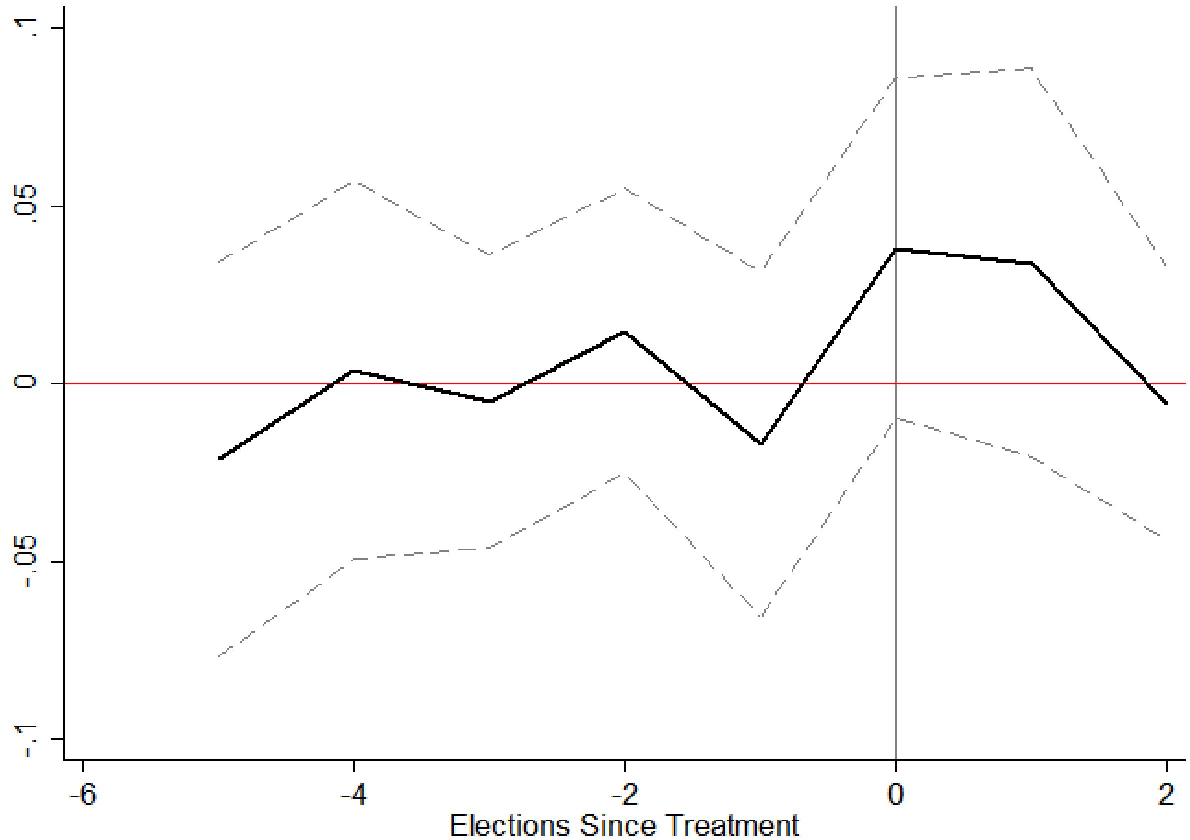

Figure 5: The average effect of Universal VBM over the first three elections of implementation was 2.22 percentage points in Utah.

Notes: This graph displays the relationship between VBM and turnout rate (ballots cast as a proportion of 18+ population) in Utah by presenting estimations of $\hat{\theta}_D(t)$ for each election t relative to treatment, where $\hat{\theta}_D(t) = \sum_{g=2010/2}^{2018/2} \sum_{e=2000/2}^{2018/2} \mathbf{1}\{e - g = t\} \widehat{ATT(g,e)} \mathbb{P}(G = g | e - g = t)$. g indexes timing groups and e indexes even-yeared, general elections. All years are divided by two so that the data is a county-election panel, with observations every election. $\widehat{ATT(g,e)}$ is the difference between changes in turnout rate in counties in group g in elections e and $g - 1$ and changes in turnout rate in counties not-yet-treated by election e in elections e and $g - 1$.

Table 4: Summary statistics: baseline characteristics

	Mean	Standard Deviation
Registration Rate	0.75	0.09
Democrat Vote Share	0.42	0.15
Logged Population	10.67	1.55
Median Age	37.14	6.27

Notes: This table reports the mean and standard deviation of the baseline characteristics of interest. Baseline characteristics are measured in the election just prior to a county's first election with VBM. For example, if a county introduced VBM in 2006, then its baseline characteristics represent the respective variables' values in 2004.

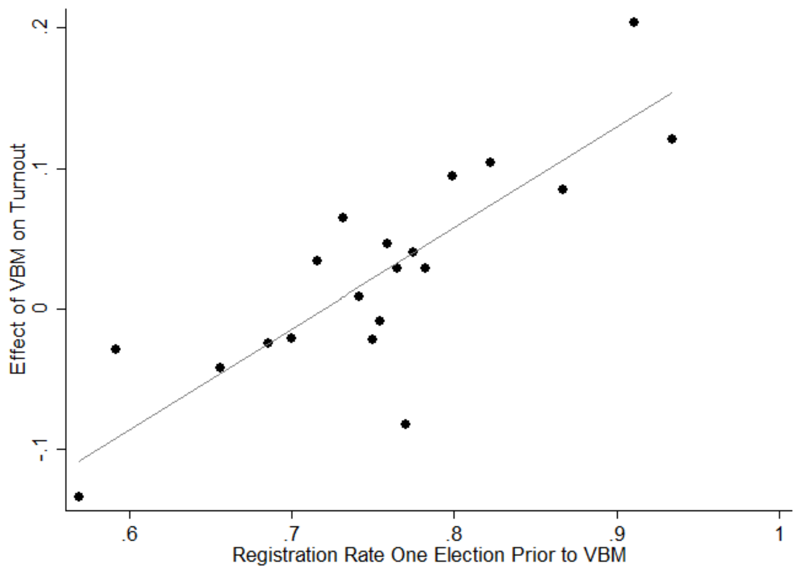

Figure 6: A county's VBM turnout effect is positively correlated with its pre-VBM registration rate.
Notes: This graph displays a binned scatterplot, with the number of registered voters as a proportion of voting-age population measured one election prior to the implementation of VBM on the x-axis and the coefficient from a stacked regression of VBM on turnout on the y-axis. The correlation coefficient is 0.6828.

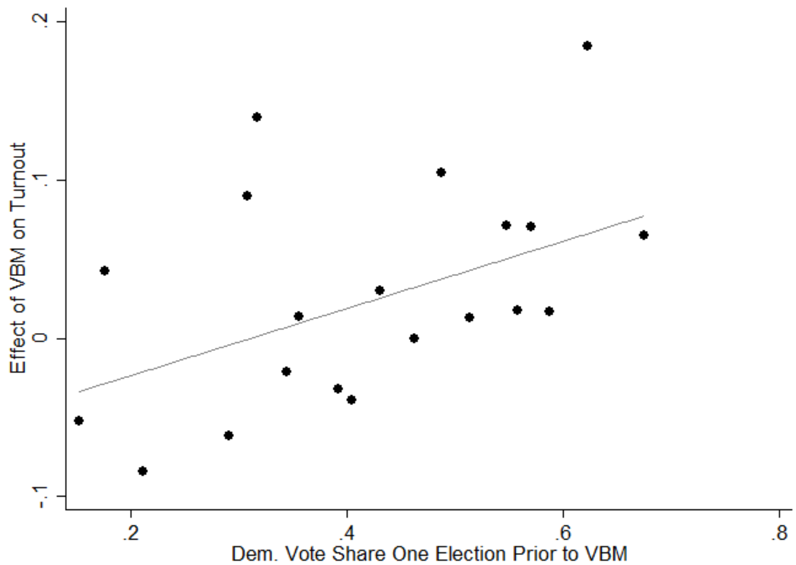

Figure 7: A county's VBM turnout effect is positively correlated with its pre-VBM Democratic vote share.

Notes: This graph displays a binned scatterplot, with the average Democrat vote share (over all federal and gubernatorial races) measured one election prior to the implementation of VBM on the x-axis and the coefficient from a stacked regression of VBM on turnout on the y-axis. The correlation coefficient is 0.3356.

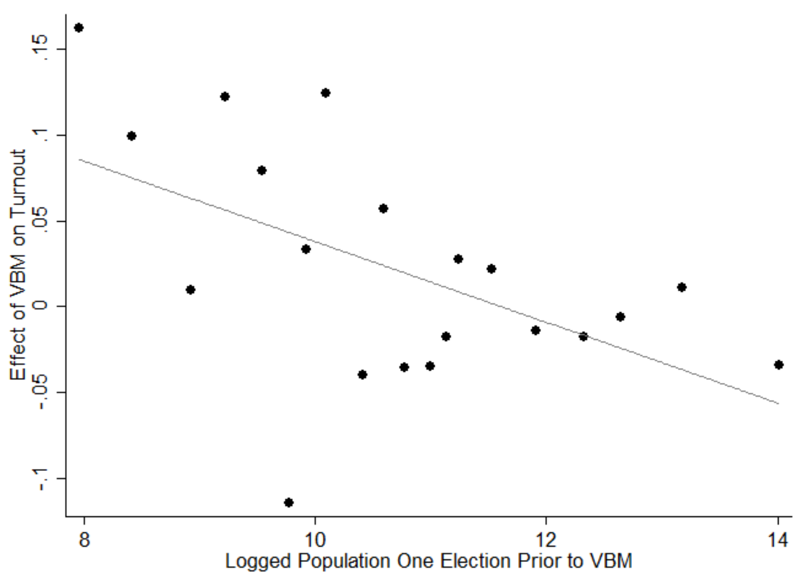

Figure 8: A county's VBM turnout effect is negatively correlated with its pre-VBM (logged) population.

Notes: This graph displays a binned scatterplot, with the logged county population measured one election prior to the implementation of VBM on the x-axis and the coefficient from a stacked regression of VBM on turnout on the y-axis. The correlation coefficient is -0.3859.

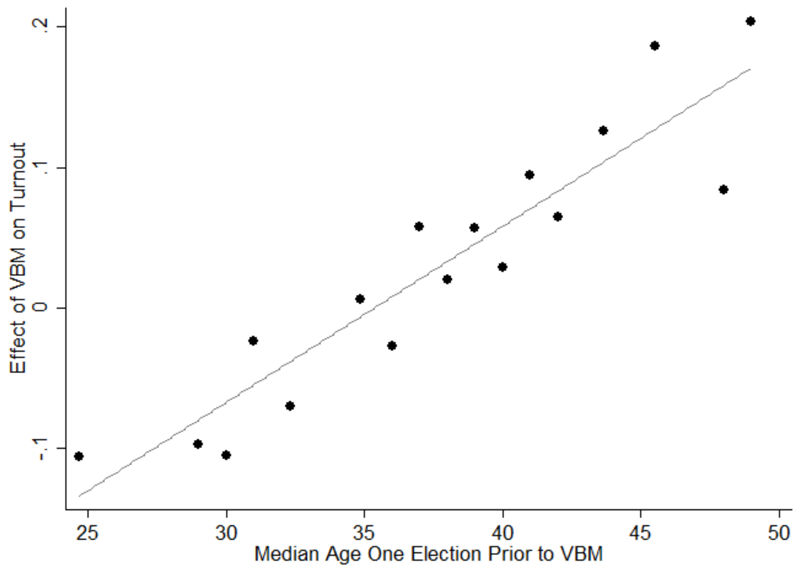

Figure 9: A county's VBM turnout effect is positively correlated with its pre-VBM median age.
Notes: This graph displays a binned scatterplot, with the median age measured one election prior to the implementation of VBM on the x-axis and the coefficient from a stacked regression of VBM on turnout on the y-axis. The correlation coefficient is 0.8371.

Table 5: Heterogeneity in turnout effect by county-level characteristics in election prior to VBM.

	Registration Rate	Democrat Vote Share	Logged Population	Median Age
Above median	0.0144**	0.0173***	0.00797**	0.0202***
	(0.00691)	(0.00558)	(0.00406)	(0.00576)
N	1099	980	763	903
Below median	0.00781	0.00560	0.0139*	0.00243
	(0.00533)	(0.00733)	(0.00776)	(0.00707)
N	840	1001	1218	1057

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table shows results from a "stacked" specification in the style of Deshpande and Li [2019]. In particular, I estimate $y_{cst} = \alpha_0 + \beta VBM_{cst} + \alpha_{cs} + \alpha_{st} + \epsilon_{cst}$, where y_{cst} is the turnout rate in county c, stack s, and election year t. Stacks are composed of the "treated" unit (county) of interest and comparison counties *from the same state* which are not-yet-treated. County-stack fixed effects, α_{cs}, act as county fixed effects, and stack-election year fixed effects, α_{st}, act as election year fixed effects. To examine heterogeneity by pre-existing county characteristics, I estimate this regression separately for stacks defined by treated counties which are above and below the sample medians of various county-level characteristics measured in the election just prior to treatment: registration rate, Democrat vote share, (logged) population, and median age. Standard errors are clustered at the county level.