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An abstract of
a thesis submitted to the Faculty of Emory College of Arts and Sciences
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Abstract

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The COVID-19 pandemic saw many states change their voting laws in a variety of ways leading up to the 2020 presidential election. Following that election and prior to the midterm election last November, states chose either to make voting even easier or harder, or else maintain a similar cost of voting as before. After broadening my analysis to include the 2016 and 2018 elections, I examine the possible impact that a state's cost of voting had on its voter turnout. I also examine the possibility of a state's partisanship impacting its cost of voting, which would in turn affect turnout, as well as the potential direct impact of partisanship on turnout. In order to determine the success of some states changing their voting regimes in an effort to favor Republicans, I also test the impact of voter turnout on Republican vote share, as well as the impact of a state's cost of voting on Republican vote share. I find that more stringent voting laws do tend to decrease turnout, while the other relationships are less clear.

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Introduction

The COVID-19 pandemic was especially disruptive to the 2020 presidential election. This disruption caused every state in the country to alter how they conducted their elections, although how they did so beforehand varied widely. State rules could differ about voting by mail, voting early, ID requirements, voting equipment, poll hours, and every other aspect of voting prior to 2020, which is why it is so remarkable that each state changed theirs (or had them changed by judicial order) in response to the pandemic. When President Trump declared a national emergency and many states implemented lockdowns of some kind, presidential primary season was also just getting underway (Procter 2022). While 24 states held presidential primaries prior to the declaration on March 13th, 23 changed the date, method of voting, or both for the primaries held afterwards (NCSL 2020). These large-scale changes are by far the most basic and most visible, but they represent only the beginning of the ways in which states modified their elections. Of course, they are also the beginning of the election modification timeline because more developments occurred between the primary and general elections.

It is worth describing in general terms some aspects of voting that were changed in 2020. Drop boxes were one change to voting methods introduced for the first time in some areas, such as Georgia; Georgia is just one state that had no precedent for ballot drop boxes but introduced them for the 2020 elections. Drop boxes are secure containers similar to mailboxes where a voter can simply walk up and drop in a sealed mail ballot. Mail voting, meanwhile, has been a widely established method of voting for decades, but saw its use expanded greatly in 2020 (MIT Election Lab 2021). Finally, some other aspects were more under the radar, such absentee voting excuses, ballot processing time windows, mail ballot postmark requirements and reception deadlines, and more.

Following the November 2020 general elections, states began to diverge in what they chose to do with their voting regulatory regimes. The preexisting diversity in state voting regimes as well as the broad range of changes made for 2020 leaves open a variety of possibilities for state action on voting rules from the rest of November 2020 through the first part of 2022. There are four main paths that each state could have taken. The first is that changes for the 2020 election remained in place, whether via temporary actions enshrined into law or statutes or regulations allowed to stand. The second is that such changes expired, whether by executive, legislative, or judicial action, and pre-pandemic regimes came back into effect. The third is that allowances made for 2020 were expanded open, allowing for even greater permissiveness than early pandemic-era regimes. The fourth is that changes made for 2020 were either reversed or allowed to expire, and furthermore states made new rules even more stringent than those in place prior to March 2020. It is important to note that there is no reason to assume that any given state uniformly followed any one of these paths, and there it is a highly plausible that it pursued different paths for different areas of voting regulations. Again, Georgia is an example here, in that the law passed in March of 2021 retained drop boxes as a voting option, in comparison with the total lack of drop boxes prior to June of 2020, but simultaneously banned and penalized certain types of assistance to citizens waiting in line to vote (Georgia SB 202).

Research Questions

The wide variety of possibilities for states' alterations of their voting regimes after the 2020 general elections means that there is a plethora of different factors combining to affect, or potentially affect, turnout in the 2022 general elections. That means that key factors influencing post-2020 changes may also have influenced changes in 2020. These factors must be examined before any determination can be made about the impact states' new voting regimes may have on voter turnout in November. Several questions are key to establishing this starting fact pattern. The first is, is there a correlation between the partisanship of state governance, defined as the party of the state's governor and legislative majorities, and the types of allowances or lack thereof made for voting in 2020? Answering this question takes the first step in sorting out where things stood when the pandemic began and potentially explains states' actions at that time. The second question is, is there a correlation between the partisanship of state governance, defined as the party of the state's governor and legislative majorities, and the stringency of new voting regimes implemented following the 2020 general election? States had common cause to react in a cautious manner in 2020, whether or not they acted on it, but partisanship may have played a bigger role in contrasting choices in 2021 and beyond. The third and final question is, did factors not directly able to affect changes to voting regimes, such as a presidential candidate's win margin of a state and racial polarization, influence changes made to voting regimes following the 2020 general elections? Answering this question is important because while partisanship is an important factor in state actions, it is highly plausible that governors and legislatures did not act purely on the basis of partisanship. The preliminary questions all build up to the primary question of this thesis, which is, is there a correlation between stringency of voting regimes and turnout change across the 2016, 2018, 2020, and 2022 elections? The 2020 and 2022 elections are the main subjects of interest because they are the two COVID-era federal general elections. Nevertheless, it is worthwhile to also examine the two preceding elections of 2016 and 2018 in order to both make comparisons with those pre-COVID elections and determine if any correlation between voting regime stringency and voter turnout also existed prior to 2020.

The significance of this question has multiple facets. One is for state governments themselves. They likely want to find out how their regulations affected voter turnout, if at all, and naturally must look to the results of the first major general election for which the regulations were in place, as compared to previous election in which they were not, to do so. Depending on the state, the goal could be to boost or suppress turnout in various sectors of the population, or even as a whole. States may then have a further interest in changing voting regimes again or leaving the 2022 iterations in place. Meanwhile, scholars would have multiple reasons to be interested in the question. The first is to measure the effects that regulatory barriers or facilitators to voting actually have. The second is to determine the impact of various measures on turnout when accounting for electoral circumstances, such as a midterm as opposed to a presidential election year. Finally, the answers to this question begin to fill in the gap of scholarship on the impact of the 2022 voting regimes on elections, as that body of work by definition can only begin to be constructed after the election.

Literature Review

While scholars have studied rules and regulations touching on various areas of voting, there has not been any research presenting a comprehensive view of all of the types of regulations and their impacts on turnout. This makes sense, because the plethora of rules, rule types, and their interactions make it difficult to tease out the effects of each particular rule or even type of rule. Research on a particular aspect of voting, or several related aspects of voting is much easier to come by. For example, one relatively recent paper studies election-day registration, early voting, and same-day registration and their effects on turnout but examines the impact of several other factors as well (Burden et al 2014). They calculated the correlation of several different combinations of these options with turnout. Based on the data from the 2004 and 2008 presidential elections, they find that states with early voting, when implemented by itself, saw a slight but negative impact on turnout. This corroborates findings from an earlier paper that early in-person voting has a negative impact on turnout,(Gronke et al 2007). At the same time, states with both early voting and same day registration saw no significant correlation with turnout, nor did those with both early voting and election day registration. Those states with either early voting, same day registration, and election day registration, or just election day registration, saw a small but positive correlation with turnout. These findings provide some background for what would take place in 2020.

Nonetheless, there are some key distinctions between the state of voting in 2008 and 2019, prior to the beginning of pandemic-related voting changes. One is that 31 states had some form of universal early voting then, while by 2019, 41 did (NCSL 2019). Another is that in the 2008 election, 17 states had same day registration in effect, while by 2019, 21 did (NCSL 2019). The same National Conference of State Legislatures (NCSL) article shows that no state implemented it for 2020 (ibid). Moreover, the proportion of vote cast early has increased drastically since 2008. Setting aside the unique circumstances of the 2020 election, the proportion of the total vote cast before election day or by absentee ballot increased from 30.7% in 2008 to 40.1% in 2016 (US Census Bureau 2020). This is roughly a 25% increase in the proportion of the vote cast early. In 2020, due to the pandemic, early or absentee voting increased to a stratospheric 69.4% of total votes cast, itself roughly a 75% increase over 2016 (ibid). Here the Census Bureau combines early in-person and absentee voting but absentee votes are cast before election day in any case. A Pew survey classified all absentee ballots as being cast either “before the week leading up to Election Day” or “in the week leading up to Election

Day", suggesting that absentee votes were uniformly early votes as well as opposed to both early votes and votes by mail (Pew Research 2020). Regarding the specific impact of early in-person voting in 2020 on turnout, a 2022 dissertation found that despite the large increase in early voting, it did not cause a significant increase in turnout, except for black voters (Murphy 2022).

In contrast, the introduction of drop boxes was a novel addition to available voting methods in 2020, and therefore existing literature that predates 2020 covers the relatively small part of the country that had drop boxes at that time. Washington state was one of the few jurisdictions that used ballot drop boxes prior to 2020. Since 2011, state law has required counties to provide drop boxes to return ballots. A 2018 paper examined the impact of drop boxes on turnout after King County (Seattle) greatly expanded its use of drop boxes (Collingwood et all 2018). The authors found that decreased distance from a drop box increased likelihood to vote and turnout, but simultaneously discovered that this effect was crowded out by other factors that influenced the 2016 general election. Collingwood et al is the only paper I found about drop boxes in a presidential election prior to 2020, so it is significant to point out that the 2011 law that provided for drop boxes also switched Washington's voting system to be almost entirely vote-by-mail. Thus, it was particularly important to have ballot drop boxes as a ballot return measure in addition to post offices. This means that while the data out of Washington and King County specifically do provide an idea of the impact of drop boxes, it is difficult to fully generalize this data when considering states that were not entirely vote-by-mail as Washington is. Still, another paper studying the 2017 general election in Washington, this time in Pierce County, corroborated Collingwood et al's finding that decreased distance to a drop box was correlated with an increase in turnout (McGuire et al 2020). At the very least, this corroboration reinforces expectations about the impact of drop boxes.

In 2020, only five states (Colorado, Hawaii, Oregon, Utah, and Washington) had universal vote-by-mail in place, while four states (California, Nevada, New Jersey, and Vermont) and the District of Columbia implemented it either for the first time that year or just for the general election (NCSL 2020). This makes the 2016 effects of drop boxes more salient in those locations. Other states, though they did not implement universal vote-by-mail, either introduced drop boxes temporarily for 2020 or already. So, while the concurrent presence of traditional voting methods made drop boxes comparatively less important than in universal vote-by-mail states, the two aforementioned papers still provide a baseline for their potential impact. The states that introduced drop boxes temporarily for November 2020 are New York, Pennsylvania, Texas, and Virginia, while Arizona, Kansas, Montana, Nebraska, and New Mexico had laws permanently allowing them (NCSL 2020). Even though there is accessible information about which states had a drop box option in 2020, only one paper attempted to predict their impact in the 2020 general election. The authors expected based on the August primary in Washington state as well as the earlier literature that the presence of new drop boxes in more jurisdictions would increase turnout in November (Collingwood and Gonzalez O'Brien 2021).

Voting by mail writ large presents a stark contrast to drop boxes, because of its much longer history and its universal presence across the country, albeit with variations on who is eligible to do so. The MIT Election Lab notes that voting by mail first became available for soldiers on both sides of the Civil War, while states introduced it for sick and traveling civilians near the end of the nineteenth century (MIT Election Lab 2021). California introduced the first widely available mail voting in the 1980s. By the 2018 elections, four states sent ballots automatically to voters, sixteen required excuses to receive an absentee mail ballot, and thirty sent ballots to voters upon request with no reason necessary (Ballotpedia 2019). The literature about the impact of voting by mail prior to 2020 tends to show that it does have some positive impact on turnout. A 2007 paper found that vote by mail (VBM), which at the time had only been instituted in its present form in Oregon, had a small but statistically significant positive impact on turnout solely in presidential years (Gronke et al 2007). As Oregon's current VBM regime was first employed in 1998, these results only apply to the presidential election years of 2000 and 2004. Regarding more recent elections, relevant scholarship comes to similar yet stronger conclusions. One paper focused on Colorado's all mail elections found that the state's switch to all-mail voting in 2013 caused an overall significant increase in turnout and higher turnout specifically among known low propensity voting groups such as students, blue collar workers, and non-college-educated voters in 2014. It also noted that in 2016 and 2018, Colorado maintained higher turnout than its non-all-mail-voting neighbors (Bonica et al 2021).

For this paper and the Gronke et al paper, the caveat must be made that states that conduct elections solely by mail voting cannot be easily compared to states that do not. To elaborate, if mail voting does have a positive effect on turnout compared to other voting methods, results from a state with all mail voting will not fully generalize to states with a mix of voting methods. Nonetheless, the fact that mail voting has a positive effect on turnout in elections in two states up to fourteen years apart reinforces the possibility of similar effects in other jurisdictions. Indeed, a paper that studied the impact of voting by mail in Utah, Washington, and California found a moderate increase in turnout with a negligible partisan differential (Thompson et al 2020). For this paper, the inclusion of California for the years the authors studied means that they included counties that voted only by mail within a state that otherwise did not. Unfortunately, the lack of intrastate comparison between the 5 all-mail counties and the 53 others makes it difficult to tease out a simple contrast between mail voting and all other methods.

While the 50 states and the District of Columbia implemented a wide variety of changes to their voting regimes in 2020, it is difficult to say whether these changes alone had an appreciable impact on turnout. One paper examined each state's use of no-excuse absentee voting or lack thereof as well changes in turnout (Yoder et al 2021). The authors find that for the states that changed to no-excuse absentee voting for the first time in 2020, there was no significant impact on turnout. More specifically, in comparing state-level turnout changes between 2016 and 2020, those states that implemented no-excuse absentee voting had an increase in turnout slightly higher than the states that did not. The comparative turnout diagram between the two groups of states also shows that the states that had no-excuse absentee voting prior to 2020 already had had higher turnout than those that did not in several previous presidential elections. This suggests a similarity amongst the states in the former group that has no connection to their 2020 expansion of voting methods, an idea supported by their increase in turnout from 2012 to 2016 even as turnout declined a minuscule amount in the latter group. Yoder et al also demonstrate negligible impact of absentee voting on turnout through a natural experiment involving two states which only had no-excuse absentee voting for voters 65 and older, Texas and Indiana. Both states saw an increase in absentee voting for that age group, but concurrently saw a decrease in early in-person voting and Election Day voting, making the net turnout effect of absentee voting nil. Finally, the third primary finding of this paper, extrapolating from the natural experiment in Texas and Indiana is that no-excuse absentee voting likely did not turn out any appreciable number of lower propensity voters and further had no significant partisan turnout effect. Meanwhile, another paper found that counties that sent ballots to registered voters experienced higher turnout (Amlani and Collitt 2022). Among the possible changes to mail voting conditions adopted in 2020, only sending ballots, as opposed to sending absentee ballot applications or eliminating the excuse requirement to vote absentee, has this positive turnout effect.

The past few papers examined the impact on turnout of some election administration changes adopted for the 2020 election, with a goal of setting expectations for how those and similar changes would affect turnout in the 2022 midterm election. However, the goal of this thesis is ultimately to examine the impact partisan considerations, if any, had on changes in election administration and in turn on turnout. With that in mind, it is necessary to refer to literature that does explicitly consider the partisan element in states' voting regime changes. One paper published in April of this year noted that sixteen states made voting by mail easier in some way for the 2020 election (Herrnson et al 2022). Of those, California, Nevada, the District of Columbia, New Jersey, and Vermont switched to universal voting by mail (VBM), while Missouri, Arkansas, Alabama, South Carolina, Kentucky, West Virginia, Virginia, Delaware, New York, Connecticut, and New Hampshire either eliminated the excuse requirement for absentee voting or allowed concern about COVID-19 to be a valid excuse. Four of the five states that switched to universal vote by mail (VBM) had their governorship and legislature controlled by Democrats, as did four of the states that eliminated or loosened their absentee excuse requirement. The other eight states controlled by Democrats either already conducted universal VBM elections or already had no excuse requirement. Among the 22 states controlled by Republicans, five eliminated or loosened their absentee excuse requirement, one already had universal VBM, twelve already had no excuse requirement, and four made no change. Among the thirteen states with divided government, one changed to universal VBM, two eliminated or loosened their absentee excuse requirement, nine already had no excuse requirement, and one made no change.

Accounting for the fact that many states already had no excuse requirement, the authors suggest that Republican control of at least some part of state government meant that no change or lesser change, i.e., eliminating the excuse requirement but not switching to universal VBM, was more likely. They also find that party control may have also influenced decisions to mail absentee ballot applications to all registered voters. Among the 40 states that did not conduct a universal VBM election, four out of eight states under Democratic control did so, compared to five out of twelve states under divided government and two out of twenty states under Republican control. Herrnson et al also corroborate Amlani and Collitt's finding that mailing ballots to voters unsolicited had a positive impact on turnout, while mailing absentee ballot applications did not.

One reference that Herrnson et al use in determining how difficult states made voting is to Schraufnagel et al's Cost of Voting Index (COVI). Every four years, the authors of the COVI examine many different aspects of voting and calculate the cost of voting each state imposes upon its citizens, and so is worth analyzing on its own (Schraufnagel et al 2020). The issue areas under consideration for 2020 were a state's voter registration deadline (number of days before the election a voter must be registered), voter registration restrictions, registration drive restrictions, pre-registration laws, automatic voter registration, voting inconvenience, voter ID laws, poll hours, and early voting. Every issue area was combined into one index number, and then the states were ranked. Index scores ranged from -1.69 for Oregon, the easiest state to vote in in 2020, to 1.29 for Texas, the hardest state to vote in in 2020.

The authors did not specifically account for state partisanship, but using Ballotpedia's list of state government trifectas, where a trifecta means control of the governorship and both chambers of the state legislature, it is possible to do so. Prior to November 2020, 21 states had a Republican trifecta, 15 states had a Democratic trifecta, and 14 states had divided government (Ballotpedia 2022). Looking again at the COVI scores, 12/15 states with Democratic trifectas (80%) had negative scores, meaning that their regulations overall made voting easier. 4/21 of states with Republican trifectas (19%) had negative scores, while 7/14 of states with divided government (50%) did. The inverse of this, of course, is that 3/15 (20%) of Democratic states had positive scores, meaning their regulations overall made voting harder, while 17/21 (81%) of Republican states and 7/14 (50%) of states with partisan splits did. To be fair, the index rankings do not necessarily reflect changes made by the 2020 election, which is why ranking changes from 2018 to 2020 may be more instructive as to the possible impact of partisanship on state voting stringency. The authors compiled a chart of state ranking changes, and from there it is possible to see that in 8/15 (53%) of Democratic states, voting became easier relative to other states in 2020, compared to 8/21 (38%) of Republican states and 5/14 (36%) of split states. Meanwhile, in 6/15 (40%) of Democratic states, voting became harder relative to other states, compared to 12/21 (57%) of Republican states and 9/14 (64%) of split states. Oregon, a Democratic state, and Alabama, a Republican state had no ranking change from 2018 to 2020. Considering the authors' caveat that a state's drop in the rankings could be attributed in part to taking no action when other states made voting easier, it still seems highly plausible that partisanship played a role in changes to state voting regimes.

When considering all of the above literature, it remains evident that prior research does cover many different aspects of voting and election administration, but no one paper provides one comprehensive perspective. Collectively it discusses different kinds of voting as well as actions that facilitate voting and their impact on turnout, both prior to 2020 and for the 2020 election itself. Herrnson et al (2022), Amlani and Collitt (2022), and Schraufnagel et al (2020) provide the clearest insight into the impact of voting changes made in 2020, and so set reference points for the potential impact of voting changes made by Election Day 2022 on turnout. Given that the 2022 elections were conducted fairly recently as of this writing, there is not yet any literature that analyzes this impact. However, the corpus of earlier scholarship means that neither will it be entirely new territory. Moreover, a February 2022 column for the Crystal Ball, the University of Virginia's Center for Politics' election handicapping and analysis site, by Professor Alan Abramowitz of Emory University sets another baseline expectation for 2022: Abramowitz believes that despite the efforts of many states to make voting harder after the 2020 presidential election, turnout in those states will not be hindered by such "voter suppression efforts" (Abramowitz 2022). He corroborates previous findings that drop boxes and mailing ballots to voters had significant but small positive effects on turnout, and further finds that no voting modality had any significant impact on Democratic presidential vote share. Thus, he reasons that efforts by states to reduce access to nontraditional voting modalities such as drop boxes and mail are unlikely to either reduce turnout or reduce Democratic vote share in 2022.

Theory

Examining the potential impact of partisanship on changes to state voting rules and consequent impact on turnout requires scrutinizing partisan motivations for making those changes. Just as in 2020, Schraufnagel et al's Cost of Voting Index provides some information on this front. As mentioned above, state partisanship seems to have played a role in influencing states' costs of voting in 2020. Examining the 2022 Index helps to affirm whether this continued to be the case in 2022. The authors note that states amended or passed so many voting laws after 2020 that it became necessary and worthwhile for the first time to calculate values for a midterm election. For the 2022 edition of the index, their main alteration was to create a tenth issue area dedicated solely to absentee voting laws. The other changes include items for Election Day wait times, postage requirements for ballots, and giving food and water to voters in line to vote under the voter inconvenience sub-index, among others. Under the parameters of the new index, the easiest state to vote in in 2022 was Oregon, with a score of -2.54, while the hardest state to vote in was New Hampshire, with a score of 1.69.

Once again, the authors did not directly account for state partisanship, but it is possible to do so with Ballotpedia's trifecta list. As a result of the 2020 and 2021 elections respectively, Montana and New Hampshire gained Republican trifectas, while Virginia fell under divided control. This put the number of Republican trifectas at 23, the number of Democratic trifectas at 14, and the number of divided states at 13 (Ballotpedia 2022). The index scores themselves provide the first clue as to the continuing influence of partisanship. In 2022, 13/14 (93%) of Democratic states had negative scores, meaning their regulations made voting easier overall by the authors' calculations. Meanwhile, 4/23 (17%) of Republican states had negative scores, and 6/13 (46%) of divided states did. The Republican and divided percentages did not change markedly from their 2020 values of 19% and 50%. However, the Democratic number rose 13% from 80%. As in 2020 it is mathematically necessary for the inverse to be true for all three groups of states. 1/14 (7%), 19/23 (83%), and 7/13 (54%) of Democratic, Republican, and divided states, respectively, had positive index scores. These numbers do appear to show that Republican control of state governments made more restrictive voting regimes more likely.

Republican trifectas had by far the largest proportion of positive index scores, followed by divided governments (where Republicans control at least one part of the government), and then by Democrats.

Because changes to state voting laws are being studied, changes to state COVI rankings should have more explanatory power. In the context of the COVI, these changes are the best proxy for whether states made their voting regimes more or less stringent, at least relative to other states. Schraufnagel et al did not make the ranking change table available in the 2022 edition, so I have made comparisons by hand. An important consideration to make it that as absentee voting became a new issue of the COVI, many states experienced ranking changes attributable in part to their absentee voting rules. 7/14 (50%) Democratic states made voting easier relative to other states, while 12/23 (52%) of Republican states and 5/13 (39%) of divided states did so. Meanwhile, 4/14 (29%) of Democratic states made voting harder relative to other states, while 11/23 (48%) of Republican states and 7/13 (54%) of divided states did so. The remaining three Democratic states and one divided state had no ranking change. Particularly given that those three Democratic states had negative scores and therefore already made voting easier overall, these numbers appear to provide more evidence for the notion that having Republicans control one or more part of a state's government made it more likely that that state would make voting harder.

A FiveThirtyEight analysis of new state voting laws passed in 2021 and 2022 also seems to show a connection between the partisanship of state governments and the passage of more stringent voting laws. The authors show that 24 states passed laws that in some way made it harder for their citizens to vote (Mejia and Samuels 2022). Of those, 17 (71%) were controlled solely by Republicans, 2 (8%) were controlled solely by Democrats, and 5 (21%) had divided governments. To make the contrast even more stark, those 24 states passed 56 new laws. Republican states passed 46 (82%) of them, while Democratic states passed 3 (5%) and divided states passed 7 (12.5%). The analysis also breaks out the number of restrictive measures regarding each component of voting each law implemented. The components addressed are voter registration, mail ballot application, mail ballot return, early voting, Election Day voting, and ballot verification. From there it is possible to see that Democratic states restricted one component of voting in one way at minimum (New York), and four components of voting in one way each at maximum (Nevada). Divided states restricted one component of voting in one way at minimum (Louisiana, North Carolina) and three components of voting in two ways at maximum (Kansas). In another stark contrast, Republican states restricted one component of voting in one way at minimum (Nebraska and Mississippi), which is the same as Democratic and divided states, but all six components of voting in five ways at maximum (Georgia). The states that passed the most distinct restrictions were Georgia, Florida, Texas, and Arkansas, all states with strong Republican trifectas. From several different angles, this analysis makes clear that partisanship affects the stringency of state voting regimes; Mejia and Samuels demonstrate even more clearly than Schraufnagel et al do with the COVI that Republican control of a state government means it is more likely to make voting harder.

To provide another dimension to the partisan influence on state voting laws, multiple news reports showed that lies and conspiracies about the 2020 election played a part in states' passage of so many new restrictive laws. One by the *New York Times* cited quotes from a Republican lawmaker in Iowa and the text of the Georgia voting law itself which referenced low voter confidence and concerns about the 2020 process, both stemming from beliefs in problems with the 2020 election that do not exist (Astor 2021). Another FiveThirtyEight piece notes the pervasiveness of the “Big Lie” that the 2020 election had widespread fraud and systematic issues with voting by mail (Rogers 2022). It describes the spread and adoption of the lie amongst Republican voters, activists, and state legislators. Some of the latter even acknowledge using it as a front without believing it.

State voting laws were also intended to have racially disparate impacts or were introduced because of white racial resentment, according to Kevin Morris of the Brennan Center for Justice (Morris 2022). In an analysis of laws states passed in 2021, Morris finds several connections between race and voting restrictions, at both the district and state levels. In states with a mostly white population, defined as 80 percent white, there is a small positive correlation between the white proportion of a legislative district and the probability that that district’s legislator sponsored a voting restriction. This is true in both the lower and upper chambers of a state legislature. Conversely, in states with a more diverse population, defined as 50 percent white, the correlation is much stronger. After controlling for several variables that represent partisanship, the correlations for mostly white states remain small and become nil or negative, but the those in more diverse states remain almost as strong. Morris finds even stronger correlations between a state legislative district’s racial resentment score and the probability that its legislator sponsored a voting restriction. He uses racial resentment scores from the 2020 Cooperative Election Study. Again, after controlling for partisanship, the correlations become weaker but remain clear, meaning that partisanship cannot explain the entire relationship between racial resentment and voting restrictions. Finally, on the state level, he finds a strong relationship between more diverse populations and the passage of restrictive voting laws, provided that states had a Republican trifecta. This comports with the findings in the COVI and the FiveThirtyEight analysis of voting restrictions that state partisanship influenced burdens placed on voters exercising their franchise.

Using the above evidence about the impact of partisanship on changes to state voting regimes, as well as the impact of various voting modalities on turnout, I have constructed five hypotheses:

H1: Stringency of a state's voting regime correlates negatively with voter turnout

H2: Republican state partisanship correlates positively with stringency of voting laws

H3:Republican state partisanship correlates negatively with voter turnout

H4: Voter turnout correlates negatively with Republican two-party vote share at the state level

H5: Stringency of a state's voting regime correlates positively with Republican two-party vote share at the state level

Research design

For the first and fifth hypotheses, stringency of voting regime is the explanatory variable, while it is the response variable in H2, because of the two-step process of state partisanship affecting voting regimes, which in turn affect turnout. For H2 and H3, state partisanship as measured by party control of the governorship and state legislature is the explanatory variable. Republican two-party vote share is the response variable in H4 and H5. Having it as an additional explanatory variable helps to account for any differences between state partisanship in the case of state government and voters' support for a party across both state and federal contests. Voter turnout is the response variable for H1 and H3, while it is the explanatory variable in H4.

The data collection and calculation methods for the explanatory and response variables are as follows. As mentioned above, state partisanship is measured solely by party control of the governorship and state legislature. This data was collected from Ballotpedia, which has a page dedicated to tracking state government trifectas. There are a couple of important things to note about state trifecta status. One is that Nebraska has a unicameral and officially nonpartisan but functionally Republican-controlled legislature, so only the governorship and state senate are needed for the trifecta. The other is that while Alaska has a majority of seats in its state house filled by Republicans, some Republicans caucus with the Democrats and independents in a coalition that does not strictly align with members' party membership, and thus Republicans do not have a trifecta there. For stringency of state voting laws, I used COVI scores, as it is not practically feasible to construct an altered index that only includes voting issues directly addressed in the literature review.

For Republican two-party vote share, I used data from Dave Leip's Election Atlas. The Atlas compiles election results at the nation, state, and county level for president, governor, and senator. As I am looking at differences between states, results at the state level are all that is needed in this case. The Atlas includes vote shares for third party and independent voters, so to calculate Republican two-party vote share, I divided the Republican percentage by the combined Republican and Democratic percentage. In a few unique cases, such as the two-Democrat Senate races in California in 2016 and 2018, there was no Republican, so I by necessity omitted those races in order to maintain a consistent measure of Republican vote share. Different statewide races in the same state may have had different partisan outcomes and vote shares, so averaging

Republican two-party vote shares is the best way to approximate overall support for the party in a given election year. I averaged presidential, gubernatorial, and senatorial vote shares in each state, as applicable, for 2016, 2018, 2020, and 2022.

Averaging vote shares across statewide races is also helpful because even within the same state, comparing election years is not apples-to-apples. Every state holds a presidential election in a presidential year, and states vary as to when they hold gubernatorial elections. Some do so in presidential years, most do so in midterm years, and a handful do so either in the odd-numbered year before a presidential election or the odd-numbered year before a midterm election. The two exceptions are Vermont and New Hampshire, which elect governors in every even-numbered year. Additionally, Oregon held a special gubernatorial election in 2016 following the resignation of the incumbent governor. Senators are the main reason presidential and midterm election years are not exact analogues to each other in the same state. Because senators are elected for six-year terms, a senatorial seat that was previously up for election in a presidential year would next be up in a midterm year, and vice versa. As each state elects two senators, a given state could elect one senator in a presidential year and the other, or no senator at all, in the next. The same is true for midterm years. Moreover, states regularly hold special elections to fill their Senate seats following the resignation or death of the incumbent. Those special elections are unique to that election year. They may be for the same seat that was up two or four years previously, but because of their circumstances, may look very different from that preceding regular election. Special senatorial elections provide a further reason for averaging statewide vote shares: double-barreled Senate elections, which occur when a state holds special and regular elections for each of its Senate seats respectively at the same time. I also computed separate averages for each office for later comparison, to see whether elections for each office have noticeably different correlations with turnout.

Alongside these explanatory and response variables, potential confounding variables must also be considered. One is the presence of a statewide race on the ballot because turnout is likely to be lower in a midterm election year in a state without any gubernatorial or senatorial contest. In midterm years, most states did have either a governor or a senator up for election, if not both. In order to determine the impact of a statewide race, I intended to compute correlations with and without the states that did not have one. Had the absence of a statewide race had a significant impact on turnout, I would also calculate the correlations between voting regime stringency and turnout just among those states. Making a comparison just between those states would sidestep any disparities in turnout between them and the majority of states that do have statewide races. In constructing my dataset, it became clear that too few states lacked a key statewide race in a given election year to make separate calculations without them, which will be addressed further in the discussion section.

Another potential confounding variable is the Supreme Court's *Dobbs v. Jackson* decision, which may have had a positive effect on both turnout and Democratic vote share. If Democratic voters were disproportionately inspired to turn out in response to the decision in order to protect abortion rights, the disproportionate turnout could mask the possible negative impact of voting laws on turnout that would have occurred otherwise. Conversely, Republican voters could have been inspired to turn out, as the decision created new possibilities for state measures limiting access to abortion. In order to determine whether *Dobbs* could have disproportionately influenced turnout in either direction, I would input the percentage of voters in each state who believed access to abortion should be protected, according to polls. As several states also had abortion measures on the ballot, I would indicate that as well. If there is no correlation between the support for abortion rights and turnout, then it likely had no significant impact. A second way to measure this variable would be to account for abortion access or restriction measures already in place, because it is possible that voters were spurred to turn out by *Dobbs* only in states where abortion rights were at risk.

A third confounding variable is inflation or other economic concerns, which may have spurred some voters both to turn out and vote for the Republican party as the party out of power in the federal government. Here too I would input the percentage of voters concerned about economic issues and see if there is a correlation between this percentage and turnout. A fourth is presidential approval rating, because states with higher disapproval of President Biden likely voted in larger numbers for Republicans. If there were no correlation between Biden disapproval and Republican vote share, then it is possible that disparities in Republican vote share between states could instead be explained by voting regime stringency. To help to account for nationwide confounding variables, I would note both national changes in turnout and composition, known as the vote swing, and compare to them state changes. How a state changed relative to the nation is known as its trend. If a state's change in turnout and Republican vote share diverged significantly from national changes, then it becomes more likely that factors at the state level played a role in the changes.

Data construction, results, and analysis

The first data I collected for my analysis was simply the trifecta status for each state. As mentioned above, this meant visiting the Ballotpedia page on state trifectas and noting the governance of each state. For 2020, meaning prior to the 2020 general elections, I created one column for the governor's party and one for that of the legislature. In 2020, all 50 states, as well as the District of Columbia (DC), had a governor who was either a Republican or a Democrat. For the sake of this analysis, the mayor of DC, was treated as a governor. Meanwhile, 49 out of 51 legislatures were completely controlled by Republicans or Democrats. The two exceptions were Alaska, which had a state senate controlled by Republicans and a state house controlled by a coalition of Democrats, Republicans, and independents, and Minnesota, where Democrats held a majority in the state house and Republicans held a majority in the state senate. Also as mentioned above, Nebraska's unicameral legislature is officially nonpartisan but functionally Republican, so it is labeled as such. Here as well, the DC city council is treated as a legislature. I calculated two-party Republican vote share as described in the research design, by dividing the Republican vote share by the combined Republican and Democratic vote share. I did so for all 51 jurisdictions for the 2016 and 2020 presidential elections, as well as senatorial, gubernatorial, special senatorial, and special gubernatorial elections, as required, for the 50 states.

All turnout data was taken from the US Elections Project, which is run by University of Florida professor Michael McDonald. McDonald personally calculated the turnout rates for each state and year using data from state and local government sites, among other places. One important note about the 2022 turnout data is that McDonald for some reason does not have the data for Kansas, Kentucky, Minnesota, and Pennsylvania marked as "final", "certified", or "official" (the term varies by state). However, checking each state's relevant governmental site showed that those four states had in fact certified their 2022 general election results. Cost of Voting Index (COVI) values are taken from the full spreadsheet provided by Schraufnagel et al on their website (Cost of Voting Index 2022). A critical consideration for the use of this data in the four elections I am analyzing, 2016, 2018, 2020, and 2022, is that the COVI values for 2016 and 2018 are the same (Schraufnagel et al 2022). Schraufnagel et al write that though they calculated COVI values for presidential elections from 1996 through 2020, the plethora of changes to election administration states made in 2021 and 2022 necessitated separate calculations for a midterm election for the first time (ibid). Although I therefore must use the same values for 2016 and 2018, any analyses which include those years may still be informative, because they could show comparative effects of the same voting regime in a presidential and midterm election year. To account for year and state fixed effects, I created dummy variables for 2016, 2018, 2020, and 2022, and all 51 jurisdictions, and marked each data point accordingly. My dataset ultimately consisted of 204 data points representing the elections in each state in each year. Due to the various constraints placed on models as described below, the number of data points used in each varied, and many could not use all 204.

The first set of regressions conducted focused on the relationship between COVI and turnout. Before calculating actual regressions, I plotted turnout against COVI, to see if any relationship could be detected visually. The plot is below–*see Fig. 1*. Just by looking at the plot, I could see that there was overall a slight negative slope. The error band around the fitted line also trended downwards, except for at the very end. As Schraufnagel et al calculated the COVI such that negative values indicated a lower burden on voting and positive values indicated a higher burden on voting, this was the first indication, albeit not a strong one, that as states made voting more difficult, their voters tended to turn out at lower rates. The actual regression of COVI on turnout returned a $\hat{\beta}_1$ of -0.0249, significant at the α=0.01 level–*see Table 1 (1)*. This would mean that for each theoretical unit increase of COVI in a state, its turnout would decrease by about two and a half percentage points. Given that the range of turnout on the fitted line has a maximum of roughly 0.65, or 65% turnout, and a minimum of roughly 0.53, or 53% turnout, $\hat{\beta}_1$

is relatively large. Upon reexamination of the graph, I noticed that the overwhelming majority of state data fit between COVI values of -1 and 1. After creating a subset that limited COVI to that range, I again ran the regression of COVI on turnout, to see if there was a stronger relationship with outliers excluded. It returned a $\hat{\beta}_1$ of -0.0299 that was significant only at the α=0.05 level, meaning there was a tradeoff between slope and significance that did not change my overall impression of the relationship between the two variables–*see Table 1 (2)*.

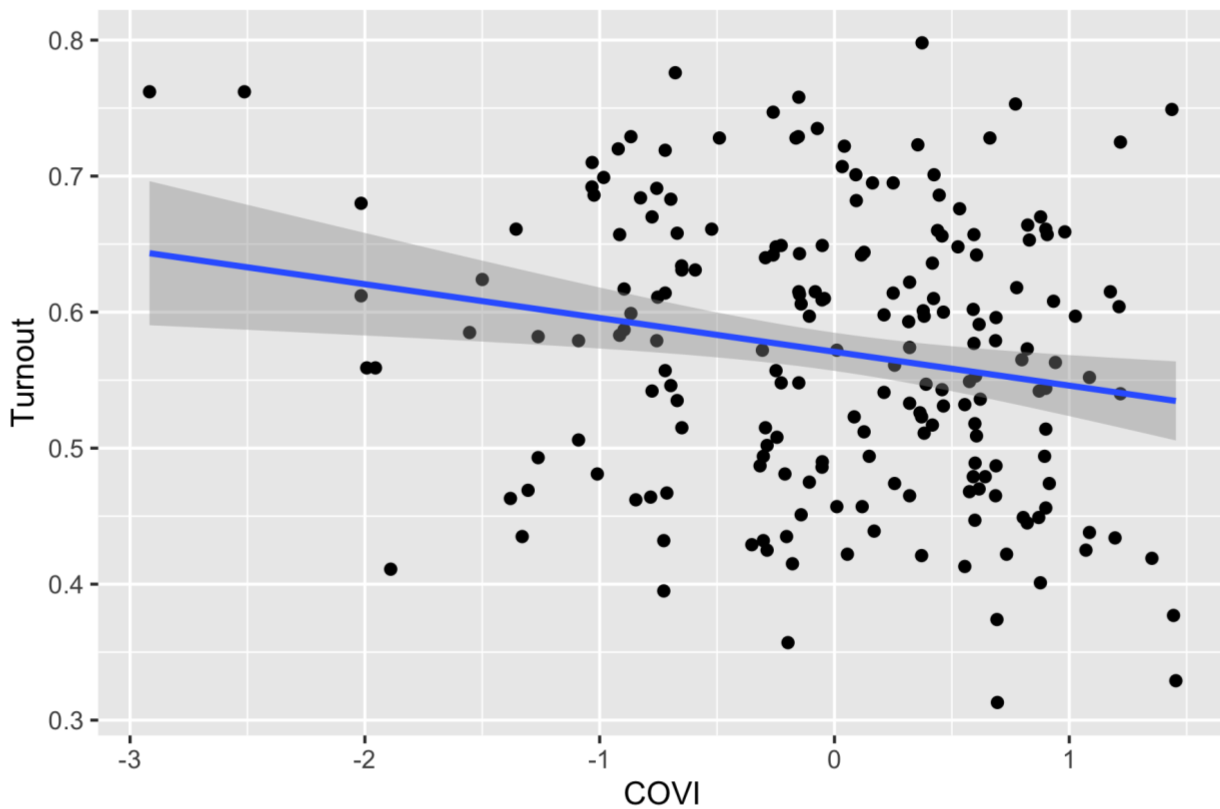

Fig. 1–a plot of turnout against COVI with the linear model and error bands added in

Influence of COVI on voter turnout

	(1)		(2)		(3)	
Predictors	*Estimates*	*t value*	*Estimates*	*t value*	*Estimates*	*t value*
(Intercept)	0.5708 *** (0.0071)	80.3105	0.5756 *** (0.0077)	74.6696	0.6160 *** (0.0088)	70.3183
COVI	-0.0248 ** (0.0089)	-2.8002	-0.0299 * (0.0137)	-2.1783	-0.0241 (0.0130)	-1.8586
Observations	200		166		50	
R^2 / R^2 adjusted	0.038 / 0.033		0.028 / 0.022		0.067 / 0.048	

* *p<0.05* ** *p<0.01* *** *p<0.001*

	(4)		(5)		(6)	
Predictors	*Estimates*	*t value*	*Estimates*	*t value*	*Estimates*	*t value*
(Intercept)	0.5143 *** (0.0078)	66.1625	0.6768 *** (0.0081)	83.3576	0.4763 *** (0.0100)	47.7326
COVI	-0.0355 ** (0.0115)	-3.0925	-0.0204 * (0.0090)	-2.2593	-0.0237 * (0.0109)	-2.1840
Observations	50		50		50	
R^2 / R^2 adjusted	0.166 / 0.149		0.096 / 0.077		0.090 / 0.071	

* *p<0.05* ** *p<0.01* *** *p<0.001*

Table 1–models on the full dataset, on the no-outlier subset, and on the four year subsets

I next created subsets for each election year, to determine if the relationship between COVI and turnout changed in any meaningful way between years. The regression of COVI and turnout, limited to 2016 data, returned a $\hat{\beta}_1$ of -0.0241, significant only at no level–*see Table 1 (3)*. This is interpreted to mean that voter turnout did tend to decrease as a state's burden on voting increased, but as it had little statistical significance, perhaps just because the model used less data. Another possible explanation for the decreased statistical significance is 2016's status as a presidential year. In a presidential election, turnout would certainly be higher across the board compared to the average of two presidential and two midterm elections. Thus, it is plausible that more people turned out in spite of voting restrictions, even if restrictions still correlated negatively with turnout. The 2018 subset regression returned a $\hat{\beta}_1$ of -0.0355, significant at the α=0.01 level–*see Table 1 (4)*. This means that for 2018, not only did voter turnout decrease more rapidly as the burden on voters increased than it did across all four years, but its statistical significance was greater as well. Comparison with 2016 is also instructive. Average turnout across all states was over ten points lower in 2018 than it had been in 2016, and voters are generally less inclined to turn out for midterm elections (Jackson 2000). The greater statistical significance of the slope corroborates the idea that a state's COVI could have a stronger impact in a midterm election. The 2020 subset regression returned a $\hat{\beta}_1$ of -0.02041 significant at the α=0.05 level–*see Table 1 (5)*. This again corroborates the idea that presidential elections would show a weaker and statistically less significant relationship between COVI and turnout. Moreover, 2020 had higher turnout than 2016, which may explain the smaller slope, assuming higher turnout does mean more voters overcome the burden placed on them casting their votes, but it does not explain the greater statistical significance of the slope in 2020 versus 2016. It is possible that outliers in the 2016 subset do explain it. The 2022 subset regression returned a $\hat{\beta}_1$ of -0.0237, also significant at the α=0.05 level–*see Table 1 (6)*. As with 2018 compared to 2016, 2022 showed a stronger relationship between COVI and turnout than did 2020, the presidential election that preceded it. The weaker statistical significance in 2022 as compared to 2018 may be related to state-specific or other nationwide effects, some of which will be analyzed later in this paper.

The next regressions in this set attempted to account for year and state effects. Year fixed effects would be otherwise unaccounted for phenomena affecting turnout in states in a particular election year. These would be nationwide, as they are common to all states in the same election cycle. They could be the effects on turnout of the presidential race, for example, in 2016 and 2020, the pandemic, in 2020 and 2022, or higher enthusiasm for voting compared to other midterms, in 2018. State fixed effects, meanwhile, account for phenomena specific to a given state, such as the competitiveness of statewide races, the presence of an incumbent on the ballot, or the accessibility of polling places. I first ran a regression identical to the original one, except that I also included the 2018 and 2022 state fixed effects. Doing so would show possible midterm-year-specific effects on turnout. This regression returned a $\hat{\beta}_1$ of -0.0249, this time significant at the $\alpha \approx 0$ level, along with estimates for the 2018 and 2022 year fixed effects which were both also significant at the $\alpha=0.001$ levels–*see Table 2 (1)*. It has a higher intercept because the presidential years are the baseline, while the year fixed effects for the midterm years of 2018 and 2022 show a significant deviation from the presidential years. Furthermore, accounting for whether a year is a midterm election year or not makes the model stronger overall and increases the statistical significance of the COVI-turnout slope.

For another angle on year effects, I then ran a regression with COVI and year fixed effects for 2016, 2018, and 2020, leaving 2022 as the baseline. This regression returned a $\hat{\beta}_1$ of -0.0248, significant at the $\alpha=0.001$ level, along with significant 2016, 2018, and 2020 fixed effects–*see Table 2 (2)*. With every election year accounted for, the model is even stronger. Each year has a large and statistically significant fixed effect on turnout, and the p-value of the slope decreases as well. I next ran another regression that added every state fixed effect, omitting DC because it lacked COVI values and excluding Wyoming to make it the baseline state. The inclusion of state fixed effects decreased the slope as well as its statistical significance, possibly meaning their collective explanatory power gave COVI a lesser impact on turnout. This regression returned a $\hat{\beta}_1$ of -0.0138, significant at the α=0.05 level, along with statistically significant 2016, 2018, and 2020 year fixed effects–*see Table 2 (3)*. The regression returned a range of estimates for the 49 state fixed effects which were statistically significant for 25 of them, suggesting that factors particular to a given state mattered much more than its cost of voting. However, the effects of nineteen states are not statistically significant at all, leaving just COVI and the election year as the statistically significant estimates in those states. The state fixed effects are not shown in the table to save space.

The final regressions in this set tested for interactions between year and COVI, to see if a given year influenced the impact of COVI on voter turnout. I first ran a regression of COVI on turnout, adding the 2016 year fixed effect as well as the interaction between COVI and 2016–*see Table 3 (1)*. The regression returned a $\hat{\beta}_1$ of -0.0250, significant at the α=0.01 level, and a statistically significant 2016 term. These values both comport with those from earlier regressions that include those terms. The new term of interest, the interaction between COVI and the 2016 year fixed effect, returned a minuscule estimate significant at no level. This indicates that a state's election being held in 2016 had no influence on the impact of COVI on turnout. The second regression included the interaction of COVI and 2018–*see Table 3 (2)*. The regression returned a $\hat{\beta}_1$ of -0.0225, significant at the α=0.05 level, and a statistically significant 2018 term. The interaction between COVI and the 2018 year fixed effect returned an estimate with no statistical significance, meaning that a state's election being held in 2018 also had no influence on the impact of COVI on turnout. The next regression included the interaction between COVI and the 2020 dummy variable–*see Table 3 (3)*. The regression returned a $\hat{\beta}_1$ of

-0.0269, significant at the α=0.01 level, and a statistically significant 2020 term. The interaction between COVI and the 2020 year fixed effect returned an estimate with no statistical

Influence of COVI on voter turnout

	(1)		(2)		(3)	
Predictors	*Estimates*	*t value*	*Estimates*	*t value*	*Estimates*	*t value*
(Intercept)	0.6464 *** (0.0065)	99.8111	0.4763 *** (0.0087)	55.0477	0.4605 *** (0.0139)	33.1239
COVI	-0.0248 *** (0.0057)	-4.3464	-0.0248 *** (0.0054)	-4.6012	-0.0138 * (0.0055)	-2.4990
X2016			0.1398 *** (0.0122)	11.4243	0.1398 *** (0.0053)	26.4796
X2018	-0.1322 *** (0.0112)	-11.7816	0.0380 ** (0.0122)	3.1059	0.0380 *** (0.0053)	7.1988
X2020			0.2005 *** (0.0122)	16.3902	0.2005 *** (0.0053)	37.9898
X2022	-0.1702 *** (0.0112)	-15.1693				
Observations	200		200		200	
R^2 / R^2 adjusted	0.605 / 0.599		0.649 / 0.642		0.951 / 0.933	

* *p<0.05* ** *p<0.01* *** *p<0.001*

Table 2–models with midterm year fixed effects, all year fixed effects, and all year and state fixed effects

significance. The fourth regression, which included the interaction between COVI and the 2020 year fixed effect, had comparable results–*see Table 3 (4)*. The regression returned a $\hat{\beta}_1$ of -0.0254, significant at the α=0.01 level, and a statistically significant 2022 year fixed effect. The interaction between COVI and the 2022 year fixed effect returned an estimate with no statistical significance. The fact that there was no significant interaction between COVI and any election year implies that COVI was a reliable predictor across years. To see if there was anything missed by finding interaction estimate for each year separately, I ran a regression with interaction terms for each year, using 2022 as the baseline–*see Table 3 (5)*. It returned similar coefficients as the earlier regression with just COVI and years–a $\hat{\beta}_1$ of -0.0237, significant at the α=0.05 level, along with statistically significant 2016, 2018, and 2020 year fixed effects. The interactions of COVI with each year returned estimates with no statistical significance. This corroborates the reliability of COVI as a predictor across years.

Influence of COVI on voter turnout

	(1)		(2)		(3)		(4)		(5)	
Predictors	*Estimates*	*t value*	*Estimates*	*t value*	*Estimates*	*t value*	*Estimates*	*t value*	*Estimates*	*t value*
(Intercept)	0.5558 *** (0.0080)	69.7902	0.5897 *** (0.0078)	75.7001	0.5355 *** (0.0065)	82.0949	0.6024 *** (0.0069)	87.1596	0.4763 *** (0.0087)	54.7532
COVI	-0.0250 ** (0.0095)	-2.6339	-0.0225 * (0.0093)	-2.4248	-0.0269 ** (0.0085)	-3.1556	-0.0254 ** (0.0091)	-2.7853	-0.0237 * (0.0095)	-2.5052
X2016	0.0603 *** (0.0159)	3.7840							0.1398 *** (0.0123)	11.3632
X2018			-0.0754 *** (0.0156)	-4.8421					0.0380 ** (0.0123)	3.0892
X2020					0.1413 *** (0.0130)	10.8291			0.2005 *** (0.0123)	16.3025
X2022							-0.1261 *** (0.0138)	-9.1236		
Observations	200		200		200		200		200	
R^2 / R^2 adjusted	0.104 / 0.090		0.142 / 0.129		0.399 / 0.389		0.325 / 0.315		0.651 / 0.638	

* *p<0.05* ** *p<0.01* *** *p<0.001*

Table 3–models with year fixed effects and their interaction terms

The second set of regressions, examining the relationship between state partisanship and COVI followed in the pattern of the first set. As aforementioned, trifecta status represented state partisanship because control of the governorship and state legislature necessarily determine the relative stringency or permissiveness of changes to the cost of voting in a state. The 51 states (including DC) had one of three statuses at any given time: Democratic trifecta, Republican trifecta, or split government. The easiest way to represent this for the purpose of running regressions was to create one dummy variable for Democratic trifectas and one dummy variable for Republican trifectas and assign each state to either if applicable or neither if the partisanship of its government was split in a given year. As COVI values could reasonably be expected to change over the course of a given two-year election cycle, I used Ballotpedia's maps of state trifecta statuses as of the election in each even-numbered year. That is, I used the "State government trifectas, pre-2016 elections" map for the 2016 elections and so on and so forth, because a state's trifecta status by Election Day 2016 was the most recent over the 2015-2016 cycle. Each state's trifecta status following an election was the same as its trifecta status preceding the next election, with the only exceptions being states that also hold elections in odd-numbered years. For the 2016-2022 period, Kentucky's trifecta status changed from Republican to split in 2019 when it elected Gov. Andy Beshear (D), and that of Virginia changed first in 2019 to Democratic when the legislature flipped to Democratic control and then in 2021 upon the election of Gov. Glenn Youngkin (R) as well as a Republican majority in the state senate. The overall impact of these three off-year elections should be minimal.

Unlike with the regressions of COVI on turnout, nearly every regression in this set had to have two primary variables, representing the two partisan levels of trifecta status. Additionally, the intercepts became much more important because they represented the states with split government. The first regression in this set simply regressed COVI on the Democratic trifecta and Republican trifecta variables. It returned a $\hat{\beta}_0$ (split government) of -0.1121, significant at no level, a $\hat{\beta}_1$ (Democratic trifecta status) of -0.6437, significant at the α=0.001 level, and a $\hat{\beta}_2$ (Republican trifecta status) of 0.5388, also significant at the α=0.001 level–*see Table 4 (1)*. This means that a state with a Democratic trifecta would have a COVI value 0.6437 lower than a state with split government, and a state with a Republican trifecta would have one that was 0.5388 higher. Considering that COVI values only range from about -3 to 1.5, both trifecta effects are very large. The fact that the intercept does not differ significantly from zero is not especially important, because it is reasonable for states with split governments to impose costs of voting that are neither especially stringent nor especially permissive. I next ran two regressions, each with only one of the trifecta variables, to double check that the intercepts would gain statistical significance when the other was omitted. I expected this to be the case because then, the intercepts would effectively be accounting for both split governments and states with a trifecta controlled by the other party. The first, which regressed the Democratic trifecta variable on COVI, returned a $\hat{\beta}_0$ of 0.207, significant at the α=0.001 level, and a $\hat{\beta}_1$ of -0.9629, also significant at the α=0.001 level–*see Table 4 (2)*. The second, which regressed the Republican trifecta variable on COVI, returned a $\hat{\beta}_0$ of -0.3708, significant at the α=0.001 level, and a $\hat{\beta}_1$ of 0.7974, also significant at the α=0.001 level–*see Table 4 (3)*. Both regressions placed the intercept estimate between the intercept estimate and omitted party trifecta estimate from the first regression, which makes sense, given that the intercepts represented the data from all states but those with the chosen party's trifecta.

The next group of regressions focused on each election year. As with the regressions of voter turnout on COVI, I used four election year subsets to see how the estimates changed in each. For these and all subsequent regressions in this set, $\hat{\beta}_0$ is the split government estimate, $\hat{\beta}_1$ is the Democratic trifecta estimate, and $\hat{\beta}_2$ is the Republican trifecta estimate. The regression of COVI on 2016 trifecta status, returned a $\hat{\beta}_0$ of -0.2515 and a $\hat{\beta}_1$ of -0.3859, both significant at no level, and a $\hat{\beta}_2$ of 0.6643, significant at the α=0.001 level–*see Table 4 (4)*. These values are lower, higher, and higher, respectively, than their counterparts from the initial regression. This may provide a hint that trifectas from both parties made voting easier in later years. The reason that the statistical significance of the split government estimate increased and that of the Democratic trifecta estimate decreased may be because there were simply so few Democratic trifectas in 2016 (7 excluding DC) and a larger number of split governments (20). The regression of COVI on 2018 trifecta status, returned a $\hat{\beta}_0$ of -0.1862 and a $\hat{\beta}_1$ of -0.5464, both significant at no level, and a $\hat{\beta}_2$ of 0.5051, significant at the α=0.001 level–*see Table 4 (5)*. There is little useful interpretation to be made of this regression because 2018 lacks COVI values. Thus, the estimates only change because some trifectas changed hands in the 2016 elections. The regression of COVI on 2020 trifecta status, returned a $\hat{\beta}_0$ of 0.1323, significant at no level, a $\hat{\beta}_1$ of -0.9169, significant at the α=0.01 level, and a $\hat{\beta}_2$ of 0.3399, significant at no level–*see Table 4 6)*. These values are all lower than the overall estimates, lending some credence to the notion that all states made voting easier in 2020. The regression of COVI on 2022 trifecta status, returned a $\hat{\beta}_0$ of -0.064, significant at no level, a $\hat{\beta}_1$ of -0.7319, significant at the α=0.05 level, and a $\hat{\beta}_2$ of 0.5846, also significant at the α=0.05 level–*see Table 4 (7)*. In this case, the split government estimate is very close to zero, likely explaining its lack of statistical significance. The Democratic trifecta estimate is slightly higher, possibly indicating some reversion to the mean among those states and likely explaining its decreased statistical significance, while the Republican trifecta estimate is appreciably higher, likely explaining its increased statistical significance. An estimated increase of about 0.37 from 2020 to 2022 is appreciable given the small range of the index. This matches my expectation that while states tended to make voting easier in 2020, Republican states changed course to make it harder in 2022.

Influence of partisanship on COVI

	(1)		(2)		(3)		(4)	
Predictors	*Estimates*	*t value*	*Estimates*	*t value*	*Estimates*	*t value*	*Estimates*	*t value*
(Intercept)	-0.1121 (0.0826)	-1.3575	0.2070 *** (0.0559)	3.7048	-0.3708 *** (0.0676)	-5.4892	-0.2515 (0.1257)	-2.0017
Dem trifecta	-0.6437 *** (0.1303)	-4.9400	-0.9629 *** (0.1205)	-7.9909			-0.3859 (0.2468)	-1.5635
GOP trifecta	0.5388 *** (0.1073)	5.0196			0.7974 *** (0.0991)	8.0493	0.6643 *** (0.1718)	3.8660
Observations	200		200		200		50	
R^2 / R^2 adjusted	0.330 / 0.323		0.244 / 0.240		0.247 / 0.243		0.351 / 0.323	

* *p<0.05* ** *p<0.01* *** *p<0.001*

	(5)		(6)		(7)	
Predictors	*Estimates*	*t value*	*Estimates*	*t value*	*Estimates*	*t value*
(Intercept)	-0.1862 (0.1410)	-1.3205	0.1323 (0.1996)	0.6628	-0.0641 (0.2104)	-0.3045
Dem trifecta	-0.5464 * (0.2611)	-2.0930	-0.9169 ** (0.2775)	-3.3038	-0.7319 * (0.2922)	-2.5043
GOP trifecta	0.5051 ** (0.1813)	2.7861	0.3399 (0.2577)	1.3191	0.5846 * (0.2633)	2.2207
Observations	50		50		50	
R^2 / R^2 adjusted	0.306 / 0.276		0.351 / 0.323		0.359 / 0.332	

* *p<0.05* ** *p<0.01* *** *p<0.001*

Table 4–models on the full dataset, on each trifecta term individually, and on the four year subsets

For the next group of regressions, I first created a subset encompassing 2020 and 2022, the two pandemic elections. This was in order to find any possible difference between those two years, and the dataset as a whole. The regression of COVI on trifecta status, across 2020 and 2022, returned a $\hat{\beta}_0$ of 0.0377, significant at no level, a $\hat{\beta}_1$ of -0.8278, significant at the α=0.001 level, and a $\hat{\beta}_2$ of 0.4597, significant at the α=0.05 level–*see Table 5 (1)*. As expected, these estimates all fall between the separate 2020 and 2022 values, and when compared to the overall regression, provide some evidence that states tended to make voting easier in those years. The next regression included the full dataset but added the 2020 and 2022 year fixed effects. That regression, of COVI on trifecta status with the 2020 and 2022 indicators, returned a $\hat{\beta}_0$ of -0.1664, significant at no level, a $\hat{\beta}_1$ of -0.6762, significant at the α=0.001 level, and a $\hat{\beta}_2$ (Republican trifecta status) of 0.5328, also significant at the α=0.001 level–*see Table 5 (2)*. Neither year indicator has a statistically significant estimate. It makes sense that there would be no statistically significant year effects on COVI, because unlike with turnout, which varies depending on the kind of election year, the decisions of state legislatures to pass laws that change their COVI values are much more important than what kind of election year it is. The third regression in this group measured the impact of all year fixed effects by including 2016, 2018, and 2020, leaving 2022 as the baseline. It returned a $\hat{\beta}_0$ of -0.0566, significant at no level, a $\hat{\beta}_1$ of -0.6754, significant at the α=0.001 level, and a $\hat{\beta}_2$ of 0.5341, also significant at the α=0.001 level–*see Table 5 (3)*. As before, the year fixed effects had no statistical significance.

Conversely, factors unique to each state may have influenced its legislature's passage of voting laws, so it would be reasonable to expect some number of statistically significant state fixed effects. Therefore, the last regression of this group included all year and state fixed effects, again with 2022 as the baseline year and Wyoming as the baseline state. This regression returned a $\hat{\beta}_0$ of 0.4139, not significant at any level, a $\hat{\beta}_1$ of -0.3753, significant at the α=0.01 level, and a $\hat{\beta}_2$ of 0.1437, also statistically insignificant–*see Table 5 (4)*. The year fixed effects once again had no statistical significance. An interesting change from previous regressions is that the split government estimate rose markedly, as did the Democratic trifecta estimate, while the Republican trifecta estimate noticeably decreased. The reason for this is very likely to be the various state fixed effects. Out of the 49 indicated states, 20 were statistically significant. The states with negative estimates were intent on making voting easier, and the states with positive estimates were intent on making it harder. In fact, in many states, the state fixed effect eclipses the trifecta effect. This means that although a state's trifecta status does have some predictive power over its relative ease of voting, other factors particular to that state may be more important. As the model with all year and state fixed effects only returned a significant Democratic estimate, it is almost as if the relationship between partisanship and COVI is only strong moving from more to less Republican states.

Influence of partisanship on COVI

	(1)		(2)		(3)		(4)	
Predictors	*Estimates*	*t value*	*Estimates*	*t value*	*Estimates*	*t value*	*Estimates*	*t value*
(Intercept)	0.0377 (0.1430)	0.2639	-0.1664 (0.0919)	-1.8114	-0.0566 (0.1201)	-0.4714	0.4139 (0.2334)	1.7732
Dem trifecta	-0.8278 *** (0.1987)	-4.1658	-0.6762 *** (0.1325)	-5.1015	-0.6754 *** (0.1329)	-5.0820	-0.3753 ** (0.1347)	-2.7857
GOP trifecta	0.4597 * (0.1817)	2.5309	0.5328 *** (0.1075)	4.9566	0.5341 *** (0.1079)	4.9505	0.1437 (0.1246)	1.1534
X2016					-0.0945 (0.1338)	-0.7061	-0.0525 (0.0790)	-0.6640
X2018					-0.1265 (0.1335)	-0.9476	-0.0611 (0.0794)	-0.7699
X2020			0.1455 (0.1160)	1.2538	0.0349 (0.1326)	0.2633	0.0133 (0.0770)	0.1727
X2022			0.1106 (0.1158)	0.9551				
Observations	100		200		200		200	
R^2 / R^2 adjusted	0.352 / 0.338		0.336 / 0.322		0.336 / 0.319		0.834 / 0.772	

* $p<0.05$ ** $p<0.01$ *** $p<0.001$

Table 5–models with the pandemic subset, pandemic year fixed effects, all year fixed effects, and all year and state fixed effects

The final group of regressions in this set included interaction terms for the year fixed effects. The first added 2016 and its interaction term, and returned a $\hat{\beta}_0$ of -0.0488, significant at no level, a $\hat{\beta}_1$ of -0.7301, significant at the α=0.001 level, and a $\hat{\beta}_2$ of 0.4799, also significant at the α=0.001 level–*see Table 6 (1)*. The 2016 year fixed effect and the two interaction terms had no statistical significance. This makes sense because if the year effect itself was insignificant, the influence of the year on each trifecta variable's impact on COVI should also be insignificant. The interaction regression with the 2018 year fixed effect returned a $\hat{\beta}_0$ of -0.0854, significant at no level, a $\hat{\beta}_1$ of -0.675, significant at the α=0.001 level, and a $\hat{\beta}_2$ of 0.5538, also significant at the α=0.001 level–*see Table 6 (2)*. The 2018 year fixed effect and the two interaction terms again lacked statistical significance. Little can be said about this regression other than that all of the estimate changes compared to the previous regression are attributable only to trifecta shifts and not COVI shifts. The interaction regression with the 2020 year fixed effect returned a $\hat{\beta}_0$ of -0.1806, significant at no level, a $\hat{\beta}_1$ of -0.5599 significant at the α=0.001 level, and a $\hat{\beta}_2$ of 0.5939, also significant at the α=0.001 level–*see Table 6 (3)*. The year fixed effect and interaction terms once again lacked statistical significance. The interaction regression with the 2022 year fixed effect returned a $\hat{\beta}_0$ of -0.1244, significant at no level, a $\hat{\beta}_1$ of -0.6121. significant at the α=0.001 level, and a $\hat{\beta}_2$ of 0.5201, also significant at the α=0.001 level–*see Table 6 (4)*. Again, the lack of statistical significance for the year fixed effect and interaction terms in this regression comports with that of the earlier regressions.

The last regression of this set included interaction terms for all years, with 2022 as the baseline. This regression returned a $\hat{\beta}_0$ of -0.0641, significant at no level, a $\hat{\beta}_1$ of -0.7319, significant at the α=0.01 level, and a $\hat{\beta}_2$ of 0.5846, significant at the α=0.05 level–*see Table 6 (5)*. The three year fixed effects and all six interaction terms were not statistically significant, as with those in the individual year interaction regressions. This provides some evidence that as with COVI and turnout, years do not have additional influence on the impact of trifecta status on COVI values. The main difference between the first set of regressions and this one is that state fixed effects seem to be more significant.

Influence of partisanship on COVI

	(1)		**(2)**		**(3)**		**(4)**		**(5)**	
Predictors	*Estimates*	*t value*	*Estimates*	*t value*	*Estimates*	*t value*	*Estimates*	*t value*	*Estimates*	*t value*
(Intercept)	-0.0488 (0.1000)	-0.4878	-0.0854 (0.0968)	-0.8816	-0.1806 (0.0935)	-1.9303	-0.1244 (0.0931)	-1.3366	-0.0641 (0.1854)	-0.3456
Dem trifecta	-0.7301 *** (0.1491)	-4.8982	-0.6750 *** (0.1470)	-4.5914	-0.5599 *** (0.1561)	-3.5859	-0.6121 *** (0.1546)	-3.9601	-0.7319 ** (0.2575)	-2.8427
GOP trifecta	0.4799 *** (0.1276)	3.7612	0.5537 *** (0.1263)	4.3844	0.5939 *** (0.1218)	4.8771	0.5201 *** (0.1224)	4.2510	0.5846 * (0.2319)	2.5207
X2016	-0.2028 (0.1789)	-1.1337							-0.1875 (0.2381)	-0.7873
X2018			-0.1008 (0.1879)	-0.5366					-0.1221 (0.2463)	-0.4958
X2020					0.3129 (0.2000)	1.5642			0.1964 (0.2575)	0.7627
X2022							0.0603 (0.2065)	0.2921		
Observations	200		200		200		200		200	
R^2 / R^2 adjusted	0.335 / 0.318		0.334 / 0.317		0.339 / 0.321		0.332 / 0.315		0.345 / 0.307	

* *p<0.05* ** *p<0.01* *** *p<0.001*

Table 6–models with each year fixed effect and its interaction terms and all year fixed effects and interaction terms

The third set of regressions tested the impact of state partisanship on turnout. No new coding was necessary as the trifecta and turnout variables were already fully coded. My expectation for these regressions was that a Republican trifecta would be associated with decreased turnout, given the likelihood of states with Republican trifectas to implement stricter voting laws and therefore have higher COVI values. A Democratic one, meanwhile, would not. My assumption for states with split government is that they would not be able to pass especially permissive or restrictive laws, so differential turnout in those states would be affected by other factors. The first model regressed the trifecta variables on turnout and returned a $\hat{\beta}_0$ (representing split government states) of 0.6023, significant at the α=0.001 level, a $\hat{\beta}_1$ (Democratic trifecta) of -0.0246, significant at no level, and a $\hat{\beta}_2$ (Republican trifecta) of- 0.0581, significant at the α=0.001 level–*see Table 7 (1)*. This regression showed that relative to a state with split government, a state with a Republican trifecta would have turnout about six points lower, while one with a Democratic trifecta would have one about three points lower. Given the lack of statistical significance for the latter, there is very low confidence for that estimate, and for many Democratic states, this is likely not the case. The Republican trifecta estimate fit my expectation, but the split government and Democratic trifecta estimates did not. Reading those results reminded me that the split government states likely lacked trifectas due to their higher electoral competitiveness, and more competitive states tend to have higher turnout compared to those safe for either party (Fraga and Hersh 2011). The next two models regressed turnout on each trifecta variable alone. The regression of turnout on the Democratic trifecta variable returned a $\hat{\beta}_0$ of 0.5679, significant at the α=0.001 level, and a $\hat{\beta}_1$ of 0.0098, significant at no level–*see Table 7 (2)*. This means that when compared to Republican states and split government states combined, Democratic states have turnout that is higher by a minuscule amount. The regression of turnout on the Republican trifecta variable returned a $\hat{\beta}_0$ of 0.5919, significant at the α=0.001 level, and a $\hat{\beta}_1$ of -0.0477, also significant at the α=0.001 level–*see Table 7 (3)*. This is not exactly a converse of the previous regression because it means Republican states have turnout that is statistically significantly lower than Democratic and split government states combined. This comports with the first regression, because it also shows Republican states having turnout that is lower than and differs significantly from other states.

For the next group of models in this set, I regressed turnout on the trifecta variables using the data subsets for each year, to determine whether the comparative effects of the three levels of state partisanship differed from year to year. For all subsequent regressions, $\hat{\beta}_0$ is the split government term, $\hat{\beta}_1$ is the Democratic trifecta term, and $\hat{\beta}_2$ is the Republican trifecta term. The 2016 regression returned a $\hat{\beta}_0$ of 0.6402 significant at the α=0.001 level, a $\hat{\beta}_1$ of -0.035, significant at no level, and a $\hat{\beta}_2$ of -0.0417, significant at the α=0.05 level–*see Table 7 (4)*. It appears to be the case that in 2016, states with trifectas of both parties had decreased turnout compared to states with split government, but Democratic states varied more in turnout and thus that estimate is not statistically significant. This decreased statistical significance of the Republican trifecta estimate may be because of increased overall turnout in a midterm year. The 2018 regression returned a $\hat{\beta}_0$ of 0.5355, significant at the α=0.001 level, a $\hat{\beta}_1$ of -0.0286, and a $\hat{\beta}_2$ of -0.0358, both significant at no level–*see Table 7 (5)*. The decreased statistical significance of the Republican estimate compared to its 2016 analogue may be attributable to particular competitive races and therefore relatively increased turnout in some Republican states. The 2020 regression returned a $\hat{\beta}_0$ of 0.711, significant at the α=0.001 level, a $\hat{\beta}_1$ of -0.0242, significant at no level, and a $\hat{\beta}_2$ of -0.0655, also significant at the α=0.001 level–*see Table 7 (6)*. The difference between the two trifecta estimates is likely because of some Republican states' decisions to not decrease the cost of voting as much as Democratic states did. That is, state trifecta status affected COVI, which in turn affected turnout. The 2022 regression returned a $\hat{\beta}_0$ of 0.5143, significant at the α=0.001 level, a $\hat{\beta}_1$ of -0.0297, significant at no level, and a $\hat{\beta}_2$ of -0.0667, significant at the α=0.01 level–*see Table 7 (7)*. The decreased statistical significance of the Republican estimate relative to its 2020 analogue may be because of the greater spread of state specific effects like competitive races in those states, similar to the decrease from 2016 to 2018.

Influence of partisanship on turnout

	(1)		(2)		(3)		(4)	
Predictors	*Estimates*	*t value*	*Estimates*	*t value*	*Estimates*	*t value*	*Estimates*	*t value*
(Intercept)	0.6023 *** (0.0124)	48.3867	0.5679 *** (0.0082)	69.4329	0.5919 *** (0.0095)	62.5204	0.6402 *** (0.0136)	47.0407
Dem trifecta	-0.0246 (0.0191)	-1.2857	0.0098 (0.0170)	0.5776			-0.0353 (0.0255)	-1.3874
GOP trifecta	-0.0581 *** (0.0162)	-3.5946			-0.0477 *** (0.0140)	-3.4035	-0.0417 * (0.0186)	-2.2397
Observations	204		204		204		51	
R^2 / R^2 adjusted	0.062 / 0.053		0.002 / -0.003		0.054 / 0.050		0.100 / 0.062	

* *p<0.05* ** *p<0.01* *** *p<0.001*

	(5)		(6)		(7)	
Predictors	*Estimates*	*t value*	*Estimates*	*t value*	*Estimates*	*t value*
(Intercept)	0.5355 *** (0.0143)	37.5145	0.7110 *** (0.0143)	49.8309	0.5143 *** (0.0190)	27.0553
Dem trifecta	-0.0286 (0.0252)	-1.1333	-0.0242 (0.0195)	-1.2380	-0.0297 (0.0260)	-1.1438
GOP trifecta	-0.0358 (0.0184)	-1.9491	-0.0655 *** (0.0184)	-3.5572	-0.0667 ** (0.0238)	-2.8064
Observations	51		51		51	
R^2 / R^2 adjusted	0.075 / 0.036		0.220 / 0.187		0.147 / 0.111	

* *p<0.05* ** *p<0.01* *** *p<0.001*

Table 7–models on the full dataset, on each trifecta term individually, and on the four year subsets

The next group of models in this tested for year and state effects. Like with the regressions of turnout on COVI, it was important here to confirm the significance of differences between midterm and presidential election years, so the first model added the 2018 and 2022 state fixed effects to the regression of turnout on trifecta status. It returned a $\hat{\beta}_0$ of 0.6713, significant at the α=0.001 level, a $\hat{\beta}_1$ of -0.0175, significant at no level, and a $\hat{\beta}_2$ of -0.0489, significant at the α=0.001 level–*see Table 8 (1)*. Both year indicators were statistically significant. As expected, turnout differs significantly between midterm and presidential elections, but it was interesting to see that even when accounting for those year fixed effects, the Republican trifecta estimate remained very statistically significant. The second model in this group included all year dummy fixed effects, leaving 2022 as the baseline. It returned a $\hat{\beta}_0$ of 0.5061, significant at the α=0.001 level, a $\hat{\beta}_1$ of -0.0259, significant at the α=0.05 level, and a $\hat{\beta}_2$ of -0.0511, also significant at the α=0.001 level–*see Table 8 (2)*. Once again, the year indicators are all statistically significant, and for the first time, the Democratic trifecta estimate is as well. This means that when year fixed effects are accounted for, states with Democratic trifectas have statistically significantly lower turnout than split government states.

The last regression in this group included all year and state fixed effects, again with 2022 as the baseline year and Wyoming as the baseline state. It returned a $\hat{\beta}_0$ of 0.4487, significant at the α=0.001 level, a $\hat{\beta}_1$ of -0.0009, and a $\hat{\beta}_2$ of -0.0035, both of which were significant at no level–*see Table 8 (3)*. All three year indicators were statistically significant. Of the 49 indicated states, 30 had statistically significant effects. What stands out most about this regression is that the size and statistical significance of the trifecta variables disappears entirely, making the difference in turnout between a split government state and a Democratic or Republican state attributable wholly to year and state fixed effects. Although trifecta status itself has some impact on COVI, which was shown previously to have an impact on turnout, it does not fully explain it. Thus, the state fixed effects in this case likely include some of the impact of COVI on turnout that is not directly explained by trifecta status.

Influence of partisanship on turnout

	(1)		(2)		(3)	
Predictors	*Estimates*	*t value*	*Estimates*	*t value*	*Estimates*	*t value*
(Intercept)	0.6713 *** (0.0090)	74.1848	0.5061 *** (0.0109)	46.6326	0.4487 *** (0.0122)	36.6511
Dem trifecta	-0.0175 (0.0124)	-1.4158	-0.0259 * (0.0117)	-2.2043	-0.0009 (0.0087)	-0.1084
GOP trifecta	-0.0489 *** (0.0104)	-4.6923	-0.0511 *** (0.0098)	-5.2177	-0.0035 (0.0081)	-0.4329
X2016			0.1368 *** (0.0120)	11.3757	0.1403 *** (0.0055)	25.5695
X2018	-0.1309 *** (0.0110)	-11.8869	0.0367 ** (0.0120)	3.0597	0.0374 *** (0.0055)	6.8085
X2020			0.1995 *** (0.0119)	16.7261	0.2008 *** (0.0054)	37.4325
X2022	-0.1687 *** (0.0110)	-15.3194				
Observations	204		204		204	
R^2 / R^2 adjusted	0.616 / 0.608		0.662 / 0.654		0.949 / 0.930	

** p<0.05 ** p<0.01 *** p<0.001*

Table 8–models with midterm year fixed effects, all year fixed effects, and all year and state fixed effects

The final group of models in this set, as with the previous two, included interaction terms for the year fixed effects. The first added 2016 and its interaction terms to the regression of turnout on the trifecta variables, and returned a $\hat{\beta}_0$ of 0.5851, significant at the α=0.001 level, a $\hat{\beta}_1$ of -0.0129, significant at no level, and a $\hat{\beta}_2$ of -0.0588, significant at the α=0.01 level. The 2016 year fixed effect was statistically significant. The interaction terms of Democratic trifecta with 2016 and Republican trifecta with 2016 lacked statistical significance. Logically, if it is indeed the case that states safe for either party, represented here by their trifecta status, have lower turnout due to their lesser competitiveness, then it follows that the year should not influence the impact of trifecta status on turnout. The second model added 2018 and its interaction terms to the basic regression, and returned a $\hat{\beta}_0$ of 0.6265, significant at the α=0.001 level, a $\hat{\beta}_1$ of -0.0342, significant at no level, and a $\hat{\beta}_2$ of -0.0651, significant at the α=0.001 level. The 2018 year fixed effect was statistically significant. The interaction terms again lacked statistical significance. The pattern of statistical significance here is the same as with the 2016 interaction model, except possibly that picking out a midterm year makes the turnout differential between it and the other years greater and thus more statistically significant. The 2020 interaction model returned a $\hat{\beta}_0$ of 0.5719, significant at the α=0.001 level, a $\hat{\beta}_1$ of -0.0505, significant at the α=0.01 level, and a $\hat{\beta}_2$ of -0.0572, significant at the α=0.001 level. The 2020 year fixed effect was statistically significant, and the interaction terms were not. The increased statistical significance of the Democratic trifecta estimate compared with the previous two models may be because 2020 was the highest-turnout election overall, and thus the relatively decreased turnout in the other three years is more significant.

Influence of partisanship on turnout

	(1)		(2)		(3)		(4)		(5)	
Predictors	*Estimates*	*t value*	*Estimates*	*t value*	*Estimates*	*t value*	*Estimates*	*t value*	*Estimates*	*t value*
(Intercept)	0.5851 *** (0.0146)	40.1065	0.6265 *** (0.0138)	45.4240	0.5719 *** (0.0111)	51.6371	0.6247 *** (0.0117)	53.2326	0.5143 *** (0.0168)	30.5726
Dem trifecta	-0.0129 (0.0213)	-0.6080	-0.0342 (0.0205)	-1.6717	-0.0505 ** (0.0179)	-2.8195	-0.0034 (0.0189)	-0.1789	-0.0297 (0.0230)	-1.2925
GOP trifecta	-0.0588 ** (0.0186)	-3.1568	-0.0651 *** (0.0180)	-3.6159	-0.0572 *** (0.0144)	-3.9712	-0.0488 ** (0.0154)	-3.1644	-0.0667 ** (0.0210)	-3.1712
X2016	0.0551 * (0.0261)	2.1127							0.1259 *** (0.0216)	5.8260
X2018			-0.0910 *** (0.0268)	-3.4006					0.0212 (0.0223)	0.9470
X2020					0.1391 *** (0.0237)	5.8762			0.1967 *** (0.0234)	8.4194
X2022							-0.1104 *** (0.0260)	-4.2404		
Observations	204		204		204		204		204	
R^2 / R^2 adjusted	0.127 / 0.105		0.167 / 0.146		0.429 / 0.414		0.345 / 0.329		0.668 / 0.649	

* *p<0.05* ** *p<0.01* *** *p<0.001*

Table 9–models with each year fixed effect and its interaction terms and all year fixed effects and interaction terms

The 2022 interaction model returned a $\hat{\beta}_0$ of 0.6247, significant at the α=0.001 level, a $\hat{\beta}_1$ of -0.0034, significant at no level, and a $\hat{\beta}_2$ of -0.0488, significant at the α=0.01 level. The 2022 year fixed effect was statistically significant, while the interaction terms for the fourth time were not. The final model of this group and the set overall added every year fixed effect and their interaction terms, leaving 2022 as the baseline year. It returned a $\hat{\beta}_0$ of 0.5143, significant at the α=0.001 level, a $\hat{\beta}_1$ of -0.0297, significant at no level, and a $\hat{\beta}_2$ of -0.0667, significant at the α=0.01 level. The 2016 and 2020 year fixed effects were statistically significant. The 2018 year fixed effect presumably lacks statistical significance because as the other midterm year its turnout relative to 2022 is less than those of the two presidential election years. All six of the interaction terms lacked statistical significance. These models taken together show that what year it is does not influence a trifecta's impact on turnout one way or the other, and furthermore that trifecta status is a reliable predictor to use, even it if only partially explains changes in turnout.

The fourth large regression set tested the association of turnout with Republican two-party vote share. My hypothesis was that higher turnout would be associated with lower Republican vote share. As described in the methods section, I averaged Republican vote share across all topline statewide races (president, governor, senator) in each state in each year, to attempt to mitigate the impact the effects of incumbency, scandal, and any other race-specific effects. As there were only four instances of double-barreled Senate elections across the entire dataset, those values were only included in the average vote share variable. Nonetheless, I decided to still run regressions with the vote share values from each separate race type, to determine whether the relationship between turnout and vote share changed between race types. This was with the understanding that by doing so, the possible effects of every factor intended to be dealt with by averaging would come back in to play.

Some races, all Senate elections, had unique circumstances that led to their omission or other decisions about how to account for them, and I make note of them here. The California Senate races in 2016 and 2018 featured two Democrats, so they were omitted. The Alaska Senate in 2022 used ranked choice voting, and the final round featured Sen. Lisa Murkowski (R) and challenger Kelly Tshibaka (R). I used Murkowski's vote share as she was the incumbent, even if a case can be made that she was effectively the candidate with Democratic support, given that Tshibaka had President Trump's endorsement. The Louisiana Senate races in 2020 and 2022 featured incumbents Bill Cassidy (R) and John Kennedy (R) respectively running against many challengers, as Louisiana has all candidates run together in a jungle primary, with a runoff needed only if no candidate wins a majority of the vote. In those two elections, the incumbents won, and I decided as in Murkowski's case to use their vote shares as the Republican vote shares, even though some challengers were also Republicans. The most common special case was runoff elections, always required in Georgia and Louisiana, if no candidate wins a majority of the vote, and required in Mississippi for special elections. The regular elections in Georgia in 2020 and Louisiana in 2016 went to runoffs, as did the special elections in Georgia in 2020 and Mississippi in 2018. In those cases, the turnout values are from the general election. The vote share values are taken from the runoffs. Regardless, using those values was still the best choice, as the general elections featured many candidates and no clear or common way to decide how to pick one Republican vote share value. Matching turnout and vote share values from different election dates is far from optimal, but I think that the unique turnout dynamics of a runoff election would be less logical fit for the dataset than the discrepancy created by the turnout-vote share mismatch. Moreover, some states had no senatorial or gubernatorial race in 2018 or 2022, and thus have no vote share value at all for those years. In the case of DC, there was a districtwide race for mayor both years, but as the Election Atlas did not include it, I also did not. The states with no gubernatorial or senatorial race were Kentucky, Louisiana, and North Carolina in 2018, and Delaware, Mississippi, Montana, New Jersey, Virginia, and West Virginia in 2022. As these comprise only nine data points in total out of 204, I decided not to make any comparison between them and the states that did have statewide races.

The first group of models in this set regressed each Republican two-party vote share variable on turnout. For this group, $\hat{\beta}_1$ represents the slope of the fitted line of vote share against turnout. For all tables in this set, each group of three or four models uses the average, presidential (if applicable), gubernatorial, and senatorial vote share variables, in that order. The t-value columns have been omitted to save space. The first model of this group regressed average Republican two-party vote share on turnout, and returned a $\hat{\beta}_1$ of -0.1964, significant at the α=0.05 level–*see Table 10 (1)*. This means that for every percentage point increase in turnout, Republican two-party vote share would decrease by about a fifth of a percentage point. For the entire regression set, as opposed to those with the trifecta variables, the intercept values are nonsensical, as they represent predicted Republican vote share at a level of zero turnout. For reference, the lowest turnout value in the dataset is 0.313. The second model regressed Republican presidential two-party vote share on turnout, and returned a $\hat{\beta}_1$ of -0.5123, significant at the α=0.01 level–*see Table 10 (2)*. This suggests that increased turnout has a statistically stronger connection to presidential vote share than average vote share, although of course topline turnout is the same for other races on the same ballot as a presidential contest. It may be the case that for senatorial and gubernatorial contests, voters splitting tickets against their presidential party choice decreases the overall association between turnout and Republican two-party vote share. The third model regressed Republican gubernatorial two-party vote share on turnout, and returned a $\hat{\beta}_1$ of -0.0397, significant at no level–*see Table 10 (3)*. In gubernatorial races, ticket splitting is more common than for other races, and that may be a factor here (Jain 2022). The fourth model regressed Republican senatorial two-party vote share on turnout, and returned a $\hat{\beta}_1$ of -0.0549, significant at no level–*see Table 10 (4)*. This suggests that the presidential vote share values were the main cause of statistical significance in the first model.

The next group of models in this set regressed each Republican two-party vote share variable on turnout for each year, to ascertain how the relationship varied across years. Rather than reporting the specific results for all fourteen regressions in this group, I will report the results for regressions with statistically significant results and more briefly summarize the others.

For the four 2016 models, $\hat{\beta}_1$ values ranged from -0.4535 for the presidential vote share regression to -0.2036 for the senatorial vote share regression, and none were statistically significant–*see Table 10 (5-8)*. For the three 2018 models, $\hat{\beta}_1$ values ranged from -0.2947 for the average vote share regression to -0.2119 for the gubernatorial vote share regression, and again, none were statistically significant–*see Table 10 (9-11)*. For the 2020 subset, the average vote share regression returned a $\hat{\beta}_1$ of -0.6052, significant at the α=0.05 level–*see Table 10 (12)*. The decreased statistical significance compared to that of the estimate from the first model may be simply because of the smaller subset being analyzed. The presidential vote share regression returned a $\hat{\beta}_1$ of -0.7005, also significant at the α=0.05 level–*see Table 10 (13)*. This would mean that a one percentage point increase in turnout, Republican vote share would drop by nearly three-quarters of a percentage point. The gubernatorial vote share regression returned a $\hat{\beta}_1$ of -0.865, which would be an impressive value, but it was not statistically significant–*see Table 10 (14)*. This is partially because there were only eleven gubernatorial elections in 2020. The senatorial vote share regression returned a $\hat{\beta}_1$ of -0.8723, significant at the α=0.01 level, by far the steepest slope out of all the models up to that point–*see Table 10 (15)*. For the 2022 subset, the average vote share regression returned a $\hat{\beta}_1$ of -0.6098, significant at the α=0.01 level, which is almost an identical estimate to the analogous 2020 regression. The gubernatorial vote share regression returned a $\hat{\beta}_1$ of -0.4023, with no statistical significance. The senatorial vote share regression returned a $\hat{\beta}_1$ of -0.7096, significant at the α=0.05 level, also similar to its 2020 analogue.

Influence of turnout on vote share

	(1)	(2)	(3)	(4)	(5)	(6)
Predictors	*Estimates*	*Estimates*	*Estimates*	*Estimates*	*Estimates*	*Estimates*
(Intercept)	0.6281 *** (0.0483)	0.8427 *** (0.1150)	0.5535 *** (0.0651)	0.5472 *** (0.0626)	0.7234 *** (0.1853)	0.7995 *** (0.1756)
Turnout	-0.1964 * (0.0824)	-0.5123 ** (0.1771)	-0.0397 (0.1189)	-0.0549 (0.1077)	-0.3277 (0.2993)	-0.4535 (0.2837)
Observations	193	102	95	133	51	51
R^2 / R^2 adjusted	0.029 / 0.024	0.077 / 0.068	0.001 / -0.010	0.002 / -0.006	0.024 / 0.004	0.050 / 0.030

** p<0.05   ** p<0.01   *** p<0.001*

	(7)	(8)	(9)	(10)	(11)	(12)
Predictors	*Estimates*	*Estimates*	*Estimates*	*Estimates*	*Estimates*	*Estimates*
(Intercept)	0.7717 (0.3844)	0.6706 * (0.2691)	0.6479 *** (0.1048)	0.6313 *** (0.1234)	0.5880 *** (0.1564)	0.9203 *** (0.1925)
Turnout	-0.3823 (0.6091)	-0.2036 (0.4350)	-0.2954 (0.2014)	-0.2314 (0.2379)	-0.2525 (0.3002)	-0.6052 * (0.2835)
Observations	12	33	47	36	32	51
R^2 / R^2 adjusted	0.038 / -0.058	0.007 / -0.025	0.046 / 0.024	0.027 / -0.002	0.023 / -0.010	0.085 / 0.066

** p<0.05   ** p<0.01   *** p<0.001*

	(13)	(14)	(15)	(16)	(17)	(18)
Predictors	*Estimates*	*Estimates*	*Estimates*	*Estimates*	*Estimates*	*Estimates*
(Intercept)	0.9769 *** (0.1898)	1.1897 * (0.3904)	1.1246 *** (0.1777)	0.8264 *** (0.1092)	0.7302 *** (0.1369)	0.8644 *** (0.1462)
Turnout	-0.7005 * (0.2796)	-0.8650 (0.5659)	-0.8723 ** (0.2618)	-0.6098 ** (0.2236)	-0.4023 (0.2766)	-0.7096 * (0.3011)
Observations	51	11	34	44	36	34
R^2 / R^2 adjusted	0.114 / 0.095	0.206 / 0.118	0.258 / 0.234	0.150 / 0.130	0.059 / 0.031	0.148 / 0.121

** p<0.05   ** p<0.01   *** p<0.001*

Table 10–models with turnout and each applicable vote share variable on the full dataset and each year subset

As only regressions from the 2020 and 2022 subsets had slope estimates with any statistical significance, I decided to next run regressions with each vote share variable as well as the 2020 and 2022 dummy year fixed effects. The average vote share regression returned a $\hat{\beta}_1$ of -0.2888, significant at the α=0.05 level, and statistically insignificant estimates for the 2020 and 2022 year fixed effects–*see Table 11 (1)*. It made no sense to run the presidential vote share regression with the 2022 year fixed effect, so I omitted it. That regression returned a $\hat{\beta}_1$ of -0.5697, significant at the α=0.05 level, and a statistically insignificant estimate for the 2020 year fixed effect–*see Table 11 (2)*. The gubernatorial vote share regression returned a $\hat{\beta}_1$ of -0.2583, significant at no level, a significant 2020 year fixed effect, and an insignificant 2022 year fixed effect–*see Table 11 (3)*. The fact that the slope is not statistically significant at the α=0.05 level while the 2020 year fixed effect is suggests that factors specific to 2020 influenced gubernatorial vote share more strongly than turnout. The senatorial vote share regression returned a $\hat{\beta}_1$ of -0.1863 with no statistical significance and estimates for the 2020 and 2022 year fixed effects that were also statistically insignificant–*see Table 11 (4)*. The fact that no variable had statistical significance challenges the previous consistency of $\hat{\beta}_1$.

The next four regressions were intended to account for year effects in general. I added the 2016, 2018, and 2020 year fixed effects, although for the presidential vote share regression, I had to omit the 2018 year fixed effect because it was inapplicable and the 2020 year fixed effect because every data point included in the regression belonged to either the 2016 election or the 2020 election. The average vote share regression returned a $\hat{\beta}_1$ of -0.4652, significant at the α=0.001 level, insignificant 2016 and 2018 fixed effects, and a significant 2020 fixed effect–*see Table 11 (5)*. This corroborates the idea that 2020 had factors particular to it that affected Republican vote share. The presidential vote share regression returned a $\hat{\beta}_1$ of -0.5697, significant at the $\alpha=0.01$ level, and an insignificant 2020 fixed effect–*see Table 11 (6)*. The gubernatorial vote share regression returned a $\hat{\beta}_1$ of -0.3861, significant at the $\alpha=0.05$ level, insignificant 2016 and 2018 fixed effects, and a significant 2020 fixed effect–*see Table 11 (7)*. The senatorial vote share regression returned a $\hat{\beta}_1$ of -0.5307, significant at the $\alpha=0.01$ level, a $\hat{\beta}_2$ of 0.0937, significant 2016 and 2020 fixed effects, and an insignificant 2018 fixed effect–*see Table 11 (8)*. The last two regressions strengthen the idea of a 2020 year fixed effect, though the last one also indicated a 2016 year fixed effect.

After running regressions with just year fixed effects, I incorporated state fixed effects into the next group. The average vote share regression returned a $\hat{\beta}_1$ of -0.3509, significant at the $\alpha=0.05$ level, and statistically significant 2016 and 2020 year fixed effects–*see Table 11 (9)*. All state fixed effects had statistically significant estimates. The presidential vote share regression returned a $\hat{\beta}_1$ of 0.1153, significant at no level, and a significant 2016 year fixed effect–*see Table 11 (10)*. Every state fixed effect but that of West Virginia was statistically significant. This implies that once year and state fixed effects are accounted for, the statistical significance of the relationship between turnout and presidential vote share disappears. The gubernatorial vote share regression returned a $\hat{\beta}_1$ of 0.0695, with no statistical significance, and no statistically significant year fixed effects–*see Table 11 (11)*. In this case, 37 out of 44 state fixed had statistically significant estimates. This means that once year and state fixed effects are accounted for, only state fixed effects have any statistical significance, and there is no strong relationship between gubernatorial vote share and turnout. The senatorial vote share regression returned a $\hat{\beta}_1$ of -0.3291, significant at no level, significant 2016 and 2018 fixed effects, and an insignificant 2020 fixed effect–*see Table 11 (12)*. Of the 49 state fixed effects, 43 were statistically significant. This again suggests that the strength of the relationship between senatorial vote share and turnout all but disappears when year and state effects are accounted for.

The fourth group of regressions, as in the previous three sets, tested for the significance of interactions between the predictor variable, turnout in this case, and the year fixed effects. Rather than reporting the specific results for all fourteen regressions involving one year fixed effect and its interaction term, I will report the results for regressions with statistically significant results and more briefly summarize the others. The average vote share regression with the 2016 year fixed effect and its interaction term returned a $\hat{\beta}_1$ of -0.2043, significant at the α=0.05 level, and statistically insignificant year fixed effect and interaction estimates–*see Table 12 (1)*. The presidential vote share regression returned a $\hat{\beta}_1$ of -0.7005, significant at the α=0.05 level, and statistically insignificant year fixed effect and interaction estimates–*see Table 12 (2)*. The gubernatorial and senatorial vote share regressions both lacked any estimate with statistical significance other than the intercept, which again has no useful meaning–*see Table 12 (3-4)*. The average vote share regression with the 2018 year fixed effect and its interaction term returned a $\hat{\beta}_1$ of -0.2644, significant at the α=0.01 level, and statistically insignificant year fixed effect and interaction estimates–*see Table 12 (5)*. As with their 2016 counterparts, the gubernatorial and senatorial vote share regressions both lacked any estimate with statistical significance other than the intercept–*see Table 12 (6-7)*. In these models, once interactions were accounted for, the year fixed effects lost their statistical significance, and the interaction terms themselves did not have any.

The average vote share regression with the 2020 year fixed effect and its interaction term returned a $\hat{\beta}_1$ of -0.2311, significant at the α=0.05 level, and statistically insignificant year fixed effect and interaction estimates–*see Table 12 (8)*. The presidential and gubernatorial vote share

Influence of turnout on vote share

Predictors	(1) Estimates	(2) Estimates	(3) Estimates	(4) Estimates	(5) Estimates	(6) Estimates
(Intercept)	0.6731 *** (0.0656)	0.8711 *** (0.1235)	0.6573 *** (0.0820)	0.6076 *** (0.0874)	0.7565 *** (0.0641)	0.8884 *** (0.1354)
Turnout	-0.2888 * (0.1136)	-0.5697 ** (0.1986)	-0.2583 (0.1483)	-0.1863 (0.1518)	-0.4652 *** (0.1281)	-0.5697 ** (0.1986)
X2016					0.0516 (0.0285)	-0.0173 (0.0269)
X2018					-0.0209 (0.0238)	
X2020	0.0332 (0.0231)	0.0173 (0.0269)	0.1152 ** (0.0412)	0.0532 (0.0307)	0.0691 * (0.0337)	
X2022	-0.0018 (0.0227)		0.0024 (0.0246)	0.0049 (0.0290)		
Observations	193	102	95	133	193	102
R^2 / R^2 adjusted	0.039 / 0.024	0.081 / 0.063	0.080 / 0.050	0.025 / 0.002	0.078 / 0.058	0.081 / 0.063

* *p<0.05* ** *p<0.01* *** *p<0.001*

Predictors	(7) Estimates	(8) Estimates	(9) Estimates	(10) Estimates	(11) Estimates	(12) Estimates
(Intercept)	0.7223 *** (0.0839)	0.7783 *** (0.0812)	0.9220 *** (0.0691)	0.6566 *** (0.0504)	0.7454 *** (0.1754)	0.8882 *** (0.1010)
Turnout	-0.3861 * (0.1676)	-0.5307 ** (0.1636)	-0.3509 * (0.1432)	0.1153 (0.0759)	0.0695 (0.3711)	-0.3291 (0.2079)
X2016	0.0518 (0.0419)	0.0937 ** (0.0357)	0.0489 * (0.0210)	0.0242 *** (0.0050)	-0.0445 (0.0615)	0.0621 * (0.0303)
X2018	-0.0113 (0.0250)	-0.0463 (0.0289)	-0.0187 (0.0102)		-0.0229 (0.0181)	-0.0332 * (0.0159)
X2020	0.1381 ** (0.0490)	0.1153 ** (0.0423)	0.0595 * (0.0291)		0.0073 (0.0823)	0.0504 (0.0425)
Observations	95	133	193	102	95	133
R^2 / R^2 adjusted	0.105 / 0.066	0.147 / 0.120	0.903 / 0.865	0.997 / 0.993	0.820 / 0.631	0.901 / 0.835

* *p<0.05* ** *p<0.01* *** *p<0.001*

Table 11–models with pandemic year indicators, year indicators, and year and state indicators

Influence of turnout on vote share

Predictors	(1) Estimates	(2) Estimates	(3) Estimates	(4) Estimates	(5) Estimates	(6) Estimates
(Intercept)	0.6273 *** (0.0516)	0.9769 *** (0.1973)	0.5465 *** (0.0705)	0.5583 *** (0.0661)	0.6788 *** (0.0556)	0.5705 *** (0.0748)
Turnout	-0.2043 * (0.0899)	-0.7005 * (0.2905)	-0.0271 (0.1318)	-0.0937 (0.1161)	-0.2644 ** (0.0919)	-0.0472 (0.1324)
X2016	0.0961 (0.1664)	-0.1774 (0.2601)	0.2252 (0.3760)	0.1122 (0.2350)		
X2018					-0.0309 (0.1530)	0.0608 (0.1724)
Observations	193	102	95	133	193	95
R^2 / R^2 adjusted	0.036 / 0.021	0.085 / 0.057	0.005 / -0.028	0.027 / 0.004	0.056 / 0.041	0.029 / -0.003

* *p<0.05* ** *p<0.01* *** *p<0.001*

Predictors	(7) Estimates	(8) Estimates	(9) Estimates	(10) Estimates	(11) Estimates	(12) Estimates
(Intercept)	0.6346 *** (0.0689)	0.6412 *** (0.0607)	0.7995 *** (0.1695)	0.6393 *** (0.0758)	0.5463 *** (0.0789)	0.5867 *** (0.0638)
Turnout	-0.1700 (0.1149)	-0.2311 * (0.1107)	-0.4535 (0.2738)	-0.2218 (0.1440)	-0.0692 (0.1450)	-0.1273 (0.1043)
X2018	-0.0466 (0.1941)					
X2020		0.2791 (0.1929)	0.1774 (0.2601)	0.5503 (0.3871)	0.5782 * (0.2388)	
X2022						0.2397 (0.1350)
Observations	133	193	102	95	133	193
R^2 / R^2 adjusted	0.092 / 0.071	0.048 / 0.033	0.085 / 0.057	0.093 / 0.063	0.060 / 0.039	0.046 / 0.031

* *p<0.05* ** *p<0.01* *** *p<0.001*

Predictors	(13) Estimates	(14) Estimates	(15) Estimates	(16) Estimates	(17) Estimates
(Intercept)	0.4803 *** (0.0881)	0.4710 *** (0.0842)	0.8264 *** (0.1178)	0.7302 *** (0.1211)	0.8644 *** (0.1519)
Turnout	0.0897 (0.1524)	0.0698 (0.1377)	-0.6098 * (0.2412)	-0.4023 (0.2446)	-0.7096 * (0.3130)
X2016			-0.1030 (0.1954)	0.0414 (0.3761)	-0.1938 (0.2607)
X2018			-0.1785 (0.1844)	-0.0990 (0.1940)	-0.2764 (0.2328)
X2020			0.0939 (0.2163)	0.4594 (0.4020)	0.2602 (0.2637)
X2022	0.2500 (0.1520)	0.3934 * (0.1815)			
Observations	95	133	193	95	133
R^2 / R^2 adjusted	0.031 / -0.001	0.037 / 0.015	0.085 / 0.050	0.116 / 0.045	0.168 / 0.121

* *p<0.05* ** *p<0.01* *** *p<0.001*

Table 12–models with each year fixed effect and its interaction term and all year fixed effects and interaction terms

regressions had no estimate with statistical significance other than the intercept–*see Table 12 (9-10)*. Conversely, the senatorial vote share regression returned a $\hat{\beta}_1$ of -0.0692, significant at no level, and statistically significant 2020 year fixed effect and interaction estimates–*see Table 12 (11)*. In this case, not only was there a year fixed effect, but it also decreased the impact of turnout on senatorial vote share, perhaps factoring into its lack of statistical significance. The average vote share regression with the 2022 year fixed effect and its interaction term returned a $\hat{\beta}_1$ of -0.1273 and statistically insignificant year fixed effect and interaction estimates–*see Table 12 (12)*. Here again, there was a year fixed effect with some statistical significance, and the year fixed effect decreased the impact of turnout on average vote share. The gubernatorial vote share regression returned a $\hat{\beta}_1$ of 0.0897, and statistically insignificant 2022 year fixed effect and interaction estimates–*see Table 12 (13)*. There is little evidence of any meaningful relationship in this model, as no term had a statistically significant estimate. Similar to its 2020, counterpart, the senatorial vote share regression returned a $\hat{\beta}_1$ of 0.0699, significant at no level, and statistically significant 2022 effect and interaction estimates–*see Table 12 (14)*. As the year fixed effect and its interaction with turnout were statistically significant while the impact of turnout on senatorial vote share was not, this as well as the previous regressions suggest a weak relationship between turnout and Republican vote share.

The last three regressions of this group included all year fixed effects and their interaction terms, to see if the previous year and interaction effects would hold when all were incorporated into the same model. 2022 served as the baseline year. The average vote share regression with 2016, 2018, 2020, and their interaction terms returned a $\hat{\beta}_1$ of -0.6098, significant at the α=0.05 level–*see Table 12 (15)*. The 2016, 2018, and 2020 year fixed effects were statistically insignificant. None of the interaction terms were statistically significant. The presidential vote share regression would have had the same terms as either the 2016 or 2020 presidential vote share regressions, given that only the 2016 or 2020 year fixed effect could be included, so it was omitted. The gubernatorial vote share regression returned a $\hat{\beta}_1$ of -0.4023, significant at no level–*see Table 12 (16)*. The 2016, 2018, and 2020 year fixed effects were statistically insignificant. Once again, none of the interaction terms were statistically significant. The senatorial vote share regression returned a $\hat{\beta}_1$ of -0.7096, significant at the α=0.05 level–*see Table 12 (17)*. All year fixed effects and interaction terms were not statistically significant. Altogether, these models show that the negative effect of turnout on Republican vote share tends to hold even when year and state fixed effects are accounted for.

The fifth and final large regression set tested the association of COVI and Republican vote share. My hypothesis was that an increase in COVI would be associated with an increase in Republican two-party vote share, because if the purpose of at least some state voting laws was to make voting more difficult for demographic groups less likely to vote for Republicans, then Republican vote share would rise with COVI provided those laws accomplished their intended effect. The details of the construction of the four vote share variables can be found in the description of the fourth regression set. COVI values were once again used as provided by Schraufnagel et al. Similar to the other regression sets that used COVI, I used duplicated 2016 COVI values in lieu of separate 2018 values. As with the previous regression sets, only topline $\hat{\beta}_1$ estimates will be written out, while year fixed effects will be provided in tables and state fixed effects and interaction terms will only be referenced. T-values are again omitted to save space and standard errors are included in parentheses underneath the estimates. For this set, all intercepts do have some meaning, as they represent Republican vote share when the COVI of a given state is 0, which is roughly true for some states in some years. Nonetheless, they are not important for interpretative purposes.

Influence of COVI on vote share

	(1)	(2)	(3)	(4)	(5)	(6)
Predictors	*Estimates*	*Estimates*	*Estimates*	*Estimates*	*Estimates*	*Estimates*
(Intercept)	0.5198 *** (0.0069)	0.5209 *** (0.0096)	0.5333 *** (0.0108)	0.5147 *** (0.0096)	0.5312 *** (0.0155)	0.5298 *** (0.0144)
COVI	0.0556 *** (0.0085)	0.0612 *** (0.0121)	0.0307 * (0.0128)	0.0654 *** (0.0117)	0.0586 * (0.0229)	0.0620 ** (0.0213)
Observations	191	100	95	133	50	50
R^2 / R^2 adjusted	0.184 / 0.179	0.206 / 0.198	0.058 / 0.048	0.193 / 0.186	0.120 / 0.102	0.150 / 0.132

* *p<0.05* ** *p<0.01* *** *p<0.001*

	(7)	(8)	(9)	(10)	(11)	(12)
Predictors	*Estimates*	*Estimates*	*Estimates*	*Estimates*	*Estimates*	*Estimates*
(Intercept)	0.5237 *** (0.0346)	0.5465 *** (0.0246)	0.4956 *** (0.0119)	0.5116 *** (0.0138)	0.4566 *** (0.0176)	0.5201 *** (0.0127)
COVI	-0.0278 (0.0396)	0.0554 (0.0363)	0.0307 (0.0173)	0.0407 (0.0204)	0.0428 (0.0289)	0.0615 *** (0.0142)
Observations	12	33	47	36	32	50
R^2 / R^2 adjusted	0.047 / -0.049	0.070 / 0.040	0.066 / 0.045	0.105 / 0.079	0.068 / 0.037	0.281 / 0.266

* *p<0.05* ** *p<0.01* *** *p<0.001*

	(13)	(14)	(15)	(16)	(17)	(18)
Predictors	*Estimates*	*Estimates*	*Estimates*	*Estimates*	*Estimates*	*Estimates*
(Intercept)	0.5120 *** (0.0130)	0.5982 *** (0.0349)	0.5276 *** (0.0166)	0.5323 *** (0.0144)	0.5328 *** (0.0193)	0.5271 *** (0.0142)
COVI	0.0607 *** (0.0144)	0.0316 (0.0331)	0.0602 ** (0.0190)	0.0619 *** (0.0154)	0.0442 * (0.0211)	0.0813 *** (0.0138)
Observations	50	11	34	44	36	34
R^2 / R^2 adjusted	0.269 / 0.254	0.092 / -0.009	0.239 / 0.216	0.278 / 0.260	0.115 / 0.089	0.521 / 0.506

* *p<0.05* ** *p<0.01* *** *p<0.001*

Table 13–models with COVI and each applicable vote share variable on the full dataset and each year subset

The first eighteen regressions in this set regressed each vote share variable on COVI, first using the full dataset and then using each year subset. The model regressing average vote share on COVI returned a $\hat{\beta}_1$ of 0.0556, significant at the α=0.001 level–*see Table 13 (1)*. This would mean that for a unit increase in COVI, a state's two-party Republican vote share would increase by about five and a half percentage points. Given the full COVI range of about 4.5, according to this model, a state would be able to increase its Republican vote share by nearly 25 percent if it raised its COVI from -3 to 1.5. The states able to do this almost certainly would not, but this still shows the possibly large effect COVI could have on vote share. The models with presidential, gubernatorial, and senatorial vote share as the response variable all had statistically significant $\hat{\beta}_1$ estimates of 0.612, 0.0307, and 0.0654 respectively. The presidential and senatorial estimates were significant at the α=0.001 level, while the gubernatorial estimate was significant at the α=0.05 level–*see Table 13 (2-4)*. While the gubernatorial estimate may have lower statistically significance because there are fewer gubernatorial elections than presidential or senatorial elections in a given cycle, the fact that its estimate is about half that of the other three suggests that it may have to do with ticket-splitting. This is because it is possible that some states with high COVI scores elect Democratic governors and vice versa.

For the 2016 subset models, the average vote share regression returned a $\hat{\beta}_1$ of 0.0587, significant at the α=0.05 level, while the presidential vote share regression returned a $\hat{\beta}_1$ of 0.062, significant at the α=0.01 level–*see Table 13 (5-6)*. Neither the gubernatorial nor the senatorial 2016 models had a statistically significant estimate–*see Table 13 (7-8)*. None of the three 2018 models had a statistically significant estimate–*see Table 13 (9-11)*. Out of the 2020 models, the average, presidential, and senatorial vote share models returned statistically significant $\hat{\beta}_1$ estimates of 0.0615, 0.0607, and 0.0602 respectively. The average and presidential estimates were significant at the $\alpha=0.001$ level, while the senatorial estimate was significant at the $\alpha=0.01$ level–*see Table 13 (12-13, 15)*. The gubernatorial estimate was not statistically significant, again possibly because of ticket-splitting but likely in this case also because there were only eleven gubernatorial elections in 2020–*see Table 13 (14)*. By contrast, all three of the 2022 models returned statistically significant $\hat{\beta}_1$ estimates of 0.0619, 0.0442, and 0.0813 respectively. The average and senatorial estimates were significant at the $\alpha=0.001$ level, while the gubernatorial estimate was significant at the $\alpha=0.01$ level–*see Table 13 (16-18)*. It is again plausible that the difference between the gubernatorial and senatorial estimates is attributable to ticket-splitting, as Vermont, New Hampshire, Nevada, Georgia voted for Democrats for Senate and Republicans for governor and Kansas did the opposite (Rosenbaum 2022).

Since 2020 and 2022 were the two election cycles in which states made concerted efforts to either increase or decrease the cost of voting, depending on the state, the next four regressions included those two year fixed effects. The exception to that was the presidential model, which by necessity could only include the 2020 year fixed effect. All four of the models returned statistically significant $\hat{\beta}_1$ estimates of 0.0556, 0.0612, 0.0312, and 0.065–*see Table 14 (1-4)*. All were significant at the $\alpha=0.001$ level but the gubernatorial $\hat{\beta}_1$ estimate, which was significant at the $\alpha=0.05$ level. The gubernatorial model was the only model with any statistically significant year fixed effect. The way to interpret that is that some factor particular to 2020 affected Republican gubernatorial vote share, in this case positively. This may be the case because the states voting for governor in 2020 skewed Republican overall relative to those electing a governor in 2022, and moreover New Hampshire and Vermont elected Republican governors while voting for Joe Biden. The subsequent four regressions included all year fixed effects, leaving 2022 as the baseline year. The presidential model in this case was the same as the previous one except that it swapped the 2020 year fixed effect for the 2016 year fixed effect. Again, all four models returned significant $\hat{\beta}_1$ estimates, of 0.0556, 0.0612, 0.0322, and 0.066–*see Table 14 (5-8)*. For the second time, all were significant at the α=0.001 level but the gubernatorial $\hat{\beta}_1$ estimate, which was significant at the α=0.05 level. These four estimates were nearly identical to the previous four, which makes sense because the none of the year fixed effects were significant except for the 2018 fixed effect in the senatorial model. Republican senatorial vote share would likely be statistically significantly less in 2018 than in the other years because Democrats won by far most seats up for election that year.

To test whether state fixed effects would have any impact not accounted for by the previous models, the next four regressions added all state fixed effects, leaving Wyoming as the baseline state. None returned a statistically significant $\hat{\beta}_1$ estimate, however the average and senatorial models returned statistically significant 2018 year fixed effects, again plausibly because of the Democratic-favorable Senate map–*see Table 14 (9-12)*. Since COVI was hypothesized to increase Republican vote share, it would be highly unlikely to have an effect when set against a baseline of one of the most Republican states. All four models had many statistically significant state fixed effects, likely primarily because most states simply had a significantly smaller Republican vote share than Wyoming.

Influence of COVI on vote share

	(1)	(2)	(3)	(4)	(5)	(6)
Predictors	*Estimates*	*Estimates*	*Estimates*	*Estimates*	*Estimates*	*Estimates*
(Intercept)	0.5140 *** (0.0097)	0.5298 *** (0.0136)	0.5188 *** (0.0150)	0.5022 *** (0.0137)	0.5322 *** (0.0143)	0.5120 *** (0.0136)
COVI	0.0556 *** (0.0086)	0.0612 *** (0.0121)	0.0312 * (0.0126)	0.0650 *** (0.0118)	0.0556 *** (0.0085)	0.0612 *** (0.0121)
X2016					-0.0010 (0.0196)	0.0178 (0.0193)
X2018					-0.0364 (0.0199)	
X2020	0.0060 (0.0166)	-0.0178 (0.0193)	0.0794 * (0.0347)	0.0248 (0.0234)	-0.0121 (0.0196)	
X2022	0.0182 (0.0173)		0.0141 (0.0229)	0.0240 (0.0234)		
Observations	191	100	95	133	191	100
R^2 / R^2 adjusted	0.188 / 0.175	0.213 / 0.197	0.109 / 0.080	0.203 / 0.184	0.203 / 0.186	0.213 / 0.197

** p<0.05 ** p<0.01 *** p<0.001*

	(7)	(8)	(9)	(10)	(11)	(12)
Predictors	*Estimates*	*Estimates*	*Estimates*	*Estimates*	*Estimates*	*Estimates*
(Intercept)	0.5329 *** (0.0173)	0.5263 *** (0.0182)	0.7565 *** (0.0229)	0.7366 *** (0.0075)	0.7818 *** (0.0474)	0.7237 *** (0.0383)
COVI	0.0322 * (0.0127)	0.0657 *** (0.0113)	0.0091 (0.0096)	-0.0063 (0.0039)	-0.0135 (0.0202)	0.0226 (0.0135)
X2016	0.0073 (0.0348)	0.0204 (0.0259)	0.0024 (0.0090)	0.0178 *** (0.0020)	-0.0309 (0.0428)	0.0185 (0.0122)
X2018	-0.0212 (0.0245)	-0.0700 ** (0.0262)	-0.0303 ** (0.0092)		-0.0211 (0.0154)	-0.0457 ** (0.0141)
X2020	0.0654 (0.0358)	0.0007 (0.0258)	-0.0087 (0.0090)		0.0262 (0.0451)	-0.0127 (0.0137)
Observations	95	133	191	100	95	133
R^2 / R^2 adjusted	0.116 / 0.077	0.270 / 0.247	0.879 / 0.832	0.996 / 0.992	0.821 / 0.635	0.902 / 0.836

** p<0.05 ** p<0.01 *** p<0.001*

Table 14–models with 2020 and 2022 year fixed effects, all year fixed effects, and all year and state fixed effects

The last models in this set regressed each applicable vote share variable on COVI, first with each year fixed effect and its interaction term separately and then with all of them together. The 2016 models returned statistically significant $\hat{\beta}_1$ estimates of 0.0549, 0.0607, 0.0392, and 0.0682 respectively–*see Table 15 (1-4)*. All were significant at the α=0.001 level but the gubernatorial $\hat{\beta}_1$ estimate, which was significant at the α=0.01 level. Neither the 2016 year fixed effect nor its interaction term with COVI was statistically significant. From here it can be seen that when the state fixed effects are excluded, $\hat{\beta}_1$ regains statistical significance, and the given year fixed effect is neither significant nor influences the impact of COVI on Republican vote share. The three 2018 models, on the other hand, varied greatly from each other. The average vote share model returned a $\hat{\beta}_1$ of 0.061, significant at the α=0.001 level, the gubernatorial model returned a statistically insignificant estimate, and the senatorial model returned a $\hat{\beta}_1$ of 0.0689, significant at the α=0.001 level–*see Table 15 (5-7)*. The average and senatorial models had statistically significant 2018 year fixed effects, while none of the three had significant interaction terms. Like the 2016 interaction models, the four 2020 interaction models each had statistically significant associations between COVI and their respective vote share variables. Their $\hat{\beta}_1$ estimates were 0.0528, 0.062, 0.0314, and 0.0666 respectively. The average and senatorial estimates were significant at the α=0.001 level, while the presidential estimate was significant at the α=0.01 level and the gubernatorial estimate was significant at the α=0.05 level–*see Table 15 (8-11)*. Only the gubernatorial model had a significant 2020 year fixed effect, and none of the interaction terms were significant. Of the three 2022 models, the average and senatorial models had statistically significant associations between COVI and their respective vote share variables, while the gubernatorial model did not–*see Table 15 (12-14)*. Their $\hat{\beta}_1$ estimates were 0.0528 and 0.0552, significant at the α=0.001 level. None of these three models had a significant 2022 year fixed effect or interaction term. For the models with all year fixed effects and their interaction terms, the presidential model was omitted, as with the turnout-vote share models, because it would have been identical to either the 2016 or 2020 version. All three models that were used returned $\hat{\beta}_1$ estimates of 0.0619, 0.0442, and 0.0813, with the average and senatorial model estimates significant at the α=0.001 level and the gubernatorial model estimate significant at the α=0.05 level–*see Table 15 (15-17)*. Only the 2018 year fixed effect in the senatorial model was significant, while none of the interaction terms were significant. Altogether, these models seem to show that the positive effect of COVI on Republican vote share holds when year fixed effects are included but all but disappears when state fixed effects are additionally incorporated.

Influence of COVI on vote share

Predictors	(1) *Estimates*	(2) *Estimates*	(3) *Estimates*	(4) *Estimates*	(5) *Estimates*	(6) *Estimates*	(7) *Estimates*
(Intercept)	0.5158 *** (0.0080)	0.5120 *** (0.0137)	0.5324 *** (0.0115)	0.5040 *** (0.0110)	0.5276 *** (0.0078)	0.5463 *** (0.0137)	0.5331 *** (0.0105)
COVI	0.0549 *** (0.0095)	0.0607 *** (0.0153)	0.0392 ** (0.0137)	0.0682 *** (0.0128)	0.0610 *** (0.0093)	0.0287 (0.0147)	0.0688 *** (0.0120)
X2016	0.0154 (0.0157)	0.0178 (0.0194)	-0.0087 (0.0340)	0.0424 (0.0220)			
X2018					-0.0321 * (0.0158)	-0.0347 (0.0223)	-0.0765 *** (0.0215)
Observations	191	100	95	133	191	95	133
R^2 / R^2 adjusted	0.188 / 0.175	0.213 / 0.189	0.088 / 0.058	0.216 / 0.198	0.209 / 0.196	0.084 / 0.054	0.269 / 0.252

* *p<0.05* ** *p<0.01* *** *p<0.001*

Predictors	(8) *Estimates*	(9) *Estimates*	(10) *Estimates*	(11) *Estimates*	(12) *Estimates*	(13) *Estimates*	(14) *Estimates*
(Intercept)	0.5197 *** (0.0080)	0.5298 *** (0.0137)	0.5248 *** (0.0114)	0.5105 *** (0.0112)	0.5161 *** (0.0079)	0.5328 *** (0.0139)	0.5111 *** (0.0111)
COVI	0.0528 *** (0.0104)	0.0620 ** (0.0203)	0.0314 * (0.0139)	0.0666 *** (0.0140)	0.0528 *** (0.0103)	0.0198 (0.0173)	0.0552 *** (0.0152)
X2020	0.0004 (0.0157)	-0.0178 (0.0194)	0.0734 * (0.0335)	0.0171 (0.0222)			
X2022					0.0162 (0.0164)	-0.0000 (0.0225)	0.0160 (0.0220)
Observations	191	100	95	133	191	95	133
R^2 / R^2 adjusted	0.185 / 0.171	0.213 / 0.189	0.106 / 0.076	0.197 / 0.178	0.189 / 0.176	0.067 / 0.036	0.203 / 0.185

* *p<0.05* ** *p<0.01* *** *p<0.001*

Predictors	(15) *Estimates*	(16) *Estimates*	(17) *Estimates*
(Intercept)	0.5323 *** (0.0143)	0.5328 *** (0.0173)	0.5271 *** (0.0183)
COVI	0.0619 *** (0.0153)	0.0442 * (0.0189)	0.0813 *** (0.0178)
X2016	-0.0011 (0.0196)	-0.0091 (0.0360)	0.0194 (0.0261)
X2018	-0.0367 (0.0199)	-0.0211 (0.0245)	-0.0704 ** (0.0263)
X2020	-0.0122 (0.0196)	0.0654 (0.0359)	0.0005 (0.0260)
Observations	191	95	133
R^2 / R^2 adjusted	0.211 / 0.181	0.148 / 0.079	0.279 / 0.238

* *p<0.05* ** *p<0.01* *** *p<0.001*

Table 15–models with each year fixed effect and its interaction term and all year fixed effects and interaction terms

Robustness checks

The major potential weakness with all regressions that included the COVI variable was that the lack of distinct 2018 scores. As described above, I duplicated the 2016 scores in lieu of 2018 scores. Again, some or many states may well have passed various voting laws that would change the burden of voting on their citizens between the 2016 and 2020 general elections. But Schraufnagel and his coauthors made their next calculations in 2020, so there was no possible consistent way to interpolate 2018 scores using their data. In order to address this issue after the fact, I created 2018 COVI scores that averaged the 2016 and 2020 values in order to see whether any results noticeably changed. If any did, it would mean that duplicated 2016 scores could not accurately represent the nonexistent 2018 scores. The robust COVI variable thus consisted of the actual 2016, 2020, and 2022 values with the constructed 2018 values inserted. I created models with this variable to check the key regressions of each set that included COVI. Here I will mention only the topline results but will include these regressions in a table below. The robust regression of turnout on COVI returned a $\hat{\beta}_1$ of -0.0238, differing from the original estimate by 0.001. This difference was the same between the estimates from the robust regression with year fixed effects and the original, while the estimates from regressions with year and state fixed effects differed by less that 0.0002. The regressions with year fixed effects and their interaction terms had identical estimates–*see models 1-8 of the robustness check table, printed in the order they were mentioned*.

The robust regression of COVI on the trifecta variables returned a $\hat{\beta}_0$, $\hat{\beta}_1$, and $\hat{\beta}_2$ of -0.133, -0.6521, and 0.5876, compared to the original estimates of -0.1121, -0.6437, and 0.5388. The difference between the Republican trifecta estimates could be because Republican states increased their COVI scores by more than other states in 2020, and thus the constructed 2018 pulled the apparent impact of a Republican trifecta on COVI up. All three differences are similar between the robust and original models with year fixed effects. The robust model with year and state fixed effects returned estimates of 0.4151, -0.3274, and 0.1693, compared to the original estimates of 0.4139, -0.3753, and 0.1437. The estimates in the robust and original models with all year fixed effects and interaction terms were nearly identical–*see models 9-16 of the robustness check table*. For the models regressing average vote share on COVI, the general model, model with year fixed effects, and model with year fixed effects and interaction terms returned estimates identical to four decimal places. The robust model with year and state fixed effects returned a $\hat{\beta}_1$ of 0.0005, compared to the original estimate of 0.0091, and neither estimate was statistically significant–*see models 17-24 of the robustness check table*.

It became apparent that another set of robustness checks would make sense as well: the effect of COVI on vote share, as well as the effect of turnout on vote share before it, seemed to disappear when state fixed effects were included, but it seemed possible that this was at least in part because the baseline state of Wyoming was consistently one of the states with the highest Republican vote share. Therefore, I created two models each with Nevada, a state with closer to average Republican vote share, as the baseline state that included the Wyoming state fixed effects. The robust regression of vote share on turnout with year and state fixed effects returned a $\hat{\beta}_1$ of -0.3509, the same as the original estimate. The second robust regression of vote share on COVI with year and state fixed effects returned a $\hat{\beta}_1$ of 0.0091, also the same as the original estimate–*see models 25-28 of the robustness check table*.

Robustness checks

Predictors	(1) Estimates	(2) Estimates	(3) Estimates	(4) Estimates	(5) Estimates	(6) Estimates	(7) Estimates	(8) Estimates
(Intercept)	0.5708 *** (0.0071)	0.5708 *** (0.0071)	0.4763 *** (0.0087)	0.4763 *** (0.0087)	0.4611 *** (0.0140)	0.4605 *** (0.0139)	0.4763 *** (0.0087)	0.4763 *** (0.0087)
Robust COVI constructed 2018	-0.0238 ** (0.0087)		-0.0238 *** (0.0053)		-0.0140 * (0.0059)		-0.0237 * (0.0095)	
COVI		-0.0248 ** (0.0089)		-0.0248 *** (0.0054)		-0.0138 * (0.0055)		-0.0237 * (0.0095)
X2016			0.1398 *** (0.0123)	0.1398 *** (0.0122)	0.1398 *** (0.0053)	0.1398 *** (0.0053)	0.1398 *** (0.0123)	0.1398 *** (0.0123)
X2018			0.0380 ** (0.0123)	0.0380 ** (0.0122)	0.0380 *** (0.0053)	0.0380 *** (0.0053)	0.0380 ** (0.0123)	0.0380 ** (0.0123)
X2020			0.2005 *** (0.0123)	0.2005 *** (0.0122)	0.2005 *** (0.0053)	0.2005 *** (0.0053)	0.2005 *** (0.0123)	0.2005 *** (0.0123)
Observations	200	200	200	200	200	200	200	200
R^2 / R^2 adjusted	0.037 / 0.032	0.038 / 0.033	0.648 / 0.640	0.649 / 0.642	0.951 / 0.933	0.951 / 0.933	0.648 / 0.635	0.651 / 0.638

* *p<0.05* ** *p<0.01* *** *p<0.001*

Predictors	(9) Estimates	(10) Estimates	(11) Estimates	(12) Estimates	(13) Estimates	(14) Estimates	(15) Estimates	(16) Estimates
(Intercept)	-0.1330 (0.0832)	-0.1121 (0.0826)	-0.0765 (0.1209)	-0.0566 (0.1201)	0.4152 (0.2195)	0.4139 (0.2334)	-0.0641 (0.1866)	-0.0641 (0.1854)
Dem trifecta	-0.6521 *** (0.1312)	-0.6437 *** (0.1303)	-0.6847 *** (0.1337)	-0.6754 *** (0.1329)	-0.3274 * (0.1267)	-0.3753 ** (0.1347)	-0.7319 ** (0.2591)	-0.7319 ** (0.2575)
GOP trifecta	0.5876 *** (0.1080)	0.5388 *** (0.1073)	0.5829 *** (0.1086)	0.5341 *** (0.1079)	0.1693 (0.1171)	0.1437 (0.1246)	0.5846 * (0.2335)	0.5846 * (0.2319)
X2016			-0.0958 (0.1347)	-0.0945 (0.1338)	-0.0458 (0.0743)	-0.0525 (0.0790)	-0.1875 (0.2397)	-0.1875 (0.2381)
X2018			-0.1308 (0.1344)	-0.1265 (0.1335)	-0.0559 (0.0746)	-0.0611 (0.0794)	-0.2008 (0.2479)	-0.1221 (0.2463)
X2020			0.0370 (0.1334)	0.0349 (0.1326)	0.0134 (0.0724)	0.0133 (0.0770)	0.1964 (0.2591)	0.1964 (0.2575)
Observations	200	200	200	200	200	200	200	200
R^2 / R^2 adjusted	0.350 / 0.343	0.330 / 0.323	0.356 / 0.340	0.336 / 0.319	0.859 / 0.807	0.834 / 0.772	0.365 / 0.328	0.345 / 0.307

* *p<0.05* ** *p<0.01* *** *p<0.001*

Predictors	(17) Estimates	(18) Estimates	(19) Estimates	(20) Estimates	(21) Estimates	(22) Estimates	(23) Estimates	(24) Estimates
(Intercept)	0.5197 *** (0.0068)	0.5198 *** (0.0069)	0.5322 *** (0.0142)	0.5322 *** (0.0143)	0.7612 *** (0.0231)	0.7565 *** (0.0229)	0.5323 *** (0.0143)	0.5323 *** (0.0143)
Robust COVI constructed 2018	0.0556 *** (0.0083)		0.0556 *** (0.0083)		0.0005 (0.0102)		0.0619 *** (0.0152)	
COVI		0.0556 *** (0.0085)		0.0556 *** (0.0085)		0.0091 (0.0096)		0.0619 *** (0.0153)
X2016			-0.0010 (0.0195)	-0.0010 (0.0196)	0.0023 (0.0090)	0.0024 (0.0090)	-0.0011 (0.0195)	-0.0011 (0.0196)
X2018			-0.0366 (0.0198)	-0.0364 (0.0199)	-0.0304 ** (0.0092)	-0.0303 ** (0.0092)	-0.0368 (0.0198)	-0.0367 (0.0199)
X2020			-0.0121 (0.0195)	-0.0121 (0.0196)	-0.0088 (0.0090)	-0.0087 (0.0090)	-0.0122 (0.0195)	-0.0122 (0.0196)
Observations	191	191	191	191	191	191	191	191
R^2 / R^2 adjusted	0.192 / 0.188	0.184 / 0.179	0.211 / 0.194	0.203 / 0.186	0.878 / 0.831	0.879 / 0.832	0.218 / 0.188	0.211 / 0.181

* *p<0.05* ** *p<0.01* *** *p<0.001*

Predictors	(25) Estimates	(26) Estimates	(27 Estimates	(28) Estimates
(Intercept)	0.6555 *** (0.0681)	0.9220 *** (0.0691)	0.5003 *** (0.0225)	0.7565 *** (0.0229)
Turnout	-0.3509 * (0.1432)	-0.3509 * (0.1432)		
COVI			0.0091 (0.0096)	0.0091 (0.0096)
X2016	0.0489 * (0.0210)	0.0489 * (0.0210)	0.0024 (0.0090)	0.0024 (0.0090)
X2018	-0.0187 (0.0102)	-0.0187 (0.0102)	-0.0303 ** (0.0092)	-0.0303 ** (0.0092)
X2020	0.0595 * (0.0291)	0.0595 * (0.0291)	-0.0087 (0.0090)	-0.0087 (0.0090)
Observations	193	193	191	191
R^2 / R^2 adjusted	0.903 / 0.865	0.903 / 0.865	0.879 / 0.832	0.879 / 0.832

* *p<0.05* ** *p<0.01* *** *p<0.001*

Discussion

H1 stated that stringency of a state's voting regime correlates negatively with voter turnout, especially for regimes in place for the 2022 election. The basic model, including just the predictor and response variables, found a statistically significant decrease in turnout with each increase in COVI, which held true for the 2018, 2020, and 2022 subsets, as well as a subset that excluded COVI outliers. This remained the case for the models with midterm year fixed effects, all year fixed effects, all year and state fixed effects, every single-year interaction, and all year interactions. Robustness checks showed that all slope estimates remained statistically significant when interpolated 2018 COVI scores were used. The bottom line for H1 and the models run to test it is that COVI does have a small but significant negative impact on turnout, even when state and year fixed effects are accounted for. Thus, it can be said that when states make it harder to vote, they tend to decrease their voter turnout. If the governor and legislature in charge of the state intended for this to happen, believing that lower turnout would benefit them, then they will be vindicated and continue to increase the cost of voting, further decreasing turnout in a vicious cycle. Moreover, as fewer prospective voters turn out, a smaller and smaller proportion of the state's population has a say in electing the officials passing the voting laws in the first place.

H2 stated that Republican state partisanship correlates positively with stringency of voting laws. More specifically, a state with a Republican trifecta would have higher COVI scores than a state with split government, which in turn would in turn have higher scores than a state with a Democratic trifecta. Increased Republican state partisanship could be either a Democratic state changing to split government, or a state with split government gaining a Republican trifecta. The simple regression across all data did return a statistically significant negative impact on COVI in Democratic states and a positive one in Republican states, with the estimate for split government states falling in between the two. This spread of estimates held true for every year subset, although the Republican and Democratic estimates were only both statistically significant in 2018 and 2022. This held true for a model that used a subset just from the pandemic years of 2020 and 2022, providing more support to the notion from the hypothesis that increased Republican state partisanship correlated positively with voting stringency.

The trifecta terms remained statistically significant when first 2020 and 2022 and then all year fixed effects were introduced. Conversely, the statistical significance of the Republican trifecta term disappeared when all year and state fixed effects were included, although the split government term gained it. This left the hypothesis partially intact, because voting stringency increased between Democratic and split government states even though it could not be shown to increase further at a statistically significant level between split government and Republican states. Both trifecta terms retained their statistical significance in all interaction models. The bottom line from this set of regressions is that there is some evidence of an association between increased Republican partisanship and increased cost of voting, even when year and state fixed effects are accounted for. This was true for new post-2020 laws, as in those in place by 2022, but it was also true across 2016, 2018, 2020, and 2022. Thus, it can be said that Republican states were generally the states that made voting harder, and that could plausibly be true in future years as well.

H3 stated that Republican state partisanship correlates negatively with voter turnout. As with H2, the meaning of the correlation was that increased Republican partisanship correlated negatively with turnout, or that turnout would decrease between Democratic and split government states and further between split government states and Republican states. The initial assumption neglected to account for the possibility that split government states could be more likely to be competitive states, and thus see higher turnout rates compared to states run solely by either party. The hypothesis therefore started out on shaky footing when the simplest model with only the two trifecta variables found that Republican states had statistically significantly lower turnout than split government states, but Democratic states also had lower turnout than split government states, albeit with no statistical significance. For the models using year-specific subsets, this held true for 2016, 2020, and 2022. The model with midterm year fixed effects found a statistically significant decrease in turnout only in Republican states, meaning that even when the decrease in turnout typical in midterm years was accounted for, Republican states still had a notable decrease in turnout compared to split government states. When all year fixed effects were accounted for, both Democratic and Republican states had statistically significant decreases in turnout compared to split government states. When all year and state fixed effects were included, both trifecta variables lost statistical significance. When only year fixed effects and their interaction terms were included, the Republican state decrease in turnout was statistically significant in all four year interaction models and the all-year interaction model, while the Democratic state decrease in turnout was only statistically significant in 2020. The bottom line for H3 and the models run to test it is that there is some evidence that Republican trifectas have decreased turnout compared to split government states and less evidence that Democratic trifectas do, but it all is wiped out when all year and state fixed effects are accounted for. This suggests that partisanship does not have a very strong effect on turnout, rather that state-specific factors overwhelm whatever impact partisanship might have. Of course, one state-specific factor in this case might just be its COVI score.

H4 stated that voter turnout correlates negatively with Republican two-party vote share at the state level. Many more models were used here, to test for potential differences between Republican vote share at the overall, presidential, gubernatorial, and senatorial levels. The basic models found a statistically significant negative relationship between turnout and Republican vote share only at the overall and presidential levels. A similar statistically significant negative relationship was found in the 2020 overall, presidential, and senatorial models and the 2022 overall and senatorial models. For the models with the two pandemic year fixed effects, 2020 and 2022, the slope was significant in the overall and presidential models. The negative relationship between turnout and Republican vote share held in all four models with all year fixed effects as well.

Out of the four year-state models, only the average vote share model had a statistically significant negative slope. Out of the four 2016 interaction models, the slope was statistically significant in the average and presidential 2016 models. For the three 2018 interaction models, the slope was statistically significant just in the overall model, which was mostly true for the four 2020 interaction models. None of the three 2022 interaction models had a statistically significant slope. Lastly, the slope was statistically significant in the average and senatorial all-year interaction models. The bottom line for H4 is that there is evidence that lower turnout had some correlation with higher Republican vote share, even with year fixed effects, state fixed effects, and interactions accounted for, at least when all topline statewide races were averaged. The robustness check used here, which added the Wyoming state fixed effect and removed the Nevada state fixed effect to set it as the baseline state, validated this finding. The broader implication here is that even if increased COVI in a state does succeed in decreasing voter turnout, that decreased turnout may not produce any more votes for Republicans compared to the previous voting regime–in spite of any intention to do so that the authors of stringent voting laws might have.

H5 stated that stringency of a state's voting regime correlates positively with Republican two-party vote share at the state level. The models used to test H5 were the same as those used to test H4, simply swapping out turnout for COVI as the explanatory variable. All four of the models regressing each vote share variable on COVI returned a positive slope estimate, as did the average and presidential 2016 subset models, average, gubernatorial, and senatorial 2020 subset models, and all three 2022 subset models. The relationship between COVI and Republican vote share also held in every model with the 2020 and 2022 year fixed effects and every model with all year fixed effects. It was not statistically significant in the models with all year and state fixed effects. Out of the fourteen single-year interaction models and three all-year interaction models, the relationship was statistically significant in all but the 2018 and 2022 gubernatorial models. The first robustness check used for H5, which substituted the interpolated 2018 COVI scores, also found no statistically significant relationship between COVI and vote share when all year and state fixed effects were included. Furthermore, the second robustness check, which added the Wyoming state fixed effect and removed the Nevada state fixed effect to set it as the baseline state, found an identical statistically insignificant relationship. The bottom line for H5 is that there is little evidence that increased stringency of voting laws led to increased Republican vote share, which holds when year fixed effects are included and vanishes when state fixed effects are also included. The broader implication here is that even as increased cost of voting decreased turnout and that decreased turnout did not greatly increase Republican vote share, if at all, it is not clear that increased cost of voting directly increased Republican vote share either.

After going through the process of constructing the various models with the variables of interest as well as year and state indicators, it became apparent that it was not possible in a timely manner to construct variables to represent all of the possible confounding variables mentioned in the methods section. Because they were not ultimately incorporated, not being able to address them is the major methodological weakness of this thesis. Here I will discuss each possible confounding variable and how each could have affected the models. The first confounding variable was the presence or absence of a statewide race. It ultimately did not make sense to include this variable, because across the entire dataset, only eleven state-year datapoints lacked a statewide race. The nine non-DC datapoints were included regardless in all models with turnout, and the two DC datapoints were only included in the average and presidential vote share models. The second confounding variable was the impact of the Supreme Court's *Dobbs* decision. I detailed how it could have a possible effect on turnout and Republican vote share in either direction. Constructing this variable would have necessitated using a single exit poll from 2022, or otherwise combing poll aggregators for abortion issue polls in each state, and then determining how to account for an additional continuous numerical variable (Fox News 2022). Furthermore, doing so would have been questionable in any case as the wording of abortion-related questions varied between polls. As for the presence of a statewide referendum on abortion rights, the widely disparate wording of the several states' referendum texts would have undermined the validity of this dummy variable. The third confounding variable was the impact of inflation or more broadly economic concerns. This would have faced the same technical issue of incorporating it into the models as well as the same issues with poll usage as *Dobbs* or abortion rights. The fourth confounding variable was presidential approval, which would have again faced the same technical issues as *Dobbs* or abortion rights and inflation or economic concerns. All three of these confounding variables may had affected turnout and Republican vote share and may also factored into year and state fixed effects.

Republican vote share and turnout trend relative to national swing were supposed to account for otherwise unaddressed state-level effects, but this also presented a technical challenge. However, state fixed effects were accounted for in any case, and this would simply have been another way to test for them. Furthermore, finding swing and trend values for 2016 would have necessitated adding 2014 turnout and vote share data, which in turn would have required altering the dataset at a late stage. Perhaps more important than any of the confounding variables was that I was unable to construct a so-called "focused" version of the COVI that excluded voting issue areas not directly addressed in the literature review. The COVI data provided by Schraufnagel et al included values for each voting issue area for each state in each year but did not include the calculations used to compile the final index scores. Thus, unlike my expectation, constructing the focused COVI values would have involved guesswork to determine what calculations to perform, or alternatively performing my own and ending up with vastly different values. Had I constructed a focused COVI score, it is likely that its range would have differed from the original, but the regression coefficients would not have changed significantly. This is because if the excluded issue areas were removed across all states, it is plausible that the differences between state values would not have changed significantly.

Conclusion

Testing the five hypotheses of this thesis showed that some were better supported than others, with possible implications for how elections are run in the US. As each state is free to implement any number of laws or regulations that govern voting, beyond what is required by federal law and the Constitution, these findings could mean different things for different states. It seems to be the case that increased stringency of voting regimes is associated with a decrease in turnout. So, states intending to increase turnout may want to decrease their cost of voting, and vice versa. As Republican states were likelier to increase their cost of voting, voters seeking to avoid this would want to elect Democratic governors and legislative majorities. This is self-evident, but it also illustrates a sort of feedback loop wherein it is more difficult for voters to electorally oust the officials who made it harder to vote in the first place. Even if Democratic states generally made voting easier than both split government and Republican states, split government states tended to see higher turnout regardless, likely because they tended to feature more competitive statewide races. It is not necessarily the case that the decreased turnout seemingly achieved by a higher cost of voting increases Republican two-party vote share in a state, possibly because those states with the highest cost of voting had some tendency to already have high Republican vote shares. So, while governors and state legislatures may have the goal of making voting either easier or harder, it is not clear that that alone will have the desired impact on the partisan outcomes of elections.

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