The Effect of “Prevailing Party” Laws on General Election Outcomes*

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Abstract: This paper examines the most common ballot ordering procedure used in U.S. general elections, which gives the most advantageous ballot position to the currently-prevailing political party. It usually increases the favored candidate’s vote share by two to three percentage points, enough to flip the result of roughly 1% of major elections nationwide. This effect is substantially larger than that of more innocuous ballot ordering schemes, due to “endorsement effects” these other schemes lack. The existing literature, which exclusively analyzes these other schemes, substantially understates the degree to which ballot order can be used to maintain political power.

Keywords: ballot order; elections; voting; primacy effects; endorsement effects

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1. Introduction

Democracy is glorious; administering elections is tedious. It amounts of a sequence of pedestrian activities: registering voters, validating candidates, generating ballots, conducting the election, and tallying the vote. Yet experience tells us that the way in which these activities are executed can have genuine effects on electoral outcomes and their legitimacy.

One electoral detail exploited in this way is the order in which candidates for a given office are arranged on the ballot. To the extent this ordering influences vote share, it can be used to bolster the fortunes of the party in power. States usually do this via "Prevailing Party" (PP) laws which grant that party the most advantageous ballot position in general elections. This paper documents the prevalence of these laws and estimates their effects.

It is the first paper in the ballot order literature to do so. Previous studies, discussed below, exclusively examine more innocuous systems that order candidates quasi-randomly, not purposively. This is a distinction with a difference. A long line of behavioral economics and political science research, discussed below, implies the relevance of "endorsement effects" that pertain only to purposive orderings. These would augment the effects of "cognitive bias" that are estimated in the existing literature, generating a larger impact on vote share. In order to know how PP laws affect electoral outcomes, we must analyze them directly.

We find that these laws are surprisingly prevalent and, because of endorsement effects, surprisingly effective. Over half of all general election ballots are cast under a PP ordering scheme, which (in most contests) adds at least 2-3 percentage points to the favored candidate's vote share, far more than quasi-randomized ordering systems do. Both in our data and nationwide, these laws determine the outcome of roughly 1% of general election contests for

U.S. Representative and state executive office. Laws intended to favor the prevailing party in ballot order determination substantially tilt the electoral playing field, calling into question their legitimacy in states committed to free and fair elections.

The beneficial statistical properties of quasi-random orderings do not apply to PP laws. Analyzing them requires the development of suitable estimation methods and their application to suitable data. Accordingly, the paper proceeds as follows. Section 2 documents the prevalence and controversial nature of PP laws, and Section 3 explains why the existing ballot order literature is ill-equipped to answer the most pressing economic and legal questions about them. Section 4 introduces our data: nearly half a century of general elections from the state of Wyoming, whose unusual ballot ordering procedure facilitates estimation. Section 5 develops our two estimation methods and Section 6 presents the results, which Section 7 applies to Wyoming and national elections in order to determine counterfactual outcomes under impartial ordering procedures. Section 8 concludes.

2. Procedures for Determining Ballot Order and Prevailing Party Laws

State legislatures specify how ballot order is determined. Broadly speaking, they can approach this task in three ways. They can treat it as a nuisance, and seek to minimize the trouble or inconvenience ballot ordering rules cause election officials, election workers, and voters. They can treat it as a responsibility, and seek to maximize the fairness of the election and the integrity of the results. Or they can treat it as an opportunity and seek to optimize political advantage, which usually amounts to ensuring that the party in power remains there. Despite the American presumption of free and fair elections, enshrined in many state constitutions, this last perspective often can be put into practice, because of uncertainty about the effects of such rules, tradeoffs involving their cost to voters or election administrators, or evolving legal standards.

The opportunity motive is muted for primary elections, which are intra-party contests. It comes into full force only in general elections, which are the focus of this paper. To document the prevalence of each approach, Table 1 groups all states' general election ballot ordering procedures according to its motivating principle: convenience, fairness, or power. (To gauge magnitudes, the table lists the number of Congressional seats elected under each method.)

As the table shows, most states use fairness-oriented or power-oriented systems, in roughly equal number. Only eight less-populous states use convenience-oriented systems that order candidates alphabetically or leave it to election officials' discretion.

Fairness-oriented systems, in turn, take three types: randomization, rotation, and placement of the minority party first on the ballot. The first two types are the most common, particularly west of the Mississippi River. When executed at the county level, as in Arkansas, randomization balances out ballot order reasonably well across the state, muting any effect in statewide races (but not local ones). Rotation systems distribute ballot positions even more evenly, especially when implemented across precincts (as in North Dakota), though small deviations may occur for incidental reasons. Some states snuff out even those.

Power-oriented systems almost always give the prevailing party first ballot position, consistent with a primacy effect long believed to hold anecdotally (Krosnick, Miller, and Tichy, 2004) and, to a lesser degree, academically, as discussed in the next section.[1] Most such laws are explicit, giving first position to the party that won the most recent election for Governor,

[1] The one exception, Massachusetts, gives first position to incumbents, which usually but not universally favors the Democratic Party, the dominant party in the state.

Secretary of State, or President (within that state); a few others specify the first-listed party directly (Delaware, Missouri) or use other indirect means. While the number of *states* using PP laws almost matches the number using fairness-oriented systems, the first set of states are more populous. Thus more than half of all votes nationwide are cast under a PP system.

One could argue, as did several election officials in *Jacobson v. Florida Secretary of State*, that a PP law primarily promotes convenience, as it "allows voters to more quickly find their preferred candidate [and] promotes uniformity in administering elections" (957 F. 3d 1199, 2020). This interpretation is undermined by the ballot-ordering procedures used in these states' primary and nonpartisan contests, most of which emphasize fairness. Of the sixteen de jure PP states, eleven—including Wyoming and Florida—use randomization or rotation systems for all primaries, while a twelfth uses them for some primaries (also see Grant, 2023). If such schemes are not overly burdensome in these elections, one cannot claim otherwise for inter-party contests.

PP laws are not only numerous, but persistent. Two decades ago, Krosnick, Miller, and Tichy (2004) detailed all states' general election ballot ordering procedures. The changes in PP laws since then are few in number and split in direction: Tennessee and Missouri implemented them while New Hampshire and North Carolina discarded them. This last change could itself have been opportunistic. North Carolina's Republican-dominated legislature eliminated ballot placement according to the most recent gubernatorial vote after Democrat Roy Cooper was elected governor in 2016. Its counterpart, New Hampshire, changed its law after a 2006 state Supreme Court mandate.

While changes have been few in number, PP laws have come under increasing legislative and judicial scrutiny. In 2021, bills requiring other ballot-ordering methods were filed in the state legislatures of Pennsylvania and Wyoming, though neither made it to a final vote, while recent federal lawsuits sought to overturn PP laws in five states.[2] Three of these, in which the plaintiffs were voters and party organizations (such as the Democratic Senatorial Campaign Committee), foundered on the issue of standing. A fourth, in Arizona, had similar plaintiffs, one of whom was found to have standing. The fifth suit, in West Virginia, included a candidate for state office among its plaintiffs and also passed the test of standing. However, in a 2-1 ruling, a panel for the 4th Circuit Court of Appeals determined that the burden imposed by West Virginia's statute was "at most…modest" and justified the state's interest in reducing voter confusion (912 F. 4th 380, 2021). At present, then, case law surrounding PP laws is evolving and evidence on its effects is timely.

3. Ballot Order Effects and Prevailing Party Laws

To date, the effects of PP laws on electoral outcomes have been imputed from the existing ballot order literature, which estimates the causal effect of ballot position on vote share. In low-information, down-ballot contests and primary elections in which party affiliation cannot guide voters' choices, this literature largely agrees that a primacy effect exists for first position, which can be as large as ten percentage points (e.g., Meredith and Salant, 2013; Grant, 2017). Much weaker effects are observed in U.S. general elections, as summarized in Table 2. Five studies each find an average first-position bonus of about one percentage point in vote

[2] In Florida, *Jacobson v. Florida Secretary of State* was filed in May 2018 and decided upon appeal in Sept. 2020; in Texas, *Miller v. Hughs* was filed in Nov. 2019 and dismissed in July 2020; in Georgia, *S.P.S. ex rel. Short v. Raffensperger* was filed in Nov. 2019 and administratively closed in Sept. 2020; in Arizona, *Mecinas v. Hobbs* was filed in Nov. 2019 and dismissed in June 2022; in West Virginia, *Nelson v. Warner* was filed in Dec. 2019 and decided upon appeal in Sept. 2021. Academic evidence featured prominently in most of these cases.

share, while three others obtain a null effect.[3]

But there is less to this than meets the eye. As Table 2 documents, this literature exclusively examines states that rotate candidates' ballot position across jurisdictions (e.g., precincts), which is beneficial for estimation. If all ballot ordering schemes influenced voters identically, this literature would suffice. But one cannot assume this to be true. Rotation systems are qualitatively different from PP and need not have equivalent effects. This vital point has been overlooked in this literature and deserves further explication.

To date, primacy effects observed with ballot order and related phenomena (see Grant, 2023) have been interpreted in terms of cognitive biases described in the psychology literature (e.g., Miller and Krosnick, 1998; Mussweiler, 2003), such as satisficing, in which the voter selects the first acceptable option. Under sequential decision-making, these mental "short cuts" privilege the item considered first. However, this interpretation has rarely been tested, and has fared poorly when it has (Grant, 2017).

For cognitive bias, the method used to create the ordering is immaterial. Thus, this explanation is well-suited to the random or quasi-random (through rotation) ballot orderings studied to date, which have no inherent meaning. But this need not be so in other systems. Even without direct knowledge of a PP law's existence, no voter would interpret a ballot listing every Republican first and every Democrat second (or vice versa) to be arbitrarily determined, much less a string of such ballots over years of general elections. This ordering is purposive and thus potentially meaningful, and this meaning can generate framing effects of its own.

[3] Even this modest finding overstates the case, for current purposes, as it does not compare first position with second position, the change in ballot order effectuated by PP laws. Rather, first position is compared to last position or to all other ballot positions (see Table 2). This comparison yields larger effects, as primacy effects are not limited to first position, but favor second position relative to third, and so on (Meredith and Salant, 2013, or Grant, 2017).

What could this type of ordering mean? First ballot position is a place of prominence, which could be associated with positive qualities that favorably dispose the voter towards that option. This is the case in a large literature on defaults—"pre-selected" options that must be affirmatively overturned by the decision maker. These strongly influence many important decisions, including charitable giving and retirement saving (e.g., Bruns and Perino, 2021, and Hagen, Hallbey, and Lindquist, 2022). One source of this influence is an "endorsement effect," whereby the prominence of the default option is interpreted as an implicit endorsement by the designer of the choice architecture (Alonso-Garcia, 2021; Benhassine et al., 2015; Brown, Farrell, and Weisbenner, 2012; McKenzie, Liersch, and Finkelstein, 2006; Madrian and Shea, 2001). Such effects disappear when the default is understood to have been randomly determined (Blumenstock, Callen, and Ghani, 2019; Goswani and Urminsky, 2016). If purposive orderings lend an endorsement effect to first ballot position, it would increase that candidate's vote share beyond that generated by any cognitive bias, and the existing ballot order literature would understate the effect of Prevailing Party laws on vote share.

There is no direct evidence on this point for U.S. elections, but there is closely related evidence from European elections. There, candidates are often selected from "party lists," which are ordered by the party itself. It would be reasonable for voters to interpret first position on this list as a party endorsement.[4] Evidence indicates that they do and react accordingly. Ballot order effects in these elections are quite large, often exceeding twenty percentage points (e.g., Faas and

[4] "If names are not listed alphabetically, it is very likely that [they] follow a certain logic…The name order then reflects how much parties want to push certain candidates and/or how well candidates are embedded in their constituencies" (Lutz, 2010). "The order in which names appear on the party list is decided by the party. Ballot position can hence be meaningfully interpreted as a signal of endorsement by the party leadership…The position on the ballot signals to the voters that from the point of view of a party a candidate is particularly well qualified to become a legislator" (Marcinkiewicz, 2014).

Schoen, 2006; Marcinkiewicz, 2014). However, when parties list their candidates in alphabetical order—so that first position carries with it no implied endorsement—this bonus in vote share diminishes greatly or vanishes (Ortega Villodres, 2003; Lutz, 2010).

This evidence establishes the plausibility of an endorsement effect associated with PP laws but not rotation ordering schemes, in which some voters perceive the first-listed candidates to be preferred or endorsed by the designer of the ballot—government. Its effect would operate separately from, and in addition to, any cognitive bias, increasing the benefit to being listed in first position. The small or nil ballot order effects in existing studies of general elections need not apply to PP laws. These laws must be evaluated directly.

Endorsement effects would have not only empirical consequences, but legal ones too. To date, PP laws have been challenged on the basis of unequal treatment that burdens lower-listed candidates (and their supporters), and adjudicated using the *Anderson-Burdick* framework that compares the magnitude of injury with the state's justification for any burden imposed by that law. However, it is generally accepted that government endorsement of political candidates is unconstitutional (Tebbe, 2013, Part IB develops this point at length). The presence of endorsement effects would thus foster a second basis for challenging the constitutionality of PP laws, to which the *Anderson-Burdick* framework would not apply.

4. Wyoming Elections and the Data

The political consequences of PP laws can be uncovered by estimating their effects on vote share. In general, this is hard to do. Most such laws assign ballot order uniformly across the state, so that these laws' effects are identified solely from those rare occasions in which the statewide vote for the "index contest" that governs ballot placement switches parties. Credible estimates cannot be formed in this way.

However, three states implement PP at the county level instead. The oldest of these laws, adopted in 1973, is Wyoming Statute § 22-6-121 (a), which states the following:

> Political party position shall be determined on the general election ballot according to the number of votes received by each party within the county for the office of representative in congress at the last preceding general election. The party receiving the highest number of votes shall appear first following the names of the offices to be voted for and other parties shall follow in the order of their respective numbers of such votes.[5]

This law is also unique in assigning ballot order biennially, based on the contest for Wyoming's sole U.S. House seat, instead of quadrennially. Consequently, it generates sufficient *within-state* variation in ballot placement over time that its effects on vote share can be estimated.

Accordingly, we use county-level election results in Wyoming to analyze the effect of its PP law on all contests that are decided statewide: three for federal office (President, Senate, House) and five for state executive office (Governor, Secretary of State, Treasurer, Auditor, Superintendent of Public Instruction). There are 24 biennial electoral cycles between 1973 and 2020 and 23 counties in Wyoming, yielding over 2,000 county*year*office observations for analysis. This lets us estimate the effects of ballot order reasonably precisely.

The two-way fixed effects regression specification introduced below identifies the effect of interest through changes in ballot order within counties over time. Table A1 in the Appendix shows that such changes are reasonably common. In addition to the national ebb and flow of partisan lean, Wyoming's politics have evolved locally over time (see Jacobs, 2022). Coal-rich counties in the southern half of the state became more conservative as union influence waned,

[5] This text is from 2020. The law, but not the ordering procedure, has been revised since 1973.

while more populous counties in the southeast and northwest corners of the state became more liberal, in line with similar developments nationwide. The sample period has a total of 51 ballot order switches, spread widely across time and space and almost evenly split in direction.

This table also shows that the number of contested offices in our sample is also almost evenly split, between federal and state. While some general elections for state office were uncontested—this happened for every office but governor—95 contested elections remain available for analysis. County-level data on each of these contests was gathered from various sources: Wyoming's Secretary of State, the political data website Our Campaigns, and Wyoming's historical Blue Book.[6] We analyze this data using the techniques described next.

5. Methods

In practice, PP laws determine which of the two major parties is placed first on the general election ballot, the other being placed second. Accordingly, our analysis estimates the advantage in major-party vote share in general election contests that comes from being listed first on the ballot instead of second. Following the literature, we allow this "ballot order effect" to vary across offices (e.g., Governor, U.S. Representative, etc.). We estimate this effect with two complementary methods, panel regression and regression discontinuity.

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[6] Election results from 1996 forward are available from Wyoming's Secretary of State online at https://sos.wyo.gov/Elections/ElectionResults.aspx and previously in printed form in biennial editions of the *Official Directory of Wyoming and Election Returns for the Preceding Year*. "Our Campaigns" results can be accessed via the links here: https://www.ourcampaigns.com/ContainerDetail.html?ContainerID=14. The three most recent editions of the Blue Book are at https://wyoarchives.wyo.gov/pdf/WyomingBlueBookThree.pdf, https://wyoarchives.wyo.gov/pdf/WyomingBlueBookFour.pdf, and https://wyoarchives.wyo.gov/pdf/BlueBookFinal.pdf.

A. Panel Regression

Specification. This county-level regression relates candidates' vote shares to their ballot order and controls. The controls are threefold. Fixed effects identifying each general election contest—for Auditor in 1990, for example—capture the (relative) appeal of the candidates in that contest statewide, including any advantage due to incumbency, while county fixed effects capture time-invariant differences in party preference across counties.

While the contest-specific fixed effects will reflect statewide shifts in party preference over time, we must also account for local, county-specific variation in such preferences beyond that captured by these fixed effects. It is impractical to do this using party registration or objective demographic measures such as mean age and education, which are not measured with the necessary frequency over the full sample period. Instead, we take a more direct approach, utilizing that county's vote share in the congressional race, the only contest held biennially. (Note that this variable's inclusion also addresses the "variable trends" issue pertinent to two-way fixed effects models.)

To develop our regression specification, it helps to begin with a hypothetical in which Republicans were listed first on half of each county's ballots and Democrats were listed first on the other half. In this scenario, let $H^*_{c,t}$ be the Republican share of the major party vote in the congressional (House) race in county c in year t and $Y^*_{c,t}$ be the analogous quantity for the office of interest, such as governor. Having removed the influence of ballot order, our previous logic implies that $Y^*_{c,t}$ is related to $H^*_{c,t}$ as follows:

$$Y^*_{c,t} = \alpha H^*_{c,t} + \sigma_c + \tau_t + \varepsilon_{c,t} \quad \textbf{(1)}$$

where α is a parameter, σ and τ represent county and year fixed effects, which subsume the regression constant, and ε is an error term.

Observed vote shares are related to these hypotheticals as follows:

$$H_{c,t} = H^*_{c,t} + \beta_H(F_{c,t} - ½) \quad \textbf{(2)}$$

$$Y_{c,t} = Y^*_{c,t} + \beta_Y(F_{c,t} - ½) \quad \textbf{(3)}$$

where β_H and β_Y represent the ballot order effect in the House contest and contest of interest and $F_{c,t}$ is a dummy variable that equals one if Republicans are listed first on the ballot in that county and zero otherwise.[7] Substituting eqq. (2) and (3) in eq. (1) yields the following:

$$\begin{aligned} Y_{c,t} &= \alpha H_{c,t} - \alpha\beta_H(F_{c,t} - ½) + \beta_Y(F_{c,t} - ½) + \sigma_c + \tau_t + \varepsilon_{c,t} \\ &= \alpha H_{c,t} + (\beta_Y - \alpha\beta_H)F_{c,t} + (\sigma_c + (\alpha\beta_H - \beta_Y)/2) + \tau_t + \varepsilon_{c,t} \quad \textbf{(4)} \end{aligned}$$

In this equation, the coefficient of interest, β_Y, is not identified. This is because all partisan races on the same ballot are subject to ballot order influences, and all such races are identically ordered within each county. Thus, the coefficient estimate on *F* reflects a weighted difference between the ballot order effects in the two contests.

This exercise demonstrates that our regression cannot use the contemporaneous congressional vote share as a control. Thus, we use the lagged congressional vote share instead. Replacing $H^*_{c,t}$ in equation (1) with $H^*_{c,t-2}$, the analog to equation (4) becomes:

$$Y_{c,t} = \alpha H_{c,t-2} - \alpha\beta_H F_{c,t-2} + \beta_Y F_{c,t} + (\sigma_c + \frac{\alpha\beta_H}{2} - \frac{\beta_Y}{2}) + \tau_t + \varepsilon_{c,t} \quad \textbf{(5)}$$

where two year lags are used because Wyoming general elections occur in even-numbered years. Now β_Y is identified. However, two lagged election cycles are needed to determine $F_{c,t}$ and $F_{c,t-2}$, shortening the estimation period to 1976-2020.

[7] Because *H* is the major-party vote share, ignoring third-party votes, defining *H* and *F* in terms of Republicans is inconsequential. Identical results would obtain were *H* and *F* defined in terms of Democrats instead. Because third party candidates invariably received a small vote share and were never listed first on the ballot, omitting them from the analysis is also inconsequential. There were several such candidates in the data, from the Libertarian and Constitutional parties.

The control $H_{c,t-2}$ is not just serviceable, but advantageous.[8] Since it determines current ballot order, it ensures that $F_{c,t}$ does not indirectly pick up party preference as well, removing a potential source of bias. An additional sign restriction is generated, since lagged ballot order is also included as an independent variable. And $H_{c,t}$ can now be used as the dependent variable, allowing the ballot order effect for U.S. House races to be estimated as well.

<u>Estimator</u>. Equation (5) is nonlinear in the parameters, which unnecessarily complicates estimation. We therefore replace it with a simpler, linear "reduced form" analog:

$$Y_{c,t} = \alpha H_{c,t-2} + \beta_Y F_{c,t} + \gamma F_{c,t-2} + \tilde{\sigma}_c + \tau_t + \varepsilon_{c,t} \quad \textbf{(6)}$$

Primacy effects consistent with the "opportunity" motive for PP laws imply that β_Y is positive and γ negative, as it backs out the effect of ballot order on the House race used as a control. When House contests are analyzed using this regression, $Y_{c,t} \equiv H_{c,t}$ and $\beta_Y \equiv \beta_H$ in equation (5), thus the restriction $\alpha\beta_H = -\gamma$ should hold in equation (6).

We estimate this equation using standard methods, making two adjustments to address issues with the residuals. First, we account for heteroskedasticity by weighting the observations slightly. As discussed in detail in Grant (2017), small counties should have greater "sampling error," in which randomness in the subset of registered voters who actually cast a ballot materially influences the vote share. Grant's scheme, which sets the weight to the logarithm of the number of voters, adequately accounts for this heteroskedasticity, so we use it here too.

Second, a small number of substantial outliers were observed, thickening the tails of the residual distribution relative to normality. At least some of these outliers can be traced to

[8] An alternative, the lagged value of Y, is unavailable whenever the previous election for that office went uncontested; this sharply reduces the number of usable observations.

"favorite son" effects, in which a candidate from a given county earned many crossover votes in that county. These are difficult to code or control for directly: some challengers' biographies aren't obtainable, and many candidates have lived in multiple places within the state. We address this issue conservatively by conducting estimation using least absolute deviation instead of least squares. This common approach for dealing with thick-tailed residuals is well supported in the literature (Dasgupta and Mishra, 2004; Bassett and Koenker, 1978; Koenker and Bassett, 1978) and absorbs no additional degrees of freedom.[9]

B. Regression Discontinuity

This estimator is motivated by the fact that $F_{c,t}$ is determined by a threshold of 0.5 for $H_{c,t-2}$. This threshold, while politically and logically sensible, is econometrically arbitrary; for *H* values sufficiently near 0.5, we can think of the resulting value of *F* as equally arbitrary. Thus, one can estimate β_Y by comparing vote shares (*Y*) on either side of this threshold, using well-established regression discontinuity methods.

The data suit this approach in that the relation between the running variable, $H_{c,t-2}$, and the dependent variable, $Y_{c,t}$, is smooth and well-represented by simple polynomials, as urged by Gelman and Imbens (2018). On the other hand, most observations lie to one side of the threshold—the right, or Republican, side, as shown by the scatterplots of the data in Figure A1 in the Appendix. In addition, the running variable is itself influenced by the previous cycle's ballot

[9] On balance, this estimator does not increase $\hat{\beta}_Y$. Compared to the four Table 3 estimates presented below, least squares estimates are larger in two cases, smaller in a third, and comparable in a fourth.

order. We thus present several specifications, some of which control for state and year effects and for previous ballot order, and one of which addresses the "one-sidedness problem."

Our basic regression discontinuity specification uses quadratic trends on each side of the 0.5 threshold:

$$Y_{c,t} = \theta_0 + \theta_1(H_{c,t-2} - .5) + \theta_2(H_{c,t-2} - .5)^2 + \beta_Y F_{c,t} + \theta_3 F_{c,t}(H_{c,t-2} - .5) + \theta_4 F_{c,t}(H_{c,t-2} - .5)^2 + \varphi X + \varepsilon_{c,t} \quad \textbf{(7)}$$

As before, β_Y represents the ballot order effect; φ and the θ's are coefficients, and X is a possibly-empty vector of controls that can include state and year fixed effects and the lagged ballot order dummy, $F_{c,t-2}$. This equation and simple variants of it are estimated using ordinary least squares. In estimates that do not include lagged ballot order, the sample can be pushed back two years, to begin in 1974; otherwise it begins in 1976 as before.

6. Results

A. Panel Regression

Table 3 presents four sets of coefficient estimates: for the most frequently contested federal office (U.S. Representative) and all federal offices together, and for the most frequently contested state office (Governor) and all state offices together. When offices are grouped together, a uniform ballot order effect is estimated for all of them, but the fixed effects are expanded to allow the year and time dummies to vary by office.[10] Grouping offices together

[10] This is necessary to ensure there is a separate fixed effect for each contest (such as Auditor in 1990). When offices are grouped together, year dummies no longer serve this purpose.

generates needed statistical power at this cost of uniformity.

In three of these four regressions—for U.S. House, Governor, and all state executive offices together—the results are similar and consistent with expectations. The coefficient estimates on the lagged congressional vote share run from 0.4 to 0.7, indicating that this variable meaningfully reflects local (countywide) variation in party preference. The estimated ballot order effects in each of these regressions are also similar, ranging from two to three percentage points in vote share. Finally, the coefficient estimates on lagged ballot order are all negative, as expected (though none are statistically significant).

Taking these three regressions in turn, the U.S. House estimates imply a two percentage point advantage for first ballot position, a sizeable, statistically significant effect. A Wald test of the coefficient restriction generated above for this race, $\alpha\beta_Y = -\gamma$, yields a test statistic of 0.14, insufficient to reject this null.

Turning to state office, the estimated ballot order effect for the governor's race alone is nearly three percentage points. It is statistically insignificant, however, as the standard error is large. This problem is remedied in the "all state contests" estimates, which utilize many more observations. Here the estimated effect is easily significant, with a point estimate resembling that for governor and a much lower standard error;[11] the estimate on lagged ballot order has the right magnitude as well.[12] On balance, then, the evidence indicates that ballot order effects for

[11] When separate regressions are conducted for each executive office, the $\hat{\beta}_Y$'s are positive and fairly similar but rarely significant (standard errors in parentheses): Secretary of State 7.55 (2.59); Auditor 1.78 (1.92); Treasurer 1.37 (1.86); Superintendent of Public Instruction 3.74 (2.09). For President $\hat{\beta}_Y$ is 0.20 (0.89); for U.S. Senator, -1.31 (0.71).

[12] The absolute value of $\hat{\gamma}$ is about 0.5 in these two regressions. It should equal the product of $\hat{\alpha}$ (which is about 0.45) and $\hat{\beta}_H$ in the House race in column one (which is about 2). This product equals 0.9, which is within one standard error of $\hat{\gamma}$.

state executive positions are sizeable.

The remaining Wyoming regression, in the second column of the table, groups all three federal offices together. Here the estimates are inconsistent with primacy effects: the ballot order coefficient is insignificant and the lagged ballot order coefficient takes the wrong sign. This is not surprising. The literature consistently indicates weaker effects in races where voters have more information, in general elections (Chen et al., 2014; Pasek et al., 2014; MacInnis et al., 2021) as well as primaries. Contests for President and U.S. Senator fall into this category. Our finding that ballot order effects obtain only in U.S. House contests and further down-ballot is consistent with this literature.

Finally, as a falsification test, we apply equation (6) to a PP state that assigns ballot order identically in all counties. Since county-level vote share does not determine ballot order, if *F* is assigned at the county level as before, β_Y should be zero and its estimate insignificant. Any statewide ballot order effect is subsumed in the year dummies.

We implement this approach with data from the state of Texas, which assigns ballot order in all counties according to the *statewide winner* of the previous gubernatorial election. We assign *F* based on the prevailing party *in that county* in the previous gubernatorial election, and use it to predict vote share in the current gubernatorial election for the years 2002-2022. The large number of Texas counties (254) compensate for this shorter time span, maintaining a similar degree of statistical power.

The results are presented in the rightmost column of Table 3. The effect of previous vote share is similar to that in Wyoming, but $\hat{\beta}_Y$ is slightly negative and insignificant. There is no sign that our estimation approach "artificially" generates favorable coefficient estimates.[13]

B. Regression Discontinuity

Table 3's clearest empirical findings obtain from contests for U.S. House and for state executive office. Regression discontinuity estimates for these two sets of contests are presented in Table 4, using linear trendlines, quadratic trendlines as in equation (7), and a simple modified linear estimator that addresses the "one-sidedness problem" by comparing the mean value just left of the threshold with its predicted value from the well-estimated right side trendline.[14]

The estimates are imprecise in the absence of controls, but the addition of county and year dummies remedies this problem substantially. The addition of lagged ballot order further increases the estimates, as expected. This last set of estimates is quite similar across specifications and greater than or equal to those in the corresponding panel regression. For U.S. House races, the ballot order effect is a little over two percentage points; for state executive office, it is five percentage points, though this estimate is only one standard error above our panel estimate of the same quantity.

In summary, for all but the two most prominent offices elected in Wyoming, ballot order

[13] This is not surprising. The two econometric concerns that theoretically pertain to equation (6) are unlikely to apply with force. The use of the lagged dependent variable as a regressor in the U.S. House regression only affects the estimate of interest, $\hat{\beta}_Y$, indirectly, and these effects can be expected to be small. The "Goodman-Bacon problem," which occurs whenever treatment is staggered across cross-sectional units over time, is most salient when treatment is "irreversible," which is not the case here; most of the time, treatment is indeed "reversed."

[14] Formally, all observations for which $H_{c,t-2} < 0.48$ are dropped, and in equation (7) θ_1, θ_2, and θ_4 are set to zero and 0.5 is replaced with $(\widehat{H}_{c,t-2} \mid H_{c,t-2} < 0.5) \approx 0.49$. This specification adheres to Lee and Lemieux's (2010) stricture to estimate the right-side trend using only right-side observations; it only extrapolates the value of this trend slightly below its range.

effects are sizeable. Switching from second to first position raises vote share by at least two percentage points in U.S. House races and at least three percentage points in contests for state executive office.

C. Mechanisms

Our findings markedly exceed those in the existing literature on general elections, summarized in Table 2, where first position effects ranged from nil to one percentage point. This suggests PP laws possess endorsement effects lacking in the rotation systems studied previously. To complete this argument, we must provide empirical evidence supporting this mechanism.

We can do so by leveraging some of the last variation remaining in our data: last names. In Section 3, some evidence marshalled in support of the plausibility of endorsement effects came from candidate lists ordered by the parties participating in European elections. Primacy effects were far weaker when these lists were ordered neutrally, in alphabetical order, rather than purposively, which generally would not be in alphabetical order. Extending the same idea to our data implies that ballot order effects will be weaker when candidates' last names happen to fall in alphabetical order and stronger otherwise.[15]

To investigate, we return to contests for U.S. House and state executive office and let ballot order effects differ by whether or not the candidates are listed alphabetically. Third parties (Constitutional, Libertarian) are prevalent in Wyoming general elections, and it is unclear

[15] For this to hold, the voter must not allow the preeminent listing of one party throughout the ballot to override the presence or absence of alphabetical order in particular races. This possibility makes this test somewhat less discriminating, because the irrelevance of alphabetical order need not imply the absence of endorsement effects. On the other hand, this means that the case for endorsement effects is further enhanced should alphabetical order be relevant.

whether voters would include them in perceiving whether the top two candidates are listed neutrally or purposively. Thus, we conduct two sets of estimations: one that throws out all contests with third-party candidates, and another that codes a contest as being in alphabetical order only if this holds across all candidates. The terms of interest interact alphabetical order and ballot order; any *independent* effect of the candidate's place in alphabetical order (Edwards, 2015) is absorbed into the contest-specific dummies.

The results are placed in Table 5. In every instance, the estimated first position effect is weaker when ballot order corresponds to alphabetical order and stronger when it doesn't. The difference between the two estimates, though imprecise, is usually sizeable. When candidates happen to be ordered alphabetically, being in first position rather than second adds an average of one percentage point to vote share, roughly in line with the Table 2 estimates from rotation ordering schemes in which endorsement effects are absent. When candidates are not listed alphabetically, however, this effect rises to about three percentage points instead. The cognitive biases discussed in the ballot order literature to date cannot account for this difference, but endorsement effects can. On balance, the evidence indicates that Prevailing Party laws impact vote share more than rotation ordering systems do, because of endorsement effects.

7. Counterfactual Outcomes

Ultimately, vote share is not valued in its own right, but because it increases the probability of winning. In this section we quantify this effect, first for the Wyoming elections in our data and then for recent elections across the United States.

A. Wyoming

Of the 95 contests in our data, two were won with less than 51% of the major party vote: the 1978 governor's race, where Democrat Ed Herschler's vote share was 50.9%, and the 2006 U.S. House race, where Republican Barbara Cubin had 50.3%. In both contests, the winner was listed first on about 75% of ballots statewide, and so was assisted by the ballot order effect.

To calculate hypothetical outcomes absent this effect, we apply eqq. (2) and (3) to each county, taking into account whether each candidate was listed first or second on the ballot in that county, and using the office-specific coefficient estimate from Table 3. (The gubernatorial estimate, though insignificant, almost matches the significant estimate for all state offices together; both are smaller than their Table 4 counterparts.) The results estimate $H^*_{c,t}$ (in 2006) and $Y^*_{c,t}$ (in 1978) in each county; these counterfactual vote totals are then added together to determine the winner of this hypothetical election. Table A2 in the Appendix has the results.

In the 2006 U.S. House race, this exercise reverses the vote shares of the two candidates. Cubin won that race by about 1,000 votes; in the counterfactual, she loses by over 700. If not for Wyoming's PP law, her opponent Gary Trauner probably would have represented Wyoming in the 110th U.S. Congress. For the 1978 gubernatorial race, the results are too close to be definitive; in the counterfactual, Herschler is likely but not certain to win.[16] That may not have

[16] The qualifiers "probably" and "likely" are necessitated by imprecision in the estimates. If the true β_Y is far from its estimated value, these calculations misstate Cubin's and Herschler's ballot order bonus. From a Bayesian perspective, the true coefficient varies asymptotically normally around the estimated value, with variance given by the square of the standard error. From this, one can calculate the probability that β_Y is such that these candidates would not have won without the help of ballot order. These calculations show that, in the counterfactual, there is an 88% chance Trauner would have won against Cubin, and a 31-46% chance John Ostlund would have won against Herschler (depending on which standard errors are used in the computations).

been the only consequence of PP here, as Herschler went on to be re-elected in 1982.

B. Recent U.S. Elections

Altogether, PP laws directly affected the outcome of 1-2% of the 95 general elections in our Wyoming data, with additional indirect effects possible through incumbency. A similar story unfolds nationally.

To examine national impact, Table 6 lists the number of Congressional and gubernatorial contests since 2010 that plausibly could have been determined by PP laws. These contests are placed in two groups: "possible," in which the winner was listed first on the ballot and the winning margin was within the relevant coefficient estimate in Table 3, and "probable," in which the winning margin was within half of that estimate.[17]

For Congressional seats, gerrymandering limits the number of close contests, blunting the impact of PP laws. A serviceable heuristic is that about a dozen contests each year are close; about half of those lie in PP states, where the winner was favored about half the time. Thus, approximately three Congressional seats are decided by these laws each year, roughly split between the two parties. Currently, PP laws have a small but noticeable impact on representation in the U.S. House.[18]

For state executive office, gerrymandering is impossible and more elections are close.

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[17] Following eqq. (2) and (3) and Bayesian logic, the latter group had a more than 50% chance of being decided by PP laws, relative to a rotation system that equalized ballot placement, and the former group a material chance that was under 50%.

[18] The impact was probably larger before 1970, when elections were closer. Since 2010, 15% of Congressional elections have had a winning margin under ten percentage points. From 1946-1970, the comparable number was 20% (Abramowitz, Alexander, and Gunning, 2006).

Accordingly, the fraction of affected gubernatorial contests is larger: four "probable" and two "possible," mostly favoring Republicans.[19] This includes an unusual "hat trick" in the state of Florida, where the Republican won by a whisper in 2010, 2014, and 2018. The favorable ballot placement afforded the Republican in 2010 probably determined his victory in that year, generating favorable placement for his narrow re-election in 2014, which then generated favorable placement for his successor in 2018.

These results are typical for state executive positions. PP laws affected a similar fraction of Attorney General and Secretary of State contests. Altogether, these laws currently alter the outcome of about 0.5% of Congressional races and about 2% of elections for state executive office. These findings accord with a varied body of evidence confirming that ballot ordering procedures affect electoral outcomes in local (Meredith and Salant, 2013), primary (Koppell and Steen, 2004; Ho and Imai, 2008; Edwards, 2015), and general elections (Miller and Krosnick, 1998; Pasek et al., 2014). When the opportunity motive is present, favorable electoral outcomes can be effectuated by the procedure used to order candidates' names on the ballot.

This conclusion understates matters for two reasons. It is based on our more conservative panel estimates instead of the regression discontinuity estimates in Table 4. It also ignores the indirect effect of incumbency, which independently boosts vote share. An official elected via PP will likely serve multiple terms, not just one—as in the Florida and Wyoming governor's races discussed above. This "multiplier" effect would be larger for U.S. Representatives than for governors, as they do not face term limits. The 14 U.S. Representatives that were plausibly elected via PP laws in 2010 or 2012 subsequently served an average of 1.5 additional terms.

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[19] More Republican states use PP laws, but the Democratic states doing so are larger on average. Thus one should expect rough party parity in Congressional elections but not those for governor.

8. Discussion and Conclusion

Pedestrian details matter. In general elections, most voters fall under a de facto or de jure "Prevailing Party" ballot ordering system that favors the politically powerful at the expense of the politically weak. This system is enduring; just two states have abandoned it, and two adopted it, in the last twenty years. However, lawsuits and legislation seeking change are increasingly frequent, occurring in at least seven states since 2018. Now is a propitious time to document the prevalence of these laws and ascertain their effects.

We do so in this paper, examining almost fifty years of general elections in Wyoming, whose unusual, county-level ballot ordering mechanism permits estimation using modern panel regression and regression discontinuity methods. The results indicate that these laws increase vote share in most statewide contests by at least two to three percentage points for the major-party candidate who is listed first, relative to their counterpart listed second. Third party candidates placed further down the ballot—unexamined in this study—are likely to suffer even more, as the literature shows that second ballot position is itself preferable to lower positions.

These findings are markedly stronger than those in previous studies of U.S. general elections, which exclusively analyze rotation ordering systems instead. This is because the effects of an ordering are influenced by the method of its creation. The purposive orderings generated by Prevailing Party laws have endorsement effects that rotation systems lack. These raise the effect of being listed first on the ballot well above the 0-1 percentage points found in the existing literature.

These findings have academic, legal, and electoral implications. In terms of research, they raise questions about the existing ballot order literature and generate topics for future study.

While the literature has uniformly attributed ballot order effects to cognitive bias, this mechanism has been assumed more than tested. This should change. More broadly, endorsement effects have received limited attention in the orderings literature generally and could apply to non-electoral contexts as well.

Our findings impact law in two ways. The *Anderson-Burdick* framework regularly used by courts to assess the constitutionality of election statutes, including PP laws, involves comparing the magnitude of injury with the state's justification for any burden imposed by the law. Our findings are far larger than in the existing literature on U.S. general elections, potentially altering any such comparison decisively. Furthermore, the presence of endorsement effects introduces a second, novel basis for challenging these laws' constitutionality.

It may be tempting to view Wyoming, which gave the Republican presidential candidate his largest margin in 2020, as so partisan that it is impervious to ballot order effects, with such large electoral margins that any effects would be inconsequential even if they did occur. This is not so. Favorable ballot placement changes the outcome of one or two of the 95 contests in our data and a similar fraction of Congressional and state races nationwide. Ballot order matters, and in Wyoming and many other states, Prevailing Party laws have successfully altered electoral outcomes in favor of the party in power.

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Table 1. Procedures for Ordering Names on General Election Ballots, with the Number of Congressional Seats Elected in 2020 in Each Group (relevant statute in parentheses, with states that changed their procedure since Krosnick, Miller, and Tichy, 2004 underlined).

Fairness-Oriented Systems (163 seats)	Convenience-Oriented Systems (36 seats)	Power-Oriented Systems (236 seats)
Randomization (80 seats)	*Alphabetical (15 seats)*	*De Jure PP (197 seats)*
AR* (§ 7-5-207(3)(c)(1))	HI (§ 11-115)	AZ* (§ 16-502(E))
CO (§ 1-5-404)	LA (§ 18:551(C)(c)(i))	CT (§ 9-249 (a))
NM (§ 1-10-8.1)	ME (21-A § 601.1.2)	FL (§ 101.151.6(3)(a))
NC (§ 163-165.6(c))	NV (§ 293.267)	GA (§ 21-2-285(c))
OH (§ 3513.052(e)(1))	VT (17 V.S.A § 2472 (b)(2))	IN* (§ 3-11-2-6)
OK (§ 26-6-106)		KY (118.215(1))
OR (§ 254.155)		MD (§ 9-210(j)(2)(i))
SD (§ 12-16-3.1)		MI (§ 168.703)
UT (§ 20A-6-305)		NE (§ 32-815)
VA (§ 24.1-613(c))		NY (§ 7-116-1)
WA (§ 29A.36.151) **		PA (§ 25.2963)
		TN (§ 2-5-208(d)(1))
		TX (§ 52.091(b))
		WV (§ 3-6-2(c)(3))
		WI (§ 5.64(1)(b))
		WY* (§ 22-6-121(a))
Rotation (64 seats)	*Official's Choice (21 seats)*	*De Facto PP (30 seats)*
AK (§ 15.15.030.6)	NJ* (§ 19.49.2)	DE (§ 4502(a)(5)) [#]
CA (§ 13111)	RI (§ 17-19.6)	IA (§ 43.73, § 49.37)
ID (§ 34-903)	SC* (§ 7-13-320)	IL (10 ILCS 5/16-3) **
KS (§ 25-610)		MO (§ 115.237.3) [#]
MT (§ 13-21-205)		
ND (§ 16.1-06-05.4, § 16.1-11-27)		
NH (§ 656:5-a)		
Inverse PP[†] (19 seats)		*Other[‡] (9 seats)*
AL		MA (§ 54.42)
MS		
MN (§ 204D.13(2))		

Note: Statutes current as of Jan. 2022.

* The procedure is carried out at the county level.

** A primary is followed by a runoff, in which the top vote getter is listed first. In WA, this is a "jungle" primary containing all candidates irrespective of party; in IL, the primary is closed, but all candidates' vote totals are reported on a single list.

Explicitly lists the Democratic party (DE) or Republican party (MO) first; these are the dominant parties in these states.

† Reverse PP, in which the minority party is listed first. In AL and MS, this is de facto, in MN, it is de jure. While Ala. Code § 17-6-25 requires names to be listed in alphabetical order, the sample ballots promulgated by Alabama's Secretary of State, to which the counties adhere, list Democrats first.

‡ Incumbents are listed first, followed by challengers in alphabetical order.

Table 2. Summary of Existing Literature on Ballot Order Effects in U.S. General Elections.

Study	State, Years, and Elections Studied	Method of Ballot Order Determination	Number of Estimates	Comparison Group (to first position)	Median Estimate (if available, else mean) in Percentage Points
Darcy (1986)	CO, 1984 Federal, State	rotation	22 contest-level	second position	nearly 0*
Miller and Krosnick (1998)	OH, 1992 Federal, State, Judicial, Local	rotation	118 contest-level	all other positions	1.3 (two-candidate races), 0.5 (multi-candidate races)
Krosnick, Miller, and Tichy (2004)	OH, CA, ND, 2000 not specified	rotation	306 candidate-level	last position	probably under one percentage point (see note)
Alvarez, Sinclair, and Hasen (2006)	CA, 1998 Federal, State Executive	rotation	52 candidate-level	all positions other than first and last	nearly 0*
Ho and Imai (2008)	CA, 1978-2002 Federal, State Executive	rotation	18 contest-level	all other positions	-0.2
Chen et al. (2013)	ND, 2000-2006 Federal, State, Judicial	rotation	36 contest-level	all other positions	1.2 (two-candidate races), nearly 0 (multi-candidate races)*
Pasek et al. (2014)	CA, 1976-2006 Federal, State Executive	rotation	402 candidate-level	all other positions (also last position)	0.9 (major party candidates)
MacInnis et al. (2021)	NH, 2012, 2016 Federal, Governor	rotation	33 candidate-level	all other positions	1.0 (major party candidates)

Notes: Krosnick, Miller, and Tichy (2004) only describe their estimates qualitatively. About one-quarter are positive and significant; those average 2.9 percentage points in two-candidate races and half that in multi-candidate races.
* almost identically zero in the median or by inspection of the graph or table reporting the range of estimates

Table 3. Panel Regression Coefficient Estimates (in percentage points of Republican vote share, with standard errors in parentheses), Contested Statewide General Elections, Wyoming, 1976-2020.

	Offices Analyzed				Falsification Test
Independent Variable (coefficient)	U.S. Representative	All Three Federal Offices	Governor	All Five State Executive Offices	Governor—Texas (see note)
Republican's Vote Share in the Previous Congressional Race (α)	0.71* (0.05)	0.63* (0.03)	0.47* (0.11)	0.43* (0.04)	0.59 (0.02)
Republican Listed First on Ballot (β_Y)	2.04* (0.72)	-0.33 (0.43)	2.92 (2.29)	2.88* (0.61)	-0.40 (0.85)
Republican Listed First on Ballot in Previous Congressional Race (γ)	-0.83 (0.67)	1.33* (0.41)	-0.46 (1.71)	-0.55 (0.56)	3.00 (0.53)
County Fixed Effects ($\tilde{\sigma}$)?	Yes	Yes, Interacted with Office	Yes	Yes, Interacted with Office	Yes
Year Fixed Effects (τ)?	Yes	Yes, Interacted with Office	Yes	Yes, Interacted with Office	Yes
Number of Observations	552	1219	253	966	1524

Note: Estimates were obtained using least absolute distance estimation. Federal offices are President, U.S. Senator, and U.S. Representative. State offices are Governor, Secretary of State, Auditor, Treasurer, and Superintendent of Public Instruction. Not all state offices were contested in each election. In the falsification test, the previous gubernatorial race replaces the previous congressional race for the estimation of α and in determining the "party listed first" variables. * = $p < 0.05$.

Table 4. Regression Discontinuity Estimates of First Ballot Position on Vote Share (in percentage points, with standard errors in parentheses), Contested Statewide General Elections, Wyoming, 1974-2020.

	SPECIFICATION			
Contest(s) Controls	Linear	Quadratic	Modified Linear (see note)	Number of Observations (unmodified, modified)
U.S. House	0.67	2.77	3.54	575, 520
no controls	(1.50)	(2.03)	(1.84)	
add county and year	1.31	1.58	2.35*	575, 520
dummies	(0.75)	(0.99)	(0.86)	
also add lagged	2.61*	2.29	4.11*	552, 502
ballot order	(1.18)	(1.35)	(1.43)	
State Executive Office	-0.02	0.71	3.81	1081, 964
no controls	(2.07)	(2.85)	(2.74)	
add county and year	2.44*	4.62*	4.97*	1081, 964
dummies (interacted with office)	(0.75)	(1.00)	(0.92)	
also add lagged	4.90*	5.05*	5.12*	966, 874
ballot order	(1.21)	(1.46)	(1.56)	

Note: Estimates were obtained using ordinary least squares. State executive offices are Governor, Secretary of State, Auditor, Treasurer, and Superintendent of Public Instruction. Not all state offices were contested in each election. When lagged ballot order is included, separate effects are estimated on each side of the threshold. The modified linear specification is described in the text and includes only observations for which the Republican earned at least 48% of the vote in the previous U.S. House contest. * = $p < 0.05$.

Table 5. Ballot Order Effects and Alphabetization—Panel Regression Estimation Results (in percentage points of Republican vote share, with standard errors in parentheses), Contested Statewide General Elections, Wyoming, 1976-2020.

Independent Variable	Offices Analyzed: U.S. Representative		Offices Analyzed: All Five State Executive Offices	
Republican's Vote Share in the Previous Congressional Race	0.11 (0.08)	0.70* (0.04)	0.37* (0.04)	0.43* (0.04)
Republican Listed First on Ballot (Alphabetical Order)	0.15 (1.45)	-0.97 (0.91)	1.36 (1.03)	2.63* (0.98)
Republican Listed First on Ballot (not Alphabetical Order)	1.87 (1.49)	2.77* (0.75)	2.99* (1.49)	3.13* (0.96)
Republican Listed First on Ballot in Previous Congressional Race	-0.32 (0.84)	-0.67 (0.66)	0.58 (0.78)	-0.43 (0.61)
County Fixed Effects?	Yes	Yes	Yes, Interacted with Office	Yes, Interacted with Office
Year Fixed Effects?	Yes	Yes	Yes, Interacted with Office	Yes, Interacted with Office
Contests with Third Party Candidates Included?	No	Yes	No	Yes
Number of Observations	138	552	759	966

Note: Estimates were obtained using least absolute distance estimation (least squares estimates were similar). State offices are Governor, Secretary of State, Auditor, Treasurer, and Superintendent of Public Instruction. Not all state offices were contested in each election. * = $p < 0.05$

Table 6. Major Elections Plausibly Decided by Prevailing Party Laws, 2010-2022.

	U.S. Congress		*Governor*	
Year	Probable	Possible	Probable	Possible
2022	1D, 2R	2D		
2020	1R	1D, 1R		
2018	2R	1D, 1R	2R	1R
2016		2R		
2014	1D	2D	1R	1D
2012	2D, 2R	1D, 1R		
2010	1D, 3R	4D	1R	
Total	5D, 10R	11D, 5R	4R	1D, 1R

Note: For U.S. Congress, "Probable" means a win by the 1st listed candidate of less than one percentage point in a state employing PP laws; "Possible" means a win by 1.0-2.0 percentage points. For Governor, "Probable" means a win by the 1st listed candidate of less than 1.5 percentage points; "Possible" means a win by 1.5-3.0 percentage points. Given the coefficient estimates (rounded) and standard errors in Table 3, "probable" races were more than 50% likely to have been decided by PP laws; possible had some chance of being decided by PP laws, but less than 50%. D stands for Democrat and R stands for Republican.

Appendix. Table A1. Characteristics of Wyoming General Elections, 1976-2020.

Year	Number of Counties Switching Ballot Order		Number of General Election Contests	
	R to D	D to R	Federal	State Executive
2020	0	0	3	0
2018	1	0	2	4
2016	0	0	2	0
2014	0	0	2	2
2012	0	3	3	0
2010	0	3	1	2
2008	4	0	4*	0
2006	3	0	2	4
2004	0	0	2	0
2002	0	1	2	3
2000	0	1	3	0
1998	0	2	1	4
1996	2	0	3	0
1994	0	1	2	4
1992	1	2	2	0
1990	4	0	2	5
1988	0	0	4*	0
1986	0	0	1	5
1984	0	0	3	0
1982	0	3	2	5
1980	0	11	2	0
1978	2	1	2	4
1976	6	0	3	0
Totals	23	28	53	42

* includes a 2008 special Senate election and a 1989 special House election (using the same ballot orders as the 1988 election).

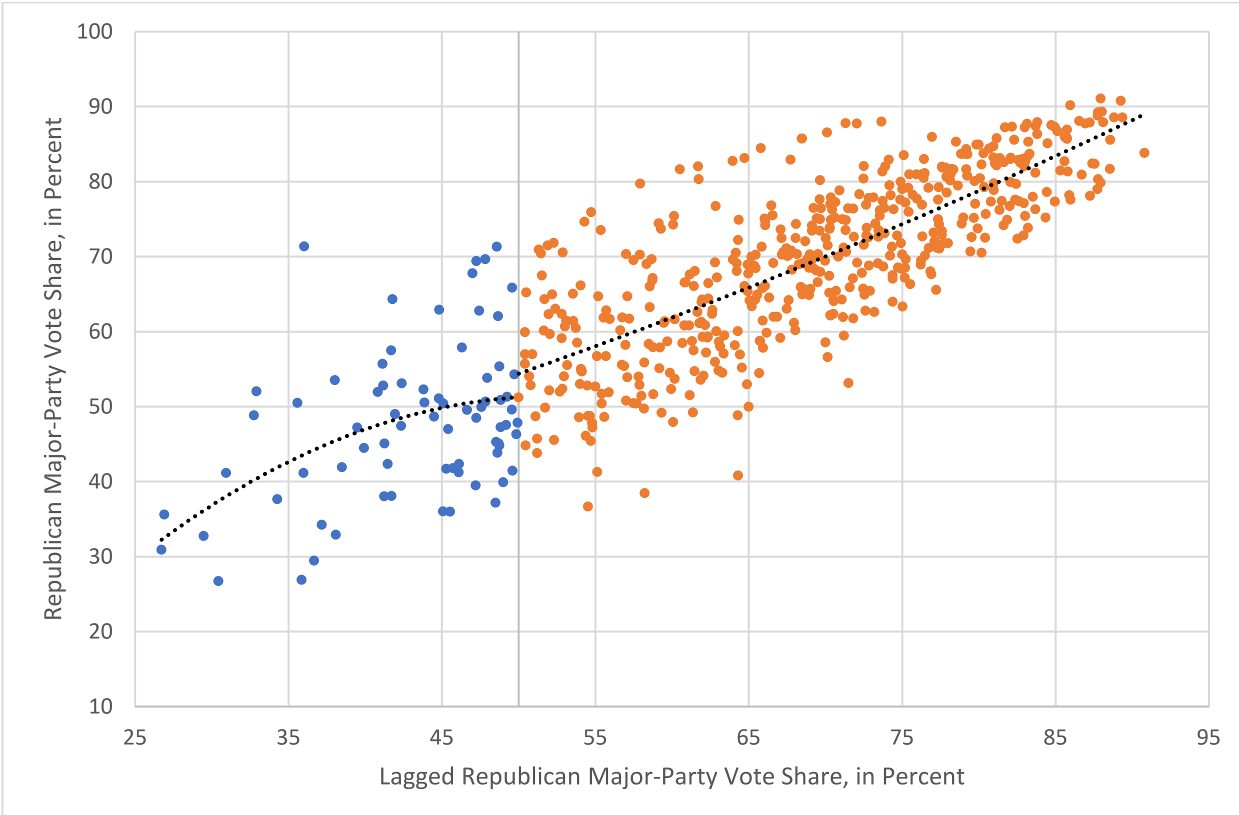
Note: A general election was considered contested if both major parties fielded a candidate. Many races classified as uncontested included independents or members of other parties, especially the Libertarian and Constitutional Parties. Federal races include president, U.S. senator, and U.S. representative. State executive positions include Governor, Secretary of State, Treasurer, Auditor, and Superintendent of Public Instruction.

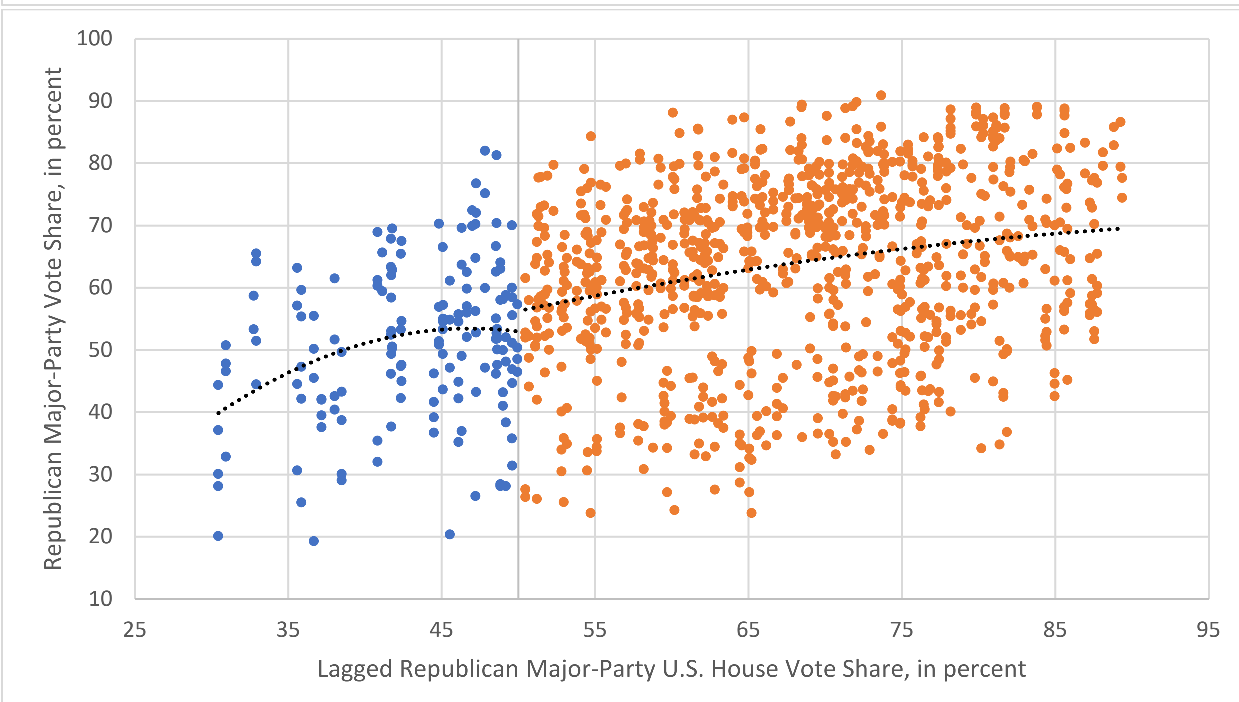
Table A2. Actual and Counterfactual Vote Totals in Two Closely Contested Races.

	2006 U.S. House of Representatives				1978 Governor			
	Actual		Counterfactual		Actual		Counterfactual	
County	Cubin (R)	Trauner (D)	Cubin (R)	Trauner (D)	Herschler (D)	Ostlund (R)	Herschler (D)	Ostlund (R)
Albany	4133	7350	4250.1	7232.9	6610	2920	6470.9	3059.1
Big Horn	2986	1328	2942.0	1372.0	1786	2331	1846.1	2270.9
Campbell	7213	3289	7105.9	3396.1	1359	3403	1428.5	3333.5
Carbon	2634	2769	2578.9	2824.1	3435	2331	3350.8	2415.2
Converse	2674	2170	2624.6	2219.4	1465	1632	1419.8	1677.2
Crook	2077	717	2048.5	745.5	755	1166	783.0	1138.0
Fremont	6541	6610	6406.9	6744.1	5128	4941	4981.0	5088.0
Goshen	2662	1991	2614.5	2038.5	2296	2374	2227.8	2442.2
Hot Springs	1160	999	1138.0	1021.0	1193	1207	1158.0	1242.0
Johnson	2116	1117	2083.0	1150.0	1312	1316	1350.4	1277.6
Laramie	11,869	18,188	12,175.6	17,881.4	11,939	9564	11,625.1	9877.9
Lincoln	3881	2008	3820.9	2068.1	2211	1715	2153.7	1772.3
Natrona	10,793	13,848	10,541.7	14,099.3	9362	9806	9082.1	10,085.9
Niobrara	685	336	674.6	346.4	492	866	511.8	846.2
Park	7177	3867	7064.4	3979.6	2448	4594	2550.8	4491.2
Platte	1967	1842	1928.1	1880.9	1897	1553	1846.6	1603.4
Sheridan	5883	5255	5769.4	5368.6	4150	3558	4037.5	3670.5
Sublette	1717	1055	1688.7	1083.3	934	833	908.2	858.8
Sweetwater	5532	6648	5407.8	6772.2	5258	4586	5114.3	4729.7
Teton	2598	6218	2687.9	6128.1	1587	2201	1642.3	2145.7
Uinta	3476	2440	3415.7	2500.3	1885	1379	1837.3	1426.7
Washakie	1851	1457	1817.3	1490.7	1371	1969	1419.8	1920.2
Weston	1711	822	1685.2	847.8	1099	1350	1134.8	1314.2
Totals	**93,336**	**92,324**	**92,469.5**	**93,190.5**	**69,972**	**67,595**	**68,880.5**	**68,686.5**

Note: Votes for a third party candidate, Thomas Rankin of the Libertarian Party, are omitted from the 2006 contest. Mr. Rankin received 4,781 votes across the state. Hypotheticals assume that each major party candidate is listed first on the ballot an equal number of times, and is listed second when not listed first. Each counterfactual relies on the office-specific coefficient estimate in Table 3.

Figure A1. Scatterplots of the Data. Top: U.S. House. Bottom: Statewide Executive Office.





Note: Quadratic trends to the left and right of the 50% threshold are shown in each graph.