

Voting Access Reforms and Policy Feedback Effects on Political Efficacy and Trust

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Abstract: In 2020, states pursued divergent voting access reforms in an effort to facilitate a safe and secure election in the midst of a global pandemic. For some voters, options like mail-in or no-excuse absentee voting were familiar; for others, they were novel. While scholars have explored how election reforms affect turnout, we know less about how state-level electoral policies influence people's political efficacy and trust, and how political context and voter partisanship condition those effects. Employing a policy feedback framework, our analysis combines original data on state changes to election procedures with ANES survey data from 1988-2020 to understand the context-specific effects of voting access reforms on people's political efficacy and trust. Using time-event difference-in-difference analysis, we find little evidence that electoral reforms affect political efficacy and trust overall; however, partisanship and state political context appear necessary to understand the true relationship between electoral reform and political behavior.

Keywords: vote by mail; elections; policy feedback; voting behavior; difference-in-difference

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The onset of the Covid-19 pandemic posed a unique problem for election administration: how could states facilitate the 2020 election while protecting both election integrity and public health? States responded to this challenge differently, creating a mosaic of voting policy changes before the 2020 general election, most commonly expanding options to vote by mail. Concurrently, a major-party presidential candidate argued—falsely—that these reforms allowed for the election to be “stolen.” In this unprecedented milieu, changes to election law could be viewed by citizens of various states as an effort to protect or undermine public health, election integrity, or ballot access—or more simply, to gain partisan advantage. These reforms thus had the potential to affect the public’s perceptions of the voting process itself—perhaps the most central mechanism of representative democracy—with potentially broad implications for their views towards government and politics.

Of course, the 2020 election was not the first U.S. presidential contest in which states enacted new reforms for how citizens cast their ballots. It did, however, highlight the salience of those reforms, and it also illuminated the importance of considering how factors like partisanship and state-level dynamics might influence the way voters respond to electoral reforms. The weaponization of Covid-era voting reforms further called into question not only how these policies influence turnout, but also how they might change voters’ trust in political systems and feelings of political efficacy. While scholars have explored how different election reforms affect voter turnout, we know less about their capacity to influence voters’ feelings of political efficacy and trust in government. Moreover, scholars have yet to systematically examine how state-level

electoral policies and partisan dynamics influence these outcomes.

The relationship between election reform and political behavior is an especially critical issue as election administration becomes increasingly the target of partisan rhetoric. In this paper we ask how the adoption of election reforms intended to expand access to voting influence voters' political efficacy and trust in government. We also consider how state context and partisanship condition these effects. We employ a policy feedback framework to examine how individual characteristics and state contexts combine to generate interpretive effects that influence individuals' perceptions of their own political efficacy and trust in government after an electoral policy change to expand mail voting.

Consider, for example, two Pennsylvania voters—a Republican and a Democrat. In late 2019, Pennsylvania passed a law known as Act 77, allowing no-excuse absentee voting (NEAV) for the upcoming 2020 elections, meaning that voters did not need to provide a reason why they cannot vote in person, and any voter could request a mail-in ballot. While this law no doubt made it far safer to vote in an election held in the midst of the pandemic, in the lead up to and aftermath of President Trump's failed reelection bid, he and his campaign claimed (without evidence) that mail voting allowed for widespread election fraud. The Pennsylvania Republican may observe the state's decision to expand mail voting as a Democratic effort to "steal" the election. Because they attribute the change to partisan favoritism, this voting reform may decrease that voter's external political efficacy and trust in government. By contrast, the Pennsylvania Democrat, who is likely less receptive to Trump's claims of fraud, may perceive these changes as salutary efforts to protect public health and may feel greater trust towards government and feel more capable of effectively participating in the political process.

We test our expectations using data on state changes in election procedures matched to

pre- and post-election survey data from the American National Election Studies (ANES) cross-sectional waves from 1988 through 2020. In so doing, we shed new light on the political consequences of the battle unfolding across states to protect or undermine democratic elections. We also contribute to the evolution of policy feedback research, which is beginning to more fully grapple with the ways that federalism and partisanship mediate feedback effects (e.g., Michener 2018; Michener 2019; Clinton and Sances 2021; SoRelle and Fullerton 2024).

Ultimately, we find very little evidence to suggest that election reforms designed to expand access to mail voting systematically influence people’s feelings of political efficacy or trust across time or in response to the unique circumstances of specific elections, including 2020. While we describe how future work might interrogate these questions more closely—particularly with more robust measures of efficacy and trust on long-running time-series data like the ANES—our findings are consistent with other recent policy feedback studies that find counterintuitive relationships between policy experience and political efficacy and trust (see, for example, Shanks-Booth and Mettler 2019). Our findings also add to a growing body of literature suggesting that voting reforms to enable ballot access do not produce significantly disparate partisan shifts in electoral behavior—which may encourage policymakers to consider questions of electoral access reform on normative rather than instrumental grounds.

The Political Consequences of Election Reform: A Policy Feedback Approach

Much of the emphasis to date on evaluating election reforms has focused on their effect on voter turnout. For example, as more states implement policies like NEAV and universal vote-

by-mail (UVBM, in which all registered voters are provided with a mail ballot¹), studies have found that such reforms typically have a small effect on overall voter turnout (Barber and Holbein 2020; Amlani and Collitt 2022), although earlier studies found the effect of voting by mail to be null (Gronke and Miller 2012) or even negative (Kousser and Mullin 2007). Small, positive increases in turnout have also been found for the introduction of NEAV (Larocca and Klemanski 2011; but see Springer 2012), and scholars have found that the rapid introduction of both policies in response to the pandemic in 2020 had significant effects on voting method and turnout (Herrnson and Stewart 2023; Ritter 2023). Moreover, scholars have determined that neither UVBM (Thompson et al. 2020; Barber and Holbein 2020; McGhee et al. 2022; Amlani and Collitt 2022) nor expanded absentee voting (Yoder et al. 2021) translated to a partisan “advantage” in turnout for one party relative to the other, despite claims by Republicans that these reforms would unduly benefit Democrats.² The relatively small turnout effects of implementing reforms to ease voting are also consistent with work demonstrating that reforms to voter registration, and not the process of voting itself, often reap the biggest rewards when it comes to turning out new voters (Rigby and Springer 2011).

While the emphasis on voter turnout is obviously critical, it is by no means the only outcome of interest when it comes to election reform. In this article, we turn our attention to

¹ Even under a UVBM policy, states typically also allow some form of in-person voting in some circumstances; the primary distinction from NEAV is that registered voters are automatically provided with a mail ballot rather than upon request.

² In contrast, scholars have found that false claims of election fraud during and after the 2020 election had negative effects on turnout among Republicans, but not Democrats, in subsequent elections (Fraga, Peskowitz, and Szewczyk 2024).

whether reforms like NEAV and UVBM influence overall political efficacy and trust among voters. These are especially critical outcomes amidst increasingly partisan efforts to erode democratic institutions. Scholars have long acknowledged that new policies can create new politics (Schattschneider 1935; Lowi 1972). Perhaps in no domain might the potential for these policy feedback effects be more obvious than in the context of electoral reforms, which affect the most central and visible of democratic processes and have the potential to personally impact how each member of the polity engages in the political process. Yet, scholars have largely focused their attention on exploring feedback effects from the creation and implementation of social programs (e.g., Soss 1999; Campbell 2003; Mettler 2005; Moynihan, Herd, and Harvey 2015; Michener 2018; Barnes 2020; Jacobs, Mettler, and Zhu 2022), regulatory policies (e.g., Galvin 2019; SoRelle 2020, 2023; Walker 2020; Stokes 2020), and punitive forms of contact with the state (e.g., Lerman and Weaver 2014; White 2019; Walker 2020). Despite its limited application to the realm of election law to date (but see Springer 2012), the mechanisms of policy feedback have considerable potential to help us understand how voters' experiences with new election reforms might shape not only their propensity to participate in politics (as policy feedback scholarship often considers; see Larsen 2019), but also their trust in government and self-perception of their internal and external political efficacy—all critical psychological components of robust democratic citizenship.³

³ In the context of electoral reform, these effects may be especially relevant for young and new voters who are still being socialized to participation in democracy. But other voters' perceptions may also shift in response to reforms that significantly alter the voting process, because voting is such a core component of the relationship between citizen and state.

Resource and Interpretive Effects of Election Reform

Scholars delineate two major feedback mechanisms that can shape people's political behavior: resource effects, which influence people's capacity and incentives to engage in politics, and interpretive effects, which influence people's norms, values, and associated attitudes in a manner that can influence policy preferences and political action (Pierson 1993; Mettler and Soss 2004; Mettler and SoRelle 2023). In the context of election reform, policies that make it easier for people to vote, for example, by expanding access to voting locations, extending voting hours, or providing election material in accessible formats, might generate resource effects that functionally lower the costs of voting. (And, of course, election reforms that make access more difficult might increase the costs of voting.) Scholars have offered convincing evidence for how the costs of voting, and people's ability to overcome those costs, can influence whether eligible participants turn out to vote in elections (e.g., Brady, Schlozman, and Verba 1995; Blais 2000; Springer 2012).

Resource effects, however, are only one half of the puzzle. Policies can generate interpretive effects—through their design and implementation—that shape how people view their own citizenship vis-a-vis the state for a particular policy issue. One major avenue through which interpretive effects are generated is in how policies shape people's interactions with the state. As Soss explains, “policy designs structure participant program experiences in ways that teach alternative lessons about the nature of government” (1999, 362). When a policy makes it easier to claim benefits or participate in a program, people generally report positive experiences that can increase their self-perceptions of both internal and external political efficacy.

Beneficiaries experience increased internal political efficacy—the sense that one can engage in participatory processes effectively (Craig 1979; Craig et al. 1990)—when policies

make it easier to navigate governmental processes (Moynihan and Soss 2014). In the electoral context, reforms that make voting easier could strengthen feelings of internal efficacy. Indeed, Wolak and Stapleton (2023) find that expanded options for voting by mail in 2020 allowed voters to cast higher quality ballots that better aligned with their policy preferences (referred to in the literature as “correct” voting; see Lau and Redlawsk 1997). They argue that by allowing voters to complete their ballots at home and at their own pace, some voters took that opportunity to carefully consider their choices and conduct research on candidates at their leisure, thus increasing the rate of “correct” voting. Wolak (2018) also finds that the quality of election administration in a state corresponds with increased evaluations of efficacy among residents. We contend that these same dynamics may have improved voters’ sense of competence in democratic participation—that is, their internal efficacy. So, for example, if a state adopts UVBM or NEAV, people may find fewer roadblocks to securing and casting a ballot compared to their previous experience. This reduction in administrative burden could increase their feelings of internal efficacy as voters are more easily able to navigate the election process (Moynihan and Soss 2014; Moynihan, Herd, and Harvey 2015). Thus, because these policies may ease the process of voting and lower costs of participation, we expect that:

H1 (Internal Efficacy): NEAV and UVBM increase individuals’ feelings of internal efficacy.

Relatedly, beneficiaries of government policies report increased external efficacy—their belief that government will be responsive to them (Craig 1979; Craig et al. 1990)—when they feel treated fairly by government policies and actors (Soss 1999; Mettler and Soss 2004). And, of

course, the reverse is true as well, with negative experiences of policy implementation generating decreased feelings of trust and efficacy. By this logic, adoption of UVBM or NEAV, if perceived as government action to enable voter participation in the democratic process, may therefore improve voters' sense of external efficacy. Voters may feel that, in an effort to ease participation, policymakers are demonstrating that voters' participation is valued—increasing external efficacy as well. Thus, we might further expect that:

H2 (External Efficacy): NEAV and UVBM increase individuals' feelings of external efficacy.

Beyond efficacy, scholars have found that a similar interpretive mechanism can unfold for trust in government. When policy implementation leads to more positive (negative) interactions with the state, people can report greater (lower) levels of trust in the associated governing institution (Mettler and Soss 2004; Michener and SoRelle 2022). The relationship between policy experience and trust in government has been documented in numerous contexts, for example, interactions with the carceral state (Weaver and Lerman 2014), the civil legal system (Michener and SoRelle FC), and various social welfare policies (Kumlin 2004; Rosenthal 2021). If voters perceive that an election reform is being implemented in order to enhance citizen participation, then we might expect that such election reforms may correspond with a rapid increase in trust in government. We therefore hypothesize that:

H3 (Government Trust): NEAV and UVBM increase individuals' trust in government.

Conditional Effects from Partisan Identity and State Context

Of course, it may be the case that voting reforms nominally intended to expand access are not viewed the same way by all voters. For example, while the extension of NEAV expands access to mail-in voting while preserving existing methods of in-person voting, UVBM expands mail-in voting at the expense of widespread in-person options. The latter reform may be viewed by some as removing choice—increasing the costs of choosing to *not* vote by mail—and thus could make voters who prefer in-person voting feel as though their preferences are not being respected by government, with negative consequences for external efficacy. Relatedly, some populations, such as tribal groups living on reservations, may have more limited access to postal mail; thus, UVBM may actually increase costs and lower the ease of voting for some voters, with negative effects on both perceived efficacy and trust.

Scholars are increasingly identifying how individual and contextual characteristics might generate disparate feedback effects across groups of beneficiaries (see Mettler and SoRelle 2023). Some studies focus on how varying state policy contexts can influence people’s responses to ostensibly similar policy changes, for example, in the cases of Medicaid (Michener 2018) and public assistance (Soss, Fording, and Schram 2011). Other studies emphasize how individual characteristics interact with policy experience to shape the emergence and direction of feedback effects. For example, Vannoni (2019) demonstrates how feedback effects in response to tobacco regulations vary for smokers versus non-smokers.

One of the most critical individual characteristics known to influence feedback effects is partisan identity. Writing in the context of healthcare, (Mettler, Jacobs, and Zhu 2023; Fording and Patton 2019) and emergency disaster relief (Chen 2013), scholars have demonstrated how people’s partisan identities can condition their policy feedback response, with co-partisans more

consistently exhibiting positive participatory boosts and support for policymakers who extend benefits. Personal experience with policy can also interact with partisan identity to reshape feedback effects (Lerman and McCabe 2017). Yet partisan differences in feedback effects for some outcomes can also fail to materialize, even when partisan attitudes towards a particular policy regime are quite polarized (Sances and Clinton 2021). Thus, it is important to consider the possibility that partisanship and state policy context might condition any feedback effects generated by election reforms.

In the context of mail voting reforms, for example, partisanship may have affected voters' perceptions of mail voting reforms adopted for the 2020 presidential election in which mail voting efforts became a target of Republican attack. We anticipate this difference could shape not only individual take-up of these new voting systems (Lerman, Sadin, and Trachtman 2017) but also interpretative responses to the new policy regime. Given the increasing importance of partisan polarization on people's attitudes toward politics as well as their interpretation of elite rhetoric and media, we might expect that Trump's co-partisans (and especially those who lacked experience with functioning UVBM and NEAV prior to 2020) would be more accepting of his false claims of fraud. At the same time, Democrats would likely view UVBM and NEAV as critical measures for protecting public health, because partisanship greatly conditioned perceptions of Covid risk in 2020 (Rothwell and Desai 2020), thus increasing trust in government and external efficacy.

While we focus our discussion here on the unique circumstances of the 2020 election as an exemplar, we also expect that perceptions of election reforms might be conditional on partisanship in other contexts where it is salient and associated with the enactment of the reforms themselves. In this vein, partisan control of the state government may interact with individuals'

partisan identities to shape how electoral reforms affect political efficacy and trust in government. If mail voting is enacted by a given individual's preferred party, co-partisans may be more supportive of the electoral reform, generating increased political efficacy and trust in government. However, if mail voting is enacted by the opposing party, the electoral reform could be viewed skeptically, thus decreasing political efficacy and trust in government. In addition to our primary tests of the hypotheses with respect to Americans overall, we also test the effects of NEAV and UVBM adoption on political self-efficacy and trust for (respectively) Democrats, Republicans, partisans whose preferred party enjoyed control of the state government when reforms were enacted, and partisans whose state was controlled by the opposing party or had a divided government.

Data and Methods of Analysis

We test our hypotheses using the American National Election Studies (ANES) cumulative data file⁴ for nine presidential election cycles from 1988 through 2020, supplemented with an original dataset of state-level policy changes that introduced NEAV or UVBM for presidential elections. To analyze the individual-level interpretive effects of these policy changes, we rely on repeated cross-sectional measures of external political efficacy, internal

⁴ The ANES conducts two-wave panel surveys of US adult citizens in each presidential election cycle, with one pre-election wave and one post-election wave. Although the ANES sometimes includes additional sampling frames (such as sampling from prior ANES respondents to generate a multi-year panel dataset), in each election cycle the ANES generally seeks a fresh cross-sectional sample of the target population via address-based sampling. Additional information on ANES data and methodology is available at <https://electionstudies.org>.

political efficacy, and trust in government included in the ANES time series cumulative file.⁵

To measure external efficacy, we use two questions from the ANES (Cronbach's $\alpha = 0.69$). These questions ask respondents to agree, disagree, or neither agree nor disagree with the statements "Public officials don't care much what people like me think" and "People like me don't have any say about what the government does."⁶ We reverse-code responses to both statements (such that disagreement, indicating higher external efficacy, is expressed with higher values). We then rescale each variable to vary between 0 and 1, and we take the simple average of both as a measure of external political efficacy, with higher values indicating greater external efficacy.

The ANES has included two questions on internal political efficacy since 2008 (Cronbach's $\alpha = 0.52$). The first question asks respondents "How well do you understand the important political issues facing our country?" on a five-point scale from "not well at all" to "extremely well." The second question asks, "How often do politics and government seem so complicated that you can't really understand what's going on?" on a five-point scale from

⁵ As we discuss further in our concluding remarks, the efficacy and trust measures that have been fielded on the ANES are imperfect, being both very general rather than specific to election administration, and quite coarse in their measurement. Nevertheless, to our knowledge these are the only such measures available in a cross-sectional dataset for such an extensive range of U.S. presidential election years; analyzing across this extended period allows us to include more state-level changes in our aggregated estimates. To the extent that voting reforms may alter trust and efficacy through cohort-level effects from the socialization of new voters under new electoral policy regimes, our long time-horizon is also helpful for capturing these effects, in addition to any individual-level attitude change that may result directly from voting policy change.

⁶ The 1988 and 1992 cross-sections used a slightly different phrasing for the first question: "I don't think public officials care much what people like me think."

“never” to “all the time.” We reverse-code the responses to the latter question, rescale each to vary between 0 and 1, and take the simple average for each individual as a measure of internal political efficacy, with higher values indicating greater internal efficacy.

Finally, we use a single question to measure trust in government, which asks respondents “Would you say the government is pretty much run by a few big interests looking out for themselves or that it is run for the benefit of all the people?” on a dichotomous scale, with 0 indicating less trust and 1 indicating greater trust.⁷

To identify state adoption of NEAV and UVBM over time, we first used the Brookings Institute’s state-by-state scorecard for ease of voting by mail in the pandemic (Kamarck et al. 2020) to identify the states that implemented NEAV or UVBM for the first time in 2020. We refer to Springer (2014, 78) and data collected by the National Conference of State Legislatures to identify states that implemented NEAV or UVBM prior to 2020. We verified the first year of implementation by looking at the text of state statutes and local reporting on election changes. The staggered adoption of NEAV and UVBM across the states is shown in Table 1. A total of 3 states have allowed NEAV since before 1988; another 26 states and DC introduced this policy between the 1988 and 2016 presidential elections. For the 2020 election, 16 more states either allowed NEAV in full or allowed any voter to request a mail ballot because of the Covid-19

⁷ As previously noted, the ANES measures are not ideal in terms of conceptual fit or measurement properties, and that is particularly true of this single item. The ANES cumulative file includes a superior item regarding trust in the federal government “to do what is right” on a multi-item response scale, but both the question wording and the response scale were changed starting in 2016; the available data in the cumulative file thus only cover a portion of our study period from 1988 to 2012. Because many of the state-level treatments occurred later in our study period (particularly for UVBM), we opted to use the consistent dichotomous item as our measure of trust.

pandemic, functionally instituting NEAV for that election despite not adopting the policy on a broader basis. Meanwhile, Oregon became the first state to conduct a presidential-year general election entirely by mail in 2000 after experimenting with all-mail local and statewide elections for a number of years, and has continued the practice ever since. The state of Washington followed in 2012, as did Colorado starting in 2016. In 2020, six additional states and DC employed UVBM for a presidential election for the first time. We use the variation in NEAV and UVBM adoption across states over time to estimate the causal effect of these policies on internal efficacy, external efficacy, and trust in government.

Table 1: State Implementation of NEAV and UVBM by Year, 1974-2020

State	NEAV	UVBM	State	NEAV	UVBM	State	NEAV	UVBM
Alabama	2020	–	Kentucky	2020	–	North Dakota	1997	–
Alaska	1996	–	Louisiana	2020	–	Ohio	2005	–
Arizona	1991	–	Maine	1999	–	Oklahoma	1991	–
Arkansas	2020	–	Maryland	2009	–	Oregon	1980	1998
California	1978	2020	Massachusetts	2020	–	Pennsylvania	2020	–
Colorado	1992	2013	Michigan	2018	–	Rhode Island	2020	–
Connecticut	2020	–	Minnesota	2013	–	South Carolina	2020	–
Delaware	–	–	Mississippi	–	–	South Dakota	2020	–
D.C.	1992	2020	Missouri	2020	–	Tennessee	–	–
Florida	2001	–	Montana	1999	–	Texas	–	–
Georgia	2005	–	Nebraska	1999	–	Utah	2004	2020
Hawaii	1993	2020	Nevada	1991	2020	Vermont	2001	2020
Idaho	1994	–	New Hampshire	2020	–	Virginia	2020	–
Illinois	2009	–	New Jersey	2005	2020	Washington	1974	2011
Indiana	–	–	New Mexico	1993	–	West Virginia	2020	–
Iowa	1990	–	New York	2020	–	Wisconsin	1999	–
Kansas	1995	–	North Carolina	2001	–	Wyoming	1991	–

Note: Table indicates the year of the first election for which each state implemented no-excuse absentee voting (NEAV) and universal vote-by-mail (UVBM), respectively, from 1974 to 2020.

To identify party control of each state’s government, we compiled data from Dubin (2007), the National Conference of State Legislatures, and the National Governors Association. For each state-year, we looked at the party in control of the governor’s office and each chamber of the state legislature. If Democrats (Republicans) held a majority in each chamber and a Democrat (Republican) occupied the governor’s office, then the state was coded as Democrat-

controlled (Republican-controlled).⁸ Any other cases were coded as divided.

A traditional strategy is to estimate the average treatment on the treated (ATT) via a difference-in-differences analysis with two-way fixed effects (TWFE) to account for both time-varying contextual factors and time-invariant state-level characteristics. A TWFE model in this setting takes the form:

$$Y_{ist} = \alpha_s + \lambda_t + \tau^{DD}D_{st} + X_i + \varepsilon_{ist}$$

where α_s expresses time-invariant state-level fixed effects, λ_t expresses time-period fixed effects, D_{st} is an indicator variable for treatment in a given state in a given time period, and X_i expresses a vector of individual-level covariates. The coefficient τ^{DD} is typically interpreted as the estimated effect of treatment on treated units in treated periods, i.e., the ATT.

Such models risk biased estimates of the ATT when the treatment adoption is staggered across multiple periods (de Chaisemartin and D’Haultfœuille 2020; Goodman-Bacon 2021; Sun and Abraham 2020), as is the case here. ATT estimates provided by a standard TWFE estimator will be unbiased only when the treatment adoption is not staggered, or the effect of adoption is both constant over time and across treated units (Baker, Larcker, and Wang 2022). We cannot reasonably assume that these conditions hold for the adoption of NEAV or UVBM. The particularities of state-level election administration, political context, and historical context are likely to influence both the rollout and effects of policy adoption, making a uniform treatment effect across states improbable. Further, because these policy changes may disrupt voters’ existing turnout habits (Aldrich, Montgomery, and Wood 2011), the treatment effects of either policy may not be realized immediately, suggesting that the treatment effect may vary over time

⁸ Because Nebraska has a unicameral, non-partisan legislature, we code each state-year for Nebraska based solely on control of the governorship.

as well.

The crux of the issue is that the τ^{DD} coefficient of a TWFE estimator with staggered treatment adoption is actually a weighted average of multiple constituent 2x2 (two group, two period) comparisons, in some cases where some treated units serve as a control for other treated units, and for which the weights are sensitive to the number of time periods observed (Goodman-Bacon 2021). The consequence is that simply observing another time period (i.e., another election) without any new treatment adoption or changes to the true treatment effects will nevertheless induce changes in τ^{DD} because of changes in the weights.

In light of these concerns, we instead employ an alternative ATT estimator proposed by Callaway and Sant'anna (2021). This event-study difference-in-difference estimator defines groups of units G_g (states, in this case) that are first treated in a given time period g , and estimates the effect of treatment for each such treated group at that time period and for each subsequent time period. That is, each state belongs to exactly one group G_g , the group of states that first experienced treatment in a given presidential election year, or to group C of never-treated states. Following the standard logic of a two-period difference-in-differences analysis, when the data is subset to units in group G_g or C at time g or the previous time period, a regression of the form:

$$Y_{ist} = \alpha_g + \lambda_t + \tau^{GT} D_{gt} + X_i + \varepsilon_{igt}$$

where D_{gt} is an indicator for membership in group G_g at time g (the treatment period) yields an unbiased estimate of the *group-time* ATT estimand (τ^{GT}). These several group-time ATT estimates may then be aggregated by several methods to provide a single-parameter estimate.

Callaway and Sant'anna offer four different types of aggregation to define a single-parameter ATT estimate. These include: a group-level average that aggregates within each group

G_g over time and then averages across groups; a dynamic-exposure average that aggregates for a given length of exposure, then averages across all lengths of treatment exposure; a calendar-time average that aggregates treatment effects for a given time period (i.e., a given election year), then averages across time periods; and finally, a simple average that provides an average of all group-time treatment effects, weighted proportionally to group size. To test our hypotheses, we preference the “group” method of aggregation, which most closely mimics the underlying logic of a difference-in-differences analysis to estimate a single ATT across all treatment adoptions (i.e., across states) over the period of study.

We analyze the effects of NEAV and UVBM separately throughout, using slightly different subsets from the ANES cumulative file. To analyze NEAV, we subset to state-year observations where UVBM is not observed—that is, we limit the analysis to states that never implemented NEAV or implemented NEAV between 1988 and 2020. We exclude any state-year observation in which a state switched from NEAV to UVBM and observations from states that maintained NEAV for the entire study period up until adopting UVBM (California, Oregon, and Washington). We are thus able to compare a single level of treatment (NEAV), roughly equivalent across states, against control observations in which mail balloting was either not permitted or strictly limited.

Similarly, to analyze UVBM, we subset to state-year observations where NEAV was already in place or where UVBM was implemented, excluding all state-year observations in which neither of these policies was in place. We can thus compare the effect of shifting from NEAV to UVBM as a (again roughly equivalent) single level of treatment across states. There are no cases of states that shifted from no or limited mail-in voting directly to UVBM. We conduct our analyses in R using the “did” package (Callaway and Sant’anna 2021) and cluster

standard errors at the state level.

Results

Table 2 summarizes our main results, reporting the estimated single-parameter ATT of adopting NEAV (columns 1-3) or UVBM (columns 4-6) across the full sample and subsamples. Broadly speaking, we find that the estimated ATT for each policy on political efficacy and trust is null. Across nearly all outcomes and subsamples, the estimated ATT is close to zero and not statistically significant at standard thresholds.

We first consider the overall effect of adopting NEAV or UVBM on internal political efficacy (columns 1 and 4 of Table 2, respectively). H1 expects that these reforms reduce the costs of participation, thus increasing treated individuals' perceived ability to engage effectively in politics—that is, increasing their feelings of internal efficacy. We find no evidence of a positive (or negative) effect on internal efficacy from the adoption of either policy: the aggregate ATT estimate is not distinguishable from zero for either NEAV (-0.70 percentage points; s.e. 1.57 p.p.) or UVBM (-0.53 p.p.; s.e. 2.61 p.p.), and the estimated effect on internal efficacy for each individual adoption group (reported in Tables A1 and A2 in the Appendix) is quite small and not statistically significant for nearly all adoption groups for both policies.⁹

⁹ The sole exception to this pattern is Washington state's adoption of UVBM in 2012, which is estimated to have a sharply negative effect on internal efficacy. While unique unobserved state-level characteristics may have affected how the policy adoption was perceived to produce this result, we consider the most likely explanation to be the small state-level sample size for Washington state, particularly in the cross-sections before 2012. The ANES cumulative file only provides 23 respondents from Washington state in 2008, which is the first year that the internal efficacy variables were fielded, but 4 to 8 times as many respondents in subsequent waves.

Table 2: Estimated Average Treatment on the Treated (Group Aggregation by Sample)

Sample	<i>No-excuse Absentee Voting (NEAV)</i>			<i>Universal Vote by Mail (UVBM)</i>		
	Internal Efficacy (1)	External Efficacy (2)	Government Trust (3)	Internal Efficacy (4)	External Efficacy (5)	Government Trust (6)
Full Sample	-0.0070 (0.0157)	0.0487 (0.0310)	0.0091 (0.1789)	-0.0053 (0.0261)	0.0293 (0.0338)	-0.0582 (0.0298)
Democrats	-0.0116 (0.0280)	0.0349 (0.0422)	-0.0092 (0.1110)	-0.0184 (0.0263)	0.0236 (0.0536)	-0.1074 (0.0664)
Republicans	0.0045 (0.0367)	0.0517 (0.0960)	-0.0311 (0.2290)	0.0153 (0.0549)	0.0199 (0.0967)	0.0314 (0.0664)
Preferred Party State Trifecta	0.0275 (0.0284)	0.0741 (0.0541)	0.0621 (0.0284)	0.0093 (0.0632)	0.0635 (0.0617)	-0.2091** (0.0841)
Outparty Trifecta or Divided	-0.0014 (0.0500)	0.0385 (0.1838)	-0.0107 (0.1436)	-0.0172 (0.0411)	-0.0412 (0.1017)	0.0575 (0.0689)

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

Note: Table provides full sample and subsample single-parameter ATT estimates via the “group” aggregation method proposed by Callaway and Sant’anna (2021). Data from the ANES cumulative file.

Turning to external efficacy, we find that neither NEAV nor UVBM adoption (reported in columns 2 and 5 of Table 2, respectively) results in increased external political efficacy overall, in contrast to the expectations of H2. The aggregate ATT estimate for NEAV is moderate but not statistically significant (4.87 p.p.; s.e. 3.10 p.p.), while the estimate for UVBM is slightly smaller and again not statistically significant (2.93 p.p.; s.e. 3.38 p.p.). That said, we do find some limited evidence of positive effects from NEAV adoption in states that have adopted the policy more recently, as shown in Appendix Table A1. Specifically, we see positive and statistically significant estimated effects for all adoption groups from 2008 onward, on the order of a 5-12 percentage point increase.¹⁰ We do not observe this same pattern for UVBM, for which the individual adoption group-level estimates are nearly all null (see Appendix Table A2).¹¹

¹⁰ We note that the pretreatment parallel trends assumption fails to hold for this outcome, so this pattern should be interpreted with caution.

¹¹ Washington state (the sole member of the 2012 adoption group) is again an outlier, with an estimated positive effect on external efficacy. While feelings of internal and external efficacy are not strongly correlated across

We next consider whether NEAV and UVBM adoption improves individuals' trust in government (H3). We fail to find any such positive effect overall, as shown in columns 3 (NEAV) and 6 (UVBM) of Table 2. The estimated aggregate ATT of NEAV adoption on government trust is negligible (0.91 p.p.; s.e. 17.89 p.p.) and not statistically significant. For UVBM adoption, the aggregate ATT estimate is modestly negative but again not statistically significant (-5.82 p.p.; s.e. 2.98 p.p.).¹²

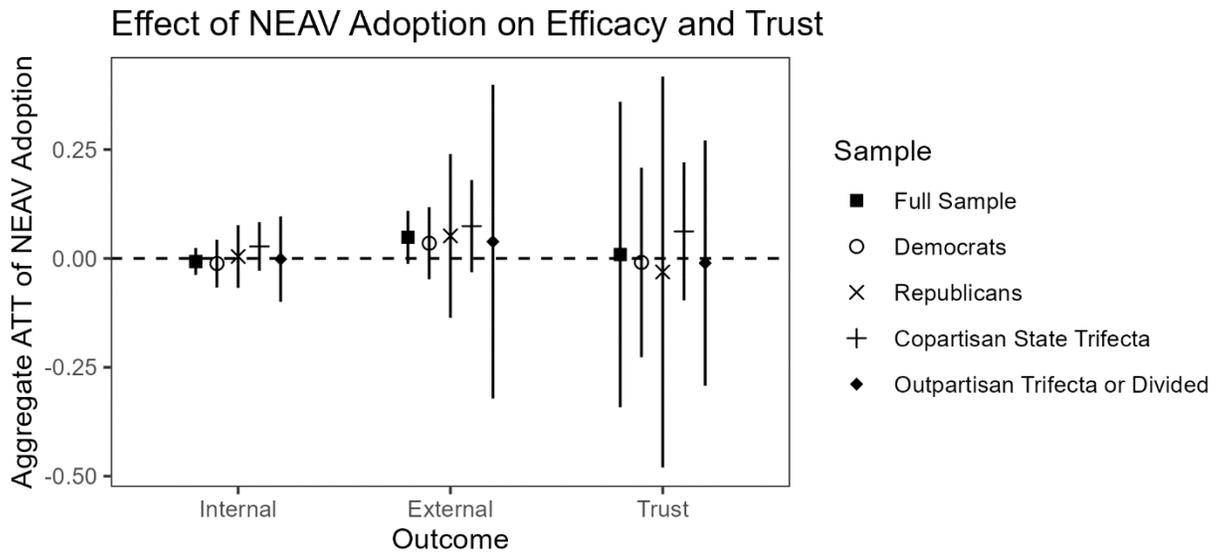


Fig. 1 Figure displays the group-average ATT estimate of the effect of NEAV adoption on each outcome by subsample.

individuals, particularly vis-à-vis state politics—for example, Wolak (2018, 771) finds almost no relationship ($r = 0.02$)—Washington state's movement in opposite directions on these two variables reinforces the plausibility of spurious results driven by small, less representative samples in the pre-adoption periods (see footnote 8).

¹² At the level individual adoption groups (see Appendix Table A2), we find significant estimated ATTs with opposite signs. Oregon, the earliest adopter of UVBM, indicates a large positive effect on trust, whereas the estimated ATT for the seven states that adopted the policy for the 2020 election indicates a moderate negative effect. As we note for other group-level estimates, the positive estimated effect for Oregon should be regarded with particular caution due to small sample sizes in pre-treatment cross-sections of the ANES.

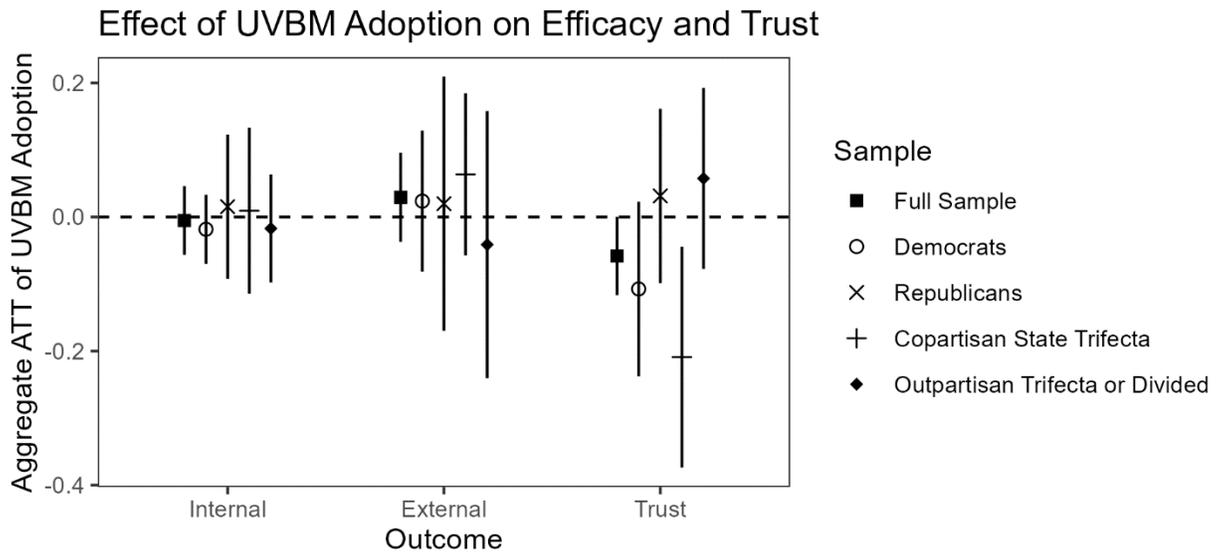


Fig. 2 Figure displays the group-average ATT estimate of the effect of UVBM adoption on each outcome by subsample.

While we fail to find positive effects from NEAV and UVBM adoption overall, we also consider the possibility of effects on efficacy and trust that are conditional on partisanship.¹³ We explore this possibility by analyzing four specific subsamples (reported in Table 2 above). First, we analyze Democrats and Republicans (including Independents who lean towards the respective party, but excluding “true” Independents who do not lean towards either party) separately. Second, we analyze partisans (including leaners) whose preferred party either enjoyed a “trifecta” of power in their state government—that is, control of the governorship and both

¹³ In the Appendix, we also report analyses considering possible effects conditional on political engagement (see Appendix Table A9). Most estimates indicate null effects of both policies on efficacy and trust for both high- and low-engagement respondents.

chambers of the state legislature¹⁴—or whose state government was controlled by the opposing party or divided between the parties, because these partisanship-conditioned relationships with the government might produce divergent effects on political efficacy and trust.¹⁵ Specifically, we might expect that those whose preferred party holds power might experience a positive effect of NEAV and UVBM because the state is likely to be perceived as taking actions aligned with their interests, whereas those whose party is out of power might view these reforms as against their interests. For these analyses on state government control, we drop treated observations after the first adoption period to ensure a clean comparison: partisans who enjoyed preferred-party state control prior to treatment versus those whose preferred party was in power when the election reform went into effect. Figure 1 (for NEAV) and Figure 2 (UVBM) present the aggregate group-average ATT estimate of policy adoption on each outcome across these subsamples, compared against the main full-sample aggregates.

In aggregate, we find no evidence that adopting either policy affected efficacy or trust specifically for Democrats (hollow circles in Figure 1 and Figure 2) or Republicans (X-marks). Meaningfully, we also find null effects on all outcomes for both Democrats and Republicans in states that adopted a new mail voting policy specifically for the 2020 election (see Appendix Tables A3 and A4). We find similar effects for partisans who enjoy, or do not enjoy, a trifecta of state power (see Appendix Tables A5 and A6); for nearly all outcomes, the estimated aggregate

¹⁴ For Nebraska, which has a nonpartisan, unicameral legislature, we use control of the governorship as the measure of state power.

¹⁵ In some cases, we lack sufficient observations in the ANES cumulative file for a given subsample in a given year to estimate an adoption group-level aggregate estimate; however, in all subsamples, we are able to generate an aggregate ATT estimate across all adoption groups for which we have sufficient data.

ATT of either policy is not statistically significant.

The sole exception is that we find a large and statistically significant negative effect of UVBM adoption on trust in government among partisans whose state is controlled by their preferred party (-20.91 p.p.; s.e. 8.41 p.p.). This effect is driven primarily by partisans in states that adopted UVBM for the 2020 presidential election, as shown in Appendix Table A6 (group-level ATT -21.81 p.p.; s.e. 8.57 p.p.). With the exception of Utah (Republican-controlled) and Vermont (divided), these partisans are Democrats who enjoy a trifecta of state power—which, given the context of the 2020 election campaign and polarized attitudes regarding public health during the ongoing pandemic, might make this negative result somewhat surprising. There are few reasons to believe that this negative group-level estimate is driven by thermostatic shifts in trust based on party control of the presidency, rather than an effect of a contemporaneous adoption of the UVBM policy. First, this group-level estimate must rely on a single post-treatment period—when a divisive Republican president occupied the White House. Second, many of the comparison states (states with NEAV but that never adopted UVBM) are Republican-leaning, whose residents can be expected to hold more trust of the then-current president than many of the UVBM adopting states. Finally, the parallel trends assumption fails to hold for this analysis, pointing to divergent shifts in government trust prior to the policy adoption that may produce a spurious result.

We consider one final possible reason for the null effects of mail voting adoption on efficacy and trust that we find above: as with any systematic change in the routine way of doing something, people may need confirmation that the new system “works” as anticipated before feeling positively about it. In the context of elections, we might expect that increased political efficacy and trust becomes more evident after an election cycle demonstrates that the new system

functions as intended. For example, voters in Oregon have been using UVBM for presidential elections since 2000. They may have better trust in this system thanks to decades of experience with it relative to voters trying out mail voting for the first time in 2020 in the midst of a major public health crisis and abundant partisan rhetoric around the security of mail ballots. Thus, positive gains to feelings of political efficacy and trust may lag behind the initial adoption of mail voting reforms because voters may need to experience repeated voting cycles with the new electoral procedures to believe that they work.

To test this possibility, we rely on an alternate “dynamic” aggregation strategy proposed by Callaway and Sant’anna (2021). By aggregating the individual ATT results dynamically by length of policy exposure—that is, 4 years after adoption, 8 years, 12 years, and so on—and examining the ATTs multiple cycles after adoption, we can evaluate the estimated effects as the public accumulates familiarity with these new voting systems. As shown in Appendix Table A7 and A8, we see little evidence that mail voting policy longevity is associated with greater political efficacy or trust in government. While we observe occasional significant estimates well after the adoption of a policy, the overwhelming majority of post-adoption estimates are not distinguishable from zero, and the estimates do not follow a consistent pattern of increasing strength as time since adoption grows.

Discussion and Conclusion

To what extent do election reforms designed to expand access to voting influence people’s political efficacy and trust—both critical prerequisites for engaged democratic citizenship? And did the introduction of these reforms in the unique turmoil of the 2020 election lead to distinctive partisan effects? With a few exceptions, we find little evidence that the

introduction of either NEAV or UVBM meaningfully influences voters’ feelings of efficacy or trust in government, as shown by the broad range of null results across outcomes and subsamples. We see some evidence that UVBM adoption negatively affects trust in government among partisans whose preferred party controls the state government, and occasional significant effects for individual adoption groups on one outcome or another—but these statistically significant results are by far the exception rather than the rule and are not much more common than we might expect by chance. Table 3 provides a succinct summary our findings across several populations and outcomes of interest.

Table 3: Summary of Results by Sample

Sample	<i>No-excuse Absentee Voting (NEAV)</i>			<i>Universal Vote by Mail (UVBM)</i>		
	Internal Efficacy (1)	External Efficacy (2)	Government Trust (3)	Internal Efficacy (4)	External Efficacy (5)	Government Trust (6)
Full Sample	Null	Null	Null	Null	Null	Null
Democrats	Null	Null	Null	Null	Null	Null
Republicans	Null	Null	Null	Null	Null	Null
Preferred Party State Trifecta	Null	Null	Null	Null	Null	–
Outparty Trifecta or Divided	Null	Null	Null	Null	Null	Null

Note: Table summarizes positive, null, and negative results across policies, outcomes, and (sub)samples from “group” aggregation of time-event ATT estimates.

We conclude that expansive voting reforms like NEAV and UVBM do not broadly increase internal political efficacy, external political efficacy, or trust in government after each reform is first adopted for presidential elections. These findings offer theoretically, methodologically, and normatively important conclusions about the relationship between election reforms and democratic participation in the United States. From a theoretical standpoint, the generally null findings for the relationship between the adoption of reforms designed to expand access to the ballot and markers of efficacy and trust help explain why existing studies may find such a minimal boost to political engagement following introduction of these policies.

Because efficacy and trust are often mechanisms that can influence political engagement (Mettler and SoRelle 2023), the absence of a positive change in one likely explains the absence of an increase in the other.

These findings suggest several implications for scholarship on policy feedback. With respect to this particular case of election administration, our null results indicate that, if there is a feedback effect at work, any boost in participation stems from resource mechanisms that curtail the costs of voting rather than the interpretive effects we consider here. It is also possible that the move to UVBM or NEAV does not constitute a significant enough reduction in the administrative burden associated with election administration that such a policy influences feelings of efficacy or trust. This possibility could also help to explain why scholars have found more positive effects on voting turnout from electoral changes that ease registration rather than voting. Perhaps the real opportunity to reduce burdens to political engagement exist within the registration process, and it is there we would be more likely to find positive feedback effects on efficacy and trust. Beyond these promising areas for future research, our study documents a new case in which policy feedback effects fail to materialize (see, for example, Galvin and Thurston 2017; Patashnik and Zellizer 2013).

Finally, these findings contribute to the growing emphasis on understanding partisan dynamics of feedback effects. To the extent that we find some case-specific deviations from our broadly null findings, the effect of expansive voting reforms on political efficacy and trust in government may be strongly context dependent. A Republican in a red state may interpret election reform differently than a Republican in a blue state or even a Democrat in a red state. The partisan dynamics may condition how a policy is interpreted by citizens and what message those citizens internalize about democracy and their place in it. Essentially, the interpretive effect

of policies may be more context dependent than the resource effects—something that future work would be well positioned to further explore. But it is also possible that, unlike in the case of healthcare (Mettler, Jacobs, and Zhu 2023) or disaster aid (Chen 2013), there may be policy issues for which partisan feedbacks do not materialize, even when the policy in question is frequently framed in partisan terms. This raises questions for future scholarship about the conditions under which partisan feedback will manifest.

From a methodological standpoint, our study also suggests the necessity of implementing improved measures of efficacy and trust in flagship national surveys of political behavior. In our analyses, we rely on measures that offer only a coarse glimpse at political efficacy and trust, in that they are both imprecise in scope (“the government”) and have poor attitude measurement properties (few response options, low scale coherence). We do so because these are the only measures that are consistently available across numerous U.S. presidential election cycles. Building on more precise conceptions of internal and external efficacy and trust vis-à-vis specific government organs—particularly those related to election administration, in our case—to field improved measurement instruments on longitudinal¹⁶ and repeated cross-sectional political behavior survey programs would better enable scholars to understand the long-term effects of electoral reform across political contexts. Similarly, measuring the method of voting that individual respondents actually use—as the ANES has recently begun to do—can help scholars unpack the effects of policy take-up on efficacy and trust (Lerman and McCabe 2017; Lerman et al. 2017).

¹⁶ Panel data would be especially helpful for distinguishing between individual- and cohort-level change. Here, our analysis of the ANES cross-sectional data only permits examining the aggregate sum of these two possible mechanisms, though our broadly null results indicate that change at either level is, at best, weak.

Moving beyond the implications of these findings for scholarship, there are critical normative implications for how we think about the adoption of election reforms. Much of the debate surrounding the implementation of new measures to expand access to voting has been bound up in partisan politics—specifically, with Republicans worrying that expanding access would increase participation among populations who typically support the Democratic party, incurring political costs for Republican candidates. As a result, the expansion of voting access has been patchwork and partisan, with voters in Democratic-led states often (though not exclusively) gaining greater access to the ballot, while voters in Republican-led states experience more restrictive measures (Grumbach 2022a, 2022b). But the largely null findings we identify here, combined with similar findings about partisan participation rates in existing studies (Barber and Holbein 2020; Thompson et al. 2020; Yoder et al. 2021), suggest that concerns about how the adoption of expansive ballot access might confer partisan advantages are overblown. Thus, debates over election reform ought to be able to proceed on normative, rather than explicitly political or instrumental grounds—focusing instead on whether democratic citizenship is normatively improved with more expansive access to voting and other forms of political engagement.

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