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# The Impact of Vote-By-Mail Policy on Turnout and Vote Share in the 2020 Election

Sharif Amlani and Samuel Collitt

## ABSTRACT

The COVID-19 pandemic spurred many states and counties to reduce public health risks by adopting policies that made voting by mail easier in the 2020 general election. Employing a two-period difference-in-difference research design, this article investigates how these policy changes affected turnout and presidential vote share. We find that counties that moved to send registered voters mail-in ballots ahead of Election Day experienced 2.6 percent higher turnout compared to counties that made no change, although lesser reforms may have hindered turnout. We also find no evidence that making voting by mail easier conferred a partisan advantage.

**Keywords:** American elections, vote-by-mail policy, 2020 election, turnout, vote share, difference-in-difference

## INTRODUCTION

ELECTED REPRESENTATIVES HARP TO their constituents that exercising the right to vote is the most important democratic action they can perform. Yet most state governments have traditionally made voting more difficult for their residents than in almost all other developed democracies, contributing to relatively lower voter turnout (Blais, Massicotte, and Dobrzynska 2003; Franklin 1996;

Jackman 1987; Powell 1986). A notable reversal occurred as the COVID-19 pandemic precipitated a historic development whereby a majority of states, intending to make voting safer in the 2020 general election, made voting easier through policies aimed at encouraging voters to cast mail-in ballots instead of voting in person.

Our study examines how turnout changes as a function of making voting by mail easier, and whether there is a partisan electoral advantage from doing so. While there is already much work on the subject, prior to the 2020 general election, studies evaluating these relationships have been limited to a handful of states that conducted elections exclusively by mail (Barber and Holbein 2020; Berinsky, Burns, and Traugott 2001; Gerber, Huber, and Hill 2013; Gronke and Miller 2012; Kousser and Mullin 2007; McGhee, Paluch, and Romero 2020; Southwell 2009; Southwell and Burchett 2000b; Thompson et al. 2020). In contrast, 1,664 counties across

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30 states made it easier, by varying degrees, for their residents to vote by mail in the 2020 general election.<sup>1</sup> Figure 1 panels a and b illustrate county-level vote-by-mail (VBM) policies in 2016 and 2020, respectively. The unprecedented sample size and variation in the restrictiveness of VBM policies across jurisdictions allow for a unique analysis of VBM liberalization (i.e., making voting by mail easier) on turnout and vote share.

We investigate these relationships using a two-period difference-in-difference (DID) design, evaluating, in turn, how both turnout and presidential vote share changed at the county level between the 2016 and 2020 general elections as a function of the change in the restrictiveness of a county's VBM policy. We find that counties that moved from not requiring an excuse to vote by mail to automatically sending their registered voters mail-in ballots experienced a turnout increase of 2.6 percent compared to those that did not change their VBM policy. In contrast, lesser reforms may have hindered turnout: counties that moved from requiring a valid excuse from residents to vote by mail to not requiring an excuse experienced a smaller increase in turnout, compared to counties that did not change their policy, by about 1.4 percent. Similarly, counties that moved from not requiring an excuse to sending voters mail-in ballot applications experienced 1.2 percent lower turnout relative to counties that did not change their policy. We speculate that this may be due to the information costs that changing voting laws may place on prospective voters, counteracting any net benefit that making voting by mail easier may provide (Brady and McNulty 2011; Burden et al. 2014; Corvalan and Cox 2018). However, after accounting for correlated errors between counties located in the same state by clustering standard errors at the state level in regression models (Cameron and Miller 2015), these latter negative effects do not reach traditional levels of statistical significance. Regardless, future studies should seek to determine the effects of these changes in subsequent elections as voters become accustomed to them.

Consistent with previous findings (Barber and Holbein 2020; Hassell 2017; Southwell and Burchett 2000a; Thompson et al. 2020), we find no significant partisan advantage associated with making voting by mail easier. Presidential vote share at the county level does not appear to be functionally related to the ease with which voters can cast their

ballots by mail. Our base model regression estimates indicate that VBM liberalization produces a statistically uncertain Democratic advantage ( $p > .05$ ). These coefficients are rendered insignificant ( $p > .1$ ) after accounting for other county- and state-level factors related to a county's change in vote share between the 2016 and 2020 elections. That being said, the two-period difference-in-difference design we employ constrains our ability to conclude with total certainty that Republicans were not disadvantaged by VBM liberalization. We are sure to explore these results and their implications in the context of our modeling strategy.

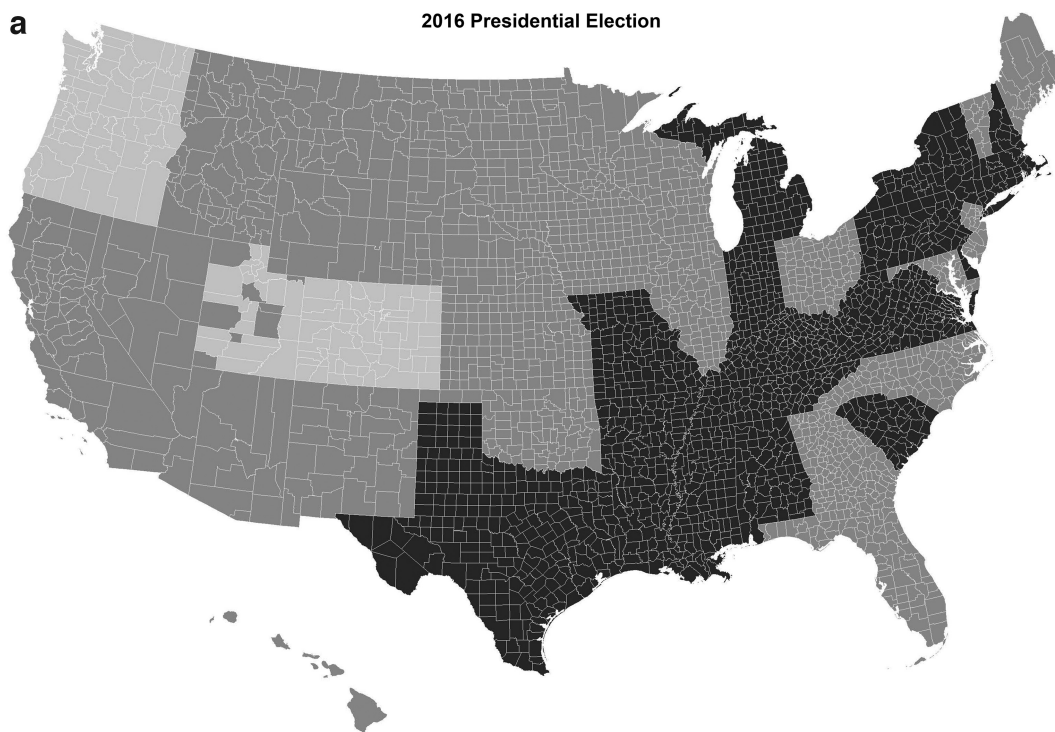
## LITERATURE AND HYPOTHESES

Theoretically, by eliminating a trip to a polling place on Election Day, voting by mail is less costly for a prospective voter than voting in person, making them more likely to turn out (Downs 1957). There is much empirical support for this proposition, with documentation of small and positive impacts of moving to VBM-only elections on turnout in Colorado, Washington, Utah, and Oregon (Barber and Holbein 2020; Berinsky, Burns, and Traugott 2001; Gerber, Huber, and Hill 2013; Gronke and Miller 2012; Kousser and Mullin 2007; McGhee, Paluch, and Romero 2020; Southwell 2009; Southwell and Burchett 2000b; Thompson et al. 2020).

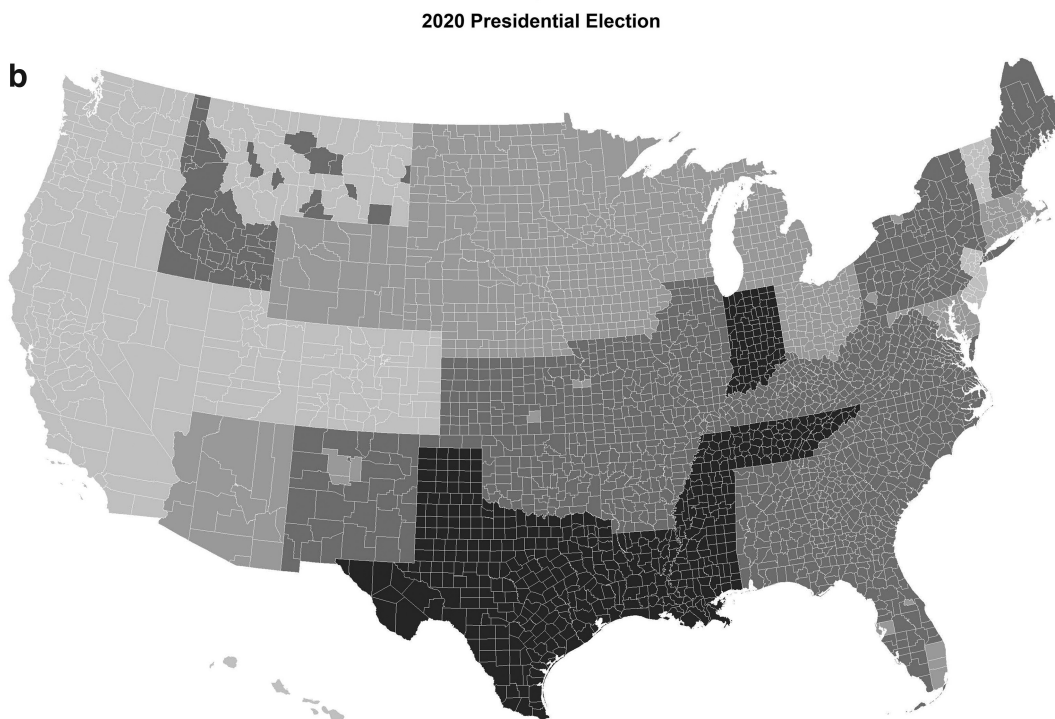
However, there has been little opportunity until the 2020 presidential general election to document the effects of VBM reform outside of four Western states. Nor has there been much opportunity to evaluate smaller (i.e., non-universal) VBM policies, which may produce adverse turnout effects. Findings from Florida's 2018 midterm elections suggest that certain populations may have mail-in ballots rejected at higher rates (Baringer, Herron, and Smith 2020; Cottrell, Herron, and Smith 2021). More broadly, liberalized convenience voting policies may encourage individuals with higher political knowledge to vote by mail at greater rates than their less knowledgeable counterparts (Shino, Suttman-Lea, and Smith 2021). Relatedly, smaller VBM reforms may produce informational costs that offset potential convenience-related benefits, which

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<sup>1</sup>To our knowledge, in no way was voting by mail made more difficult by states or counties in the 2020 general election.



Vote-By-Mail Policy    ■ Excuse-Needed Absentee    ■ No Excuse-Needed Absentee  
                                  ■ Automatic Vote-by-Mail



Vote-By-Mail Policy    ■ Excuse-Needed Absentee    ■ No Excuse-Needed Absentee  
                                  ■ Vote-by-Mail Applications Sent    ■ Automatic Vote-by-Mail

**FIG. 1.** (a) U.S. counties' vote-by-mail policies in the 2016 presidential election and (b) U.S. counties' vote-by-mail policies in the 2020 presidential election. Note: Alaska is excluded from the figure because Alaska does not use counties, but electoral districts, to administer elections. Therefore, turnout data at the county level is unavailable and we omit it from our analysis.

may lower turnout overall (Brady and McNulty 2011; Burden et al. 2014; Corvalan and Cox 2018).

Concerning the partisan effects of VBM reform, while the conventional wisdom is that eligible non-voters, who are primarily poorer, less educated, and nonwhite, would vote for Democratic candidates if they did vote, there is little evidence suggesting that increasing the convenience of voting or, specifically, moving to VBM-only elections, significantly alters parties' vote shares (Alvarez, Levin, and Sinclair 2012; Barber and Holbein 2020; Martinez and Gill 2005; Southwell and Burchett 2000a; Thompson et al. 2020). However, there may be unforeseen partisan effects of adopting other VBM policies, such as sending voters mail-in ballot applications or omitting the need for an excuse to vote by mail. We exploit substantial policy and regional variation, both new to the U.S. in the 2020 presidential general election, to systematically evaluate the effects of VBM policy on turnout and vote share.

Based on consistent findings of positive turnout effects (Barber and Holbein 2020; Berinsky, Burns, and Traugott 2001; Gerber, Huber, and Hill 2013; Gronke and Miller 2012; Kousser and Mullin 2007; McGhee, Paluch, and Romero 2020; Southwell 2009; Southwell and Burchett 2000b; Thompson et al. 2020), we hypothesize that making voting easier by liberalizing VBM policy will increase turnout. Because there is variation in the degree to which changes in VBM lowered voting costs ("dosage variation"), we can analyze the effects of multiple policy interventions. We hypothesize a higher dosage of VBM policy liberalization leads to proportionately higher turnout. Specifically, we expect counties that moved from requiring a valid excuse from voters to vote by mail in 2016 to not requiring one in 2020 to experience the smallest increase in turnout, relative to counties that made no change. We expect a relatively greater change in turnout among counties that moved from not requiring an excuse to sending mail-in ballot applications to registered voters. We expect yet a greater increase in turnout in counties that moved from requiring an excuse to sending mail-in ballot applications (and additionally not requiring an excuse to vote by mail). We expect the greatest change in turnout in counties that moved from not requiring an excuse in 2016 to sending all registered voters no-excuse-needed mail-in ballots in 2020.

Our second hypothesis derives from a near-consensus of null partisan effects findings (Barber

and Holbein 2020; Berinsky, Burns, and Traugott 2001; Hassell 2017; Southwell and Burchett 2000a; Thompson et al. 2020): easing VBM restrictions does not confer an electoral partisan advantage. Specifically, VBM liberalization should not change a county's vote share relative to counties that made no change in VBM policy.

## METHODOLOGY

In response to the COVID-19 pandemic, many states and counties made it easier to vote by mail. The degree to which they made it easier varied considerably. For example, some states enacted a policy to send all registered voters mail-in ballots. Other states did not make it so easy, requiring eligible voters to have a valid excuse to vote by mail, such as being at a high risk of contracting COVID-19. In addition, while some states administer elections uniformly, others allow their counties some autonomy. For example, in 2020, New Mexico's state legislature approved a bill allowing counties to automatically send mail-in ballot applications to voters (Gould 2020). Only ten counties sent applications to their registered voters, while the rest opted out. Thus, in addition to interstate variation, there is also intrastate variation in VBM policies. As a result, we make counties our unit of analysis ( $N = 3,113$ ).<sup>2</sup>

We use a two-period difference-in-difference (DID) design to estimate the effects of changes to VBM policy on turnout and then assess their partisan consequences (Angrist and Pischke 2008; Wing, Simon, and Bello-Gomez 2018). We employ a two-period DID design to account for the non-random assignment of VBM policies to counties. For example, counties with less restrictive VBM policies may differ in observed and unobserved ways from counties with more restrictive VBM policies (e.g., states with Republican-controlled state legislatures have more restrictive VBM policies).<sup>3</sup> Highton (2017) argues that cross-sectional models analyzing variation in voting laws do not account

<sup>2</sup>We exclude Alaska from our analysis because turnout and vote share is not measured at the county level. Instead, Alaska uses "electoral districts" that do not overlap with counties.

<sup>3</sup>In addition, Ansolabehere and Konisky (2006) and Highton (2017) argue that liberal and conservative electors may be different themselves: more liberal electorates being more inclined to vote.

for this source of bias and are highly problematic in accurately estimating the effects of public policy on turnout. Therefore, we estimate a DID model because it accounts for unobserved confounders of turnout and vote share, including the strategic selection of VBM policies.

The primary assumption underlying a difference-in-difference model is that of parallel trends. The assumption is that the control group provides a reasonable counterfactual of the trend of the treatment group by illustrating what would have happened to this group if left untreated (Angrist and Pischke 2008). Given that we examine only two-time points, we cannot verify whether the control and treatment groups track similarly before the treatments' introductions. We provide time-series estimates of turnout and vote share for each VBM condition across turnout and vote share in Supplementary Appendix Figures 6A and 7A.

We create our key independent variable, *VBM condition*, by comparing VBM policies in 2016 and 2020 (Table 1).<sup>4</sup> *VBM condition* includes a control group and four treatment groups.<sup>5</sup> We treat counties whose VBM policy stayed the same between 2016 to 2020 as our control condition (n = 1473). Our treatment conditions are as follows: (1) counties that changed their VBM policy from requiring a valid excuse from their residents to vote by mail to not requiring such an excuse: *excuse-needed in 2016 to no-excuse-needed in 2020* (n = 750); (2) counties that moved from requiring an excuse to sending mail-in ballot applications to their registered voters: *excuse-needed in 2016 to applications sent in 2020* (n = 115); (3) counties that moved from not requiring an excuse to sending mail-in ballot applications: *no-excuse-needed in 2016 to applications sent in 2020* (n = 634); and (4) counties that moved from not requiring an excuse to sending

their registered voters mail-in ballots: *no-excuse-needed in 2016 to ballots sent in 2020* (n = 169).<sup>6</sup> Due to the absence of a single centralized database on these classifications, we derive and cross-validate these changes from a variety of sources: DeSilver and Geiger (2016), Rakich et al. (2020), Swasey (2020), and the National Conference of State Legislatures (2020).<sup>7</sup>

We measure turnout as the percentage of the voting-age population (VAP) that voted (Thompson et al. 2020; Barber and Holbein 2020).<sup>8</sup> While alternative measures might be more desirable (e.g., percentage of eligible or registered voters who cast ballots), they are not reliably available at the county level. We therefore follow the lead of existing research. County-level turnout data from 2016 and 2020 are available from *Dave Leip's Atlas of U.S. Elections* (Leip 2020), and estimates of their voting-age populations are available from the U.S. Census.

TABLE 1. JOINT DISTRIBUTION OF VOTE-BY-MAIL POLICIES IN 2016 AND 2020

2016 VBM policies	2020 VBM policies			
	<i>Excuse-needed</i>	<i>No-excuse-needed</i>	<i>Applications sent</i>	<i>Ballots sent</i>
Excuse-needed	19% (587)	24% (748)	4% (115)	0% (0)
No-excuse-needed	0% (0)	23% (702)	20% (632)	5% (169)
Ballots sent	0% (0)	0% (0)	0% (0)	5% (160)

Note: Cell entries report the percentage (and number) of counties in each category.

<sup>4</sup>Prior to 2020, Oregon, Washington, Colorado, and most counties in Utah conducted elections using universal—or “mandatory”—vote-by-mail (VBM) policies. Such policies include automatically sending voters mail-in ballots, but they also include limited opportunities for in-person voting. Seven states and Washington DC were new to automatically sending voters mail-in ballots in 2020. While all 12 jurisdictions automatically sent voters mail-in ballots, their in-person voting policies varied greatly. Due to this variation, we avoid using “universal” or “mandatory” language in referencing our least restrictive VBM policy classification and instead refer to the state or county’s decision to send mail-in ballots to registered voters.

<sup>5</sup>Supplementary Appendix Table 1A displays the distribution of VBM conditions across 2020 VBM policies.

<sup>6</sup>As a robustness check, Supplementary Appendix Tables 5A–8A includes estimates for two sets of models. The first model compares counties where the control condition is *excuse-needed absentee* to (1) *excuse-needed in 2016 to no-excuse-needed in 2020* and (2) *needed in 2016 to applications sent in 2020* treatment conditions. The second model compares counties where the control condition is *no-excuse-needed absentee* to (1) *no-excuse-needed in 2016 to applications sent in 2020* and (2) *no-excuse-needed in 2016 to ballots sent in 2020*.

<sup>7</sup>While there is substantial within-group VBM policy variation, we argue that classification based on these four criteria (whether a state or county requires an excuse to apply for a VBM ballot, and, if not, whether residents are sent applications or the ballots themselves) provides the most relevant and substantial between-group variation in terms of the theoretical costs to vote by mail. Because 2020 is the first election year where such variation exists on a national scale, we are unable to follow precedent set by previous work.

<sup>8</sup>Voting-age population (VAP) turnout is calculated by dividing the total number of ballots cast by the total voting-age population (the number of individuals 18 years and older) residing in the county.

We estimate our DID model using the equation:

$$\begin{aligned} VAP \text{ Turnout} = & \alpha + \beta_1(\text{Vote by Mail Conditions}) \\ & + \beta_2(\text{Year}) + \beta_3(\text{Vote by Mail} \\ & \text{Conditions} \times \text{Year}) + \beta_i(X_i) + \sigma_c + \varepsilon \end{aligned}$$

The interaction term, *Vote by Mail Conditions x Year*, represents the average treatment effect relative to the control group.<sup>9</sup> The variable  $X_i$  represents our model's control variables. We follow the guidance of Ansolabehere and Konisky (2006), Karp and Banducci (2000), Knack and Kropf (2003), and Smith (2001) and account for a similar set of potential confounders. We control for vote share at the county level coded, *Democratic vote share*. We also add census information, such as the percentage of *African Americans*, *median household income*, *median age*, and percentage of individuals with only a *high school diploma*. We account for the possibility that the severity of COVID-19 affected turnout and vote share by including the *weekly trend in COVID-19 deaths (per 100,000)* at the county level (Baccini, Brodeur, and Weymouth 2021; Parzuchowski et al. 2021). We add an indicator for counties in *battleground states* (Arizona, Georgia, North Carolina, Michigan, Pennsylvania, and Wisconsin) and a binary indicator for the *change in automatic registration laws*.<sup>10</sup> Lastly, we include *county fixed effects* ( $\sigma_c$ ) to control for fixed observed and unobserved heterogeneity between counties, and we cluster our standard errors at the state level (Bertrand, Duflo, and Mullainathan 2004; Cameron and Miller 2015).

To analyze the partisan effects, we again employ a two-period DID design. We source county-level vote share data from Amlani and Algara (2021). Our DID model estimates the effects of changing VBM policies on changes in Republican presidential vote share while accounting for confounders and non-random assignment of VBM policies to counties.

#### *Republican Presidential Vote Share*

$$\begin{aligned} = & \alpha + \beta_1(\text{Vote by Mail Conditions}) + \beta_2(\text{Year}) \\ & + \beta_3(\text{Vote by Mail Conditions} \times \text{Year}) + \beta_i(X_i) \\ & + \sigma_c + \varepsilon \end{aligned}$$

The interaction term, *Vote by Mail Conditions x Year*, represents the average treatment effect. Positive (negative) and significant values suggest

that changes in VBM policy benefit Republicans' (Democrats') vote share. The variable  $X_i$  represents our control variables. We control for counties' *population density*, *unemployment rate* one month before Election Day, the percent of their population with a *high school degree*, with a *bachelor's degrees*, and the percent of the county that is *African American*, *multiracial*, *Latino*, and *foreign-born*. We include the *weekly trend in COVID-19 deaths (per 100,000)*, an indicator for counties in *battleground states* (Arizona, Georgia, North Carolina, Michigan, Pennsylvania, and Wisconsin), and the *change in automatic registration laws* into our model as well.<sup>11</sup> Lastly, we once again include *county fixed effects* ( $\sigma_c$ ) to control for fixed observed and unobserved heterogeneity between counties, and we cluster our standard errors at the state level.<sup>12</sup>

Despite using a difference-in-difference design, our approach has several important limitations. First, our approach is synonymous to an experiment with four observations, one for each treatment group. Therefore, a correlated shock to any of the four VBM policy groups in 2020 (or in 2016) could drive our observations. Notwithstanding this limitation, we are careful not to treat these counties as independent observations as we cluster our standard errors at the state level. Second, more pre-treatment and post-treatment observations would help to adjust for potential correlated shocks and we encourage researchers to investigate this limitation in the future as more data becomes

<sup>9</sup>It is also known as the difference-in-difference estimator.

<sup>10</sup>Changing automatic voter registration represents an electoral reform that is likely correlated with changing VBM policy and may influence both turnout and vote share. As a result, we include it in both the turnout and vote share models. Unfortunately, no comparably reliable, nationwide data is available for other electoral reforms that may be related to both our dependent variables and independent variables of interest, such as whether a state introduced paid postage on return VBM ballot envelopes or whether the availability of ballot drop boxes changed. We expand on the possibility of potential omitted variables in the Discussion section.

<sup>11</sup>We follow Chykh and Urbatsch's (2021) change in vote share model at the county level and employ the same control variables.

<sup>12</sup>For both the turnout and vote share models, we conduct a sensitivity analysis as a robustness check against potential omitted variables (Cinelli and Hazlett 2020). In this analysis, we measure the strength of a confounder necessary to decrease/increase the size of our estimates to a range where they would/would not be statistically significant. We report these results in Supplementary Appendix Figures 1A and 2A.

available. Third, we rely on a two-period difference-in-difference, which is inherently weaker than a difference-in-difference design with three or more periods. The two-period approach is prone to violations of parallel trends, downward-biased standard errors, and cannot be validated using standard difference-in-difference tools and robustness checks. Despite these limitations, we perform several checks to evaluate the robustness of our findings, which are detailed in the Supplementary Appendix.<sup>13</sup>

## RESULTS

Figure 2 illustrates a boxplot of the change in VAP turnout between 2016 and 2020 for each VBM condition. The left-most boxplot shows that turnout in the control condition increased  $6.7 \pm$  the 95 percent confidence interval of 0.17 percent. The remaining boxplots report the distribution of the change in turnout for each treatment condition. Two treatment conditions report an average change in turnout lower than the control, *excuse-needed* to *no-excuse-needed* ( $5.4 \pm 0.24$  percent) and *no-excuse-needed* to *applications sent* ( $5.6 \pm 0.26$  percent). The other two treatment conditions report an average change in turnout higher than the control, *excuse-needed* to *applications sent* ( $8.6 \pm 0.62$  percent) and *no-excuse-needed* to *ballots sent* ( $10.1 \pm 0.51$  percent).<sup>14</sup>

However, these values are naïve estimates. They do not account for potential confounders explaining both VBM policy change and the change in VAP turnout. Therefore, we specify three models aimed at isolating the relationship between VBM policy change and VAP turnout (and vote share, below). We present models with likely confounders as covariates, a county fixed effects estimator, and state clustered standard errors. Table 2 reports the results of these models from left to right, respectively. For our purposes, the critical question our DID estimates address is whether VBM policy changes significantly differ from the average change in the control group's turnout (and vote share) between 2016 and 2020 within each model.

The base, control variable-included, and county fixed effects models report that the change from *excuse-needed* to *no-excuse-needed* and from *no-excuse-needed* to *applications sent* result in negative and significant ( $p < 0.05$ ) change in VAP turnout relative to the change in the control group.

The change in turnout from *excuse-needed* to *applications sent* does not significantly differ from the change in the control group. The change in turnout among counties moving to *ballots sent* is significantly higher than the change in the control group.

In addition to Table 2, Figure 3 illustrates the results of the fully specified model with clustered standard errors. The results suggest that the change in turnout in counties shifting their VBM policy to *ballots sent* is  $2.6 \pm 1.7$  percent higher than the control group's change ( $4.7 \pm 1.2$  percent), yielding a net gain in turnout of 7.3 percent between 2016 and 2020.<sup>15</sup> Changes in turnout among the remaining treatment conditions do not significantly differ from the control group once we account for state-clustered standard errors.

In sum, we find robust evidence that counties automatically sending registered voters mail-in ballots experienced a greater change in turnout compared to the control group, affirming our initial expectation. In contrast, we find suggestive evidence that changes from *excuse-needed absentee* to *no-excuse-needed absentee* and *no-excuse-needed absentee* to *sending applications* resulted in lower turnout relative to the control.<sup>16</sup> Finally, change from *excuse-needed absentee* to *sending applications* did not result in a detectable difference in the change in VAP turnout compared to the control. Thus, our expectation of relatively greater VBM policy liberalization leading to a relatively greater increase in turnout (i.e., that higher dosage yields greater effects) is only partially met.

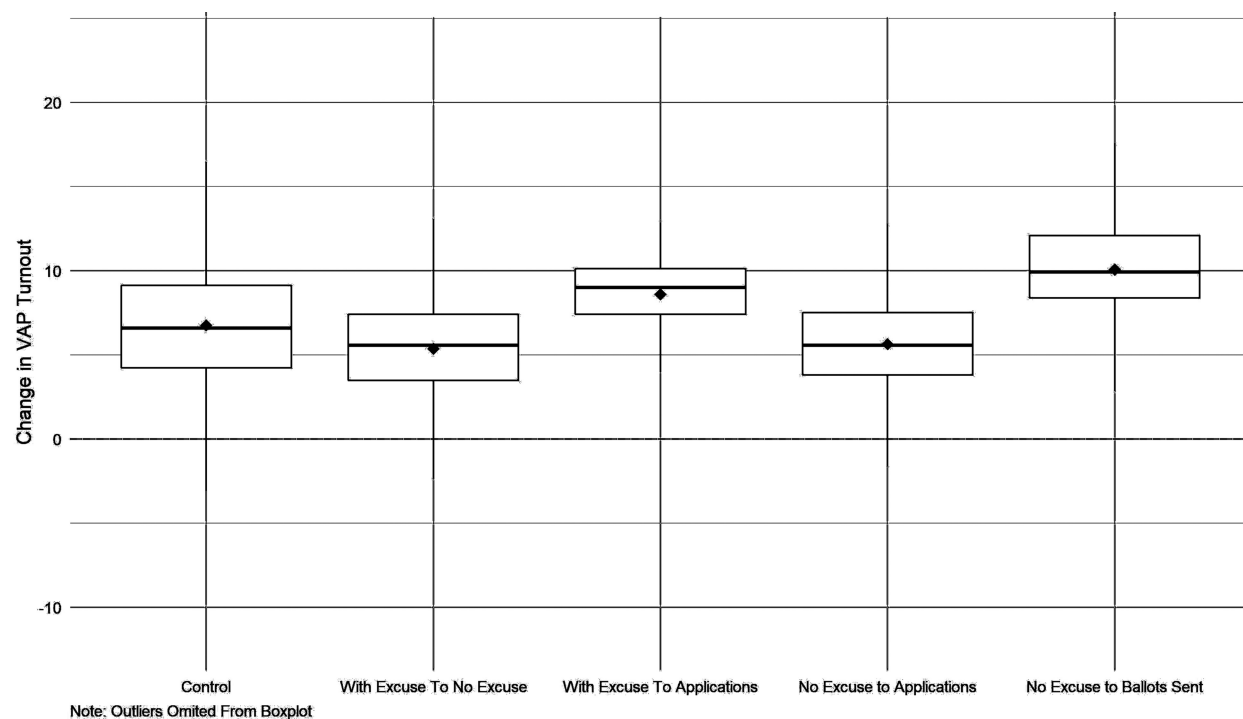
Figure 4 illustrates a boxplot of the change in Republican vote share between 2016 and 2020 for each VBM condition. The left-most boxplot reports that Republican vote share in the control condition decreased  $0.29 \pm 0.14$  percent. This change

<sup>13</sup>These robustness checks can be found in the Supplementary Appendix in Figures 4A and 5A and Tables 3A-7A.

<sup>14</sup>The average overall increase in turnout for all counties between 2016 and 2020 was  $6.4 \pm 0.13$  percent.

<sup>15</sup>Given that California accounts for 53 counties, or about one-third of the observations in the *no-excuse absentee to ballots sent* condition, we test whether California has a disproportionate influence on the estimates presented in the main text. In short, we find that California does not have a disproportionate influence on results seen in *no-excuse absentee to ballot sent condition*. The results are presented in Supplementary Appendix Table 2A and Supplementary Appendix Figure 3A.

<sup>16</sup>Although, again, this difference is rendered insignificant after accounting for correlated standard errors between counties in the same state.



**FIG. 2.** Change in voting-age population turnout between 2016 and 2020. Note: Outliers omitted from boxplot.

represents a slight Democratic swing. The remaining boxplots report the distribution of the change in Republican vote share across each treatment condition. These boxplots mirror the control condition: a slight Democratic advantage.<sup>17</sup> However, as seen in the base model in Table 3 (left-most column), the change in Republican vote share in our four treatment conditions does not differ significantly from the change in the control group.

Table 3 reports the interaction terms representing the DID estimates testing whether there is a partisan advantage to reducing the restrictiveness of VBM policy across four linear models: base, control-included, county fixed effects, and county fixed effects plus state-clustered standard errors (Fig. 5). Each model's results suggest that the change in Republican vote share between 2016 and 2020 in each treatment group does not deviate from the control in a statistically significant manner. Consistent with previous research, these results indicate that VBM liberalization does not hurt or help a party at the ballot box.

## DISCUSSION

Leading up to the 2020 election, unprecedented changes to the VBM policy landscape in the U.S.

fomented endless speculation by politicians and pundits about effects on turnout and partisan electoral prospects. Scholars simultaneously studied previous changes to VBM policies to provide informed predictions about such electoral effects (e.g., Barber and Holbein 2020; Baringer, Herron, and Smith 2020; Bonica et al. 2020; Lockhart et al. 2020; Thompson et al. 2020; West 2020; Yoder et al. 2021). This body of research provides a near-consensus that less restrictive access to the postal ballot box results in higher turnout. However, these studies' empirics are constrained to universal VBM elections.

The COVID-19 pandemic incentivized states and counties to decrease the risk of COVID-19 contagion on Election Day. Their actions to reduce the spread of COVID-19 resulted in two useful methodological opportunities. First, it pushed many counties to change their VBM policies. Second, these changes produced novel variation in VBM policy restrictiveness across the United States. Combined, these opportunities afford our analyses uniquely generalizable and nuanced insight into the electoral

<sup>17</sup>Overall, the average change in Republican vote share for all counties between 2016 and 2020 was -0.56 percent.

TABLE 2. OLS REGRESSION RESULTS FOR THE DIFFERENCE-IN-DIFFERENCE TURNOUT ANALYSIS

	<i>Dependent variable</i>			
	<i>VAP turnout (%)</i>			
	<i>Base (1)</i>	<i>Control (2)</i>	<i>County FE (3)</i>	<i>County FE and state clustered SE (4)</i>
Condition: excuse-needed to no-excuse-needed	-0.473 (0.413)	-0.333 (0.307)	0.329 (0.305)	0.329 (1.717)
Condition: excuse-needed to applications sent	6.185*** (0.889)	0.442 (0.668)	0.194 (0.795)	0.194 (1.333)
Condition: no-excuse-needed to applications sent	6.973*** (0.438)	3.878*** (0.331)	4.046*** (0.344)	4.046*** (1.349)
Condition: no-excuse-needed to ballots sent	3.294*** (0.748)	-2.282*** (0.556)	-0.184 (0.676)	-0.184 (2.611)
Year: 2020	6.714*** (0.341)	4.782*** (0.253)	4.665*** (0.204)	4.665*** (0.592)
% Black		0.027*** (0.008)	0.030*** (0.010)	0.030 (0.047)
Democratic vote share		2.846*** (0.745)	3.278*** (1.010)	3.278 (3.000)
Median income		0.0003*** (0.00001)	0.0003*** (0.00001)	0.0003*** (0.00003)
Median age		0.929*** (0.017)	0.907*** (0.022)	0.907*** (0.068)
% High school degree		-0.331*** (0.016)	-0.337*** (0.019)	-0.337*** (0.051)
Weekly trend in COVID-19 deaths (per 100k)		-0.235*** (0.082)	-0.146* (0.075)	-0.146* (0.078)
Automatic voter registration change: change		0.665** (0.282)	0.421* (0.252)	0.421 (0.847)
Battleground state		1.248*** (0.250)	1.655*** (0.305)	1.655 (1.186)
Condition: excuse-needed to no-excuse-needed x year: 2020	-1.336** (0.584)	-1.301*** (0.427)	-1.418*** (0.334)	-1.418 (0.919)
Condition: excuse-needed to applications sent x year: 2020	1.900 (1.257)	1.423 (0.930)	1.195 (0.732)	1.195 (1.616)
Condition: no-excuse-needed to applications sent x year: 2020	-1.103* (0.619)	-1.158*** (0.446)	-1.177*** (0.349)	-1.177 (0.755)
Condition: no-excuse-needed to ballots sent x year: 2020	3.244*** (1.059)	2.699*** (0.776)	2.568*** (0.610)	2.568*** (0.845)
Constant	57.796*** (0.241)	15.370*** (1.131)	15.245*** (3.911)	15.245*** (5.694)
Observations	6,209	6,168	6,168	6,168
Adjusted R <sup>2</sup>	0.192	0.584	0.747	0.747
Residual std. error	9.172 (df=6199)	6.588 (df=6150)	5.136 (df=4311)	5.136 (df=4311)

Note: \* $p < 0.1$ ; \*\* $p < 0.05$ ; \*\*\* $p < 0.01$ .

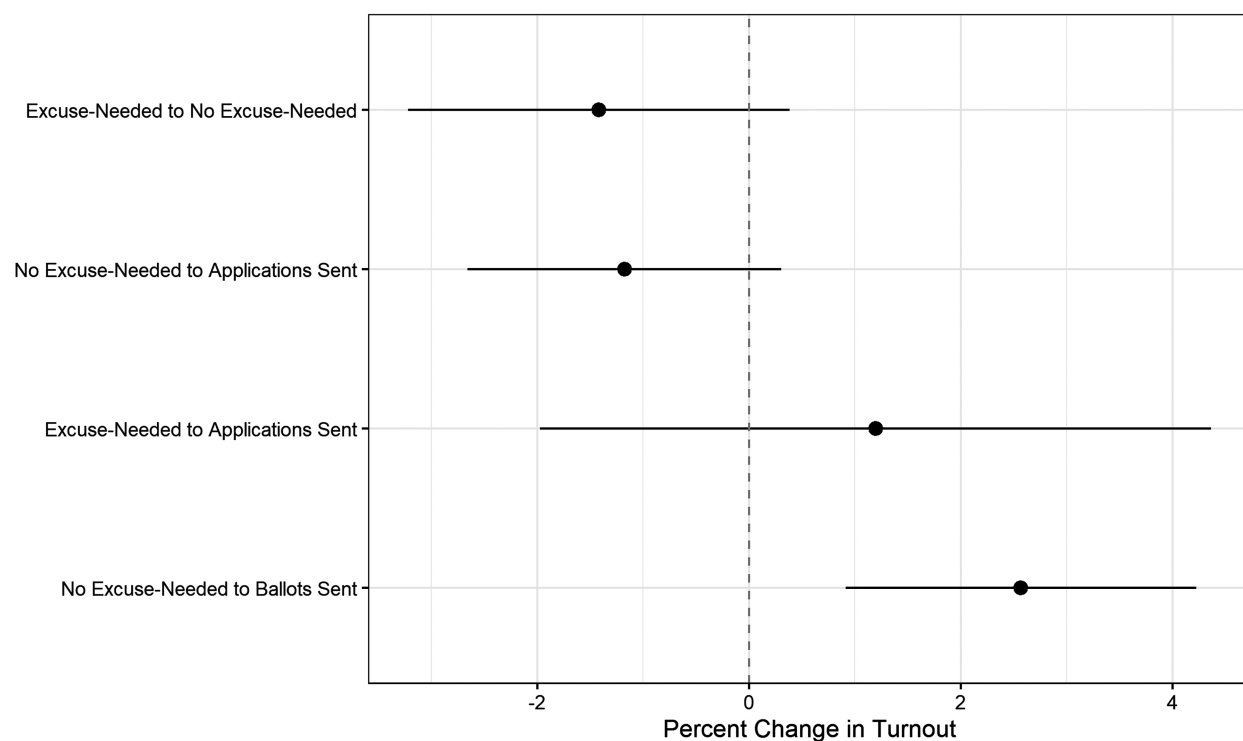
VAP, voting-age population; FE, fixed effects; SE, standard errors.

effects of VBM liberalization. We capitalize on these opportunities by using a two-period DID design to estimate the effects of changes to VBM policy on turnout and vote share.

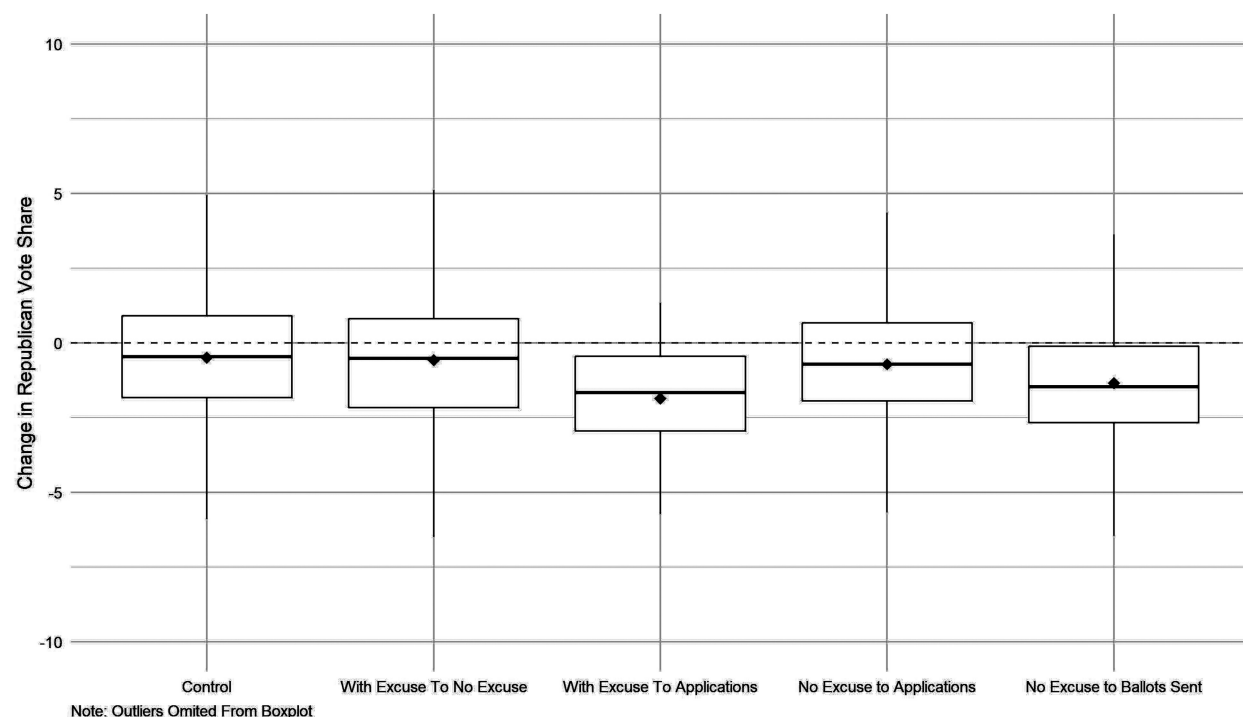
Consistent with previous work, we find no evidence that increasing access to VBM ballots influences parties' vote shares. While counties that liberalized their VBM policy trended relatively Democratic between 2016 and 2020, these effects are statistically uncertain. Further, when we control

for potential confounders, it becomes clearer that any change to the restrictiveness of VBM policy does not confer distinguishable partisan electoral benefits. Still, given the fragility inherent to our two-period design, it should not be ruled out with total certainty that VBM liberalization hurt Trump electorally in 2020.

Our results concerning turnout are considerably more nuanced. We offer robust evidence that sending voters mail-in ballots yields a 2.6 percent



**FIG. 3.** Difference-in-difference estimates of the effects of the vote-by-mail (VBM) conditions on turnout. Note: Point estimates are dots with lines indicating 95 percent confidence intervals. The baseline/reference category is no change in VBM laws. Model controls for Democratic vote share, the weekly trend in COVID-19 deaths (per 100,000), the percentage of African Americans, median household income, median age, percentage of individuals with only a high school diploma, battleground states, change in automatic voter registration, county fixed effects with standard errors clustered at the state level.



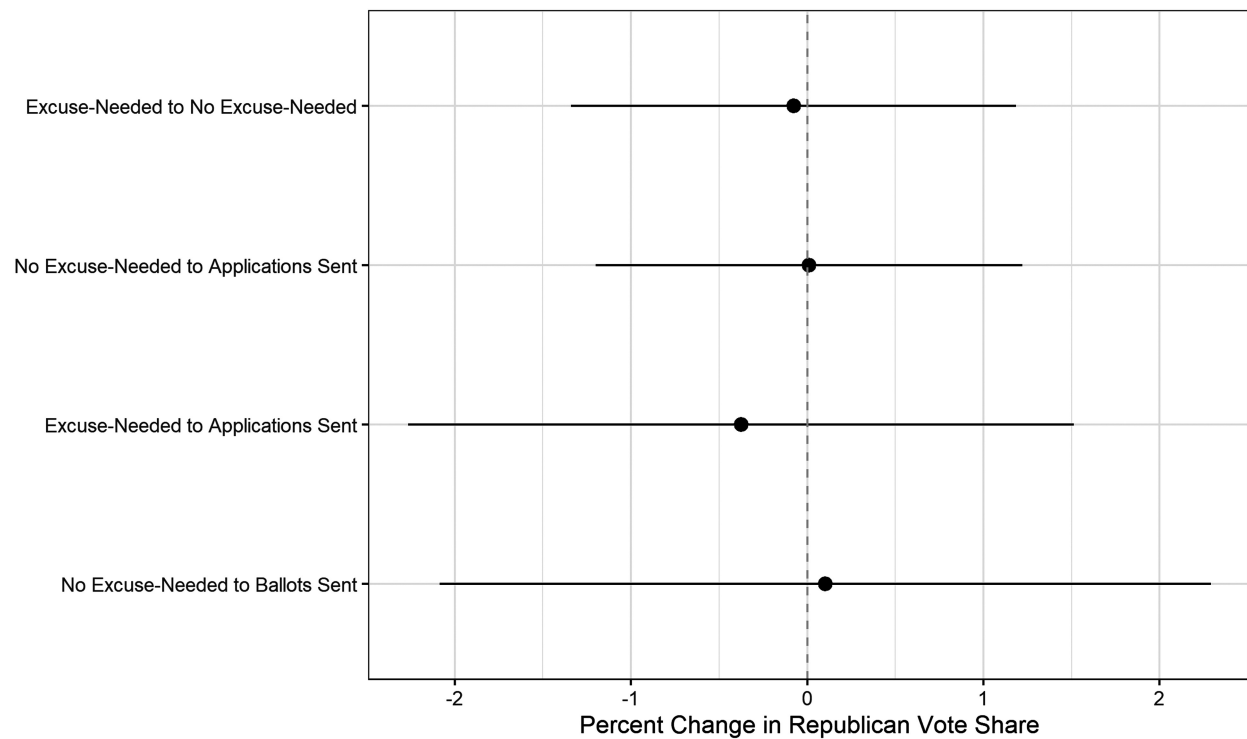
**FIG. 4.** Change in Republican vote share between 2016 and 2020. Note: Outliers omitted from boxplot.

TABLE 3. OLS REGRESSION RESULTS FOR THE DIFFERENCE-IN-DIFFERENCE VOTE SHARE ANALYSIS

	<i>Dependent variable</i>			
	<i>Two-party Republican vote share</i>			
	<i>Base (1)</i>	<i>Control (2)</i>	<i>County FE (3)</i>	<i>County FE and state clustered SE (4)</i>
Condition: excuse-needed to no-excuse-needed	-1.362* (0.712)	-5.643*** (0.492)	-2.622*** (0.440)	-2.622 (1.945)
Condition: excuse-needed to applications sent	-12.323*** (1.527)	-14.320*** (1.043)	-10.117*** (1.127)	-10.117*** (3.747)
Condition: no-excuse-needed to applications sent	-0.869 (0.753)	-7.934*** (0.525)	-6.128*** (0.493)	-6.128** (3.064)
Condition: no-excuse-needed to ballots sent	-12.484*** (1.285)	-8.557*** (0.882)	-6.656*** (0.970)	-6.656 (4.285)
Year: 2020	-0.279 (0.589)	-1.042** (0.452)	0.242 (0.331)	0.242 (0.618)
% High school degree		0.584*** (0.028)	0.449*** (0.029)	0.449*** (0.089)
% Bachelor's degree		-0.421*** (0.042)	-0.103*** (0.032)	-0.103*** (0.037)
% Black		-0.222*** (0.021)	-0.150*** (0.026)	-0.150** (0.074)
% Multiracial		0.319*** (0.019)	0.390*** (0.024)	0.390*** (0.065)
% Hispanic		-0.060*** (0.016)	0.104*** (0.024)	0.104 (0.086)
% Foreign-born		-0.202*** (0.043)	-0.373*** (0.059)	-0.373** (0.150)
Median income		-0.0001*** (0.00001)	-0.0001*** (0.00002)	-0.0001*** (0.00005)
Population density		-0.001*** (0.0001)	-0.001*** (0.0002)	-0.001* (0.001)
Oct. unemployment rate		-1.116*** (0.082)	-0.539*** (0.080)	-0.539** (0.234)
Weekly trend in COVID-19 deaths (per 100k)		0.536*** (0.130)	0.392*** (0.107)	0.392*** (0.082)
Automatic voter registration change: change		4.192*** (0.444)	1.156*** (0.361)	1.156 (1.380)
Battleground state		-1.838*** (0.395)	-2.212*** (0.434)	-2.212 (2.751)
Condition: excuse-needed to no-excuse-needed x year: 2020	-0.337 (1.007)	0.658 (0.676)	-0.077 (0.478)	-0.077 (0.644)
Condition: excuse-needed to applications sent x year: 2020	-1.580 (2.160)	1.379 (1.470)	-0.376 (1.043)	-0.376 (0.963)
Condition: no-excuse-needed to applications sent x year: 2020	-0.437 (1.065)	0.138 (0.706)	0.010 (0.497)	0.010 (0.618)
Condition: no-excuse-needed to ballots sent x year: 2020	-1.218 (1.820)	0.977 (1.231)	0.103 (0.873)	0.103 (1.116)
Constant	68.329*** (0.417)	38.647*** (2.429)	36.342*** (5.878)	36.342*** (7.427)
Observations	6,173	6,168	6,168	6,168
Adjusted R <sup>2</sup>	0.050	0.584	0.795	0.795
Residual std. error	15.757 (df=6163)	10.399 (df=6146)	7.307 (df=4307)	7.307 (df=4307)

Note: \* $p < 0.1$ ; \*\* $p < 0.05$ ; \*\*\* $p < 0.01$ .

FE, fixed effects; SE, standard errors.



**FIG. 5.** Difference-in-difference estimates of the effect of the vote-by-mail (VBM) conditions on two-party Republican vote share. Note: Point estimates are dots with lines indicating 95 percent confidence intervals. The baseline/reference category is no change in VBM laws. Model controls for population density, unemployment one month before Election Day, the weekly trend in COVID-19 deaths (per 100,000), percent of the county with a high school degree and bachelor's degrees, battleground states, change in automatic voter registration, the percent of the county that is Black, multiracial, Latino, and foreign-born, and county fixed effects with standard errors clustered at the state level.

increase in turnout. This estimate is close to Thompson et al.'s (2020) finding of a 2.1 percent bump in turnout from the adoption of universal VBM elections, as well as Gerber, Huber, and Hill's (2013) estimates that largely vary between 2.6 and 3.0 percent. Thus, this finding contributes to a common understanding of how the least restrictive VBM policies influence turnout. We additionally show that previous findings investigating the effect of fully liberalizing VBM policy on turnout in Colorado, Oregon, Utah, and Washington are likely generalizable to other geographic regions.

Even though our data contain only two measurement periods, so that we are unable to verify the parallel trends assumption, our estimates of counties that moved from no-excuse-needed to ballots sent are consistent with those of Barber and Holbein (2020), who correct for possible parallel trends violations in their model estimating the turnout effects from counties that moved to universal vote by mail elections.

Additionally, the effect of sending voters mail-in ballots on turnout may, in part, be a product of other

VBM-related policies that jurisdictions also implemented between 2016 and 2020. For example, changes in paid postage of return mail-in ballot envelopes or changes in the availability of ballot drop boxes likely occurred alongside the changes to the VBM policies we study. However, due to the lack of available, reliable, and nationwide data on such variables, we could not account for them in our analysis. Thus, researchers should incorporate these and other potential confounders into future models as this data becomes available.

Concerning the effects of lesser VBM liberalization, before we cluster standard errors at the state level, we find that counties that either moved from *excuse-needed* to *no-excuse-needed* or from *no-excuse-needed* to *sending mail-in ballot applications* (i.e., counties that received the two smallest VBM policy treatment dosages) experienced a significantly smaller increase in turnout between 2016 and 2020 relative to counties that made no change to their VBM policy. After adjusting standard errors by clustering counties in states, these negative effects are rendered insignificant. Yoder

et al. (2021) estimate the turnout effects of Texas's decision to move to no-excuse-needed absentee for individuals 65 years or older in the state's 2020 primary election. While we provide an average estimate across all counties in a particular treatment condition, their study's design facilitates a more accurate estimate in a particular jurisdiction. That being said, Yoder et al. (2021) also find a null effect for turnout when moving to a no-excuse-needed VBM policy.

However, we also acknowledge the possibility that lower doses of VBM liberalization may have negative impacts on turnout. We speculate that making new rules can, in some cases, impose additional informational costs on voters that may dissuade them from voting (Brady and McNulty 2011; Corvalan and Cox 2018), especially when those new rules only make voting by mail marginally easier. To test this possibility, future research might assess whether there is a difference in turnout change between counties that implemented VBM reforms before and after the 2018 election. For counties that liberalized their VBM policy between the 2016 and 2018 elections, the 2018 midterm election may have afforded many voters the opportunity to address any informational costs associated with a new VBM policy, making it relatively easier for them to vote by mail in the 2020 election. Thus, we would expect the change in turnout between the 2016 and 2020 elections to be higher in counties that implemented a given VBM policy between the 2016 and 2018 elections relative to counties that implemented the same reform between 2018 and 2020.

Alternatively (or perhaps additively), Burden et al. (2014) find that making voting more accessible ahead of Election Day when it is not combined with same-day registration reduces turnout, and theorize that "[l]ocal news coverage, discussions with peers, and Election Day activities all help spur turnout by providing information about candidates and the process of voting, introducing some normative pressure to vote, and enhancing the social benefits of taking part in a collective enterprise. When these activities are diluted, or at least redistributed over time, so is the stimulating effect, particularly for the peripheral voter" (Burden et al. 2014, 97). We find that moving to send voters mail-in ballot applications does not increase turnout and so it remains unclear whether this policy increases the convenience of voting in practice.

Since we only assess turnout at the aggregate level, future studies should seek to understand—at

the individual level—whether voters actually took advantage of these policies, and whether some demographic groups were encouraged more than others to vote by mail. As others have found, VBM liberalization may reduce costs for some voters but increase costs for others (Baringer, Herron, and Smith 2020; Cottrell, Herron, and Smith 2021). Thus while automatically mailing voters VBM ballots may positively influence the likelihood of turnout, on average, some voters may be put at a disadvantage in voting by mail due to higher ballot rejection rates.

Last, VBM policy reform should not solely be judged by turnout or partisan effects. There are many other important aspects of election reform that this study does not evaluate, including implementation costs, as well as voters' perceptions of efficacy, reliability, and election security. Policy recommendations should only be made after careful consideration of the costs and benefits of all factors related to VBM liberalization. This study only robustly claims that implementation of reforms to send voters mail-in ballots between the 2016 and 2020 general elections is associated with a significant increase in voter turnout.

## SUPPLEMENTARY MATERIAL

### Supplementary Appendix

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