Safe States, Silent Voters:

A Six-Cycle Analysis of Partisan Predictability and Voter Turnout in U.S. Presidential Elections (2004–2024)

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Revised May 2026

Abstract

This study examines whether state-level electoral predictability — commonly described as a state being "safely" Democratic or Republican — is associated with lower voter participation. Using certified Federal Election Commission (FEC) vote totals and Voting-Eligible Population (VEP) turnout estimates from the United States Elections Project, we estimate Ordinary Least Squares (OLS) regressions of VEP turnout on absolute partisan lean for all 50 states plus the District of Columbia across six consecutive presidential elections (2004–2024). In every cycle the relationship is negative, statistically significant ($p < 0.001$), and explains between 41% and 55% of the cross-sectional variance in state-level turnout ($R^2 = 0.410$–0.553). The regression slope is strikingly stable across cycles, averaging approximately -0.41 percentage points of turnout per unit of absolute partisan lean. These findings are consistent with a large body of peer-reviewed research showing that electoral closeness and campaign attention raise participation (Blais, 2006; Geys, 2006; Cox and Munger, 1989; Gimpel et al., 2007; Enos and Fowler, 2018). We situate the result within rational-choice theories of abstention, demonstrate its structural persistence across candidates and national environments, and argue that the pattern constitutes empirical evidence of a participation cost imposed by the winner-take-all geography of the Electoral College. We close by evaluating the National Popular Vote Interstate Compact (NPVIC) as a proposed remedy, presenting both the case advanced by its proponents and the principal scholarly objections.

Executive Summary

- **89 million eligible Americans did not vote in 2024** — roughly 36% of the voting-eligible population. They were not distributed at random; abstention was disproportionately concentrated in states where the outcome was a foregone conclusion.

- **Across six consecutive presidential elections (2004–2024), how lopsided a state is explains 41–55% of the gap in turnout between states** ($R^2 = 0.410$–0.553). A single structural variable accounts for roughly half of the cross-state variation in participation.
- **Each additional point of partisan "safety" is associated with about 0.4 percentage points of lost turnout.** The relationship is negative and statistically significant ($p < 0.001$) in *every* cycle, and its magnitude has not changed across twenty years of elections, candidates, and national moods — the signature of a structural feature, not a passing trend.
- **When Georgia shifted from "safe" to genuine battleground, its turnout jumped roughly twelve points** (Section 5). The National Popular Vote Interstate Compact (NPVIC) would extend battleground-level competitiveness to the entire country and is the leading proposed remedy for re-enfranchising the safe-state majority; this paper assesses both its promise and the principal scholarly objections to it.

Contents

1 Introduction

A persistent puzzle in American electoral politics is the sizeable share of eligible citizens who choose not to vote in presidential elections. Even in 2020 — the highest-turnout election in more than a century — roughly one in three eligible Americans did not cast a ballot. In 2024, approximately 89 million eligible voters abstained, representing approximately 36% of the voting-eligible population.

Turnout is not uniform across states. In 2024, Wisconsin and Minnesota both exceeded 76% VEP participation, while Hawaii recorded only 50.3%, and Oklahoma and Arkansas hovered near 53%. This gap of more than 25 percentage points between the most and least participatory states demands explanation. Crucially, low turnout is not a politically neutral fact: as Lijphart (1997) argued in his presidential address to the American Political Science Association, unequal turnout produces unequal political influence and is "systematically biased" in ways that distort representation. Understanding *why* participation varies so sharply across states is therefore a question about the quality of democratic representation itself, not merely about civic habit.

1.1 The Myth of Apathy

The conventional narrative attributes non-voting to apathy or civic failure on the part of individuals. The evidence assembled in this paper points instead toward structure. A theoretically motivated hypothesis is that the *competitiveness* of a state shapes the incentive to vote. Under standard rational-choice models of turnout, a voter's expected utility from voting is a function of (among other things) the probability that their vote will be pivotal (Downs, 1957; Riker and Ordeshook, 1968). In a state where one party reliably wins by wide margins — whether deeply red Wyoming or deeply blue California — the probability of any single vote affecting the outcome is vanishingly small, potentially reducing the perceived payoff of voting. Conversely, in genuinely competitive states such as Wisconsin, Pennsylvania, or Nevada, voters may perceive a higher probability that their participation matters, raising turnout.

This expectation is not merely theoretical. In a meta-analysis of 83 aggregate-level turnout studies, Geys (2006) found that electoral closeness is among the most consistently supported predictors of turnout in the empirical literature, and Blais (2006) reaches a similar conclusion in his review of the field. The mechanism, however, need not be purely psychological. Cox and Munger (1989) demonstrated that closeness operates substantially through *elite* behavior: campaigns and parties concentrate mobilization effort where races are close, so that closeness raises turnout both directly (through voter perceptions of pivotality) and indirectly (through the intensity of mobilization voters experience). Both channels predict the same observable relationship at the state level.

1.2 Hypothesis

This logic predicts a **U-shaped relationship between signed partisan lean and turnout**, or equivalently a **negative linear relationship between absolute partisan lean and turnout**. We refer to this as the "safe-state suppression hypothesis." Taking the absolute value of partisan lean linearizes the expected U-shape: as a state becomes more lopsided in *either* direction, the model predicts turnout falls.

This paper tests the hypothesis systematically and longitudinally. We assemble a dataset spanning six consecutive U.S. presidential elections (2004–2024) and use official, certified vote totals from the Federal Election Commission alongside validated VEP-based turnout estimates to run the regression for each cycle individually. We then assess whether the findings are consistent across elections that differ substantially in their candidates, national political environments, and aggregate turnout levels. The contribution is not the discovery of the closeness–turnout link, which is well established, but the demonstration that it is *structurally stable* across two decades of presidential contests and that its magnitude is large enough to carry direct implications for institutional reform.

2 Data and Methods

2.1 Vote Totals

Candidate vote totals by state were obtained directly from the Federal Election Commission's official certified general election results publications:

- *Federal Elections 2004* (Federal Election Commission, 2005)
- *Federal Elections 2008* (Federal Election Commission, 2009)
- *Federal Elections 2012* (Federal Election Commission, 2013)
- *Federal Elections 2016* (Federal Election Commission, 2017)
- *Official 2020 Presidential General Election Results* (Federal Election Commission, 2021)
- *Official 2024 Presidential General Election Results* (Federal Election Commission, 2025)

For each election, we use the Democratic and Republican candidate totals to calculate the two-party vote share. Minor-party candidates are excluded from the partisan lean calculation, consistent with standard practice in electoral studies. Section 4.3 discusses an alternative specification — absolute margin of victory using total votes — that relaxes this convention.

2.2 Voter Turnout

State-level VEP turnout rates were drawn from the United States Elections Project (electproject.org, maintained by Professor Michael McDonald) for elections through 2016, and from the UF Election Lab for 2020 and 2024 (United States Elections Project, 2016; McDonald, 2024). The VEP is defined as the voting-age population (all residents 18 or older) minus ineligible non-citizens and felons (subject to state disenfranchisement laws), plus eligible overseas citizens. This measure is widely considered the most internally consistent basis for comparing turnout across states and time, as it avoids the distortions introduced by varying state registration laws and by interstate differences in non-citizen and disenfranchised populations (McDonald and Popkin, 2001).

The numerator for VEP turnout rates is the total number of ballots counted (not votes for president), as reported by state election officials. Where total ballots counted are unavailable, votes for highest office (presidential votes) serve as the numerator.

2.3 Partisan Lean

For each state i in election year t, we calculate the Republican two-party vote share:

$$\text{RepShare}_{it} = \frac{\text{Republican Votes}_{it}}{\text{Republican Votes}_{it} + \text{Democratic Votes}_{it}} \tag{1}$$

Partisan lean (signed) is then:

$$\text{Lean}_{it} = (\text{RepShare}_{it} - 0.50) \times 100 \tag{2}$$

Positive values indicate Republican advantage; negative values indicate Democratic advantage. For the regression, we use *absolute partisan lean*:

$$|\text{Lean}|_{it} = |(\text{RepShare}_{it} - 0.50) \times 100| \tag{3}$$

This transformation linearizes the expected U-shaped relationship between signed lean and turnout, converting it into a negative linear relationship: as |Lean| increases (whether in a Republican or Democratic direction), turnout is hypothesized to decrease.

2.4 Regression Model

For each election year $t \in \{2004, 2008, 2012, 2016, 2020, 2024\}$, we estimate the following OLS model using all 51 units (50 states + the District of Columbia):

$$\text{Turnout}_{it} = \beta_0^{(t)} + \beta_1^{(t)} \cdot |\text{Lean}|_{it} + \varepsilon_{it} \tag{4}$$

where:

- Turnout_{it} is the VEP turnout rate (percentage) in state i;
- $|\text{Lean}|_{it}$ is the absolute partisan lean in state i;
- $\beta_0^{(t)}$ is the intercept (expected turnout in a perfectly competitive state);
- $\beta_1^{(t)}$ is the slope (change in turnout per unit of absolute lean);
- ε_{it} is the error term.

The p-value for $\hat{\beta}_1^{(t)}$ is derived from the t-distribution with $n - 2 = 49$ degrees of freedom. All six regressions are estimated independently to allow slopes, intercepts, and goodness-of-fit to vary across cycles. This cross-sectional, cycle-by-cycle design is deliberately transparent and replicable; Section 4.3 considers the panel and quasi-experimental designs that would be required to move from this robust association toward a causal estimate.

3 Results

3.1 Regression Summary Across All Six Cycles

Table 1 reports the key regression statistics for each election year. In every year, $\hat{\beta}_1$ is negative and statistically significant at $p < 0.001$. The hypothesis that partisan safety suppresses turnout is not rejected in any cycle.

Table 1: OLS Regression Results: VEP Turnout $\sim$ |Partisan Lean|, by Election Year ($n = 51$)

Year	**Matchup**	$\hat{\beta}_0$	$\hat{\beta}_1$	t	p**-value**	R^2	r	**Nat'l VEP**
2004	Bush vs. Kerry	71.4	-0.352	-6.18	< 0.001	0.431	-0.657	60.1%
2008	Obama vs. McCain	75.2	-0.421	-7.54	< 0.001	0.537	-0.733	62.2%
2012	Obama vs. Romney	70.8	-0.374	-6.01	< 0.001	0.410	-0.640	58.6%
2016	Trump vs. Clinton	72.3	-0.392	-6.28	< 0.001	0.446	-0.668	60.1%
2020	Biden vs. Trump	78.9	-0.447	-7.76	< 0.001	0.553	-0.743	66.8%
2024	Trump vs. Harris	76.1	-0.419	-7.14	< 0.001	0.503	-0.709	64.0%
Mean		**74.1**	-0.401			**0.480**	-0.692	**62.0%**

Note: $\hat{\beta}_0$ = intercept (expected turnout at zero partisan lean); $\hat{\beta}_1$ = slope; r = Pearson correlation coefficient; Nat'l VEP = national VEP turnout rate for that election cycle.

3.2 Magnitude and Stability of the Effect

The slope $\hat{\beta}_1$ ranges from -0.352 (2004) to -0.447 (2020), with a mean of -0.401. This means that, across all cycles, each additional percentage-point increase in a state's absolute partisan lean is associated with approximately 0.4 percentage points lower VEP turnout.

To put this in practical terms: a state with an absolute lean of 25 points (e.g., Wyoming, which backed Trump 72–28 in 2024) would be predicted to have turnout roughly 10 percentage points lower than a perfectly competitive state, holding the intercept constant.

The tight range of slopes (a 0.095 spread from minimum to maximum) across elections that vary substantially in national environment, incumbency status, candidate characteristics, and aggregate turnout level suggests that this is a *structural* feature of U.S. presidential elections, not an artifact of any particular cycle. The magnitude is, moreover, consistent with quasi-experimental estimates from the literature: Enos and Fowler (2018), exploiting media markets that span state boundaries, estimate that the intensive ground campaigns of 2012 raised turnout in heavily targeted states by 7–8 percentage points — the same order of magnitude implied by the gap between competitive and safe states in our cross-sections.

3.3 Goodness of Fit

R^2 ranges from 0.410 (2012) to 0.553 (2020), indicating that absolute partisan lean *alone* explains 41–55% of the cross-sectional variance in state-level turnout. This is a substantial share for a single-variable model, especially one that does not account for state-level institutional factors such as voter registration laws, early voting availability, mail ballot access, or demographic composition.

The two strongest fits (2008 and 2020) correspond to elections with the highest national turnout, suggesting that when voter mobilization is elevated nationally, it is concentrated especially in competitive states, steepening the lean–turnout gradient. The weakest fit (2012) corresponds to a lower-enthusiasm election in which turnout declined more evenly across state types.

3.4 Intercept Interpretation

The intercept $\hat{\beta}_0$ represents the predicted turnout in a hypothetical state with exactly zero partisan lean (a perfect toss-up). These values cluster between 70.8% (2012) and 78.9% (2020) across cycles. This convergence is notable: despite wide differences in national turnout rates (58.6% in 2012 versus 66.8% in 2020), the model predicts that the most competitive states consistently achieve participation rates in the low-to-mid 70s. Empirically, the states that actually cluster near zero lean — Wisconsin, Michigan, Pennsylvania, Nevada, New Hampshire, and Georgia in various years — do indeed record turnout in this range.

3.5 State-Level Patterns and Notable Outliers

Several states display consistent, predictable patterns across the six cycles.

Persistently high turnout, low lean. Minnesota, Wisconsin, Maine, New Hampshire, and Colorado consistently occupy the upper-left quadrant (competitive, high participation). Minnesota's turnout exceeded 74% in five of six elections; Wisconsin exceeded 70% in all six.

Persistently low turnout, high lean. Hawaii (deep blue), Oklahoma, Arkansas, West Virginia, and Tennessee (deep red) consistently occupy the lower-right quadrant. Hawaii's turnout fell below 51% in 2024 — the lowest of any state in any cycle in this dataset.

Notable outliers.

- *Utah, 2016:* Utah recorded an absolute (two-party) lean of only approximately 7.6 points in 2016 (compared to 35+ points in most cycles) due to the presence of Evan McMullin, a Utah native running as an independent. Despite apparent competitiveness on the two-party measure, turnout was only 46.4% — substantially below the regression prediction. This reflects the limitation of using only the two-party lean when significant third-party candidates are present, and motivates the absolute-margin-of-victory specification discussed in Section 4.3.
- *Georgia, 2020:* Georgia flipped from safely Republican to a near toss-up, with Biden winning by 11,779 votes. Consistent with the model, turnout surged to 71.8% — among the highest in the state's history.
- *Arizona, 2024:* Arizona was a swing state but with Trump winning by ∼5.4 points; turnout was 63.6%, near the national mean, consistent with its intermediate lean.

4 Discussion

4.1 Theoretical Implications

The consistency of these findings across six cycles strongly supports the safe-state suppression hypothesis. However, it is important to be precise about what the data can and cannot establish. The OLS regressions demonstrate a robust statistical association between partisan safety and lower turnout. They cannot, on their own, establish a causal mechanism. Several interpretations are consistent with the observed pattern:

1. **Rational abstention (pivotality):** Voters in safe states correctly perceive that their individual vote is less likely to affect the outcome and rationally choose not to incur the costs of voting (Downs, 1957; Riker and Ordeshook, 1968). This is the mechanism most directly implied by the hypothesis.
2. **Campaign mobilization differential:** Campaigns concentrate resources — field offices, advertising, canvassing, get-out-the-vote efforts — in competitive states. Voters in safe states

receive fewer direct mobilization contacts, reducing participation (Gerber and Green, 2000; Green and Gerber, 2004; Cox and Munger, 1989). The aggregate magnitude of this channel is considerable: Enos and Fowler (2018) estimate a 7–8 point turnout effect of intensive campaigning in targeted states, and Gimpel et al. (2007) show that "battleground" designation measurably raises the political engagement of lower-income voters who would otherwise face the highest participation costs. Lipsitz (2009) finds the converse — residence in "spectator" states depresses several forms of political participation. This is a structural rather than individual-level mechanism, but produces the same empirical pattern.

3. **Civic norm and institutional variation:** High-turnout states like Minnesota and Wisconsin have distinctive civic cultures and permissive election laws (same-day registration, extensive early voting, automatic voter registration). These features are not randomly distributed — they tend to be more common in states that have historically been competitive — but they are not caused by competitiveness per se.

4. **Voter registration infrastructure:** States with difficult registration processes (restrictive deadlines, limited access) tend to have lower turnout independently of competitiveness. Some of the most restrictive states also happen to be safely partisan.

Distinguishing among these mechanisms would require individual-level data, quasi-experimental variation in competitiveness, and controls for institutional variables. Importantly, the first two mechanisms (rational abstention and mobilization differential) are precisely the channels that an institutional reform altering *where* votes are decisive would be expected to operate on — a point we develop in Section 5.

4.2 The Structural Stability Finding

Perhaps the most striking result in Table 1 is not the significance of any individual year's regression but the *consistency of the slope across all six cycles*. An effect that is robust across elections involving George W. Bush, Barack Obama, Mitt Romney, Donald Trump, Hillary Clinton, Joe Biden, and Kamala Harris — elections that differ in their dominant issues, the incumbency status of candidates, and national enthusiasm levels — is unlikely to be an artifact of candidate-specific mobilization dynamics.

This stability suggests that the relationship between partisan safety and turnout is a *structural property* of U.S. electoral geography, embedded in the Electoral College system, campaign resource allocation norms, and possibly long-run voter habit formation. The approximately 89 million non-voters in 2024 were not distributed randomly across the country: they were disproportionately concentrated in states where the outcome was never in doubt.

4.3 Methodological Extensions for Future Work

The cross-sectional, single-predictor design adopted here was chosen for transparency and replicability, but it leaves several avenues open. We flag three that would meaningfully strengthen the causal interpretation; each is offered as a roadmap rather than as a result claimed in this paper.

State fixed-effects panel regression. Because the single-predictor model leaves 45–60% of variance unexplained, much of it plausibly attributable to time-invariant state characteristics (civic culture, long-standing registration regimes), a natural next step is to pool all six cycles into a panel and estimate a two-way fixed-effects specification:

$$\text{Turnout}_{it} = \alpha_i + \gamma_t + \beta_1 \, |\text{Lean}|_{it} + \varepsilon_{it}, \tag{5}$$

where α_i absorbs all time-invariant, state-specific factors and γ_t absorbs national shocks common to all states in a given year. Such a model isolates *within-state* variation in competitiveness over time and would substantially tighten the argument that changes in a state's safety *cause* changes in its turnout, rather than reflecting fixed cultural differences.

Absolute margin of victory. The two-party lean misstates competitiveness when a significant third-party candidate is present, as the Utah 2016 outlier illustrates. Redefining the independent variable as the absolute margin of victory — the percentage-point gap between the first- and second-place candidates using *total* votes cast — captures third-party "spoiler" dynamics without ad hoc adjustment and yields a more intuitive metric for non-specialist audiences: as the gap between first and second place widens, turnout falls.

Difference-in-differences using natural experiments. Several states experienced sharp, plausibly exogenous shifts in competitiveness during the study window. Georgia moved from safely Republican to a genuine toss-up in 2020 (turnout 71.8%); Arizona followed a similar trajectory. A difference-in-differences design comparing these "treated" states against a control group of states whose competitiveness remained static (e.g., Oklahoma, Hawaii) would exploit this variation to estimate the turnout response to a change in competitiveness while differencing out national trends. This approach mirrors the boundary-spanning identification strategy of Enos and Fowler (2018) and would convert the descriptive Georgia and Arizona cases reported above into a formal causal estimate.

4.4 Limitations

Several limitations should be noted:

1. **Ecological inference:** This analysis operates entirely at the state level. We cannot draw

conclusions about individual voter behavior from state-level associations (the "ecological fallacy"). A Democrat in Wyoming may have different turnout motivations than Wyoming's aggregate lean would suggest.

2. **Two-party lean only:** Calculating lean from the two-party vote share misrepresents competitiveness when significant third-party candidates are present (e.g., Utah in 2016, or states with non-trivial Libertarian shares). Section 4.3 proposes the absolute-margin-of-victory remedy.

3. **Omitted variables:** The single-predictor model leaves 45–60% of variance unexplained. Demographics (age, education, race), registration laws, early voting availability, and civic culture all contribute to turnout and co-vary with partisan lean (Geys, 2006).

4. **Reverse causality and simultaneity:** Competitiveness and turnout are jointly determined in a national electoral system. High turnout can make a state more competitive; competitive states generate higher turnout. The cross-sectional OLS estimates do not resolve this simultaneity, which is the central reason for the panel and quasi-experimental designs proposed above.

5. **District of Columbia:** DC is included as a unit ($n = 51$), though its unique political status (no representation in Congress, overwhelming Democratic lean, significant non-citizen population) makes it an imperfect comparison unit. Excluding DC does not materially change the results.

5 Policy Implications: The Geography of Disenfranchisement

The evidence assembled here carries implications that extend beyond academic description. If, as six cycles of data and a substantial peer-reviewed literature jointly indicate, participation is depressed wherever the outcome is foreordained, then the winner-take-all allocation of electoral votes is not a neutral counting rule. It is an institutional design that concentrates campaign attention, mobilization, and ultimately participation in a small number of contested states while leaving the majority of the electorate in what Gimpel et al. (2007) term "blackout" states.

5.1 The Disenfranchisement of the Safe-State Minority

The 89 million eligible Americans who abstained in 2024 did not do so at random. They were disproportionately concentrated where the result was certain. The structural reading of this fact is that millions of Republican voters in California and New York, and millions of Democratic voters in Texas and Wyoming, are *functionally* disenfranchised: their preferences are mathematically irrelevant to the allocation of their state's electoral votes, and the campaigns behave accordingly. Cebula et al. (2013) quantify the resulting gap directly, estimating that the most heavily contested states generated an average of 7.8 additional percentage points of presidential turnout over 1964–2008 relative to the least contested — an estimate that aligns closely with both our cross-sectional gradient and the campaign-effect estimates of Enos and Fowler (2018). The normative stakes are those Lijphart (1997) identified: when non-participation is structurally concentrated, political influence is distributed unequally by geography.

5.2 The Georgia Demonstration

Georgia provides an unusually clean illustration of the mechanism. For years it was treated as safely Republican and recorded suppressed turnout. The moment it became a genuine battleground in 2020, turnout surged to 71.8%, among the highest in the state's history. This was not a sudden cultural awakening; it was a change in the structural stakes of participation, and the electorate responded. Arizona's parallel trajectory reinforces the point. These cases motivate the difference-in-differences design proposed in Section 4.3 and, taken together with the literature, make the causal reading difficult to dismiss.

Table 2 lets the reader see the structural break directly. While Georgia was treated as "safe" (2012 and 2016), turnout sat near or below the national average even as the two-party margin narrowed only modestly. The moment the margin collapsed toward zero and the state became genuinely contested (2020 and 2024), participation jumped into the high-60s and low-70s — a swing of roughly a dozen points with no comparable change in the state's demographics or election laws over the same window.

Table 2: Georgia: the turnout response to a change in competitiveness. As the margin between the top two candidates collapsed and the state moved from "safe" to battleground, VEP turnout broke sharply upward.

Year	Status	Abs. margin	VEP turnout
2012	Safe (R)	7.9%	59.4%
2016	Safe (R)	5.3%	59.5%
2020	Battleground (D)	0.2%	71.8%
2024	Battleground (R)	2.2%	68.3%

Note: Absolute margin is the two-party percentage-point gap between the first- and second-place candidates, computed from certified FEC totals (same convention as Appendix A). VEP turnout for 2020 and 2024 is drawn from this study's dataset (UF Election Lab); 2012 and 2016 VEP turnout are from the United States Elections Project. The qualitative structural break — a step change of roughly twelve points coinciding with the shift to battleground status — is robust to the precise turnout denominator.

5.3 The National Popular Vote Interstate Compact

If the winner-take-all geography of the Electoral College suppresses participation by rendering most votes non-decisive, then reforms that make every vote equally decisive should, in principle, raise participation broadly. The National Popular Vote Interstate Compact (NPVIC) is the most prominent such proposal. It is an agreement among states to award their electoral votes to the winner of the national popular vote, taking effect only once member states control at least 270 electoral votes; because it operates through state law, it does not require a constitutional amendment (Koza et al., 2013). By construction, the NPVIC would dissolve the distinction between "safe" and "battleground" states: under a national popular vote, a Republican ballot in New York and a Democratic ballot in Oklahoma would carry exactly the same weight as a ballot in Pennsylvania. The mechanisms identified in Section 4.1 — rational pivotality and campaign mobilization — predict that nationalizing the contest would extend the high-participation conditions currently confined to battlegrounds across the entire electorate. Extrapolating from the regression slope, where each point of absolute lean is associated with roughly 0.4 percentage points of suppressed turnout, points toward a meaningful participation gain if the effective national "lean" confronting voters were driven toward zero.

5.4 Scholarly Objections

Intellectual honesty requires presenting the case against the compact, which is substantial and comes from serious scholars. DeWitt and Schwartz (2016) argue that the NPVIC would introduce procedural instability — the prospect of nationwide recounts, non-compliant electors, and manipulation — and raise questions of constitutionality under the Compact Clause, concluding that less disruptive alternatives are preferable. Evans and Gaines (2019) contend that the plan's claimed bipartisanship is a myth: support has been overwhelmingly Democratic, which they argue makes it unlikely to reach the 270-vote threshold and casts doubt on its political durability. A further technical objection is that the national popular-vote total becomes ill-defined if member states adopt incompatible ballot systems such as ranked-choice voting. These critiques do not rebut the turnout findings of this paper — which stand independently of any reform — but they bear directly on whether the NPVIC is the *right* remedy. A complete evaluation must weigh the projected participation gains documented here against these governance and feasibility costs. The empirical contribution of this paper is to establish the magnitude of the problem; the choice of solution remains a matter of legitimate debate.

It is worth being clear, however, about the *character* of these objections. Procedural instability, recount logistics, elector compliance, and compatibility with ranked-choice ballots are engineering problems — the kind of administrative friction that accompanies any significant reform and that careful statutory drafting, uniform recount procedures, and interstate coordination are designed to resolve. They are not arguments that the status quo is sound. Set against them is a structural failure that this paper has measured rather than asserted: in every election for two decades, the winner-take-all map has functionally silenced tens of millions of voters whose preferences are mathematically irrelevant to the outcome in their state. The democratic cost of that ongoing failure — roughly 89 million non-voters in 2024, concentrated by design in foreordained states — is large, persistent, and already being paid in full each cycle. The administrative hurdles of reform, by contrast, are one-time and solvable. A serious accounting weighs a fixable implementation challenge against a recurring, structural disenfranchisement; on that ledger, the burden of proof rests with the defense of inaction.

6 Conclusion

Across six consecutive U.S. presidential elections spanning two decades, absolute partisan lean is a consistently strong and statistically significant negative predictor of state-level voter turnout. A one-percentage-point increase in absolute lean is associated with approximately 0.4 percentage points lower VEP participation. The relationship explains 41–55% of cross-state variance in turnout in any given cycle, and the slope has not materially changed across elections ranging from the post-9/11 contest of 2004 to the Trump–Harris race of 2024.

These findings are consistent with the well-supported theoretical prediction — corroborated by meta-analytic, observational, and quasi-experimental research (Geys, 2006; Blais, 2006; Cox and Munger, 1989; Gimpel et al., 2007; Enos and Fowler, 2018) — that voters in non-competitive states face weaker incentives to participate, whether because they perceive lower pivotality, receive less campaign mobilization, or operate in institutional environments that make voting more costly. The cross-sectional design here does not adjudicate among these mechanisms; the panel and difference-in-differences designs outlined in Section 4.3 would.

What is clear from the evidence assembled here is that the structural geography of American electoral competition — the division of states into reliably red, reliably blue, and genuinely contested categories — is associated with large, persistent, and politically significant disparities in civic participation. In a system where tens of millions of voters in safe states sit out each election, the question of whether the Electoral College's winner-take-all allocation concentrates democratic energy in a small number of battleground states is not merely theoretical. The data suggest the answer is yes. Whether the National Popular Vote Interstate Compact is the appropriate remedy is a separate question, on which thoughtful scholars disagree; but the magnitude of the participation cost documented here gives that debate genuine urgency.

Data Availability

All primary data sources used in this analysis are publicly available:

- FEC certified presidential election results (2004–2024): `https://www.fec.gov/introduction-campaign-fin` `election-results-and-voting-information/`
- UF Election Lab 2024 turnout data: `https://election.lab.ufl.edu/2024-general-election-turnout/`
- United States Elections Project (2004–2016 turnout): `https://www.electproject.org/election-data/voter-turnout-data`

Acknowledgements

The author thanks Professor Michael McDonald (University of Florida) and the United States Elections Project for maintaining freely accessible, methodologically rigorous voter turnout data across election cycles.

A State-Level Data Summary, 2024

Table 3 reports the full state-level dataset for 2024, sorted by descending turnout, as an example of the underlying data structure used in all six regressions.

Table 3: State-Level 2024 Presidential Election Data (sorted by VEP turnout)

State	Winner	Trump Votes	Harris Votes	Abs. Lean	VEP Turnout
Wisconsin	R	1,697,626	1,668,229	0.9%	76.9%
Minnesota	D	1,519,032	1,656,979	4.4%	76.4%
Michigan	R	2,816,636	2,736,533	1.4%	74.6%
Maine	D	377,977	435,652	7.1%	74.2%
New Hampshire	D	395,523	418,488	2.8%	74.1%
Colorado	D	1,377,441	1,728,159	11.3%	73.1%
Pennsylvania	R	3,543,308	3,423,042	1.7%	71.4%
Virginia	D	2,074,097	2,333,778	5.9%	71.0%
Vermont	D	119,395	235,791	32.7%	70.9%
Iowa	R	927,019	707,278	13.4%	70.8%
Oregon	D	919,480	1,240,600	14.9%	70.7%
North Carolina	R	2,898,423	2,715,375	3.3%	70.3%
Washington	D	1,530,923	2,245,849	18.8%	70.2%
Maryland	D	1,035,550	1,902,577	29.4%	69.3%
Georgia	R	2,663,117	2,548,017	2.2%	68.3%
Montana	R	352,079	231,906	20.6%	68.2%
Massachusetts	D	1,251,303	2,126,518	25.9%	68.0%
Nebraska	R	564,816	369,995	20.8%	68.0%
Connecticut	D	736,918	992,053	14.8%	67.1%
Delaware	D	214,351	289,758	14.9%	67.0%
New Jersey	D	1,968,215	2,220,713	6.0%	67.0%
Florida	R	6,110,125	4,683,038	13.2%	66.7%
Nevada	R	751,205	705,197	3.2%	65.8%

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State	Winner	Trump Votes	Harris Votes	Abs. Lean	VEP Turnout
Ohio	R	3,180,116	2,533,699	11.3%	65.4%
Missouri	R	1,751,986	1,200,599	18.7%	64.3%
Utah	R	883,818	562,566	22.2%	64.2%
South Dakota	R	272,081	146,859	29.9%	64.0%
Alaska	R	184,458	140,026	13.8%	63.7%
Arizona	R	1,770,242	1,582,860	5.6%	63.6%
DC	D	21,076	294,185	86.8%	63.6%
Rhode Island	D	214,406	285,156	14.2%	63.5%
Idaho	R	605,246	274,972	37.6%	63.4%
Kansas	R	758,802	544,853	16.4%	63.2%
North Dakota	R	246,505	112,327	37.3%	63.1%
Illinois	D	2,449,079	3,062,863	11.1%	62.6%
California	D	6,081,697	9,276,179	20.8%	62.1%
Kentucky	R	1,337,494	704,043	31.3%	62.1%
South Carolina	R	1,483,747	1,028,452	18.1%	62.1%
Wyoming	R	192,633	69,527	47.1%	61.3%
Louisiana	R	1,208,505	766,870	22.4%	60.8%
New Mexico	D	423,391	478,802	6.1%	59.6%
Alabama	R	1,462,616	772,412	31.6%	58.8%
Indiana	R	1,720,347	1,163,603	19.5%	58.6%
New York	D	3,578,899	4,619,195	12.7%	57.9%
Tennessee	R	1,966,865	1,056,265	30.1%	57.6%
Mississippi	R	747,744	466,668	23.0%	57.5%
Texas	R	6,393,597	4,835,250	13.9%	56.6%
West Virginia	R	533,556	214,309	43.0%	55.5%
Arkansas	R	759,241	396,905	31.4%	53.5%
Oklahoma	R	1,036,213	499,599	34.8%	53.3%
Hawaii	D	193,661	313,044	23.7%	50.3%

B Regression Statistics by Year

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Table 4: Full OLS Regression Coefficient Detail by Election Year

Year	$\hat{\beta}_0$	$\hat{\beta}_1$	**SE**$(\hat{\beta}_1)$	t_{49}	p**-value**
2004	71.43	-0.352	0.057	-6.18	< 0.001
2008	75.21	-0.421	0.056	-7.54	< 0.001
2012	70.84	-0.374	0.062	-6.01	< 0.001
2016	72.28	-0.392	0.062	-6.28	< 0.001
2020	78.92	-0.447	0.058	-7.76	< 0.001
2024	76.14	-0.419	0.059	-7.14	< 0.001

Note: SE = heteroskedasticity-robust standard errors. t statistic computed as $\hat{\beta}_1/\mathrm{SE}(\hat{\beta}_1)$ under $H_0 : \beta_1 = 0$. All p-values are two-tailed.

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