

How Election Rules Affect Who Wins

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ABSTRACT

Contemporary election reforms that are purported to increase or decrease turnout tend to have negligible effects on election outcomes. We offer an analytical framework to explain why. Contrary to heated political rhetoric, election policies have small effects on outcomes because they tend to target small shares of the electorate, have a small effect on turnout, and/or affect voters who are relatively balanced in their partisanship. After developing this framework, we address how the findings bear on minority voting rights. We then show that countermobilization from political parties cannot explain the small effects of election laws. We explain that even when a state passes multiple policies at the same time, the reforms will still only have a marginal effect on turnout and an ambiguous effect on who wins. Finally, we explain what policies should raise alarm about affecting outcomes.

1. INTRODUCTION

Thirty years ago, Congress passed a law allowing eligible citizens to register to vote at departments of motor vehicles. The debate leading up to the bill's passage had several features familiar to anyone who follows debates about voting laws: the law was passed largely on a partisan basis, it was accompanied by accusations that the law's supporters had a partisan motive, and it was subject to years of litigation (*'Motor Voter' After 5 Years*, 1993). The actual effect of the law did not match the political rhetoric: after the policy was implemented and scholars measured its partisan political consequences, they found basically no change at all (Knack & White, 1998).

Election laws, in general—from voter identification and felon disenfranchisement to automatic registration and no-excuse mail voting—are presumed to have the intent or effect of influencing who votes, and, in turn, they are expected to impact partisan election outcomes. Such policies are highly polarizing. They are passed by legislatures along partisan lines and sometimes litigated in court. Many people express dismay about laws they disagree with, arguing the laws have dire consequences for American democracy, such as they generate fraud or they amount to “democratic backsliding”.

And yet, the reality of research on election administration does not support the dire rhetoric from either side. Policies beget studies and evidence. And the evidence shows the laws have small effects on turnout and essentially no effect on partisan advantage in a state. This is the puzzle we address: Why do election laws bear such a modest relationship to who wins and who loses?

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Our answer is that modern election reforms target narrow shares of the population, have a small effect on turnout, and/or are imprecisely targeted at members of political parties. To see how this combination of facts results in small effects, consider an initial, hypothetical example with features that will be similar to actual examples used throughout the paper. Suppose a state recently held a close election in which 51 percent of voters supported the Democratic candidate and 49 percent of voters supported the Republican candidate. In response to the election, the Republican-controlled state legislature passes a bill that imposes additional requirements to vote and these requirements disproportionately target Democratic voters. Specifically, the additional requirements target 4 percent of the electorate and as a result of these requirements, there will be a 3 percentage point decline in turnout in this group. The targeted group is strongly Democratic: 60 percent of the targeted group supports the Democratic presidential candidate.

What would happen if the 51/49 election were held again and everything about the election was the same except for this law? The policy would cause a 0.12 percentage point decline in the overall turnout. And it would cause a 0.011 percentage point decline in the two-party vote share for the Democratic candidate. In other words, the Republican party would lose the election with nearly identical results: in the new election 50.989 percent of voters would support the Democratic candidate while 49.011 percent would support the Republican candidate. If the state had one million eligible voters, the policy would deter 720 Democratic voters and 480 Republican voters, netting the Republicans a 240-vote shift.

In the left-hand of Figure 1, we walk through the steps of the law, showing it has a small overall effect because the targeted group is small and the effect on turnout is small. The right-hand plots show that even though the group is disproportionately Democratic, the law is not perfectly targeted at Democratic voters. Taken together, the result is that the law has a small effect on the election outcome.

As we will explain, most election laws that are hotly debated have features similar to this example. The laws target a small group of voters and barely influence turnout. When turnout does change, it tends not to change disproportionately more for one party or the other. To make this case, we provide a simple theoretical framework that enables us to assess the effect of any election law that is assumed to influence the partisan balance in a district by affecting who turns out to vote.

By articulating a step-by-step process through which a law could affect partisan vote outcomes, we show why nearly all contemporary election laws have small effects on partisan election outcomes.

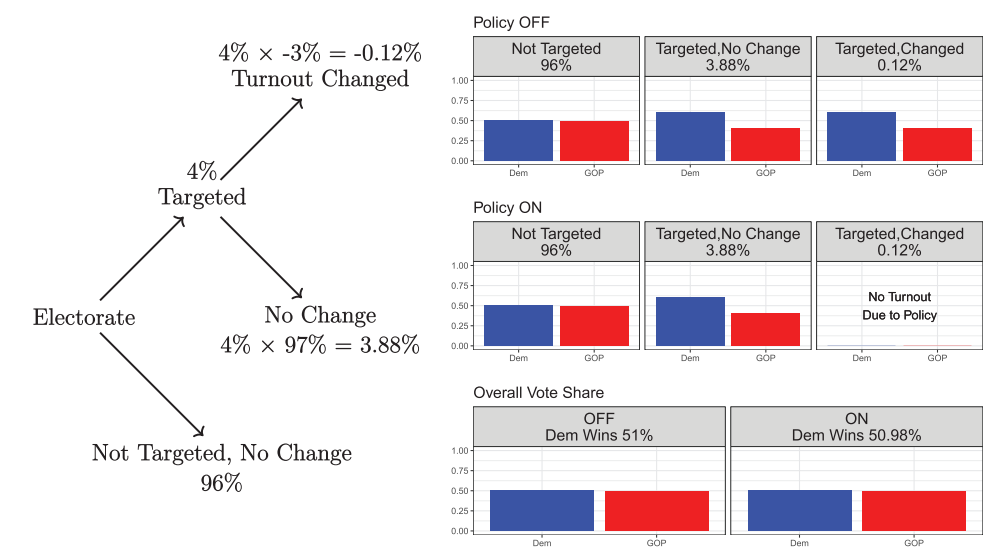


Figure 1. Calculating the vote shift from a hypothetical law change.

Our simple framework offers experts and non-experts alike a tool they can use to think through the relationship between election laws and election outcomes.

After developing the theoretical framework, and showing its logic through specific examples, we discuss four extensions. First, we address the relationship between the partisan effects of election laws and racial politics. We show how laws that disproportionately aid or hinder the participation of racial minority groups can have counterintuitive partisan consequences, depending on the preferences of racial groups and their relative share of the electorate.

Second, we address a common argument that election laws would have bigger effects on partisan outcomes if not for political campaigns and parties counteracting the effects of these laws. We show that the empirical literature on countermobilization cannot support this claim.

Third, we address what happens when states pass not just one law affecting turnout, but a series of such laws, such as a package of laws all meant to increase voter access or all meant to limit voter access. We show that even when many laws change simultaneously, the basic logic and findings remain unchanged: there are small effects on turnout and even smaller effects on the partisan balance of competition in a state.

Fourth, while our framework suggests few contemporary election policies have meaningful effects on election results, that does not mean that no election policies can have consequential effects. We point out the characteristics of laws that should raise alarm. They are the laws that target a large group of politically homogeneous voters and have potent effects on the turnout rates of these voters.

To be clear, our focus here is on the effects of voting laws on partisan election outcomes. Voting laws are hotly contested for reasons other than their effects on partisan election outcomes, such as that they are morally good or bad, that they are administered well or poorly, that they cost or save taxpayers money, that they are motivated by racism or other forms of prejudice, that they violate state or federal law, that they increase or decrease election security, and so on. Our essay does not comment on any of these legitimate concerns. We also focus our essay on how laws are administered after districts are drawn, setting aside important questions about the effects of partisan gerrymandering.

However, we believe that the public debate on nearly every aspect of election policy is clouded by incorrect assumptions about how the laws affect partisan election outcomes, and on this point, we hope our essay is clarifying. The caustic rhetoric that suggests the partisan stakes for election administration reform are very high is detached from empirical reality. Even very close elections are decided by margins larger than the magnitude of election reforms we examine in this paper. Further more, the party that benefits from changes is often unclear. In all but the absolute closest elections, modest electoral reforms cannot affect partisan outcomes.

2. ELECTION LAW AS A FORM OF POLITICAL TARGETING

US legislatures have broad authority to craft election laws affecting federal, state, and local offices. This authority affords lawmakers an opportunity to use the laws of democracy for partisan gain. The partisan motive is so obvious that nearly any change in election law, from precinct consolidation and polling hours to vote-by-mail and registration deadlines, raises concerns that the intentions of the lawmakers are partially or entirely partisan.

At times, politicians are explicit about this motivation (Hersh, 2015). In *Rucho v. Common Cause*, a landmark 2019 case on the topic of partisan gerrymandering, the Supreme Court considered election laws where the partisan motive was front and center. Said a North Carolina lawmaker cited by Chief Justice Roberts, “I think electing Republicans is better than electing Democrats. So I drew this map to help foster what I think is better for the country.” Roberts held that federal courts do not have the authority to address disputes over partisan gerrymandering. The Court effectively told lawmakers that having partisan motives does not invalidate a law.

The concern over partisan motivations appears to have trickled down to the mass public, as Democratic and Republican identifiers are increasingly at odds about election policies. More than Republicans, Democrats in the electorate support policies such as automatic voter registration, expanding access to early voting and mail voting, and enfranchising ex-felons. More than Democrats, Republicans support voter identification laws, purging obsolete records from voter registration systems, and requiring most

voters to cast ballots in person on Election Day. Particularly after the 2020 COVID-19 pandemic and subsequent politicization of mail voting, Democrats and Republicans disagree on how elections ought to be conducted.¹

2.1 The Scope of Election Laws

When we refer to election laws, we mean any law that may plausibly affect voting participation. Most such laws are explicitly about the voting process. These include changes to voting methods (e.g., in-person, early, by mail), voting eligibility (e.g., identification requirements, ex-felon voting), voting experience (e.g., number of polling stations, length of polling hours), and registration rules (e.g., same-day registration, automatic registration, online registration, pre-registration).

Other election rules are not explicitly designed to affect voter participation but may affect participation nevertheless. Our study bears on these laws as well. For instance, the creation of majority-minority districts is meant to advance minority representation in legislatures. Scholars have proposed that majority-minority districts may lead to a feeling of empowerment among minority voters and increase turnout (Fraga, 2015). Another example: the decision to hold municipal elections on or off the federal cycle may or may not be designed to influence voter turnout, but it nevertheless can have a dramatic influence on voter turnout (Anzia, 2013).

2.1.1 The Effects of Altering Voter Access on Turnout

Literature reviews from the early 2000s concluded that reforms such as early voting and mail voting may have modest (e.g., 2–4 percentage points) effects on turnout and no discernible partisan impact (Gronke et al., 2008; Berinsky, 2005). A more recent review sums up that “the research on the turnout effects of convenience voting reforms is at best mixed, leaning toward a null effect and in some instances a negative effect (Menger & Stein, 2020)”. In a review of recent litigation, Stephanopolous (2023) draws a similar conclusion.

As Menger & Stein (2020) note, much of the research on the effects of these laws on turnout relies on imprecise methods. In the last few years, scholarship has employed more precise strategies. They continue to find modest relationships between the policies and turnout. Thompson et al. (2020) and Barber & Holbein (2020) estimate about a 2 percentage point effect of universal vote by mail on turnout (see also Yoder et al. (2021)). Fowler (2017) estimates a 2 percentage point turnout effect from pre-registration policies among young adults. Kim (2022) estimates a 5.8 percentage point effect of automatic registration among voters who recently moved (a comparatively large effect that we investigate in more detail below). Bryant et al. (2022) estimates a 1 percentage point turnout effect when a state encourages registration with postcards. In this literature, the partisan effects of laws are less commonly studied, but when they are explicitly measured they are typically indistinguishable from null (e.g., Walker, Herron & Smith 2019; Yoder et al. 2021; Harden & Campos 2023).

The effects of election laws on turnout are so small that scholars analogize the effect sizes to the modest impact of campaign advertisements on participation. Studying the turnout effects of majority-minority districts—a powerful reform stemming from the Voting Rights Act—Fraga (2015) writes, “The effects I find are roughly equivalent to receiving an impersonal contact encouraging a registrant to vote.” The effects of all-mail voting, Barber & Holbein (2020) suggest, are “somewhere between one nonpartisan get-out-the-vote solicitation over the phone and one social-pressure mailer”.

Studies that have found larger effects suffer from deficiencies that make the findings unreliable. For instance, consider a recent study about the effect of online registration. The study claims that “usage of online registration by voters increases their turnout by about 18 to 20 percentage points” (Yu, 2019). This effect is estimated with an instrumental variable analysis that requires an assumption that “access to the computer or the internet is uncorrelated with voter turnout through other ways than online voter registration”, an assumption that strikes us as implausible. Even if this instrument was appropriate, the quantity Yu (2019) estimates is the complier average causal effect of registering online on turnout, which does not actually provide an estimate of the effect of a state law allowing for online registration

¹ See, for example, “Republicans and Democrats Move Further Apart in Views of Voting Access,” Pew Research Center, April 22, 2021.

on voter turnout.² In another example, McDonald et al. (2023) study the relationship between the share of ballots cast by mail and voter turnout and found that “states with greater usage of mail voting experience higher overall voter turnout” (McDonald et al., 2023). In their study, McDonald et al. (2023) examine the cross-sectional relationship between ballots cast by mail and voter turnout; their estimates are consistent with states with historically higher turnout being more likely to adopt policies that lead to an increase in the share of ballots cast by mail.

2.1.2 The Effects of Altering Voting Requirements on Turnout

Among policies that change voting requirements, the most well-researched topic is voter identification laws. An early review of the scholarship found “modest turnout effects and only minor differences across politically relevant groups” (Highton, 2017). More recent work that has focused on specific states with individual-level data (Grimmer & Yoder, 2022) or a longer time-horizon (Cantoni & Pons, 2021) has estimated even smaller effects, close to zero.

Studies of other policies that have been theorized to lower turnout cover topics such as long lines at the polls and felon disenfranchisement. The effect of long lines amounts to about 1 percentage point and is concentrated among the small fraction of the public that must wait in long lines (Cottrell, Heron & Smith, 2021; Pettigrew, 2021). Felony disenfranchisement decreases participation among affected felons and ex-felons by approximately 10 percentage points, relative to how much they might vote in the absence of the law (Miles, 2004; Meredith & Morse, 2015; Morse, 2021). Below, we will discuss how felon disenfranchisement affects outcomes in detail, building especially on the work of Michael Morse.

As with the voter access laws, some articles are published on these policies that suggest big and surprising effects, such as the Hajnal, Lajevardi & Nielson (2017) findings that voter identification laws have large turnout effects, including in some of their models very large positive effects on turnout. However, in this case, the findings have been found to be unreliable (Grimmer et al., 2018). We think a fair and uncontroversial reading of the literature on voter “suppression” is that, compared to dire warnings and predictions in the public square, scholars have found only modest relationships between these laws and election participation and no consistent relationship between “suppression” laws and partisan outcomes.

To be sure, measuring the effects of election laws is a difficult task. Researchers must try to sort out effects due to changes in law from all the other reasons why a state’s turnout levels or election results fluctuate from year to year. While the literature on election laws affecting outcomes suggests modest and null results, the occasional paper is published that suggests otherwise. How does one evaluate the merits of such a paper? One must dig into the mechanics of the research and determine if the assumptions are sound. The framework we provide below not only helps to diagnose the effects of laws but also sets up baseline expectations for evaluating claims about any election law presented by advocates and researchers.

3. MODELING HOW ELECTION LAWS AFFECT OUTCOMES

Consider any voting policy as a treatment that can be turned on or off. The status quo in a jurisdiction (e.g., a US state) is when the policy is off. A proposed change in law is when the policy is on. To formally define the partisan effect, we calculate the share of the two-party vote for the Republican party if the policy is in place, $GOP(1)$, and compare that to the vote share if the policy is not in place, $GOP(0)$. The **partisan effect** is defined as, $GOP(1) - GOP(0)$.

As noted in Table 1, we also draw attention to a different quantity of interest, the turnout when the policy is on or off. Turnout will play a key role in shaping our understanding of partisan effects. We can think of turnout as the percentage casting ballots among the Citizen Voting Age Population (CVAP) the voting-eligible population (VEP) or among registered voters.³ The **turnout effect** is defined as, $V(1) - V(0)$.

As an example, if we imagine a status quo (policy = OFF) where turnout among CVAP is 50 percent and Republicans win 60 percent of the two-party vote share, and if we imagine a new election law (policy

² In a separate analysis, Yu measures the effect of a state having online registration on turnout. The overall effect is not significant. Yu (2019) claims that online laws increase youth turnout by 3 percentage points, a finding that rests upon strong parametric assumptions about how the treatment effect varies by an individual’s age and on examining only the effect among 18 year olds.

³ In some cases, it is important that we do not condition turnout on the voting eligible public, as some policies (e.g., felon disenfranchisement) affect who would be included among the eligible.

Table 1. Two basic consequences of voting policies.

Policy is OFF		Policy is ON	
Turnout	Vote choice	Turnout	Vote choice
V(0)	GOP(0)	V(1)	GOP(1)

= ON) increases turnout to 55 percent and decreases the Republican vote share to 58 percent, then the vote share effect would be calculated as -0.02 and the turnout effect would be calculated as 0.05 .

We now decompose each quantity. We suppose that every law divides the population into those who are targeted and those not targeted. We will call the “proportion targeted” $p(T)$. We will define the “proportion of the electorate not targeted” as $p(NT) = 1 - p(T)$.

Sometimes, the targeted subset is clearly defined by the election policy in question. For example, in the case of felon disenfranchisement, $p(T)$ is the share of the citizen voting age population convicted of felonies that would disqualify those individuals from voting. In the case of voter identification, $p(T)$ is the share of the population that lacks valid photo identification to comply with a law.

A targeted group can also be defined as *any group of voters* that an advocate, a government, or a researcher is interested in evaluating in relationship to a policy question. A targeted group can be thought of as a demographic or geographic subset of the electorate, such as 18–30-year-olds, students, renters, low-income, low-education voters, and so on. Advocates may propose a policy such as same-day registration specifically designed to increase participation among one or more of these targeted groups. Or, they may propose a policy to decrease participation among one or more of these groups. In either case, one can always subset the effects on a targeted group, however defined, and on the non-targeted remainder of the electorate.

Our division of the population into “targeted” and “not targeted” subsets does not require us to make assumptions about who a law affects. This is important because an election law can plausibly affect the participation of citizens in the not-targeted set. For instance, a felony disenfranchisement law could affect the voting behavior of family members of individuals who are disenfranchised. Or, a law that targets renters may also affect homeowners. So, we need to take seriously the effects of the law on both the targeted group and the non-targeted group.

Racial minority groups can be considered as a targeted group, just as any group can be considered as a targeted population. However, it is sometimes useful to consider racial groups as *subsets of targeted and non-targeted groups*. For instance, suppose a targeted group is defined as “people without identification cards” or “low-socioeconomic-status (low SES) citizens”. We are going to want to measure the rate at which racial cohorts are present in and out of these targeted groups. The reason for this, as we will discuss in greater detail in the next section, is that the US Constitution and the Voting Rights Act draws specific attention to how election laws affect the participation of racial groups.

We assume that the size of a targeted group does not depend on whether the policy is on or off. For instance, suppose, under a status quo, there is no voter ID law and 5 percent of citizens do not have voter identification cards. If a voter ID policy is implemented (policy = ON), some of the non-ID-holders may obtain an ID. However, we would still consider them part of the targeted group of non-ID-holders on account of their position when the policy is not in place. As policies change, what can vary is the turnout rate and the share of ballots cast for Republicans.

We will define the turnout rate among the targeted when the policy is turned off as $V(T, 0)$ and the turnout rate when the policy is turned on as $V(T, 1)$. We will define the turnout rate among the not-targeted when the policy is off $V(NT, 0)$ and use $V(NT, 1)$ as the turnout rate among the not-targeted group when the policy is on. A common quantity of interest in analyses like ours is to examine the **turnout effect in the targeted group**, $V(T, 1) - V(T, 0)$. While a less commonly studied quantity, researchers could also compute the **turnout effect in the not-targeted group**, $V(NT, 1) - V(NT, 0)$.

We define $GOP(T, 0)$ as the share of votes cast for Republicans among targeted individuals when the policy is off and $GOP(T, 1)$ as the Republican share of votes cast among targeted individuals when the policy is on. Likewise, we define $GOP(NT, 0)$ and $GOP(NT, 1)$ as the share of votes cast for the Republican party among non-targeted individuals when the policy is off and on, respectively. $GOP(T, 1)$ need not equal $GOP(T, 0)$, nor must $GOP(NT, 1)$ equal $GOP(NT, 0)$.

There are two reasons why partisan vote shares in each group might differ when the policy is on or off. First, the law could systematically alter who participates in an election. For example, if a law encourages voters in the targeted group who prefer a democratic candidate to participate in the election more than it encourages voters who prefer a Republican candidate, then we would expect $GOP(T, 1)$ to be smaller than $GOP(T, 0)$.

Second, election laws could, theoretically, change voters' attitudes toward the political parties. That is, a policy may not just mobilize or demobilize certain voters but could also persuade voters to one side or another. While a theoretical possibility, we think this is unlikely. It is difficult to imagine many election laws, in and of themselves, persuading people to change their vote from Democrat to Republican or vice versa (e.g., "I would vote for Donald Trump, but because of this mail ballot policy, I am voting for Joe Biden.")

The exception that seems to prove the rule is the effect of the Voting Rights Act of 1965 on African-American participation. That law both caused a large increase in turnout among Black voters and catalyzed a partisan shift among voters. That is, the law both mobilized and persuaded. The persuasive effect of the law was special in that the law was an extraordinarily salient policy, the law was targeted to affect the voting behavior of a subset of the electorate, the law *did* affect their voting behavior, and the law influenced a major realignment of the political parties (Stanley, 1987; Black & Black, 2003). Later, in the paper, we return to the unique features of policies that disenfranchised and reenfranchised Black voters in the American South.

In general, we will suppose that laws affect turnout rates, but do not systematically affect the share of votes cast for Republicans in the targeted and untargeted groups. This simplifies Table 2, because if this is the case then $GOP(T, 0) = GOP(T, 1)$ and $GOP(NT, 0) = GOP(NT, 1)$. This assumption is implicit in most scholarship and litigation, as far as we can tell. To build intuition for this assumption, consider a simple example. Suppose an advocate proposes a law to influence turnout rates among renters. Renters are, on average, less Republicans than non-renters. To the extent that the law affects partisan outcomes in the full electorate, it is expected to do so because it increases or decreases the share of the electorate that is renters rather than the law *changing the minds* of renters or non-renters about which party they would support or differentially targeting renters with a particular partisan preference. We thus follow the implicit assumption of the scholarship that turnout of subsets of the electorate is the mechanism that affects partisan vote outcomes and that contemporary election policies do not have persuasive effects on voters. However, nothing about our argument hangs on that assumption.

Using the quantities in Table 2, and following Grimmer, Marble & Tanigawa-Lau (2024), we can calculate the number of individuals who cast ballots from a group, which we will call the group's *vote contribution*, and the total number of Republican votes from the group, which we will call the group's *GOP contribution*. We collect these terms in the top of Table 3. Vote contributions, represented by VC in Table 3, are simply the proportion of the group multiplied by the turnout of that group. GOP contributions, represented by GC in the table, are the turnout contributions multiplied by the Republican share of those who cast ballots.

The bottom facet of Table 3 provides a simple example of how a policy with a dramatic effect on turnout manifests in changes in the number of Republican votes. Suppose that in a 1,000,000-person electorate, 5 percent of the electorate is targeted by the policy (50,000 individuals) and 95 percent of the electorate is not targeted (950,000 individuals). Among the not-targeted individuals, we assume that there is no effect of the policy. With or without the policy, the non-targeted group has a 50 percent turnout rate and 80 percent votes for the Republican. In the targeted group, we suppose that when the policy is ON the turnout rate doubles from 25 percent to 50 percent and the targeted

Table 2. Turnout and Partisan effects, by group.

Group	Size	Policy is OFF		Policy is ON	
		Turnout	Vote choice	Turnout	Vote choice
Target	$p(T)$	$V(T,0)$	$GOP(T, 0)$	$V(T,1)$	$GOP(T, 1)$
Not target	$p(NT)$	$V(NT,0)$	$GOP(NT, 0)$	$V(NT,1)$	$GOP(NT, 1)$

group's rate of voting for Republicans remains constant at 20 percent (i.e., 80 percent support for the Democrats).

When the policy is OFF, the Republican candidate wins the election by 277,500 votes with 487,500 total votes cast. When the policy is ON and it doubles the targeted group's turnout rate, the overall increase in the turnout rate is 1.25 percentage points, or an additional 12,500 votes cast. Of those votes, 10,000 would be cast for the Democratic candidate and 2,500 for the Republican candidate, yielding a decrease in the Republican candidate's vote margin of 7,500 votes. Now, the Republican candidate wins the election by 270,000 votes.

Table 4 collects all the key quantities we need to assess a law's partisan impact. In Lines A and B of Table 4, we calculate the total number of votes cast when the policy is OFF (Line A) and when the policy is ON (Line B). Taking the difference between Line A and Line B calculates a policy's turnout effect. As just discussed and Line C shows, in our simple example, the policy causes a 1.25 percentage point increase in turnout. In Lines D and E, we calculate the share of votes cast for the GOP when the policy is OFF (Line D) and when the policy is ON (Line E). For intuition about these calculations, notice that the share of the electorate from the targeted group when the policy is ON is $VC(T, 1)/(VC(T, 1) + VC(NT, 1))$ and the share

Table 3. Multiplication of terms to measure effects.

Group	Policy is OFF Contribution to overall...		Policy is ON Contribution to overall...	
	Turnout	Vote choice	Turnout	Vote choice
T	$VC(T, 0) =$ $p(T) \times V(T, 0)$	$GC(T, 0) =$ $p(T) \times V(T, 0) \times GOP(T, 0)$	$VC(T, 1) =$ $p(T) \times V(T, 1)$	$GC(T, 1) =$ $p(T) \times V(T, 1) \times GOP(T, 1)$
NT	$VC(NT, 0) =$ $p(NT) \times V(NT, 0)$	$GC(NT, 0) =$ $p(NT) \times V(NT, 0) \times GOP(NT, 0)$	$VC(NT, 1) =$ $p(NT) \times V(NT, 1)$	$GC(NT, 1) =$ $p(NT) \times V(NT, 1) \times GOP(NT, 1)$
Example policy				
Group	Policy is OFF Contribution to overall...		Policy is ON Contribution to overall...	
	Turnout	Vote choice	Turnout	Vote choice
T = 0.05	$VC(T, 0) = 0.0125$	$GC(T, 0) = 0.0025$	$VC(T, 1) = 0.025$	$GC(T, 1) = 0.005$
NT = 0.95	$VC(NT, 0) = 0.475$	$GC(NT, 0) = 0.38$	$VC(NT, 1) = 0.475$	$GC(NT, 1) = 0.38$

Note: In the illustrative example, the group is 5 percent of the electorate and votes 20 percent Republican. The non-group is 95 percent of the electorate and votes 80 percent Republican. The non-group's turnout of 50 percent is unaffected by the policy. The group's turnout is 25 percent when the policy is off and 50 percent when the policy is on.

Table 4. Quantities of interest.

Line	Select quantities of interest	Calculation	Using example
A	Turnout when policy OFF	$VC(T, 0) + VC(NT, 0)$	48.75 percent
B	Turnout when policy ON	$VC(T, 1) + VC(NT, 1)$	50.00 percent
C	Effect of law on turnout	Line B – Line A	1.25 pp.
D	GOP share when policy OFF	$\frac{GC(T, 0) + GC(NT, 0)}{VC(T, 0) + VC(NT, 0)}$	78.46 percent
E	GOP share when policy ON	$\frac{GC(T, 1) + GC(NT, 1)}{VC(T, 1) + VC(NT, 1)}$	77.00 percent
F	Effect of law on GOP share	Line E – Line D	–1.46 pp.
G	Effect on share of group in electorate	$\frac{VC(T, 1)}{VC(T, 1) + VC(NT, 1)} - \frac{VC(T, 0)}{VC(T, 0) + VC(NT, 0)}$	2.44 pp.

of the electorate from the not-targeted group when the policy is ON is $VC(NT, 1)/(VC(T, 1) + VC(NT, 1))$. That means we can write the GOP share,

$$GOP(1) = \underbrace{\frac{VC(T, 1)}{VC(T, 1) + VC(NT, 1)}}_{\text{Share targeted}} \times GOP(T, 1) + \underbrace{\frac{VC(NT, 1)}{VC(T, 1) + VC(NT, 1)}}_{\text{Share not targeted}} \times GOP(NT, 1)$$

or the GOP vote share is the weighted average of the rate of supporting the GOP among the targeted voters who turned out to vote and the rate of GOP support among the not-targeted voters.

Returning to our illustrative example, as mentioned, the policy that doubles turnout in the targeted group increases overall turnout by 1.25 percentage points. Because the policy increases turnout among individuals who disproportionately vote for the Democratic candidate, it lowers the Republican candidate's vote share. The policy causes a 1.46 percentage point decrease in the Republican candidate's vote share, reducing the two-party share for the Republican candidate from 78.46 percent to 77.00 percent.

Our illustrative example shows that even massive changes in the turnout rates of targeted groups translates into relatively small changes in the vote share for the GOP candidate. The example here is purposefully exaggerated and unrealistic compared to the real policies to which we will turn to next. Consider why the example is unrealistic. We are not aware of any contemporary election administration policy that increases voter turnout by 25 percentage points (an 100 percent increase in the turnout rate). Furthermore, the difference here in partisan support between the targeted and not-targeted groups is massive, and we have assumed no spillover of the law changing turnout in the non-targeted group. And yet, even a law of this potency could only flip an election in a narrow set of circumstances of an otherwise razor-thin margin. (In fact, the law would only be decisive in a situation where the baseline (policy OFF) level of Republican support was between 50.00 percent and 50.77 percent.)

3.1 Real-World Examples

We now turn to two brief examples of real policy interventions, one that increases turnout and one that decreases turnout. These examples are useful because they build intuition for why election policies have a small effect: the policy targets a small share of the electorate, has a small effect on turnout, and/or affects a group with relatively balanced partisanship.

3.1.1 Example 1: Automatic voter registration.

Kim (2022) studies automatic voter registration. She focuses on a policy in Orange County, California where registrants who move within-county are automatically registered at their new address. Movers are only automatically re-registered if their move happened more than 90 days before an election. Kim (2022) exploits this cut-off to estimate a causal effect.

There are roughly 1.5 million registrants in Orange County and 100,000 in-county movers among registrants between two federal elections. Thus, we will approximate that $p(T) = 0.07$ and $p(NT) = 0.93$. The turnout rate for the non-targeted group is about 70 percent of registered voters in the midterm election that Kim (2022) studies and they are not expected to be affected by the policy. The turnout of the targeted group is estimated as 58 percent when the policy is OFF and 64 percent when the policy is ON.⁴ While we do not know the partisan vote choices of movers and non-movers, we use estimates of party affiliation. Leaving non-party affiliates aside, movers are approximately 52 percent Republican. The full set of registered voters is approximately 51 percent Republican, which we use as our estimate of vote choice among the non-movers.⁵ We will assume that the vote choice of the movers is not affected by the policy and as we describe above this is equivalent to assuming that the policy does not systematically affect Republicans or Democrats within the targeted group and that there is no persuasion effects of the policy within the targeted group.

Using these statistics and assumptions, we estimate the effect of the policy on the Republican vote share (Line F of Table 4) as giving the Republicans a 0.006 percentage point advantage in vote share in

⁴ If we used CVAP as the denominator, turnout estimates would be significantly lower. While the registered population in Orange County in this period is approximately 1.5 million, the CVAP population is approximately 2.1 million. See: "Citizen Voting Age Population by Race and Ethnicity," United States Census Bureau <https://www.census.gov/programs-surveys/decennial-census/about/voting-rights/cvap.html>

⁵ "2004–2018 Voter Data Trends," Orange County Registrar of Voters. <https://ocvote.gov/election-library/docs/Voter%20Data%20Trends%20Report.pdf>.

the election that Kim (2022) examines. Note, this law has a large effect on turnout within the targeted group: 6 percentage points. In fact, Kim (2022) refers to the turnout effect as “very large”. Further, note that the law targets a rather large segment of the population. These within-county movers represent more than one in fifteen registered voters. Nevertheless, the partisan effect is very small simply because the population of movers does not differ in partisan composition from the non-movers.

3.1.2 Example 2: Out-of-precinct ballot rejections.

Brnovich v. Democratic National Committee (2021) was a landmark ruling at the Supreme Court about the Voting Rights Act. The underlying case dealt with several election laws in Arizona, including a long-standing policy that rejected a voter's entire ballot if the ballot was cast in the wrong precinct. On these ballots, even votes cast for statewide or presidential contests were rejected. Among the arguments made by the law's proponents was that the law advantaged Republicans. A Republican National Committee (RNC) lawyer defended the law because a change would place Republicans “at a competitive disadvantage relative to Democrats”.

Because this policy affects voters *after* they have cast ballots, we can consider the targeted group individuals who cast in-person votes on Election Day. When the policy is OFF (i.e., out-of-precinct ballots are not rejected), turnout is 100 percent, meaning those ballots are all counted. When the policy is ON, in-person ballots that are cast out-of-precinct are rejected, yielding a turnout rate below 100 percent.

According to the Plaintiff's expert witness in the case, 3,800 voters' ballots were rejected for being cast out-of-precinct (See Jonathan Rodden's expert report in *Brnovich v. Democratic National Committee*). Based on the 618,077 votes cast in person, this implies that the out-of-precinct policy caused 0.61 percent of Election Day ballots to be rejected. When the policy is ON, turnout goes down from 100 percent to 99.4 percent.

To estimate the partisanship of the out-of-precinct voters, we start with racial estimates from Rodden's expert report, which estimated the racial identity of affected voters based on their names. Rodden estimated that 2,046 of the out-of-precinct ballots came from White voters, 1,162 from Hispanic voters, 369 from Black voters, and 223 from Native American voters. From the 2016 Cooperative Election Study, we estimate partisanship from race by observing the rates of voting for Democrats versus Republicans by racial group in Arizona. For the sake of this exercise, we assume the estimates of partisanship by racial group are precise. We calculate that the ban on out-of-precinct voting deterred 1,988 Democratic votes and 1,812 Republican votes, yielding Republicans 177 votes. By way of comparison, the margin in the 2016 Presidential election in Arizona was over 90,000 votes.⁶

The effect of this policy on election outcomes is very small, but notice it is for a different reason than in the case of automatic voter registration. In that first case, the turnout effect was large, but the population it affected was balanced on partisanship. In the case of out-of-precinct rejections, partisanship is less balanced. In a presidential election in Arizona that the Republican candidate won by 3.5 percentage points, those with rejected out-of-precinct ballots were 52/48 Democratic. But the number of ballots rejected is so small that the overall effect of the turnout-lowering policy is almost imperceptible.

3.2 Generalizing from Examples

The real-world examples illuminate that election administration policies often have small effects because they only target small shares of the population, have only a small effect on turnout, and/or affect groups that are relatively balanced in their partisanship. In general, our approach can be used to calculate the partisan implications for any policy and calculate bounds on the effect of policies under different assumptions about the size of the targeted group, the effect on turnout, and the partisan composition of different groups. We use this approach in Figure 2 to examine what sort of change in turnout

⁶ Based on the geography of voters studied in Rodden's expert report, White voters affected by the law may have plausibly been more Democratic than White voters overall in Arizona. But even if we assume White voters are more Democratic, we arrive at similar conclusions. Even if White voters were as Democratic as Black voters, the out-of-precinct policy would cause a 1,596 vote shift toward Republicans.

If we include sampling variability in our estimate of each racial group's partisanship, then there is no clear advantage for either party. The 95 percent confidence interval of votes for either party, in terms of Republican votes, is [-684, 330]. The 95 percent confidence ranges from increasing Democratic votes by 684 to increasing Republican votes by 330. Of course, this calculation ignores other types of uncertainty, such as uncertainty in the estimate of race from names. Furthermore, the conditions in any election could change substantially so it is difficult to forecast values from one election to the next.

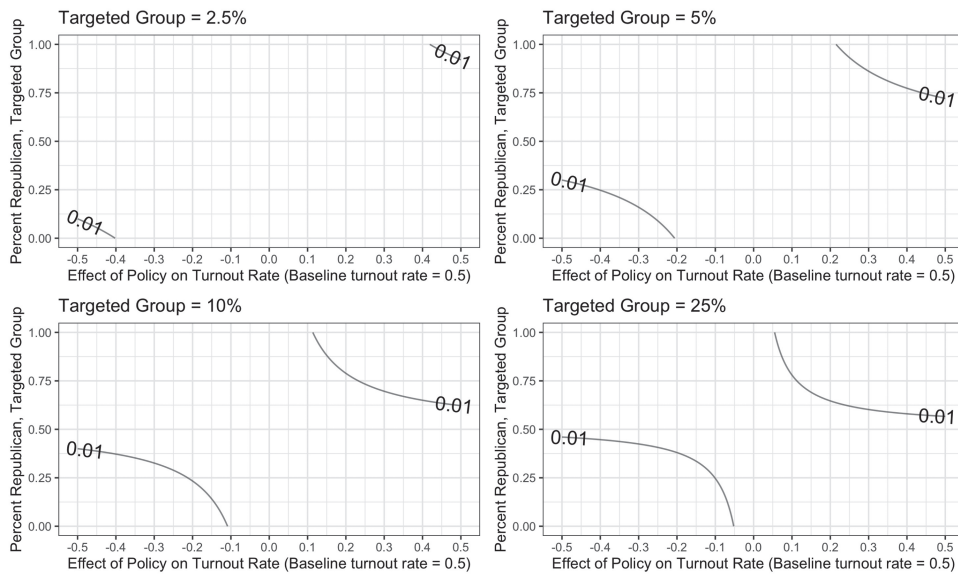


Figure 2. Size of turnout effect to increase Republican vote share by one percentage point.

and partisanship would be necessary to increase the Republican vote share by 1-percentage point. We use 1 percentage point as a representation of a “big effect” that could swing a close election.⁷

In each facet of Figure 2, we suppose that voter turnout is 50 percent among targeted and non-targeted groups when the policy is OFF. Furthermore, we suppose that the non-targeted group’s turnout rate is unaffected by the law and that the voters from the non-targeted group are evenly split between Republicans and Democrats. Across the facets, we vary the size of the targeted group as a share of the citizen voting age population. Across the facets, we will suppose that when the policy has a turnout effect, it does not systematically affect Democrats or Republican in the targeted group, nor does the policy have a persuasive effect. This means that, for this example, we will suppose that $GOP(T, 1) = GOP(T, 0)$.

The effect on partisan outcomes in our hypothetical state will depend on how Republican or Democratic the targeted group is. Each facet contains isocontours that describe the combination of turnout effect and vote choice in the group that leads to the one-percentage-point change in the Republican direction. For example, consider the top-left panel, where 2.5 percent of the electorate is targeted. There, massive changes in turnout would have to happen within a homogeneous population to yield a 1 percentage point change in Republican vote share. Suppose, for example, that everyone in the targeted group votes for Democrats. Then, for the policy to increase Republican vote share by 1 percentage point, it must decrease turnout among this group by 40.2 percentage points—a massive downward shift. As the group becomes slightly more favorable to Republicans, the size of the downward turnout shift necessary to increase Republican vote share by 1 percentage point becomes even larger: if 5 percent of the targeted group supports Republicans, then turnout would have to be decreased 44.6 percentage points. The isocontours in the top-right of each plot show that, symmetrically, a policy could increase turnout among a very pro-Republican group rather than decrease turnout among a Democratic group to yield a similar electoral shift.

As the targeted group’s size increases across the four facets, the effect of turnout to achieve a 1 percentage point shift in vote share is more modest, though the necessary changes are still large and implausible in absolute terms. Even if the targeted group is 25 percent of the voting-eligible population and, improbably, the law only targets Democratic voters, a policy would need to decrease their turnout by 5.2 percentage points to yield a 1 percentage point increase in Republican vote share.

⁷ As a point of reference if one restricts elections to contested races where the margin of victory is 10 percentage points, even among those, only 10 percent have a margin of victory less than 1 percentage point (e.g., Eggers et al. 2014; Caughey & Sekhon 2017).

Of most policies that are presumed to increase turnout (e.g., all-mail elections, same-day registration) and most policies that are presumed to decrease turnout (e.g., voter identification laws, long lines), past scholarship suggests effects on the order of, at a maximum, a few percentage points. What Figure 2 shows is that even for policies that have a 10 percentage point effect on turnout among a targeted group unless that group is both large as a proportion of the electorate and overwhelmingly lopsided in its partisan composition, the law will not affect partisan outcomes in a way that changes any but the closest elections.

In light of Figure 2, let us consider some key demographic groups that are often thought of as targets of elections laws. For instance, young adults (18–30) and renters might be targeted by an Election Day registration law to increase turnout among mobile populations. Young people might be the target of a precinct consolidation to limit their influence on voting. Or consider low-SES voters, such as those with below-median incomes and lacking a college education. One may propose a law meant to help low-SES voters overcome the logistical burdens of voting or one may propose a law that imposes an additional logistical burden that is especially hard for low-SES voters to overcome.

Table 5 reports key statistics for five groups of voters, based on data from the 2020 validated Cooperative Election Study. All of these groups have substantially lower turnout than those who are not in the groups. Moreover, the groups are all distinct from those not in the groups with respect to partisanship. Young adults, renters, and Hispanic respondents are all 15–20 percentage points more Democratic in their 2020 Presidential vote choice. African-Americans are 41 percentage points more Democratic. The largest of these selected groups, low-income/low-education voters, are 6 percentage points more Republican than those not in that group.⁸

These groups are different from targeted groups highlighted above, such as “in-county movers”, in that they are a much bigger share of the population and are more distinct in their partisanship. On account of their larger share, we might expect turnout effects of laws to be more modest and more likely to spillover into the non-targeted group. That is, perhaps same-day-registration is a law proposed by advocates explicitly to help young adults and renters. However, the law might also help some older adults and homeowners vote. That would amount to spillover.

We can use the data in Table 5 to imagine the overall partisan effects of laws on outcomes. Suppose a law increased the rate of voting of each targeted group by two percentage points and had no impact on non-targeted groups. Two percentage points are in line with the magnitude of effect sizes measured

Table 5. Select targeted groups.

GROUP	Size		Turnout		GOP share	
	p(T)	p(NT)	V(T,0)	V(NT,0)	G(T,0)	G(NT,0)
Young adults	23	77	38	66	35	50
Renters	39	61	47	68	33	54
Low Inc. & Edu.	57	43	51	71	49	43
Black	13	87	51	61	11	52
Hispanic	10	90	45	62	32	48

Note: 2020 Cooperative Election Study. $p(T)$ represents the size of the group and $p(NT)$ is the size of those not in the group. Validated voter turnout of the group and non-group are listed as $V(T)$ and $V(NT)$. Two-party Republican vote choice in the 2020 Presidential election is represented by $G(T)$ and $G(NT)$. Young adults are 18–30 year olds. Low-income/education respondents are those who have less than a 4-year degree AND have a family income of less than \$80k. $N = 61,000$, except 3,488 respondents are neither owners nor renters and are excluded from the Renters row, and 6,734 respondents declined to report a family income level.

⁸ The rate of Republican support among groups such as these depends on how one defines the groups and what election is observed. The analysis here is simply illustrative. One may particularly wonder about the definition of low-SES voters and how sensitive the definition is to variations in party support. If using the 2020 election, one defines low-SES as lacking a college degree and having a self-reported family income below \$100k, then the low-SES voters are 9 percentage points more Republican in vote choice than those not in that group. If low-SES is defined as lacking a college degree and having below \$80k, then low-SES voters are 6 percentage points more Republican (as shown in Table 5). If the cutoff is below \$70k, they are 4 percentage points more Republican. If the cutoff is below \$60k, then they are 2 percentage points more Republican. It is only when one defines low-SES as having no college degree and less than \$40k family income that the relationship flips and the low-SES group becomes slightly more Democratic than those not in the group.

for voter access laws, as noted above. Again, plugging in the numbers into Line F from Table 4, we learn the following. A policy that increased turnout by 2 percentage points just for the targeted groups would increase overall Democratic vote share by 0.10 percent in the case of young adults, 0.19 percent in the case of renters, 0.16 percent in the case of Black Americans, and 0.05 percent in the case of Hispanic Americans. The policy would increase Republican vote share by 0.06 percent in the case of low education/income voters. Thus, a two-percentage-point increase in turnout targeted to any of these demographic groups translates into an overall partisan swing of approximately one to two tenths of a percentage point.

Notice two points. First, we have assumed here no spillover, which results in an *overestimate* of the partisan effects of targeting any of the groups. If we suppose that the non-targeted group also may exhibit increased turnout, the overall effects will be more muted.

Second, while these data are national level, it is certainly true that at a state or local level, targeted groups like these may be different in their group size, turnout level, and partisanship. For instance, in most states (thirty-four out of fifty), the low-SES population is more Republican than the high-SES population, but in other states, the opposite pattern emerges, and the degree of difference varies by state. As a result, any law that hinders low-SES voters will help Democrats in most states, but not in all states. And of course, nothing in this table eliminates the possibility that a particular law could be designed that has a potent effect on turnout for a politically homogeneous group. But the table and the argument in this section suggest why that would be so difficult: it is hard to precisely target a law to affect one particular partisan group. Beyond this possibility, Table 5 demonstrates how one can connect real behavioral data with some simple assumptions and our analytical framework to estimate the effect of laws on outcomes.

4. PARTISAN EFFECTS VERSUS RACIAL EFFECTS

The legal evaluation of voting laws is typically focused not on partisan outcomes, as we address here, but on the effect of these policies on racial group participation. Federal law constrains governments from disproportionately burdening protected minority groups in the context of voting. In this section, we examine the relationship between disproportionate racial effects and partisan outcomes.

As a working example, let us return to data from the 2020 CES survey. The left of Table 6 (policy OFF) reflects the actual group sizes, turnout behavior, and support for the Republican presidential candidate in the 2020 election. Here, we observe low-SES Americans as a targeted group. Again, these are individuals who have below median income and do not hold a bachelor's degree. We subdivide high-SES and low-SES voters according to those who identify their race as Black and those who do not identify as Black. The right side of the table imagines a policy that decreases turnout by 10 percentage points among low-SES Americans, but it does not affect high-SES Americans.

If we are interested in the partisan consequences of this policy, we would make the same calculations as we had done before (Line F of Table 4). The policy decreases the Republican vote share from 46.8 percent to 46.5 percent. Because the affected population (low-SES voters) is mostly Republican, the law is bad for Republicans and good for Democrats.

This kind of policy, which burdens low-SES voters, would likely trigger political and legal concerns about racial discrimination. As Justice Kagan notes in her dissent in *Brnovich v. Democratic National Committee* (2021), citing an earlier landmark case, *Gingles*, “[Congress] saw that ‘inferior education, poor

Table 6. Race, socio-economic status, and an imagined policy that reduced turnout in a targeted group.

Group	Size	Policy is OFF		Policy is ON	
		Turnout	GOP share	Turnout	GOP share
Low-SES Black	9.2	47.5	10.1	37.5	10.1
Low-SES Not Black	47.4	51.7	57.4	41.7	57.4
High-SES Black	3.8	56.7	10.7	56.7	10.7
High-SES Not Black	39.7	72.7	46.0	72.7	46.0

Note: 2020 Cooperative Election Study.

employment opportunities, and low income’—all conditions often correlated with race—could turn even an ordinary-seeming election rule into an effective barrier to minority voting in certain circumstances.” Regardless of the intention of the law’s creators, and regardless of the partisan consequences, if it burdens low-SES voters and, therefore, disproportionately burdens Black voters, it may be viewed as discriminatory.

The key statistic relevant to assess potential racial discrimination would be derived from Line G of Table 4. That calculation would inform us that this 10 percentage point decline in voter turnout among low-SES Americans would effectively reduce the voting power of African-Americans. The Black percent of the electorate goes from 10.9 percent when the policy is OFF to 10.3 percent when the policy is ON.

In most US states, the low-income and low-education population is predominantly Republican and disproportionately non-White. That means that policies that burden lower-class voters likely help Democrats whereas policies that improve turnout rates among lower-class voters likely help Republicans. This result is interesting in light of the fact that public opinion about voter suppression/access laws is polarized such that Democrats and Republicans seem to be diverging on policies in ways that are counter to their partisan interests but are consistent with their parties’ racial coalitions. Partisans may incorrectly assume a strong link between voting rights of minorities and partisan outcomes. The effect of a law on race is not the same as the effect of a law on party.

4.1 An example: felon disenfranchisement

The example in Table 6 is merely illustrative, but it bears a striking relationship to a real-world policy that is subject to much debate: felon disenfranchisement. Disenfranchisement laws across the country affect an estimated 4.6 million people, or about 2 percent of eligible voters (i.e., $p(T) = .02$). The share of the population ranges dramatically by state, from 0 to 11 percent in the case of Mississippi.⁹

Recent scholarship on felon disenfranchisement suggests that the population affected had very low turnout rates prior to their convictions and if/when they are re-enfranchised, only approximately 8–12 percent would vote in elections (Miles, 2004; Meredith & Morse, 2015; Morse, 2021). Similarly, other research suggests a lack of spillover effects (White, 2019, 2022). That is, the turnout rates of family of disenfranchised felons are not affected by the law. For simplicity, we might then assume that enfranchising all ex-felons would lead to the targeted group increasing turnout from 0 percent to approximately 10 percent, with no change in the non-targeted group.

What are the partisan leanings of disenfranchised felons? Data on this question are hard to come by, so we use two strategies. According to Michael Morse (2021), following the passage of a Florida state constitutional amendment in 2018 in which ex-felons could vote, 94 percent of Black ex-felons who registered with a party identified as Democrats, and 36 percent of non-Black ex-felons registered as Democrats. In one set of our estimates, we will use these statistics to estimate the partisan consequences of returning voting rights to felons.

Particularly because the Hispanic population in Florida is distinctive from other states, we employ a second set of estimates by using the partisan identification of low-education (high school or less) men, broken down by racial group. Using this estimate from the CES, we estimate that 62.1 percent of low-education White men vote for Republicans, 25.6 percent of low-education Black men vote for Republicans, and 36.8 percent of low-education Latinos vote for Republicans.¹⁰

In all states with disenfranchisement laws, the disenfranchised population is disproportionately Black. If, upon re-enfranchisement, turnout in the affected population does not vary by race, then reenfranchisement would increase the Black share of most states’ electorates. At the same time, Black Americans do not make up a majority of the disenfranchised population. Across the country, Black identifiers represent about a third of the disenfranchised population. In 60 percent of states that have disenfranchisement laws (that is, in twenty-nine out of forty-eight states), the majority of those disenfranchised are neither Black nor Hispanic. These include many states in which a large share of the population is disenfranchised, such as Alabama, Tennessee, and Florida.

Calculating the partisan effect of reenfranchising former felons across states, we find that the policy change would have small and ambiguous effects. Using estimates from Morse’s (2021) study, we find

⁹ “Locked Out 2022: Estimates of People Denied Voting Rights due to a Felony Conviction.” The Sentencing Project, 2022.

¹⁰ These estimates are very close to what was found in a 2020 survey of 8,000 incarcerated people who were asked about their preference in the 2020 presidential election (Lewis, Shen & Flagg, 2020).

that in 19 states, granting former felons the right to vote improves Republican vote totals. In 29 states, granting former felons the right to vote increases Democratic vote totals. The biggest pro-Republican increase occurs in Arizona (a 2,934 pro-Republican vote shift). In the 2020 election, this pro-Republican shift would increase President Trump's vote share in the state by 0.04 percentage points. The biggest pro-Democratic shift would occur in Virginia (a 9,261 pro-Democratic vote shift), which would cause a 0.07 percentage point decrease in Donald Trump's vote share. We find similar effects if we measure the partisan preferences of felons using the partisan preferences of low-education men. For example, in Georgia, we estimate that restoring felon votes would provide Democrats an additional 3,835 votes, which would decrease Donald Trump's vote share in the state by 0.04 percentage points.

From the framework we have developed here, it is now easy to see why felon disenfranchisement laws cannot make a big impact on partisan outcomes in states and why the evaluation of racial equality is very different from the evaluation of partisan outcomes. Even if 5 percent or 10 percent of a population is disenfranchised, the turnout of this group might only increase by 10 or so percentage points if reenfranchised. More important, the population is relatively split by party. Neither we nor anyone else has precise estimates by state of the partisan preferences of disenfranchised felons, but extrapolating from the available evidence cited above, the typical state that disproportionately disenfranchises minorities but mostly disenfranchises White men is unlikely to have a politically homogeneous group of disenfranchised felons.

Thus, even though all states disproportionately disenfranchise racial minorities and even though racial minorities tend to support Democrats, enfranchising ex-felons will yield modest gains for Republicans in states where the targeted population is mostly Republican. In those states, re-enfranchisement laws will exhibit the combination of (slightly) increasing minority participation while (slightly) decreasing the probability of electing minority-preferred candidates. In other states, re-enfranchisement may modestly help Democrats. Either way, the results will be modest because the target population is not overwhelmingly Democratic or Republican.

5. THE LIMITS OF COUNTERMOBILIZATION

We have argued that election laws rarely affect partisan outcomes because the proportion of the population affected is often small, the turnout effect of the law is often small, and the partisan breakdown of the population is often balanced. Few election laws target a lopsidedly partisan subpopulation with a treatment that has a big impact on turnout.

However, when it comes specifically to policies viewed by critics as “suppression” laws, there is a common alternative explanation for why the policies do not seem to affect outcomes: countermobilization (e.g., [Komisarchik & White, 2022](#)). The argument is that election laws *would have* larger effects on election outcomes if it were not for the concerted efforts of campaigns and media to counteract those effects. If all the campaign attention to these laws disappeared, the effect sizes would be larger.

In fact, countermobilization cannot actually account for the lack of a relationship between voter suppression and election outcomes. Two specific forms of countermobilization are discussed in the literature: the direct effects of campaigns and the indirect effects of the political and media environment that might energize certain voters and inspire them to vote at higher rates than they would in absence of the laws. We will consider each version of the countermobilization story in turn. The evidence in favor of either concept is surprisingly weak, and the analytical framework we have developed in this essay helps explain why.

Consider two recent studies that measure campaign countermobilization. [Cantoni & Pons \(2021\)](#) estimates that voter ID laws do not affect turnout. They then ask whether the lack of a turnout effect is due to countermobilization. In one test, they create a scale of activism that incorporates whether survey respondents attend political meetings, post yard signs, volunteer, and donate. They find no relationship between the implementation of voter ID laws and activism. In another test, however, they use a survey measure of self-reported campaign contact. They estimate that voter ID laws led non-White respondents to be 4.1 percentage points more likely to say a campaign contacted them than White voters to say the same.

In another study, [Komisarchik & White \(2022\)](#) look not at voter ID laws but at the broader set of changes to election law in jurisdictions that had been covered under Section 5 of the Voting Rights Act. After the Supreme Court ruled in *Shelby County* to remove the protections on those jurisdictions, many

of these southern states made adjustments to their laws, such as adding voter identification requirements and purging voters from registration records. Komisarchik and White estimate that non-White voters post-*Shelby* actually vote at higher rates than before. Is countermobilization the reason? They look at the same survey question measuring self-reported campaign contact as Cantoni and Pons use. They measure whether non-White voters in the jurisdictions that were covered under Section 5 of the Voting Rights Act were more likely to report contact after *Shelby*. They find no statistically significant relationship.

Suppose, though, that there is a countermobilization effect consistent with Cantoni & Pons (2021). And further suppose that a “suppression” law such as voter identification is passed by a state, and because it is passed, campaigns expend extra resources specifically to mobilize non-White voters, who are indeed disproportionately affected by voter ID laws. Suppose that the laws lead non-White voters to receive 4.1 percentage points more campaign contact than White voters. What would be the effect of such countermobilization? As it turns out, the effect would be microscopically small.

To see why, we must estimate the effectiveness of campaign contact. Suppose all the additional campaign contact due to countermobilization comes in the form of direct personal contact from a campaign staffer or volunteer. This is a generous assumption, as personal contact, which is time-intensive and expensive, has been found to have the greatest impact on turnout (i.e., compared to mailers, phone calls, and other forms of contact). Green & Gerber (2019) suggest in-person contact typically has a 4 percentage point effect. Thus, we can imagine a countermobilization effort in which targeted groups get 4.1 percentage points more campaign appeals, and 4 percent of the recipients of those appeals, who otherwise would not have voted, do vote.

To see how much this countermobilization could matter in an election, let’s consider the 2020 presidential election in Arizona, a state Joe Biden won by just 0.31 percentage points, which amounted to under 10,500 votes.¹¹ Could countermobilization have made the difference in such a close election?

In Arizona, according to the 2020 5-year CVAP estimates, 23.69 percent of the citizen voting age population is Hispanic and, according to the CPS, 60.8 percent of those Hispanic citizens voted in the 2020 election.¹² According to the 2020 CES survey, 26 percent of Arizona Hispanic identifiers voted Republican.

Suppose countermobilization increased targeting to the 23.69 percent of electorate that is Hispanic, such that 4.1 percent of them were targeted and 4 percent of those turned out to vote. Of these new Hispanic voters, 26 percent would be expected to vote Republican and 74 percent would be expected to vote Democratic, just as Hispanics statewide voted. This would amount to about 958 new votes for Democrats statewide and 337 new votes for Republicans, out of an electorate of 3.3 million. Countermobilization would be responsible for one one-hundredth of a percentage point change in the presidential vote share. The effectiveness of the countermobilization would have to be closer to 45.5 percentage point yield (i.e., 4.1 percent of Hispanics are contacted and then 45.5 percent of those contacted vote on account of the contact) to be responsible for even the 0.31 percent vote margin in Arizona. Because such an effect size is unheard of, we would dismiss the claim that countermobilization of the magnitude estimated by Cantoni and Pons affected even the razor-thin outcome in a state like Arizona.

We can use the Cantoni & Pons (2021) study to measure countermobilization in a different way as well. The authors find no effect of voter identification laws on turnout, but they do not measure how the effect varies by state-level competitiveness. If countermobilization is responsible for the non-relationship between ID laws and turnout, presumably we would see negative effects of ID laws on turnout in uncompetitive states (where campaigns are not as active) but null or even positive effects in swing states.

We use Cantoni and Pons’ (2021) classification of state’s photo identification laws from 2004 to 2018. We then estimate a two-way fixed effect model, including fixed effects for the state and election year. We present the results in Table 7, where we present the coefficient estimates and the standard errors, clustered at the state level.

In Column 1 of Table 7, we exactly replicate Cantoni and Pons and find that, in the aggregate, strict photo identification laws appear to cause a small increase in turnout, but the effect is not statistically

¹¹ State of Arizona Official Canvass, November 24, 2020 Report, https://azsos.gov/sites/default/files/2020_General_State_Canvass.pdf

¹² November 2020 Voting and Registration Supplement, Data File, Current Population Survey, United States Census Bureau.

Table 7. Coefficient estimates for the effect of strict photo identification laws on state-level voting eligible turnout rates. The first column estimates an overall effect of the law on turnout, the second column examines how the effect of the law depends on whether a state is a swing state in a presidential election (comparing to all other states in both presidential and midterm election years), and the third column compares swing states in presidential elections to non-swing states in presidential elections.

		Dependent variable:	
		VEP Turnout rate	
Strict ID	0.006 (0.011)	0.002 (0.010)	−0.005 (0.010)
Swing state		0.025 (0.007)	0.018 (0.006)
Strict ID × Swing state		−0.0004 (0.012)	0.001 (0.010)
State-years	408	408	204
R ²	0.922	0.929	0.930
Elections	Midterm + President	Midterm + President	President
State fixed effects	✓	✓	✓
Election fixed effects	✓	✓	✓

significant. We interact the effect of the strict identification law with whether the two-party vote share difference in the state is less than 6 percentage points in a presidential election (the threshold choice does not affect results). In Column 2, we compare the effect of the law in swing states in presidential elections to all other states in both presidential and midterm elections. In Column 3, we look just at presidential election years. The key coefficient is the interaction between a strict ID state and a swing state. We do not find any significant relationship suggesting turnout in swing states with ID laws is different than turnout in swing states without voter ID laws. In other words, no evidence of countermobilization.

A separate form of countermobilization is rooted in psychology rather than campaign electioneering activity. The most prominent study on this topic is [Valentino & Neuner \(2017\)](#), who argue that media coverage of voter ID laws makes Democratic-aligned voters angry, which, in turn, boosts their civic engagement and turnout.

[Valentino & Neuner \(2017\)](#) test their hypothesis with two studies. In a survey, they collect information about respondent's self-reported anger that voter identification laws might stop someone from voting. They assess the relationship between anger and a self-reported participation index, which combines an individual's self-reported likelihood of volunteering to raise awareness about voter identification laws and a self-reported index of how likely an individual is to participate in a midterm election. [Valentino & Neuner \(2017\)](#) report a positive correlation between anger and this participation index.

In a separate, survey experimental study, Valentino and Neuner test different conditions in which respondents are asked to read articles, including, in one condition, an article that suggests voter ID laws will disenfranchise Black voters. They measure the effect of the treatment on a five-item index of participation that includes voting along with items such as willingness to volunteer and to donate.

Valentino and Neuner do not separately measure intention to vote in an election versus other forms of participation. However, upon our request, Dr Neuner generously re-ran regression analyses from his article using just turnout intention as the dependent variable.¹³ In the first study, the new results show a relationship between anger and intention to vote, but no statistically significant difference between how Democrats and Republicans respond. If intention to vote does not vary by party, then countermobilization may affect turnout but not who wins.

¹³ Email correspondence with Dr Fabian Neuner, June 19, 2023.

In the second study, the new results show no relationship between any of the treatments and voting intention among the respondents, including among the subsample of Democratic respondents. While the treatments in [Valentino & Neuner \(2017\)](#) may correspond to self-reported outcomes such as willingness to attend a political event, they do not correspond to intention to vote.

The same logic for why campaign countermobilization can only have a microscopic effect on outcomes also helps convey the limits of any psychological countermobilization effect. Suppose a state passes a voter identification law, which, in turn, leads some news outlets to produce media about the law. Only some residents are likely to see the news. Of those, only some will be made angry on account of the news. Of those, only a small number would be people who would not have otherwise voted except for the fact that the news story about the voter ID law made them angry. As far as we are aware, there is no evidence in the scholarly record of a “suppression” law leading to anger-inducing news that in turn leads to disproportionately higher turnout (or even higher intention to vote) among voters of one political party.

6. MULTIPLE POLICIES CHANGING AT THE SAME TIME

To this point, we have only considered isolated policies. But if a state makes a series of changes, can those changes add up to a large effect on partisan outcomes? Such a question is at the heart of work in political science that attempts to create indices of state democracy, such as [Li, Pomante II & Schraufnagel \(2018\)](#) and [Grumbach \(2022\)](#). As we will show through two separate examples, because most individual changes have such minuscule effects, and because it is not always easy to predict which party benefits from a particular rule change, the aggregation of multiple election reforms is unlikely to translate into one-sided partisan gains.

6.1 Shelby County and multiple policy changes

Consider a recent change in legal precedent that led to states passing numerous election policies in a short amount of time. Between 1965 and 2013, a number of states and counties, primarily in the South, were constrained by Section 5 of the Voting Rights Act in their ability to change election rules. When the Supreme Court removed the constraints on these jurisdictions in *Shelby County v. Holder*, these formerly “covered jurisdictions” were able to enact changes to election administration that previously would have required Justice Department review. Democratic-aligned advocacy groups feared that the Court “opened the floodgates to laws restricting voting” (*The Effects of Shelby County v. Holder*, 2018). They feared that states would make changes that would be hard to track but would negatively impact Black voting participation ([Levine & Rao, 2020](#)).

One way to summarize the size of the change after Shelby is to use the *Cost of Voting Index* (COVI), which aggregates up a range of policies and then constructs a single “cost of voting” for each state. [Li, Pomante II & Schraufnagel \(2018\)](#) argue that states with a higher score on their index make voting more costly. In its most recent ranking from 2022, the scores range from –2.5 (the least costly state to vote, Oregon) to 1.69 (the most costly state to vote, New Hampshire). After Shelby, previously covered states saw their COVI scores increase in absolute terms (from an average of 0.34 to an average of 0.65) and also increase relative to other states (from an average ranking of 32 to an average ranking of 37.5).

And yet, as noted above in reference to work by [Komisarchik & White \(2022\)](#), there has been no meaningful decline in turnout in formerly covered states, and in fact, there seems to have been an increase in registration and turnout among Black and Hispanic Americans in these jurisdictions. Using a similar model to [Komisarchik & White \(2022\)](#) and examining overall turnout at the state level, we find essentially no change in the share of the voting-eligible population who turned out to vote: in a two-way fixed effect specification we find an increase in overall turnout of 0.6 percentage points, but with a wide confidence interval (95-percent confidence interval [–0.027, 0.039]).¹⁴ And as noted above, we see no evidence that countermobilization would explain the lack of a decrease in turnout stemming from policy changes following *Shelby*. *Shelby*, thus, presents a case where a single Court decision suddenly allowed states to make multiple changes to their election procedures that allegedly increase the burden on voters. Evidence suggests that the aggregation of those multiple changes did not lead to a Republican advantage.

¹⁴ These estimates are consistent with aggregate turnout results presented in Appendix C in [Komisarchik & White \(2022\)](#).

To be sure, the analysis here focuses on state-level consequences, and perhaps studies of county-level or municipal-level effects could be different. So far, no such studies have been produced. A recent high-profile article that estimates the effect of *Shelby* on local election officials finds “no indication that local election officials have used their new discretion post-*Shelby* to advantage their party” (Ferrer, Geyn & Thompson, 2023). Now more than a decade out from the Court’s ruling in *Shelby*, the scholarly record has not provided evidence that the court case and accompanying policy changes have affected partisan outcomes at the local or state levels.

6.2 What Would Happen if Mississippi Made the Dreams of Progressive Election Reformers Come True?

Mississippi has laws that progressive activists and some scholars believe make it costly for people to vote. According to the Cost Of Voting Index, Mississippi consistently ranks among the bottom states (Schraufnagel, Pomante II & Li, 2020). Mississippi also has low voter turnout; in the most recent midterm in 2022, turnout was 32.5 percent among eligible voters, more than 10 percentage points lower than the national turnout rate.¹⁵ Mississippi, which tends to be a safe state for Republicans, is also a state where close to 40 percent of the population is Black, the highest share of any state in the nation.

We now engage in a thought experiment: suppose the Mississippi state government suddenly passed a package of six reforms all meant to increase voting access. With these six reforms, Mississippi would presumably be viewed by voting rights advocates as among the best states in the country for lowering the cost of voting, a model for other states. How would this bundle of reforms affect the partisan balance in the state? We will run this thought experiment in the context of the 2020 election. In that election, the voting-age population in the state was approximately 2,272,000. Of these, 1,325,000 were estimated to cast a ballot.¹⁶ The Republican presidential candidate, Donald Trump, received 58.4 percent of the two-party vote.

To be sure, estimating the Mississippi-specific effect of changing these policies is quite challenging. The best we can do is use estimates of the effects of the laws on turnout from the literature. Where possible, we will also attempt to use the literature to estimate the partisan composition of who is affected by the law. We will also make the implausible assumption that there is no overlap in who is targeted (and affected) by the laws, resulting in our calculations likely providing an upper bound on the effect of the policies. For instance, for two laws both meant to increase turnout among mobile populations such as young people, we will assume that the laws increase turnout among entirely different people.

6.2.1 Reduction in Polling Location Wait Times.

Pettigrew (2021) estimates that every additional hour spent waiting in line causes a one-percentage-point decrease in future turnout. In 2020 in Mississippi, approximately 12.2 percent of in-person voters waited at least an hour to cast their ballot. According to the CES, approximately 90.8 percent of ballots cast in Mississippi in 2020 were in person. This implies that removing wait times in voting would increase turnout by an additional 1,468 voters. Because this group is small, estimates of the partisan breakdown from the CES are necessarily noisy. But we estimate approximately 69 percent of voters who wait more than an hour are Democrats and 22 percent are Republicans. Given this breakdown, it implies that improving wait times would yield an additional 693 Democratic votes.

6.2.2 Election Day Voter Registration.

Currently, Mississippi requires individuals to register to vote 30 days before an election. Removing the requirement to register, in advance, makes it easier to vote. Grumbach & Hill (2022) estimate how the effect of same-day voter registration laws varies by age, arguing that the effects are concentrated among younger voters. We use Grumbach and Hill’s (2022) replication code to estimate the overall effect of Election Day voter registration on turnout, using self-reported turnout from the CPS and a simple two-way fixed effect model to adjust for fixed characteristics of states and election-specific changes in turnout. We estimate that imposing Election Day voter registration causes a 0.67 percentage point increase in

¹⁵ Michael P. McDonald, “2022 General Election,” *United States Elections Project*, accessed May 24, 2023.

¹⁶ Michael P. McDonald, “2020 General Election,” *United States Elections Project*, accessed May 24, 2023.

turnout, an effect that is not statistically significant (95-percent confidence interval $[-0.021, 0.034]$). Based on the citizen voting age population, we estimate Election Day registration would approximately increase turnout by approximately 15,015 voters. Because Grumbach & Hill (2022) shows that this effect is concentrated among young voters, we use the CES to estimate the partisanship of Mississippi residents by age group.¹⁷ Based on this calculation and using the effect estimates in Grumbach & Hill (2022), we estimate that same-day registration results in an additional 2,215 Democratic votes in the state.

6.2.3 Pre-Registration for Young Voters.

Mississippi currently does not allow individuals to pre-register to vote. According to Fowler (2017), pre-registration increases turnout among 18–26-year-olds by 2 percentage points. Using estimates of the Mississippi population distribution, this implies an additional 4,825 voters. If we use the same partisanship distribution used when calculating the Election Day voter registration effects, we estimate pre-registration adds an additional 712 Democratic votes.

6.2.4 Removing Strict Photo Identification Requirements.

Mississippi currently requires voters to present photo identification when casting their ballot. We are unaware of race-specific estimates of who lacks photo identification in Mississippi. According to Hosemann (2017), the state estimated that between “5% and 9% of registered voters did not have an active Mississippi driver’s license or Department of Public Safety-issued photo identification”. In 2020, approximately, 1,749,000 voters were registered to vote in Mississippi. If we use estimates from Grimmer & Yoder (2022), we estimate that removing photo identification requirements would yield between 2,361 to 4,250 additional voters, depending on whether the share of registered voters without photo identification is 5 percent or 9 percent. We do not have estimates of the partisanship of these voters, but we can use estimates from North Carolina as an approximation. In North Carolina, 57.5 percent of voters without an ID match were registered as Democrats, 19.2 percent were registered as Republicans, and 27.3 percent did not register with a particular party. If we apply these numbers to Mississippi, we estimate that removing a photo identification law would yield Democrats between 904 and 1,628 additional votes.

6.2.5 Felon Disenfranchisement.

The Sentencing Project, a non-profit group focused on restoring felony voting rights, estimates that 218,181 Mississippi voters are ineligible to vote because of prior felony convictions and that 127,130 of those voters are Black.¹⁸ To calculate the effects of restoring felon rights on the partisan balance in Mississippi, we use estimates from the literature discussed above, which is that approximately 10 percent of felons would vote, adding an additional 21,818 voters. While we do not have Mississippi-specific estimates of the political preferences of ex-felons, we extrapolate Michael Morse’s estimates from Florida. Using these numbers, we estimate that restoration of voting rights to Black felons would increase the Democratic vote total 11,642 votes ($11,642 = 129,495 \times 0.1 \times (0.94 - 0.041)$) and that restoration of voting rights to Not Black felons would cause an increase of 2,633 votes to Republicans ($2,663 = 109,714 \times 0.1 \times (0.36 - 0.60)$). In total, this implies an increase of 9,008 Democratic votes. Note that if we used the alternative strategy, discussed above, of estimating ex-felon preferences based on survey responses of non-college-educated men, the pro-Democratic shift from disenfranchisement would be closer to 4,000 votes rather than 9,000 votes.

6.2.6 No-Excuse Mail in Absentee Voting.

Mississippi currently limits mail-in absentee voting to individuals who reside outside the county, individuals with a permanent disability, caretakers of those with a permanent disability, or voters 65 or

¹⁷ Because of lack of statistical power, we divide Mississippi residents as younger or older than thirty-five.

¹⁸ In Mississippi, The Sentencing Project computes this number by combining the number of individuals serving felony sentences or on parole and estimates the number of living felons who are disqualified from voting. This necessarily requires assumptions about how many individuals who were convicted of felonies were released and their mortality rate. The estimate from the Sentencing Project is much larger than the estimate from Michael McDonald’s Voting Eligible Population estimates, which suggest that 46,032 Mississippi voters are ineligible due to felonies. This appears to be similar to the number of individuals in Mississippi prison or on parole.

older. According to estimates from Thompson et al. (2020), no-excuse mail-in voting yields a 2.1 percentage point increase in turnout. If applied to Mississippi, this would imply an additional 47,001 voters turning out for an election. But Thompson et al. (2020) estimate that there is a minimal partisan effect of the laws and the effect depends upon the state used to estimate the effect. If we estimate the effect of vote by mail using data from California, Thompson et al. (2020) estimate a 1.2 percentage point decrease in the Democratic party's share. Applying this estimate to Mississippi in 2020 would yield an increase of 40,117 votes for Republicans, more than enough to cancel out all the increases in Democratic vote share from the other reforms listed above. If, however, we use estimates from all three states that Thompson et al. (2020) use, we estimate a 0.7 percentage point increase in two-party Democratic vote share. This would imply an additional 13,609 votes for the Democratic party. Importantly, if Mississippi or any other state passed a no-excuse mail voting law, the legislators passing that law would not know with any certainty, based on the available evidence, whether the law would slightly benefit Democrats or Republicans.

6.2.7 Total Votes and Total Democratic Votes.

We estimate that making this series of policy reforms would increase overall voter turnout in Mississippi by 4.1 to 4.2 percentage points. We emphasize that these effect estimates likely are an overestimate of the effect of the laws because we have made the improbable assumption that each law is affecting disjoint groups of individuals. Of course, many of the laws are likely to affect the same population. As a maximum number of votes for Democrats, we estimate that the laws in total will add 25,985 Democratic votes. This implies a 1.6 percentage point increase in Joe Biden's vote total in 2020. At the bottom of our estimates, imposing these policies would net Republicans 26,585 votes. This would imply a 0.38 percentage point increase in Donald Trump's vote total in Mississippi.

If Mississippi implemented these six policies, we would expect an increase in voter turnout but would not be confident about which party would benefit. This is true before we even consider the uncertainty when estimating the causal effects of these policies or uncertainty in our estimates of the political preferences of individuals who are affected by the law. As a result, after these policies are implemented, we would expect the status quo to be largely unchanged: Mississippi would still have below-average voter turnout and would remain a safe state for Republicans.¹⁹

7. POLICIES THAT DO AFFECT PARTISAN ELECTIONS

Our framework suggests that most election policies are unlikely to affect even very close election outcomes. If not these policies, then what policies, if any, should concern those who are worried about laws affecting outcomes?

Two kinds of policies should generate serious concern. First, while the policies we have reviewed have small effects because they target small groups of voters and/or have minimal effects on turnout, and/or affect a politically heterogeneous group of voters, historically the USA has seen election policies that do target large groups of homogeneous voters and massively change their voter turnout rates. These are the policies that still cast a shadow over much of the debate around election laws.

Namely, the southern states, post-Reconstruction, disenfranchised Black voters nearly completely. The effect of racist laws in the South on voting is merely one component of the heinous effects these laws had on Black Americans. There is no doubt that the policies that disenfranchised Black voters as well as the policies of the civil rights era that re-enfranchised Black voters dramatically affected election outcomes and, in turn, affected legislation (Olson, 2023; Schuitt and Rogowski, 2017).

Consider participation rates by White versus Black Americans living in Louisiana (Keele, Cubbison & White, 2021). At the end of the nineteenth century, registration rates among eligible Black and White Louisiana men were 80–100 percent. When Louisiana changed its Constitution in 1898, instituting poll

¹⁹ Anticipating the effect of changing many laws in a state is challenging, in part because the effect of laws could interact with each other, causing the effect to be smaller or larger than the simple additive effect that we have supposed. Interaction effects could result in a smaller overall effect than we described here. This is particularly true if some of the laws would mobilize the same person (if implemented on their own). Our estimates would thus “double count” those individuals and would be too big. Or, it could be that our estimates are too small if there are individuals who are mobilized only if groups of policies are ON. As a hypothetical, it could be that in some states felons are more likely to vote if they also receive a ballot in the mail or if they are able to register on Election Day.

taxes and residency requirements, but exempting White men from those requirements via a “Grandfather clause”, Black registration dropped effectively to 0 percent within a few years. Then, starting in 1944, with the end of the White-only primary, followed by a series of other changes (i.e., the Voting Rights Act of 1965), Black registration rose dramatically and, by the early twenty-first century, returned to the same levels as Whites.

In 1896, according to data from [Keele, Cubbison & White \(2021\)](#), in the median parish in Louisiana, 54 percent of eligible citizens were Black, 88 percent of Black men were registered to vote, and 85 percent of White men were registered to vote. For illustrative purposes, let us imagine in 1896 in this median parish an election took place between a Black-preferred candidate and a White-preferred candidate in which all Black voters chose one candidate, all White voters chose the other candidate, and turnout did not vary by race. In this imaginary close election, the Black-preferred candidate would win with 55 percent of the vote (slightly higher than the Black share of the population because the observed registration rate is higher for Black Louisianans than White Louisianans).

Four years later, in 1900, after Louisiana adopted its new Constitution, circumstances dramatically changed. According to the data, the median parish in 1900 was 51 percent Black. The voter registration rate for White men went to 57 percent (down from 85 percent). The voter registration rate for Black men went to 1 percent (down from 88 percent). If that same hypothetical election between a Black-preferred candidate and a White-preferred candidate was held under these new 1900 circumstances, the White-preferred candidate would now win with over 98 percent of the votes.

What are the features of the post-Reconstruction South that made the disenfranchising laws so potent? The policies targeted not a small subset of people (e.g., just a small percentage of people who lack a photo ID) but all who are Black because they are Black. While the policies did have spillover—Louisiana’s 1897 Constitution reduced White registration by half—state governments used tools such as a “Grandfather Clause”, an “Understanding Clause”, and outright violence to selectively target the suppressive laws to Black people. The suppressive policies did not just reduce Black turnout by a little bit but reduced Black turnout to zero. In the Civil Rights era, policies and Court cases had a similarly large impact on participation because they, too, targeted voters based on their racial identity, doing away with policies from literacy tests to White-only primaries that had effectively and in many places completely disenfranchised Black Americans ([Fresh, 2018](#)).

Policies that target a large, politically homogeneous share of the population—a racial group, a religious group, a group of party registrants—and have a large effect on turnout specifically for that group, should raise serious alarm about the consequences for election outcomes.

To be sure, some policies—including policies we have not examined or ones that have not yet been imagined or proposed—may have effects on turnout that are larger than the contemporary policies we have studied and could disproportionately affect some groups more than others. By no means has our analysis contemplated every possible election law.

As just one example, a policy intervention that has quite dramatic effects on turnout and, potentially, disproportionate effects for one party relates to the timing of elections. States that consolidate local elections such that municipal elections are held on the same day as federal elections have dramatically higher turnout in those local contests. Since many local elections are officially nonpartisan, the consequences of election consolidation would not be measured in Democratic versus Republican outcomes, but this is nevertheless the rare kind of policy that could change outcomes ([Anzia, 2013](#); [Hajnal, Kogan & Markarian, 2022](#)).

A second kind of policy that should raise concern is one that is implemented *after* an election when the outcome is known to be a near tie: rules governing recounts. Cases such as the 2000 presidential election in Florida or the 2008 senate election in Minnesota raise concern that lawmakers, judges, or the campaigns themselves can put their thumbs on the scale and tip an election.²⁰

In recounts, the election is known ex-post to be very close, so even minor changes in votes make a difference. In the targeted population (i.e., “voters with challenged ballots”), the mechanism of invalidation of ballots or inclusion of ballots means that the turnout effect can be large even though only a small fraction of voters may be in the targeted set. If the invalidation or counting of contested ballots

²⁰ On the Minnesota recount, the selective counting of absentee ballots raised concern (“Coleman to argue Franken won by bogus recount” CNN, January 26, 2009). In the Florida recount, different standards for recounts across counties raised concern (*Bush v. Gore*, 2000).

is targeted disproportionately to voters of one party on account of an official with partisan motives or the undue influence of one side's campaign, then the gains or losses of votes will accrue disproportionately to one party. Recounts are thus a perfect storm for laws affecting election outcomes, and it is no surprise that recount experiences of the past (such as Florida 2000) carry significant weight in the minds of lawmakers and advocates about how election administration rules affect outcomes.

8. CONCLUSION

From the perspective of partisan activists, the US states are battlegrounds; the future of democracy depends on how policymakers act on issues such as no-excuse mail voting, early voting, automatic registration, voter identification, felon disenfranchisement, and so on. Partisan actors attempt to delegitimize their opponents, sow doubt about the integrity of elections, and mobilize their donors and supporters around the dramatic potential consequences of election laws. And yet, there is little evidence in the scholarly record suggesting contemporary election rules affect election outcomes. Most laws barely make a dent on voter turnout, let alone on who wins or loses. The disconnect between the perceived stakes and the available evidence presents a puzzle.

This essay sought to address the puzzle. We offered a new framework for thinking through the effects of laws on turnout and on election outcomes. Once one has a sense of the size of the population targeted by a law, the extent to which the targeted population differs from the rest of the population in terms of partisan preferences, and the likely effect on turnout, then one can evaluate the likely effect of a law on which party will win or lose an election. It is straightforward to see that most of the laws that are fought over so vigorously in statehouses and courtrooms cannot affect any but the very closest elections.

As we have argued through concrete examples, the lack of the relationship between changes in election laws and changes in partisan outcomes is not the result of countermobilization campaigns. We have also shown that the aggregation of multiple election laws in a state does not mean that small partisan effects accrue into larger effects. Because the effect sizes are so small and different laws help different party coalitions (sometimes in unexpected ways), the effect of the laws in the aggregate is often a wash and difficult to predict ahead of an election. We have further shown that policies that favor the participation of one racial group or another do not always run parallel with policies that favor the preferred party of different racial groups. To the extent that advocates believe that mobilizing or demobilizing racial groups is a path to helping their party win elections, their intuitions may be way off base.

A clear implication of our analysis is to lower the temperature on election administration policies. Lawmakers should not pass laws thinking they will help their partisan side. It would not work and it is a waste of time. And the media should not portray every change in an election law as a red-alert scenario that will determine future elections. Evidence suggests the partisan stakes for these laws are not particularly high. A typical voting law may affect outcomes on the order of a fraction of a percent, but most elections are not that close and even if they are, it is hard to determine which party would actually gain from each change in law and in each election year.

Does this mean that policymakers, judges, and ordinary Americans should no longer concern themselves with such laws?

No. The public, and its leaders, should care about election laws for reasons other than their partisan consequences, such as whether they make voting convenient, more secure, more cost effective, and whether they are motivated by discriminatory intent. They should also care about these laws for their normative value and for their effect on racial minority group participation, including participation in primary and municipal elections, which are different from the Democratic-vs-Republican elections we have focused on here. As noted above, much of the conflict over election laws is focused on ensuring racial minority groups have equal rights to participate in elections and that the historic record of discrimination and disenfranchisement of Black Americans is never repeated.

Laws that target homogeneous partisan groups and have large effects on turnout should raise red flags. Procedures surrounding post-election recounts should raise red flags. As far as the many variants of policies that might have a percentage-point or so effect on turnout, these are not policies that meet the criteria for concern over affecting partisan election outcomes. Of course, these policies still

might violate the law based on criteria such as the intent behind the policies and the burdens the policies place on voters, but they nevertheless are not cause for concern about affecting partisan election outcomes.

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