

Assessing the Current North Carolina Congressional Districting Plan

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1 Introduction

My name is Simon Jackman. Since April 2016 I have been a Professor of Political Science and Chief Executive Officer of the United States Studies Centre at the University of Sydney. Between 1996 and 2016 I was a professor of political science at Stanford University.

I have been asked by counsel representing the plaintiffs in *League of Women Voters of North Carolina v. Rucho* to analyze relevant data and provide expert opinions. More specifically, I have been asked:

- to explain a summary measure of a districting plan known as “the efficiency gap” (or *EG*) ([Stephanopoulos and McGhee, 2015](#)), what it measures, how it is calculated, and to assess how well it measures partisan gerrymandering;
- to determine if the current North Carolina Congressional districting plan is asymmetric with respect to any particular party, and if so, how large and durable is any discriminatory effect;
- to compare the efficiency gap to extant summary measures of districting plans such as partisan bias;
- to analyze data from Congressional elections in recent decades, so as to assess the properties of the efficiency gap and to identify plans with high values of the efficiency gap;
- to assess the robustness of the efficiency gap to plausible perturbations in Congressional election outcomes;
- to suggest a threshold or other measure that can be used to determine if a districting plan warrants judicial scrutiny;
- to describe how the efficiency gap for the North Carolina Congressional districting plan compares to the values of the efficiency gap observed in Congressional elections in recent decades elsewhere in the United States; and
- to describe where the efficiency gap for the current North Carolina Congressional districting plan lies relative to the threshold for determining if a districting plan ought to attract judicial scrutiny.

My opinions are based on the knowledge I have amassed over my education, training and experience, and follow from statistical analysis of the following data:

- a large, canonical data set on candidacies and results in [Congressional elections, 1898-1992](#), collected and published by Professor Gary King (Harvard University);
- more recent Congressional elections data from a data file maintained and collected by *Congressional Quarterly*; and
- presidential election returns aggregated to Congressional districts, a data collection maintained by Professor Gary Jacobson (University of California, Los Angeles) that has canonical status in the political science profession.

These data collections are augmented with data collected from the Associated Press, the Cook Political Report and Daily Kos for the 2016 Congressional elections.

My research assistant, Brad Spahn, a doctoral candidate in Political Science at Stanford University, helped with the initial acquisition and preparation of these data files.

A summary of my findings appears in section 3, below. Some highlights are presented in capsule form, below.

1.1 Highlighted findings

1. **Partisan gerrymandering and wasted votes.** In two-party, single-member district electoral systems, a partisan gerrymander operates by effectively “wasting” more votes cast for one party than for the other. Wasted votes are votes for a party in excess of what the party needed to win a given district or votes cast for a party in districts that the party does not win.
2. **The efficiency gap (EG)** is a relative, wasted vote measure, the ratio of one party’s wasted vote rate to the other party’s wasted vote rate.
3. In 2012, Democratic candidates for Congress won 50.9% of the two-party vote for Congress in North Carolina; they won 4 out of the state’s 13 seats, or 30.8%. In 2014, Democratic candidates for Congress won 46.2% of the

two-party vote for Congress (95% CI 45.4% to 47.1%, reflecting uncertainty stemming from the imputations for missing data). Democrats won 3 out of the 13 seats (23.1%). In 2016, Democratic candidates for Congress won 46.7% of the two-party vote for Congress. They again won 3 out of the 13 seats. The efficiency gaps associated with each of these elections are large: -.214 in 2012, -.211 in 2014 (95% CI -.229 to -.195) and -.194 in 2016.

4. These large, negative estimates of the efficiency gap — and the large disparities between vote shares and seat shares in North Carolina Congressional elections — are manifestations of the same phenomenon: a systematic advantage for Republican candidates in the districting plans used in these elections in North Carolina.
5. The negative *EG* estimates generated in these last three elections in North Carolina are unusual relative to North Carolina’s political history (see Figure 23), and when compared with efficiency gap scores from Congressional elections over 40 years and many states (see Figure 24). In particular, the 2012 *EG* estimate of -.214 for North Carolina is:
 - the largest *EG* estimate North Carolina has produced over the 44 year period spanned by this analysis (1972-2016);
 - the 12th largest *EG* estimate by magnitude (95% CI 12th to 17th) out of 512 *EG* estimates produced in the analysis;
 - the 5th largest *EG* estimate since 2010, by magnitude (95% CI 4th to 7th); and
 - the 7th largest *EG* estimate indicative of Republican advantage (*EG* estimates with a negative sign, 95% CI 6th to 10th).
6. The 2016 efficiency gap score of -0.194 is slightly smaller than the 2012 and 2014 efficiency gap estimates in North Carolina. The 2016 score is (see Table 4):
 - the 21st largest *EG* estimate by magnitude (95% CI 19th to 26th) out of 512 *EG* estimates produced in the analysis;

- the 10th largest *EG* estimate since 2010, by magnitude (95% CI 9th to 12th); and
 - the single largest efficiency gap produced in 2016, among the 24 states contributing data to my analysis from that year.
7. My analysis strongly suggests that the districting plan adopted prior to the 2012 Congressional election is the driver of pro-Republican change in the efficiency gap in North Carolina, systematically degrading the efficiency with which Democratic votes translate into Democratic seats in that state.
 8. In turn, this change in the efficiency gap in North Carolina accords with a more general pattern of (a) a correlation between partisan control of redistricting and the sign and magnitude of the resulting efficiency gaps; and (b) more plans being drawn under Republican control in recent decades.
 9. Analysis of the trajectories of efficiency gaps over the lives of districting plans strongly suggests that when a districting plan has an initial value as large and as negative as the one observed in North Carolina in 2016, it will continue to produce large, negative efficiency gaps (if left undisturbed), generating seat tallies for Democrats well below those that would be generated from a neutral districting plan.

2 Qualifications, Publications and Compensation

My Ph.D. is in Political Science, from the University of Rochester, where my graduate training included courses in econometrics and statistics. My curriculum vitae is attached to this report.

All publications that I have authored and published in the past ten years appear in my curriculum vitae. Those publications include peer-reviewed journals such as: *The Journal of Politics*, *Electoral Studies*, *The American Journal of Political Science*, *Legislative Studies Quarterly*, *Election Law Journal*, *Public Opinion Quarterly*, *Journal of Elections*, *Public Opinion and Parties*, and *PS: Political Science and Politics*.

I have published on properties of electoral systems and election administration in *Legislative Studies Quarterly*, the *Australian Journal of Political Science*, the

British Journal of Political Science, and the *Democratic Audit of Australia*. I am a Fellow of the Society for Political Methodology and a member of the American Academy of Arts and Sciences.

I have previously served as an expert witness in *Whitford v. Nichol* (No. 3:15-cv-00421 W.D. Wis.). I am being compensated at a rate of \$350 per hour.

3 Summary

1. **Partisan gerrymandering and wasted votes.** In two-party, single-member district electoral systems, a partisan gerrymander operates by effectively “wasting” more votes cast for one party than for the other. Wasted votes are votes for a party in excess of what the party needed to win a given district or votes cast for a party in districts that the party does not win. Differences in wasted vote rates between political parties measure the extent of partisan gerrymandering.
2. **The efficiency gap (EG)** is a relative, wasted vote measure, the ratio of one party’s wasted vote rate to the other party’s wasted vote rate. The efficiency gap can be computed directly from a given election’s results, without recourse to extensive statistical modeling or assumptions about counterfactual or hypothetical election outcomes, unlike other extant measures of the fairness of an electoral system (e.g., partisan bias).
3. The efficiency gap is an “excess seats” measure, reflecting the nature of a partisan gerrymander. An efficiency gap in favor of one party sees it wasting fewer votes than its opponent, thus translating its votes across the jurisdiction into seats more efficiently than its opponent. This results in the party winning more seats than expected, given its vote share (V) and if wasted vote rates were the same between the parties. $EG = 0$ corresponds to no efficiency gap between the parties, or no partisan difference in wasted vote rates. In this analysis (but without loss of generality) EG is normed such that negative EG values indicate higher wasted vote rates for Democrats relative to Republicans, and positive EG values the converse.
4. A districting plan in which EG is observed to be positive is consistent with

the plan being a pro-Democratic gerrymander. The magnitudes of the *EG* measures speak to the severity of the gerrymander. Conversely, a districting plan with negative values of the efficiency gap is consistent with the plan being a pro-Republican gerrymander.

5. **Operational implications of the efficiency gap.** In states with a relatively small number of Congressional districts, the operational implication of an efficiency gap greater than ± 0.08 is that it is more likely than not that a seat changes hands between the parties, relative to the historical, long-run relationship between statewide vote shares and seat shares in Congressional elections. This threshold of ± 0.08 is the point at which the efficiency gap is more likely to be producing a one seat shift between the parties than no change at all, given the statewide split of the two-party vote for Congress and the historical, long-run relationship between statewide vote shares and seat shares in Congressional elections. For states with larger numbers of Congressional districts (CDs) (above 15 CDs), the efficiency gap threshold corresponding to a one seat deficit/surplus being more likely than not is ± 0.05 . See section 11.
6. **Performance of the efficiency gap in 512 state-level, Congressional elections.** My analysis of this large number of state-level, Congressional elections between 1972 and 2016 examines empirical properties of the efficiency gap. The efficiency gap is estimated with some uncertainty in the presence of uncontested districts, but this source of uncertainty is small relative to differences in the efficiency gap across states and across districting plans.
7. **Stability of the efficiency gap.** Efficiency gaps vary from election to election under the same districting plan. But I present numerous tests in this report demonstrating that the efficiency gap is a valid measure of a districting plan *per se*. For instance, see section 9.2 on within-plan variation in the efficiency gap versus between-plan variation. See also section 12 where I report strong relationships between the *EG* observed in the first election under a districting plan and the remainder-of-plan average *EG* (the average value of the efficiency gap over subsequent elections held under the same districting plan).

8. **Recent decades show more pro-Republican gerrymandering, as measured by the efficiency gap.** Efficiency gap measures in recent decades show a pronounced shift in a negative direction, indicative of an increased prevalence of districting plans favoring Republicans. Among the 10 most pro-Democratic *EG* measures in my analysis, only two were recorded after 2000 (Massachusetts 2010 and 2014). Among the 10 most pro-Republican *EG* measures in my analysis, only two were recorded before 2000 (Washington 1994 and 1996).
9. **The current North Carolina Congressional districting plan** (the “Current North Carolina Plan”). In 2016, Democratic candidates contesting North Carolina’s 13 House of Representatives seats won 2,142,661 votes. Republican candidates won 2,447,326 votes, or 53.3% of the two-party vote. Republican candidates won 10 out of 13 seats, or 76.9% of the seats.
10. Using the definition of wasted votes given above, 1,592,127 votes for Democratic candidates in North Carolina’s 2016 Congressional elections were wasted. 702,868 votes for Republican candidates were wasted. The efficiency gap in North Carolina is accordingly -0.194 (to three digits of precision).
11. I have computed the efficiency gap in 72 state-level, Congressional elections since the 2010 round of redistricting. The magnitude of the 2016 efficiency gap score for North Carolina ranks 10th among these post-2010 efficiency gap scores (95% CI 9 to 12). Among the set of 512 efficiency gap scores computed 1972-2016, the magnitude of the 2016 efficiency gap score for North Carolina ranks 21st (95% CI 19 to 26), placing the 2016 North Carolina at the 4.1 percentile.
12. **An actionable threshold based on the efficiency gap.** Historical analysis of the relationship between the first *EG* measure observed under a new districting plan and the subsequent *EG* measures lets us assess the extent to which that first *EG* estimate is a *reliable* indicator of a *durable* and hence *systematic* feature of the plan. In turn, this lets me assess the *confidence* associated with a range of possible *actionable EG thresholds*.

13. My analysis suggests that a 1st election *EG* greater than .12 in absolute value be used as an actionable threshold, for states with a relatively small number of Congressional districts (7 to 15 CDs; my analysis does not consider states with fewer than 7 CDs). For states with a larger number of Congressional districts, I recommend a 1st election *EG* threshold of .075 in absolute value, at or beyond which the districting plan ought to attract scrutiny.
14. Plans that produce a 1st election *EG* measure in excess of these thresholds almost always go on to produce a remainder-of-plan average *EG* estimate that is consistent with the 1st election *EG* measure, and in ways that are politically consequential (it being more likely than not that a seat has changed hands as a consequence of the plan, see paragraph 5, above). A large, negative *EG* in the 1st election under a plan almost always goes on to produce a negative remainder-of-plan average *EG*; conversely for a large positive *EG* in the 1st election. See section 12.
15. **The Current North Carolina Plan is generating estimates of the efficiency gap far in excess of this proposed threshold.** In 2016, the efficiency gap was -0.194. Note too that in 2012 and 2014, the efficiency gap in North Carolina has also been large, using a districting plan quite similar to the plan in place for the 2016 elections. In 2012 and 2014 the efficiency gap in North Carolina was -0.214 and -0.212. These last three efficiency gap scores from North Carolina are well beyond the conservative threshold suggested by my analysis of efficiency gap measures observed from 1972 to the present (see Section 12.2).
16. Given (a) the historical relationship between 1st election *EG* and remainder-of-plan average *EG* observed since 2000 and (b) the 2016 value of the efficiency gap in North Carolina, I estimate that the remainder-of-plan average efficiency gap for the Current North Carolina plan will be -.12 (95% CI -.23 to -.02). This estimate corresponds to a 98.62% probability that the remainder of the plan will, on average, also produce election results with efficiency gaps disadvantageous to Democrats.
17. That is, the current North Carolina Congressional districting plan is gener-

ating efficiency gap measures that are so large that it is extremely likely that the plan has a systematic enduring Republican advantage in the translation of votes into seats in North Carolina Congressional elections.

A vivid, graphical summary of my analysis appears in Figure 1, showing the average value of the efficiency gap in 136 districting plans, spanning 25 states and 512 Congressional elections from 1972 to 2016. The Current North Carolina Plan has an efficiency gap of -0.194. Details on the interpretation and calculation of the efficiency gap come later in my report, but for now note that negative values of the efficiency gap indicate a districting plan favoring Republicans, while positive values indicate a plan favoring Democrats.

North Carolina's efficiency gap score for 2016 is the largest value of the efficiency gap observed in 24 states contributing 2016 data to my analysis; the next largest comes from my Pennsylvania with an efficiency gap score of -0.189 and South Carolina with an efficiency gap score of -0.177.

The plan used in North Carolina for 2012 and 2014 produced an average efficiency gap score that was the largest plan-average efficiency gap score in the set of states analyzed here over the entire 1972-2016 period.

4 Redistricting plans

A districting plan is an exercise in map drawing, partitioning a jurisdiction into districts, typically required to be contiguous, mutually exclusive and exhaustive regions, and — at least in the contemporary United States — of approximately the same population size. In a single-member, simple plurality (SMSP) electoral system, the highest vote getter in each district is declared the winner of the election. Partisan gerrymandering is the process of drawing districts that favor one party, typically by creating a set of districts that help the party win an excess of seats (districts) relative to its jurisdiction-wide level of support.

What might constitute evidence of partisan gerrymandering? One indication might be a series of elections conducted under the same districting plan in which a party's seat share (S) is unusually large (or small) relative to its vote share (V). Gerrymandering generates this outcome via “packing”, creating a relatively small number of districts that have unusually large proportions of partisans from party

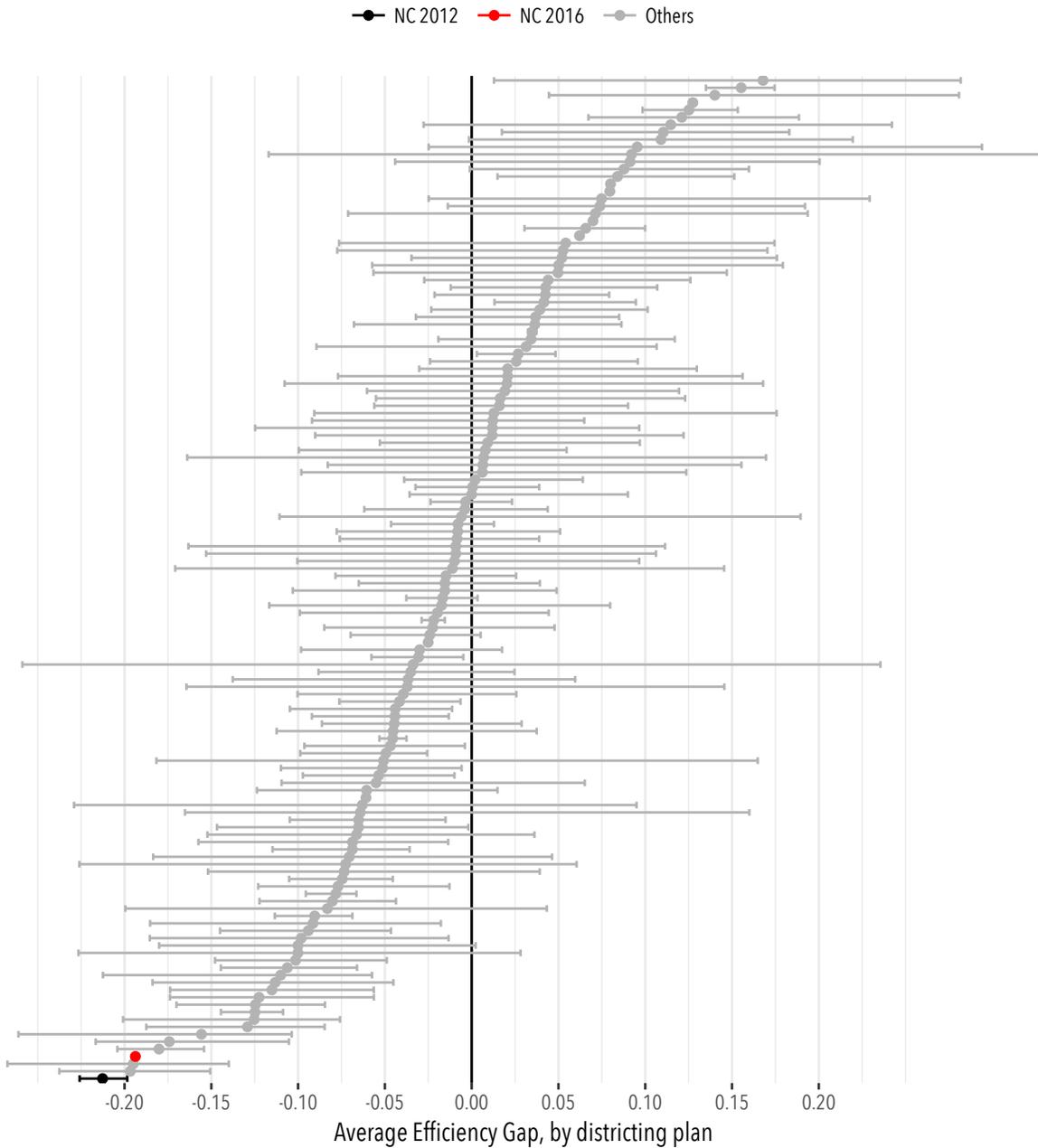


Figure 1: Average efficiency gap score, 136 districting plans, 1972-2016. Plans have been sorted from low average *EG* scores to high. Horizontal lines cover 95% confidence intervals. Negative efficiency gap scores are plans that disadvantage Democrats; positive efficiency gap scores favor Democrats. The North Carolina Plans from this decade are highlighted in red and black.

A relative to party *B*. In other districts in the jurisdiction, party *A* supporters are “cracked” such that they never (or seldom) constitute a majority (or a plurality), making those districts “safe” for party *B*. This districting plan helps ensure that party *B* wins a majority of seats even though party *A* has a majority of support across the jurisdiction; or, at the very least, the districting plan helps ensure that party *B*’s seat share exceeds its vote share in any given election.

It is conventional in political science to say that such a plan allows party *B* to “more efficiently” translate its votes into seats, relative to the way the plan translates party *A*’s votes into seats. This nomenclature is telling and will be revisited in the discussion of the *efficiency gap* measure, below.

Assessing the partisan fairness of a districting plan is fundamentally about assessing a party’s excess (or deficit) in its seat share relative to its vote share. The efficiency gap is such a summary measure. To assess the properties of the efficiency gap, I first review some core concepts in the analysis of districting plans: vote shares, seat shares, and the relationship between the two quantities in single-member districts.

4.1 Seats-Votes Curves

Electoral systems translate parties’ vote shares (V) into seat shares (S). Both V and S are proportions. Plotting the two quantities V and S against one another yields the “seats-votes” curve, a staple in the analysis of electoral systems and districting plans.

Two seats-votes curves are shown in Figure 2, one showing a non-linear relationship between seats and votes typical of single-member district systems,¹ the other showing a linear relationship between seats and votes observed under proportional representation systems.

In pure proportional representation (PR) voting systems, seats-votes curves are 45 degree lines by design, intersecting the (50%, 50%) point: i.e., under PR, $S = V$ and a party that wins 50% of the vote will be allocated 50% of the seats.

Single-member district systems tend to produce steeper and non-linear seats-votes curves. In single-member, simple plurality (SMSP) systems with an ap-

¹The curve labeled “Cube Rule” in Figure 2 is generated assuming that $S/(1-S) = [V/(1-V)]^3$, an approximation for the lack of proportionality typically observed in single-member district systems, though hardly a “rule” or “law.”

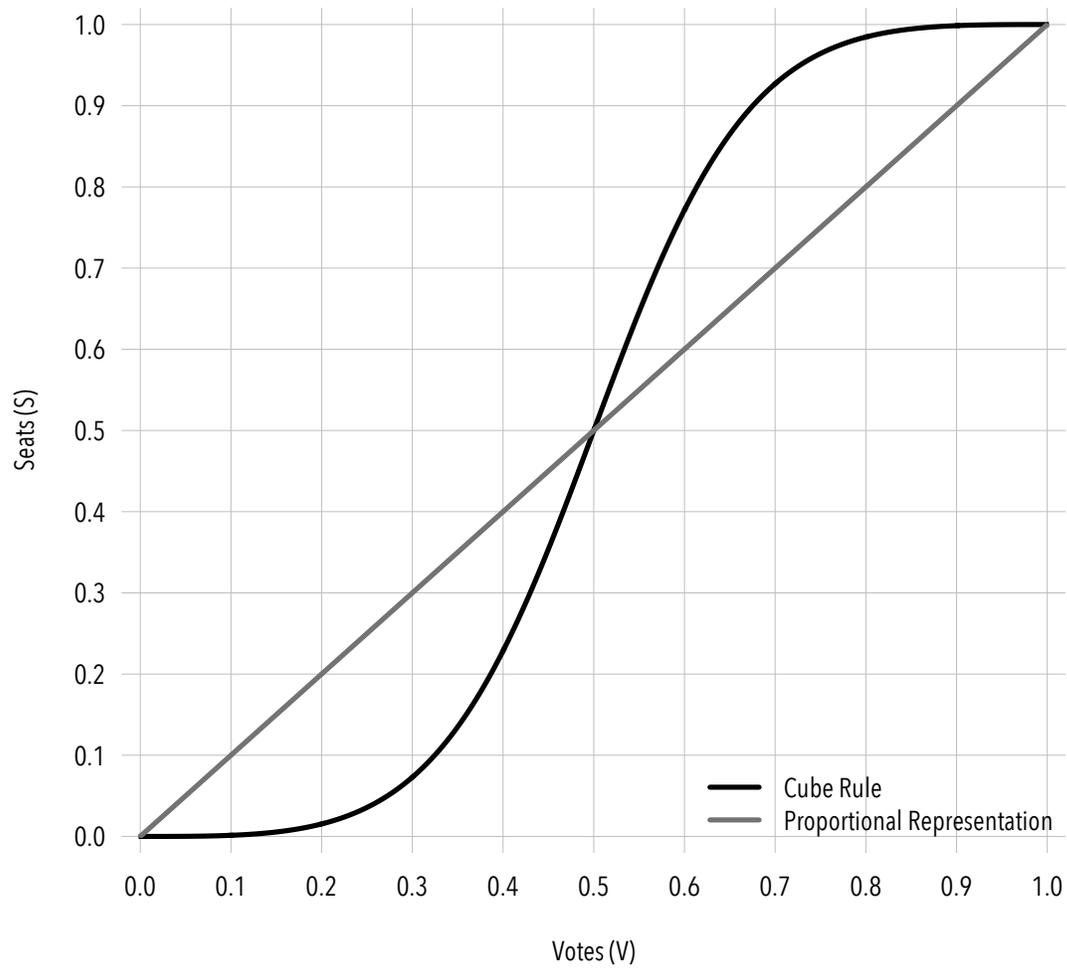


Figure 2: Two Theoretical Seats-Votes Curves

proximately symmetric mix of districts (in terms of partisan leanings) close to the 50-50 point on the horizontal “votes” axis, large changes in seat shares (S) can result from relatively small changes in vote shares (V) at the middle of the distribution of district types.

5 Partisan bias

Both of the hypothetical seats-votes curves in Figure 2 run through the “50-50” point, where $V = .5$ and $S = .5$. An interesting empirical question is whether *actual* seats-votes curves run through this point, or more generally, whether the seats-votes curve is symmetric about $V = .5$.² The vertical offset from the $(.5, .5)$ point for a seats-votes curve is known as *partisan bias*: the extent to which a party’s expected seat share lies above or below 50%, conditional on that party winning 50% of the jurisdiction-wide vote.

Figure 3 shows three seats-votes curves, with the graph clipped to the region $V \in [.4, .6]$ and $S \in [.4, .6]$ so as to emphasize the nature of partisan bias. The blue, positive bias curve “lifts” the seats-votes curve; it crosses $S = .5$ with $V < .5$ and passes through the upper-left quadrant of the graph. That is, with positive bias, a party can win a majority of the seats with *less* than a majority of the jurisdiction-wide or average vote; equivalently, if the party wins $V = .5$, it can expect to win *more* than 50% of the seats. Conversely, with negative bias, the opposite phenomenon occurs: the party can’t expect to win a majority of the seats until it wins more than a majority of the jurisdiction-wide or average vote.

5.1 Multi-year method

With data from multiple elections under the same district plan, partisan bias can be estimated by fitting a seats-votes curve to the observed seat and vote shares, typically via a simple statistical technique such as linear regression; this approach has a long and distinguished lineage in both political science and statistics (e.g., Edgeworth, 1898; Kendall and Stuart, 1950; Tufte, 1973). Niemi and Fett (1986) referred to this method of estimating the partisan bias of an electoral system as

²Symmetry of the seats-vote curve implies that $E(S|V) = 1 - E(S|1 - V)$, where E is the expectation operator, averaging over the uncertainty with respect to S given V .

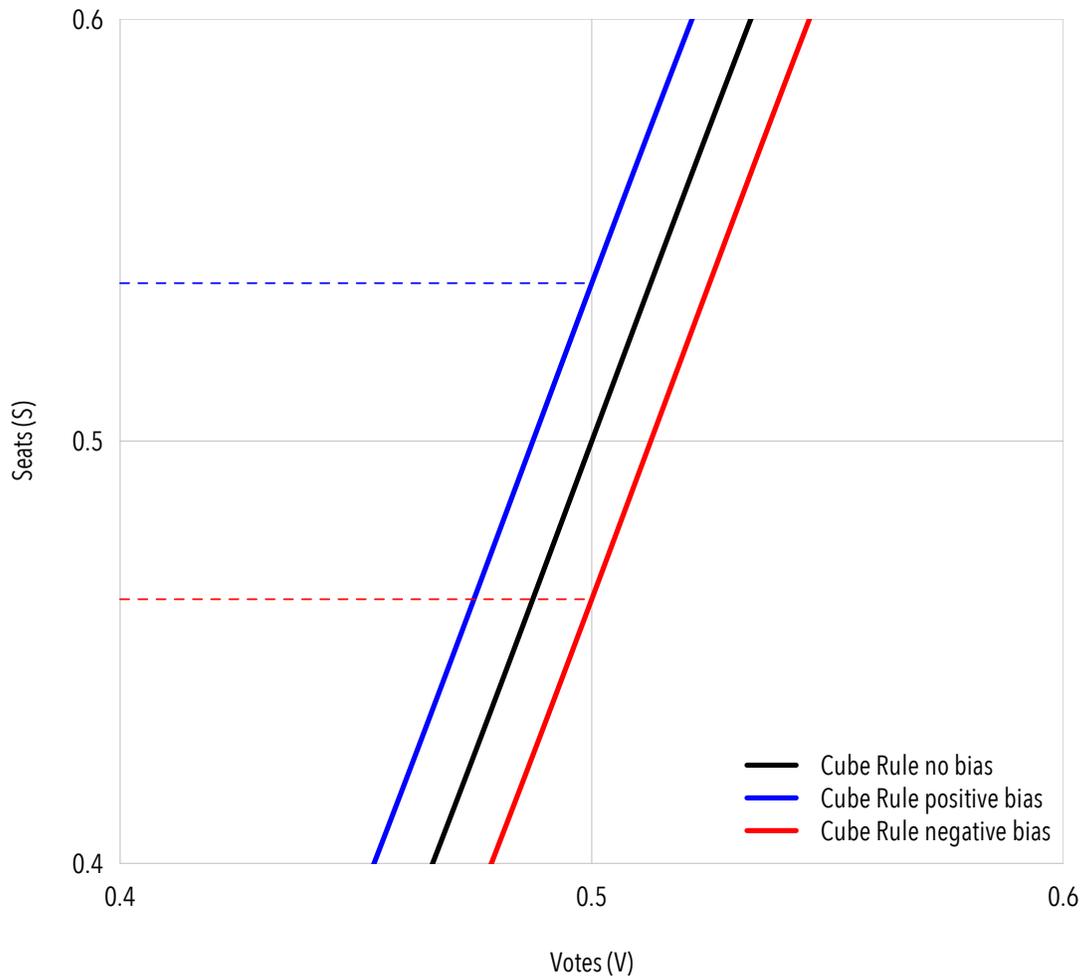


Figure 3: Theoretical seats-votes curves, with different levels of partisan bias. This graph is “zoomed in” on the region $V \in [.4, .6]$ and $S \in [.4, .6]$; the seats-votes “curves” are approximately linear in this region.

the “multi-year” method, reflecting the fact that the underlying data comes from a sequence of elections.

This approach is of limited utility when assessing a new or proposed districting plan. More generally, it is of no great help to insist that a sequence of elections must be conducted under a districting plan before the plan can be assessed. Few plans stay intact long enough to permit reliable analysis in this way: districting plans in the United States might generate as many as five elections between decennial censuses.

One solution is to combine multiple plans and/or jurisdictions in a single analysis, so as to estimate average levels of partisan bias. For instance, [Niemi and Jackman \(1991\)](#) estimated average levels of partisan bias in state legislative districting plans, collecting data spanning multiple decades and multiple states, and grouping districting plans by the partisanship of the plan’s authors (e.g., plans drawn under Republican control, Democratic control, mixed, or independent).

Assessing the properties of a districting plan after a tiny number of elections — or *no* elections — requires some assumptions and/or modeling. A single election yields just a single (V, S) data point, through which no unique seats-vote curve can be fitted. In this case partisan bias can not be estimated without further assumptions. Absent *any* actual elections under the plan, one might examine votes from a previous election, say, with precinct level results re-aggregated to the new districts.

5.2 Uniform swing

One approach—dating back to Sir David Butler’s [\(1974\)](#) pioneering work on British elections—is the uniform partisan swing approach. I introduce some notation as to explain the method.

Let $\mathbf{v} = (v_1, \dots, v_n)'$ be the set of vote shares for party A observed in an election with n districts. Party A wins seat i if $v_i > .5$, assuming just two parties (or defining v as the share of two-party vote). s_i is a binary indicator denoting who wins seat i ; i.e., $s_i = 1$ if $v_i > .5$ and otherwise $s_i = 0$. Party A ’s seat share is $S = \frac{1}{n} \sum_{i=1}^n s_i$. V is the jurisdiction-wide vote share for party A , and if each district had the same number of voters $V = \bar{v} = \frac{1}{n} \sum_{i=1}^n v_i$, the average of the district-level v_i . Districts are never *exactly* equal sized, in which case V is defined

as follows: let t_i be the number of voters in district i , and $V = \sum_{i=1}^n t_i v_i / \sum_{i=1}^n t_i$.

The uniform swing approach perturbs the observed district-level results \mathbf{v} by a constant factor δ , corresponding to a hypothetical amount of *uniform swing* across all districts. For a given δ , let $v_i^* = v_i + \delta$ which in turn generates $V^* = V + \delta$ and an implied seat share S^* . Now let δ vary over a grid of values ranging from $-V$ to $1 - V$; then V^* varies from 0 to 1 and a corresponding value of S^* can also be computed at every grid point. The resulting set of (V^*, S^*) points are then plotted to form a seats-vote curve (actually, a step function). Partisan bias is simply “read off” this set of results, computed as $S^*|(V^* = .5) - .5$.

There is an elegant simplicity to this approach, taking an observed set of district-level vote shares \mathbf{v} and shifting them by the constant δ . The observed distribution of district level vote shares observed in a given election is presumed to hold under *any* election that might be observed under the redistricting plan, save for the shift given by the uniform swing term δ .

5.3 Critiques of partisan bias

Among political scientists, the uniform swing approach was criticized for its determinism. Swings are never exactly uniform across districts. There are many permutations of observed vote shares that generate a statewide vote share of 50% other than simply shifting observed district-level results by a constant factor. A less deterministic approach to assessing partisan bias was developed over a series of papers by Gary King and Andrew Gelman in the early 1990s (e.g., [Gelman and King, 1990](#)). This approach fits a statistical model to district-level vote shares — and, optionally, utilizing available predictors of district-level vote shares — to model the way particular districts might exhibit bigger or smaller swings than a given level of state-wide swing. Perhaps one way to think about the approach is that it is “approximate” uniform swing, with statistical models fit to historical election results to predict and bound variation around a state-wide average swing. The result is a seats-vote curve and an estimate of partisan bias that comes equipped with uncertainty measures, reflecting uncertainty in the way that individual districts might plausibly deviate from the state-wide average swing yet still produce a state-wide average vote of 50%.

The King and Gelman model-based simulation approaches remain the most

sophisticated methods of generating seats-votes curves, extrapolating from as little as one election to estimate a seats-votes curve and hence an estimate of partisan bias. Despite the technical sophistication available in estimating partisan bias, legal debate has centered on a more fundamental issue, the *hypothetical* character of partisan bias itself. Recall that partisan bias is defined as “seats in excess of 50% had the jurisdiction-wide vote split 50-50.” The premise that $V = .5$ is the problem, since this will almost always be a counter-factual or hypothetical scenario. The further V is away from $.5$ in a given election, the counter-factual being contemplated (when assessing the partisan bias of a districting plan) becomes all the more speculative.

As I demonstrate below, the method for assessing gerrymandering proposed here — the efficiency gap — does not rely on counter-factual “re-runs” of an election with V set to 50%. When an actual election does produce a result close to $V = .5$ the efficiency gap and partisan bias tend to coincide. But critically, the efficiency gap approach makes it possible to assess the fairness of districting plans when election results lie some distance away from the benchmark of $V = .5$ contemplated by partisan bias.

6 The Efficiency Gap

The efficiency gap (EG) is also an asymmetry measure. But unlike partisan bias, the interpretation of the efficiency gap is *not* explicitly tied to any counter-factual election outcome. In this way, the efficiency gap provides a way to assess districting plans that is free of the criticisms that have stymied the partisan bias measure.

6.1 Wasted Votes

Stephanopoulos and McGhee (2015) derive the EG measure with the concept of wasted votes. A party only needs $v_i = 50\% + 1$ of the votes cast for two-party candidates to win district i . Anything more are votes that could have been deployed in other districts. Conversely, votes in districts where the party doesn’t win are “wasted,” from the perspective of generating seats: districts in which $v_i < .5$ yield no seats.

6.2 Partisan Asymmetry in Wasted Votes: a hallmark of gerrymandering

Wasted votes get at the core of what partisan gerrymandering is, and how it operates. A gerrymander against party A creates a relatively small number of districts that “lock up” a lot of its votes (“packing” with $v_i > 50\%$) and a larger number of districts that disperse votes through districts won by party B (“cracking” with $v_i < 50\%$). To be sure, both parties are wasting votes. But partisan advantage ensues when *one party is wasting fewer votes than the other*, or, equivalently, more efficiently translating votes into seats. Note also how the efficiency gap measure is closely tied to asymmetry in the distribution of v_i .

Some notation will help make the point more clearly. If $v_i > 50\%$ then party A wins the district and $s_i = 1$; otherwise $s_i = 0$. The efficiency gap is defined by McGhee (2014, 68) as “relative wasted votes” or

$$EG = \frac{W_B}{n} - \frac{W_A}{n}$$

where

$$W_A = \sum_{i=1}^n s_i(v_i - .5) + (1 - s_i)v_i$$

is the sum of wasted vote proportions for party A and

$$W_B = \sum_{i=1}^n (1 - s_i)(.5 - v_i) + s_i(1 - v_i)$$

is the sum of wasted vote proportions for party B and n is the number of districts in the jurisdiction. If $EG > 0$ then party B is wasting more votes than A , or A is translating votes into seats more efficiently than B ; if $EG < 0$ then the converse, party A is wasting more votes than B and B is translating votes into seats more efficiently than A .

7 Congressional elections, 1972-2016

I estimate the efficiency gap in Congressional elections over a large set of states and districting plans, covering the period 1972 to 2016. I begin the analysis in

1972 since districting plans and sequences of elections from 1972 onwards can be reasonably considered to be from the post-malapportionment era. Further, I restrict my analysis to states with seven or more Congressional districts in a given election because the efficiency gap becomes less reliable as the number of districts gets small (Stephanopoulos and McGhee, 2015, 868). I also exclude Louisiana from this analysis because that state uses a unique run-off election system when no candidate receives a majority of the vote in the November election.

For each contested election I recover an estimate of the efficiency gap based on the election results actually observed in that election.

Figure 4 provides a graphical depiction of the elections that satisfy the selection criteria described above.

- Alaska, Arkansas, Connecticut, Delaware, Hawaii, Idaho, Iowa, Kansas, Louisiana, Maine, Mississippi, Montana, Nebraska, Nevada, New Hampshire, New Mexico, North Dakota, Oklahoma, Oregon, Rhode Island, South Dakota, Utah, Vermont, West Virginia and Wyoming all drop out of the analysis entirely, because they have six or fewer congressional districts throughout the period under analysis or because of the use of a run-off system (Louisiana).
- South Carolina, Kentucky, Colorado and Arizona only supply data for part of the timespan because they have too few districts for part of the period.

In summary, the data available for analysis span 7,949 district-level Congressional elections, from 512 state-level Congressional elections across 25 states and 23 election years.

7.1 Grouping elections into redistricting plans

Districting plans remain in place for sequences of elections. An important component of my analysis involves tracking the efficiency gap across a series of elections held under the same districting plan. A key question is how much variation in the *EG* is observed *within* districting plans, versus variation in the *EG between* districting plans.

If the efficiency gap is a feature of a districting plan *per se*, there should be a small amount of within-plan variation in *EG* scores relative to between-plan

variation. To perform this analysis I first group sequences of elections within states by the districting plan in place at the time.

[Stephanopoulos and McGhee \(2015\)](#) provide a unique identifier for the districting plan in place for each Congressional election, which I adopt here.

Figure 4 displays how the elections available for analysis group by districting plan. Districts are typically redrawn after each decennial census; the first election conducted under new district boundaries is often the “2” election (1982, 1992, etc). Occasionally there is just one election under a plan: examples include Virginia, Florida and North Carolina in 2016 and Texas in 2002, 2004 and 2006.

7.2 Uncontested races

Uncontested races are not uncommon: for 14.0% of the districts in this analysis, it isn’t possible to directly compute a two-party vote share, either because the seat was uncontested or not contested by both a Democratic and Republican candidate. This percentage is too large to be ignored.

A graphical summary of the prevalence of uncontested districts appears in Figure 5, showing the percentage of districts without Democratic and Republican vote counts, by election and by state. While most Congressional races are contested in every state, Massachusetts, Alabama and Florida have rates of uncontested seats above 30%.

8 Imputations for Uncontested Races

[Stephanopoulos and McGhee \(2015\)](#) use a statistical model to impute vote shares in uncontested districts. They write:

We strongly discourage analysts from either dropping uncontested races from the computation or treating them as if they produced unanimous support for a party. The former approach eliminates important information about a plan, while the latter assumes that coerced votes accurately reflect political support.

I concur with this advice, utilizing an imputation strategy for uncontested districts with *two* distinct statistical models, predicting Democratic, two-party vote



Figure 4: 512 Congressional elections available for analysis, 1972-2016, by state, grouped by districting plan (horizontal line).

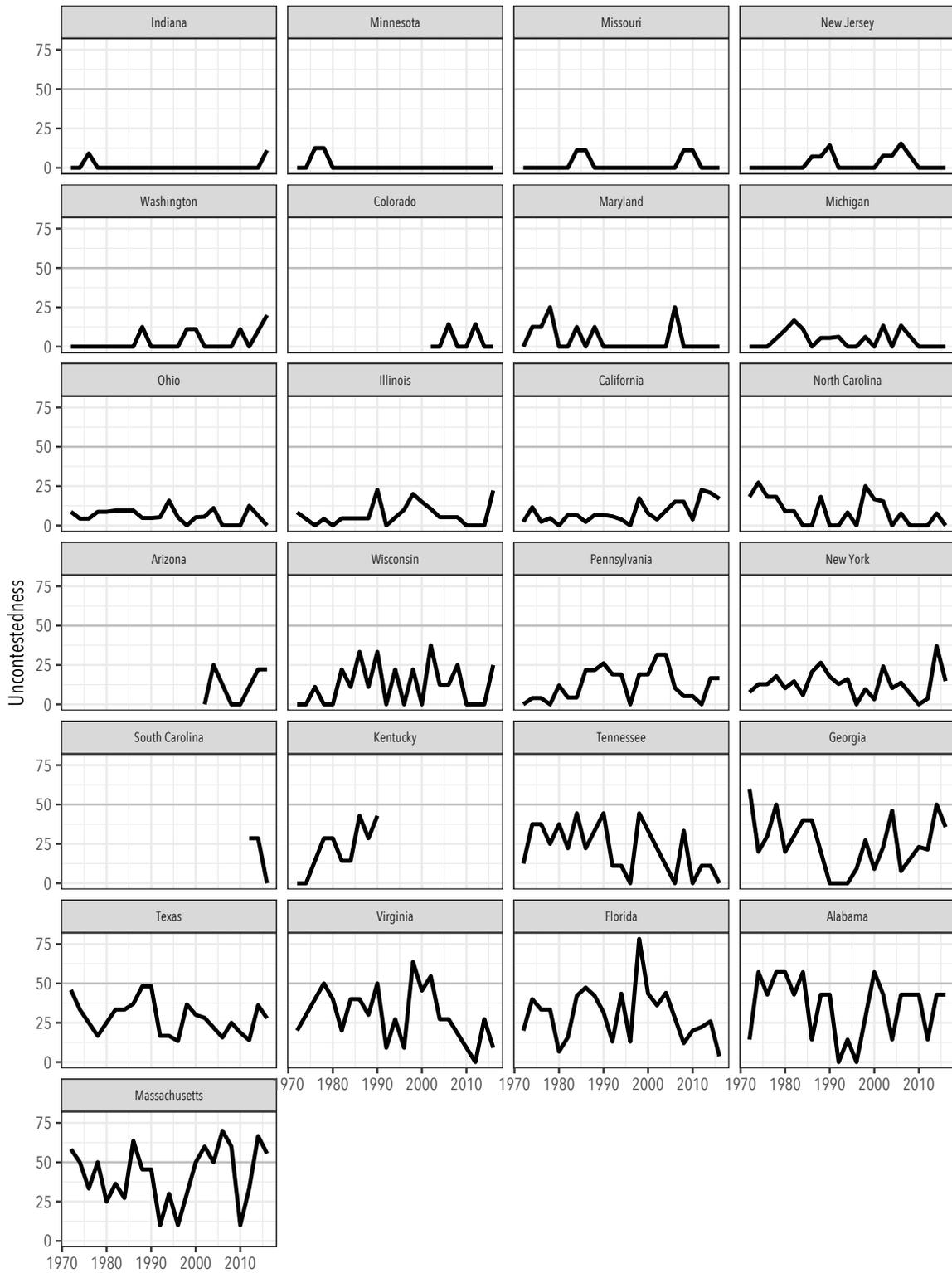


Figure 5: Percentage of districts missing two-party vote shares, by election, in 512 state-level, Congressional elections, 1972-2016. Missing data is always due to districts being uncontested by both major parties.

share in Congressional districts (y_i) and the total turnout one would expect if the district had actually been contested. The key idea here is that I use seats where *both* Congressional vote *and* Presidential vote shares are observed, to build a model where the former is predicted as a function of the latter, where the former (Congressional vote shares) are not observed, but vote shares in Presidential election outcomes are observed. Note that the Presidential election outcomes are always observed, irrespective of whether the Congressional election was a two-party contest.

The model I fit for Congressional vote shares is:

$$y_i \sim N(\mu_i, \sigma_{p(i)}^2) T(l_i, u_i)$$

$$\mu_i = \alpha_{s(i)t(i)q(i)} + \beta_{t(i)} x_i$$

where

- i indexes district level elections,
- y_i is the Democratic share of the two-party vote for Congress in district i ,
- x_i is the Democratic share of the two-party vote for President in district i ,
- s indexes states, with $s(i)$ denoting the state of district i ,
- q indexes the three incumbency classes,
- t indexes elections,
- α are intercepts and β are slopes.

The notation $T(l_i, u_i)$ indicates truncation. For seats won by a Democrat, $l_i = 50, u_i = 100$; for seats won by Republicans, $l_i = 0, u_i = 50$.

Hierarchical models are used for the intercept and slope parameters:

$$\alpha_{stq} \sim N(\mu_q^\alpha, \omega_q^2)$$

$$\mu_q^\alpha \sim N(0, 10^2)$$

$$\omega_q \sim \text{Unif}(0, 1)$$

$$\beta_t \sim N(\mu^\beta, \nu^2)$$

$$\mu^\beta \sim N(1, 2^2)$$

$$\nu \sim \text{Unif}(0, 1)$$

Bayesian inference for this model is performed using Markov chain Monte Carlo (MCMC), implemented in the computer program JAGS (Plummer, 2011).³ Uncontested seats have missing data for y_i ; in these cases I recover samples from the posterior predictive density for the vote shares in these conditional on (a) presidential election outcomes in that seat, x_i ; (b) the relationship between Congressional election outcomes and presidential election outcomes observed in that seat.

The model is run for 25,000 “burn-in” iterations, followed by 150,000 iterations with every 30th iteration saved to yield 5,000 approximately independent draws from the posterior density of the model’s parameters, and from the posterior predictive density of the missing vote shares in uncontested seats.

8.1 Imputing turnout in uncontested elections

I also utilize a model predicting the actual *number* of votes cast in Congressional elections for uncontested seats. My modeling strategy here is reasonably simple, premised on the idea that an excellent predictor of turnout in a given seat is turnout in a temporally proximate *contested* election, if one is available.

To this end, recall that a redistricting plan typically generates five Congressional elections, the “2”, “4”, “6”, “8” and “0” elections. Index these elections by t . I fit regressions of turnout in election t on turnout in all elections $t' \neq t$, for all t , where the data entering these regressions are seats with contested (D vs R) elections in both elections t and t' . This provides us a basis for imputing turnout in a district where the election was uncontested in election t , but contested in election t' . Clearly, if a district is uncontested over the entire redistricting plan, then there is no t' election available to generate a prediction for turnout. I deal with these cases separately, below.

The models I fit have the form

$$v_{it} = h_{t,t'}(v_{it'}) + \alpha_{jt'} + \delta_{st'} + \epsilon_{itt'}$$

³The object of Bayesian statistical inference is to compute and summarize the distribution of unknown quantities (parameters in models, predictions) given observed quantities (data). Markov chain Monte Carlo is a computational-intensive, simulation-based methodology for Bayesian inference; the distribution of unknown quantities is characterized by using computer programs to sample from this distribution an arbitrarily large number of times; see Jackman (2009).

where

- v_{it} is the number of votes recorded for Democratic and Republican Congressional candidates in district i in election t ; this quantity is unobserved when the election does not have a Democratic candidate running against a Republican candidate;
- $v_{it'}$ is the number of votes recorded for Democratic and Republican Congressional candidates in district i in election $t' \neq t$; typically, $t' = t - 1$ or $t' = t + 1$. Different values of t' are utilized so as to find an election under a given districting plan in which $v_{it'}$ is observed for cases where I seek an imputation for an unobserved v_{it} ;
- $h_{t,t'}$ is a thin-plate smoothing spline, fit using the `mgcv` package in R (Wood, 2006);
- the $\alpha_{jtt'}$ are incumbency offsets, where $j \equiv j(it)$ indexes the three incumbency classes and the subscripts t and t' indicate that these parameters are estimated separately for each t -on- t' run of the model; and
- the $\delta_{stt'}$ are offsets (or fixed effects) for states indexed by $s \equiv s(i)$.

Although there is a considerable amount of notation to rigorously describe the models, they are simply a series of (semi-parametric) regressions of district turnout totals on leads or lags of turnout totals, with fixed effects for incumbency and states. The t, t' notation is a little cumbersome, but refers to modeling votes from election t as a function of votes from election $t' \neq t$.

I use these models to generate predictions for turnout in uncontested districts given turnout in that district in a contested election under the same districting plan (with controls for incumbency and state fixed effects). I also note the standard errors for the predicted values from the model, letting us put confidence intervals around the model's predictions, and ultimately, around our predictions as to Democratic vote shares in uncontested districts.

Districts that are uncontested over the life of a redistricting plan. A small number of districts remain uncontested over the life of the corresponding redistricting plan. These districts will be missing imputations for turnout in uncontested races given the methodology outlined above. Many of these are cases

where the district exists for just a single election until mid-decade redistricting intervenes. For these districts I impute turnout by regressing Congressional turnout on Democratic share of the two-party vote for President in the most recent Presidential election.

Imputations for counts of votes. I convert predictions of Democratic vote proportions and predictions of two-party turnout into counts by simple multiplication. That is, if in uncontested district i I impute Democratic vote share $y_i \in [0, 1]$ and two-party votes as v_i then the imputed *number* of votes for the Democratic candidate is $d_i = y_i v_i$. This imputed count generally will not be an integer, but this is of no great consequence.

Moreover, for uncontested districts, both the imputed Democratic vote shares y and the turnout counts v_i are accompanied by some uncertainty, reflecting the fact that the imputation models will not fit the data perfectly. This uncertainty carries over into the imputations for counts of Democratic and Republican votes in uncontested districts, and ultimately into the efficiency gap computed for the corresponding election. This uncertainty is carried forward in my analysis, with statements about the efficiency gap’s stability over time or rank-orderings of efficiency gap estimates often equipped with 95% confidence intervals etc. The more districts in a given election that are subject to uncontestedness the greater than uncertainty in “downstream” estimates such as the efficiency gap.

9 The efficiency gap, by individual state elections

I now turn to the centerpiece of the analysis: assessing variation in the efficiency gap across districting plans.

I compute 512 efficiency gap measures, spanning 25 states and 23 election years. In each election I compute the efficiency gap for a given state-level, Congressional election as

$$\frac{\sum_{i=1}^n W_i^R - W_i^D}{\sum_{i=1}^n v_i} \quad (1)$$

where i indexes n districts, W_i^R is the number of wasted votes for the Republican candidates in district i , W_i^D are wasted votes for the Democratic candidate and v_i is the number of votes for Democratic and Republican candidates.

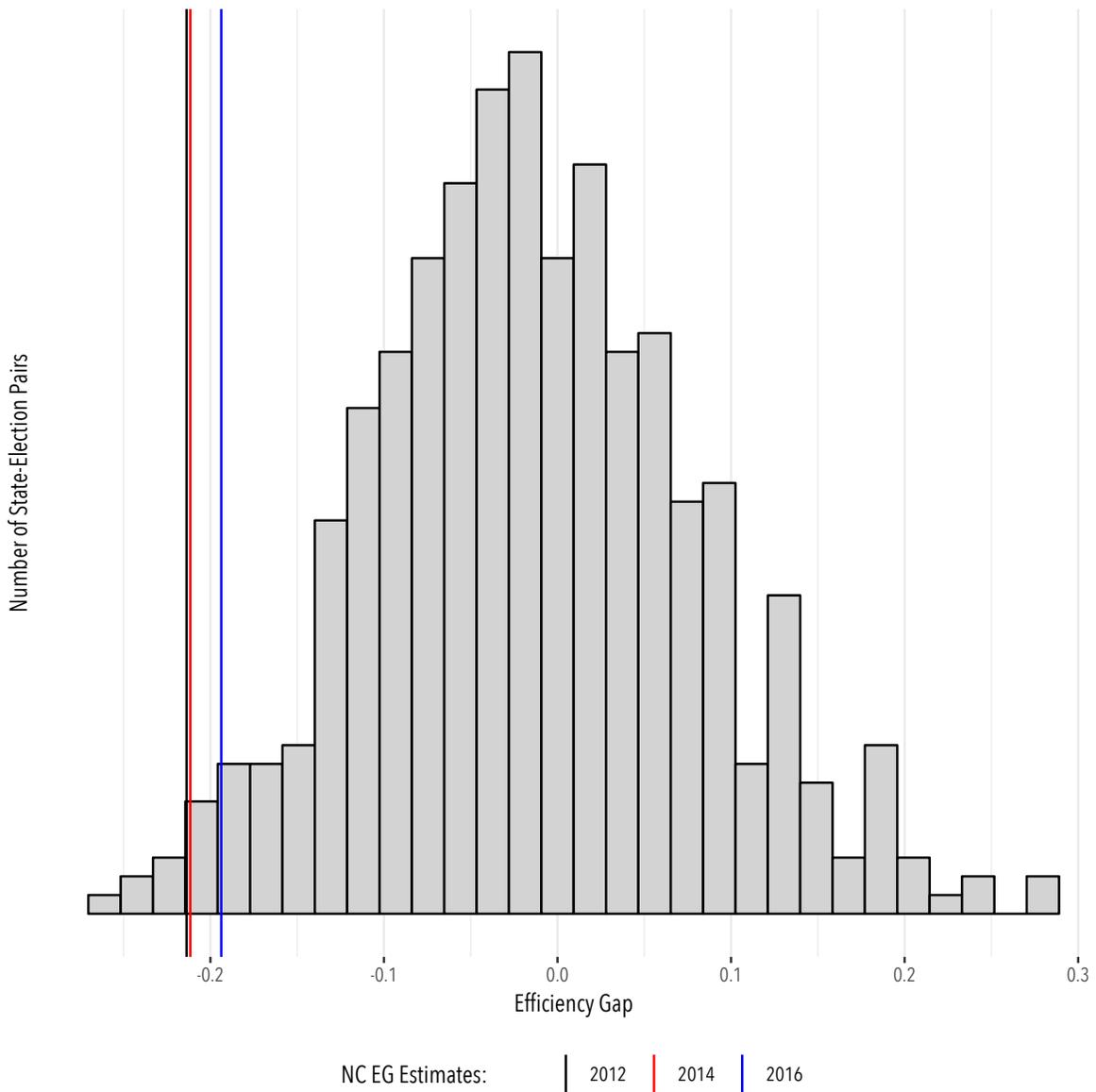


Figure 6: Histogram of efficiency gap estimates in 512 elections, 1972-2016. The three vertical lines indicate where North Carolina’s three most recent elections lie in the distribution of efficiency gap scores.

Figure 6 summarizes the distribution of the 512 efficiency gap scores. There is considerable variation in the efficiency gap estimates across states and elections, but North Carolina stands out for having its three most recent elections generate the 7th, 8th, and 13th most pro-Republican efficiency gaps of the 512 efficiency gap measures. No other state makes three appearances in the list of the fifteen most pro-Republican efficiency gaps.

The overall distribution of efficiency gap scores is roughly symmetric and centered on zero (mean of -0.014 and median of -0.018). This indicates that when averaged over many elections and many states, the districting plans used in Congressional elections in the United States display no systematic efficiency gap advantage towards one party or the other. Particular districting plans *do* have large efficiency gap scores, including the North Carolina elections and plans in the left tail of the distribution of efficiency gap scores in Figure 6.

9.1 Over-time change in the efficiency gap

Are large values of the efficiency gap less likely to be observed in recent decades? This is relevant to any discussion of a standard by which to assess districting plans. If recent decades have generally seen smaller values of the efficiency gap relative to past decades, then this might be informative with respect to contemporary districting plans and their corresponding values of the *EG*.

Figure 7 plots *EG* estimates over time, with the median measure indicated by the heavy blue line. The distribution of *EG* measures in the 1970s and 1980s appeared to slightly favor Democrats; 58% of all *EG* measures in this period were positive. The distribution of *EG* measures trends in a pro-Republican direction through the 1990s, such that by the 2000s, *EG* measures were more likely to be negative, indicative of pro-Republican advantage (see Figure 7). In the 2010s, 78% of efficiency gap scores were negative, indicative of pro-Republican advantage in their underlying districting plans. Figure 8 plots the *magnitude* of *EG* estimates over time, with the median absolute value indicated by the heavy blue line. The magnitude of the median efficiency gap was roughly constant until the current cycle, when it spiked to the highest value on record.

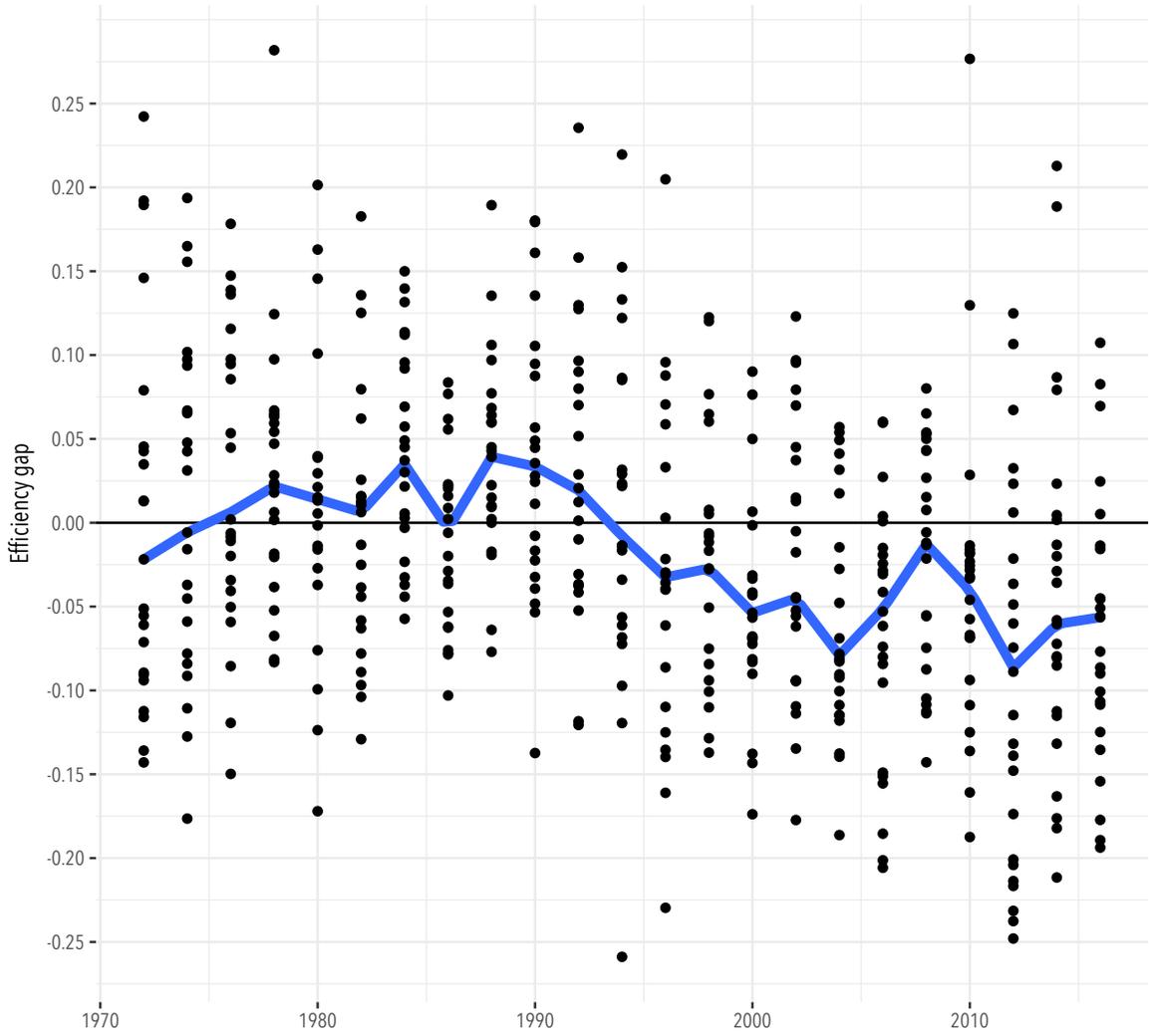


Figure 7: Efficiency gap estimates, over time. The line is a smoothed estimate of the median efficiency gap.

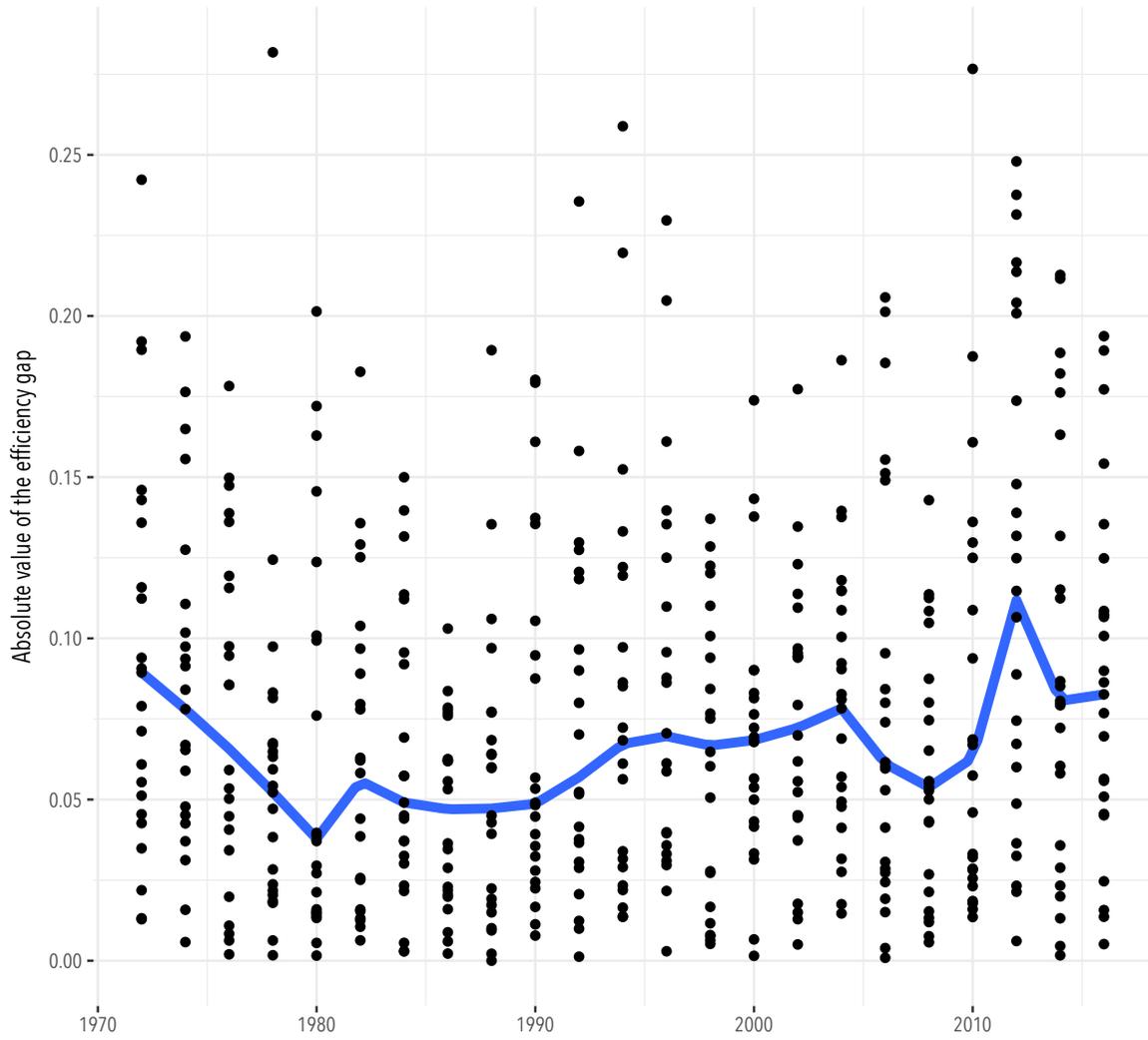


Figure 8: Absolute value of efficiency gap measures, over time. The blue line is a smoothed estimate of the median absolute value of the efficiency gap measure.

9.2 Within-plan variation in the efficiency gap

The efficiency gap is measured at each election, with a given districting plan typically generating up to five elections and hence five efficiency gap measures. Efficiency gap measures will change from election to election as the distribution of district-level vote shares varies over elections. Some of this variation is to be expected. Even with the same districting plan in place, districts will display “demographic drift,” gradually changing the political complexion of those districts. Incumbents lose, retire or die in office. Sometimes incumbents face major opposition, sometimes they don’t. Variation in turnout — most prominently, from on-year to off-year — will also cause the distribution of vote shares to vary from election to election, even with the districting plan unchanged. All of these election-specific factors will contribute to election-to-election variation in the efficiency gap.

Precisely because I expect a reasonable degree of election-to-election variation in the efficiency gap, I assess the magnitude of “within-plan” variability in the measure. If a plan is a partisan gerrymander — with a systematic advantage for one party over the other — then the “between-plan” variation in *EG* should be relatively large relative to the “within-plan” variation in *EG*.

59% of the variation in the *EG* estimates is between-plan variation. The *EG* measure varies election-to-election, but there is a moderate to strong “plan-specific” component to variation in the *EG* scores. I conclude that efficiency gaps *are* measuring an enduring feature of a districting plan.

10 Party control of redistricting drives change in the efficiency gap

Districting plans for Congressional elections have tended to produce pro-Republican average efficiency gaps in recent years, but over the entire 1972-2016 period, the average efficiency gap is very close to zero (see Figure 7). There is thus no sign of a pro-Republican advantage in the dataset as a whole. Efficiency gap scores indicative of Republican advantage are much more likely to be found in recent decades.

A leading cause of this is the fact that Republican control of Congressional

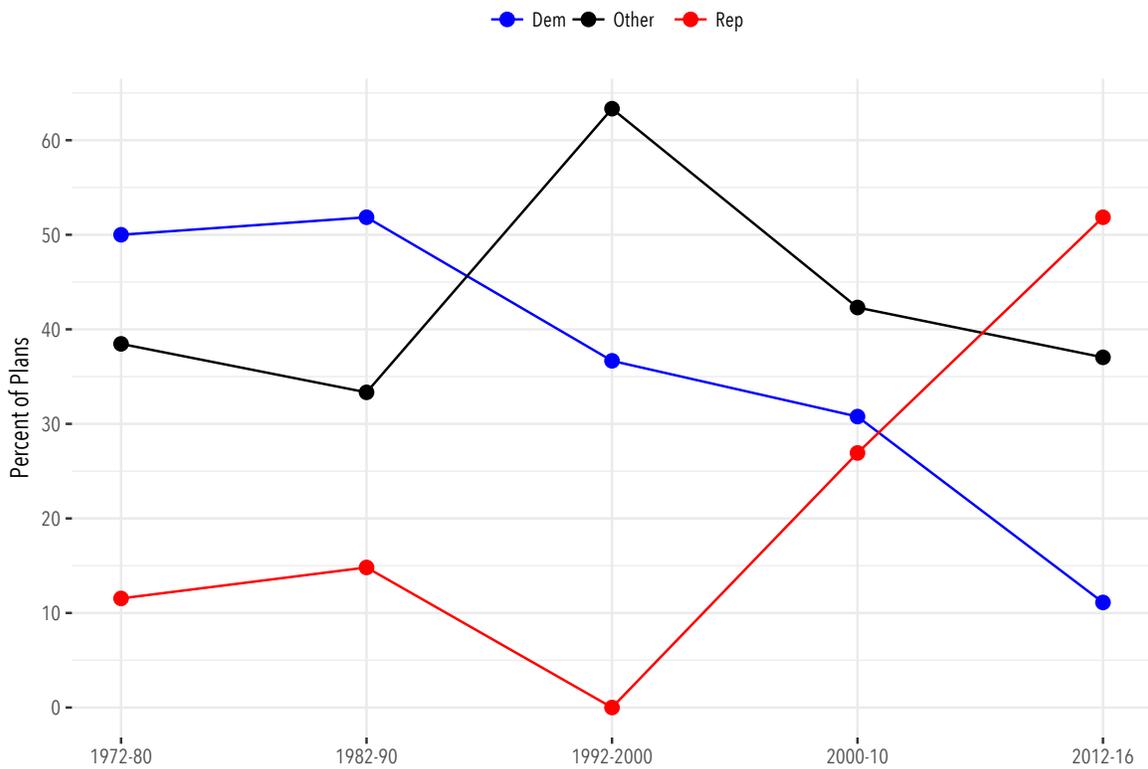


Figure 9: Share of 136 Congressional districting plans used in the efficiency gap analysis, grouped by decade, designed by Democrats in unified control of state government, by Republicans in unified control of state government, or by another institution (divided state government, commission, or court).

redistricting has increased markedly in recent decades. As Figure 9 displays, no Congressional maps were designed by Republicans in full control of the redistricting process in the 1990s, compared to about 30% by Democrats in full control and about 70% by another institution (divided government, a commission, or a court). But in the 2000s, Republicans were fully responsible for about the same proportion of plans as Democrats (about 30%). And in the 2010s, the distribution changed again such that about 50% of plans were designed entirely by Republicans, versus about 10% designed entirely by Democrats.

To determine the impact of this change in party control on the change in the efficiency gap over the last generation, I created five separate boxplots, one for each decadal redistricting cycle in the dataset. In each case, the distribution of efficiency gaps for Congressional districting plans is shown for each type of plan-making body: Democrat-controlled bodies, Republican-controlled bodies

and other institutions (divided government, courts and commissions). Figure 10 displays the average efficiency gap and the distribution of the efficiency gaps.

		Democratic	Republican
2002-2016	Estimate	.131	-.070
	<i>t</i>	(3.85)	(-2.10)
1992-2010	Estimate	-.028	-.088
	<i>t</i>	(-1.17)	(-2.24)
1992-2016	Estimate	.032	-.146
	<i>t</i>	(0.94)	(-3.35)
1972-2016	Estimate	.034	-.055
	<i>t</i>	(0.96)	(-1.79)

Table 1: Estimates of average effect of change in efficiency gap resulting from change from non-partisan control of redistricting to Democratic or Republican control of redistricting. Regression models include unreported fixed effects for states and years. *t*-statistics are based on estimates of standard errors computed via clustering on state/districting plans.

I employ regression analysis to assess the effects of partisan control on the efficiency gap. Table 1 reports estimates of the average effects on the efficiency gap of a change from non-partisan control of Congressional redistricting to partisan control, for various time slices spanning the years encompassed by my analysis. Each regression includes fixed effects for election years and states and weights taking into account the uncertainty associated with some of the efficiency gap scores. Standard errors are estimated by clustering on state/districting-plans, acknowledging that efficiency gap scores from a given state under a given districting plan are unlikely to be conditionally independent given the predictors in the regression model. The fixed effects for states means that the party control estimates are within-state estimates and identified by states that change party control of redistricting over the period spanned by a particular regression model.

Efficiency gaps in the 2002-2010 cycle would have been substantially less pro-Republican had Republicans not gained control of more state governments in this cycle relative to the 1990s. The shift from non-partisan control of Congressional redistricting in the 1990s to Republican control in the 2000s produces a -.088 average shift in the resulting efficiency gaps ($t = -2.24$). Conversely, shifting from

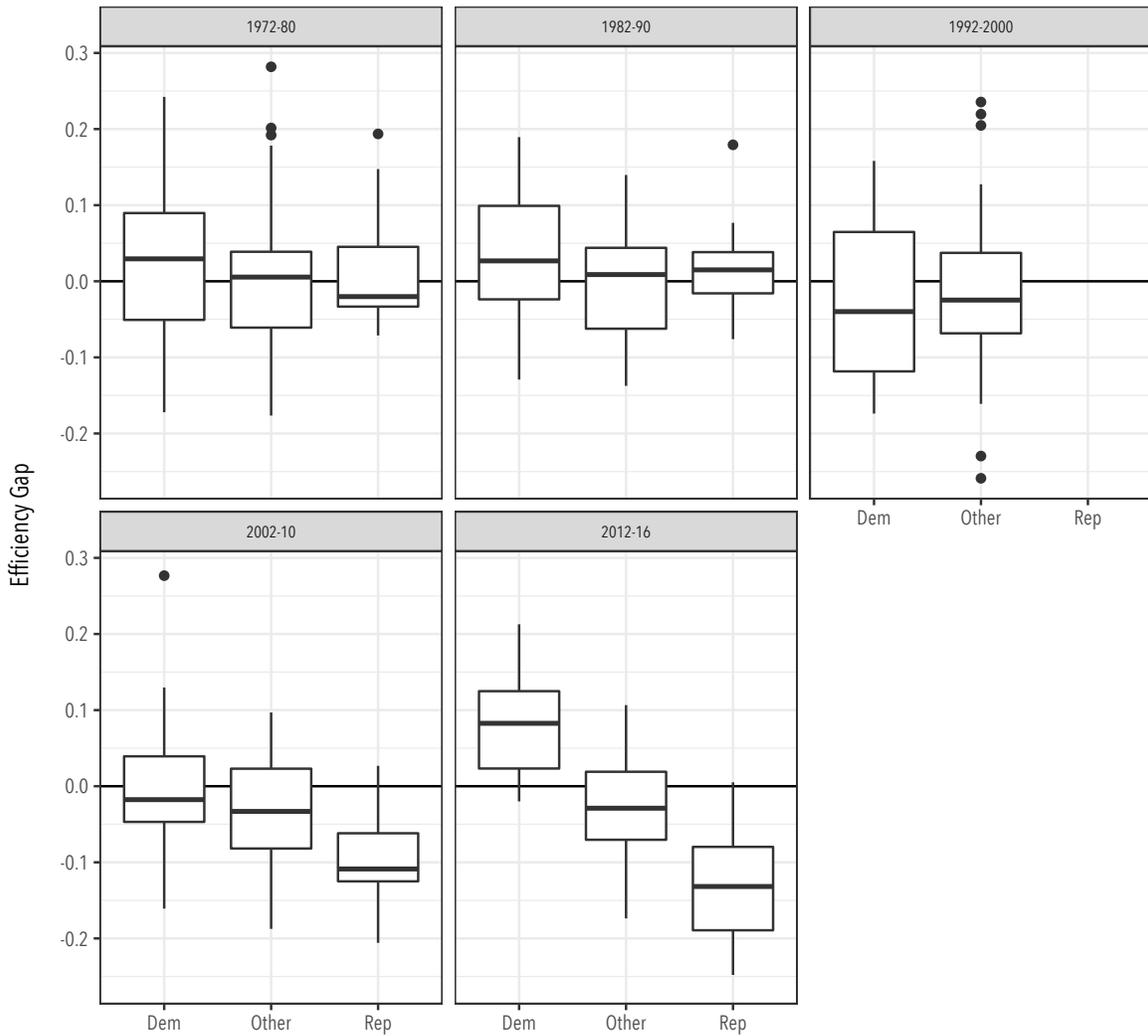


Figure 10: Distribution of efficiency gap scores, by decade, by control of corresponding redistricting process: Democrats in unified control of state government, Republicans in unified control of state government, or control by other institutions (divided state governments, commissions or courts).

non-partisan control of Congressional redistricting in the 1990s to Democratic control in the 2000s produces a $-.028$ average shift in the resulting efficiency gaps ($t = 1.17$) which is not distinguishable from “no change” at conventional levels of statistical significance).

Between the 2000s and the 2010s (1st row of Table 1), changing from non-partisan control of Congressional redistricting to Republican control produces a shift in the efficiency gap of $-.070$ ($t = -2.10$), while a shift from non-partisan control to Democratic control produces a shift in the efficiency gap of $.131$ ($t = 3.85$).

Over the last two decades, the regression analysis reported in the 3rd row of Table 1 finds large effects associated with switching to Republican control: an average change of the efficiency gap in a negative/pro-Republican direction of $-.146$ ($t = -3.35$) but no statistically significant movement associated with change from non-partisan control to Democratic control.

Finally, the last row of Table 1 confirms that the effects of partisan control of redistricting are largely confined to recent decades. Averaging over the entire 44 year period spanned by my analysis, there are much smaller effects of changes in the partisan control of redistricting than in recent decades. Averaged over the 1972-2016 period, there is no statistically significant average effect of switching from independent/non-partisan control of Congressional redistricting to Democratic control ($t = .96$) and the average effect of a switch to Republican control is a modest $-.055$ and on the cusp of being distinguishable from zero at conventional levels of statistical significance. Again, this pattern of results confirms that the association between partisan control of Congressional redistricting and large and consequential changes in the efficiency gap is a relatively recent development.

Much of the observed change in the efficiency gap in recent decades is due to the effects of change in control of the redistricting process. If the composition of partisan control of redistricting had stayed as it was in the 1990s — but the effects of partisan control of redistricting on efficiency gaps were as observed in the 2000s and 2010s — then the average efficiency gap in the 2002-2010 period would be $-.028$ versus the $-.041$ actually observed; see Figure 11. In the 2012-2016 period, the corresponding estimate is $.003$ versus the observed average efficiency gap of $-.067$; given that these are multiple-election, multiple-state *average* efficiency gap scores, these differences are large and of substantial consequence.

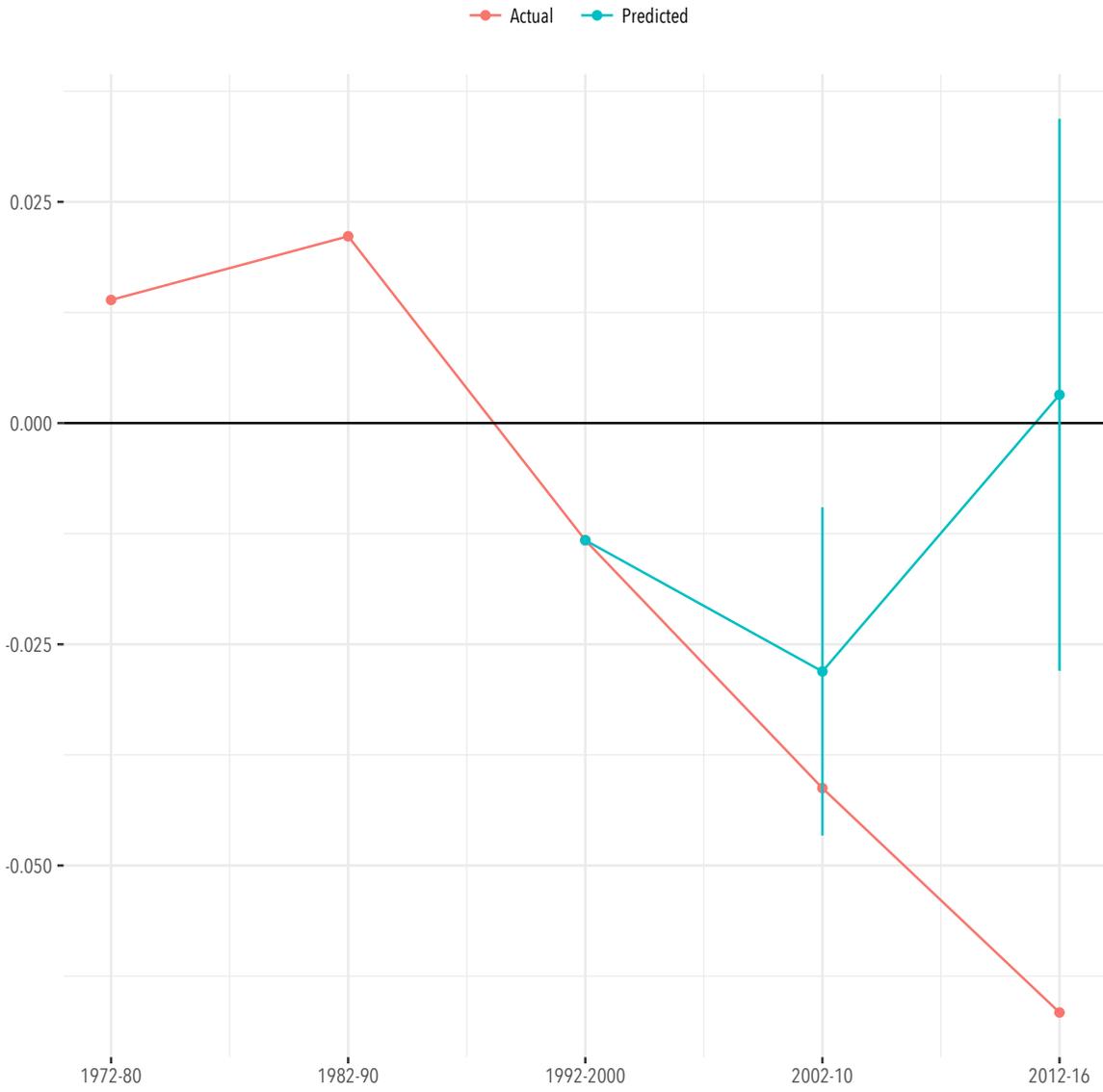


Figure 11: Average efficiency gap by decade and predicted efficiency gap had partisan control of redistricting stayed as it was in the 1990s. Vertical lines cover 95% credible intervals for predictions of the average efficiency gap.

In summary, changes in the distribution of efficiency gap scores in recent decades can be confidently attributed to changes in partisan control of redistricting, and, in particular, to the increased prevalence of Republican control of Congressional redistricting.

11 Operative consequences of the efficiency gap

How large must the efficiency gap be before it can be said to have triggered a discrete, politically meaningful outcome such as a seat changing hands?

Here it is helpful to look at the historic relationship between seats and votes, by state, in the Congressional elections spanned by this analysis. For each election, in each state, I compute the Democratic share of the statewide, two-party vote (after any imputations for uncontested seats) and the percentage of the states' CDs won by Democratic candidates. Note that again I discard states with fewer than seven CDs.

The orange line in Figure 12 is a linear regression, not constrained to run through the 50-50 point. Instead, the fact that the regression line *does* run through the 50-50 point reflects a regularity in the data: i.e, averaging over many districting plans and many elections over some 50 years of American political history, if a party wins 50% of the statewide, two-party vote in a Congressional election, it generally wins half of the CDs in that state. The slope of the regression line is 2.36, meaning that for every additional percentage point in state-wide vote share won (or lost) by Democrats, Democrats win (or lose) an extra 2.36 percentage points of seat share.⁴

I treat the regression relationship in Figure 12 as a benchmark. Each deviation (in the vertical direction) from the orange regression line — or “residuals” as they are called in regression analysis — is that portion of seat share in a given election that lies above or below that which is expected given the party's statewide share of the vote. This allows me to calibrate efficiency gap values with “surplus seats” won or lost by the parties. This exercise is designed to provide us with guidance

⁴I acknowledge that the linear form of the regression means that it will predict seat shares greater than 100% or lower than 0% conditional on high (or low) values of vote shares. But the linear regression model generates a good fit to the data in the region of observed vote shares, with little indication that predictions of seat shares will lie outside of the feasible zero to 100% interval.

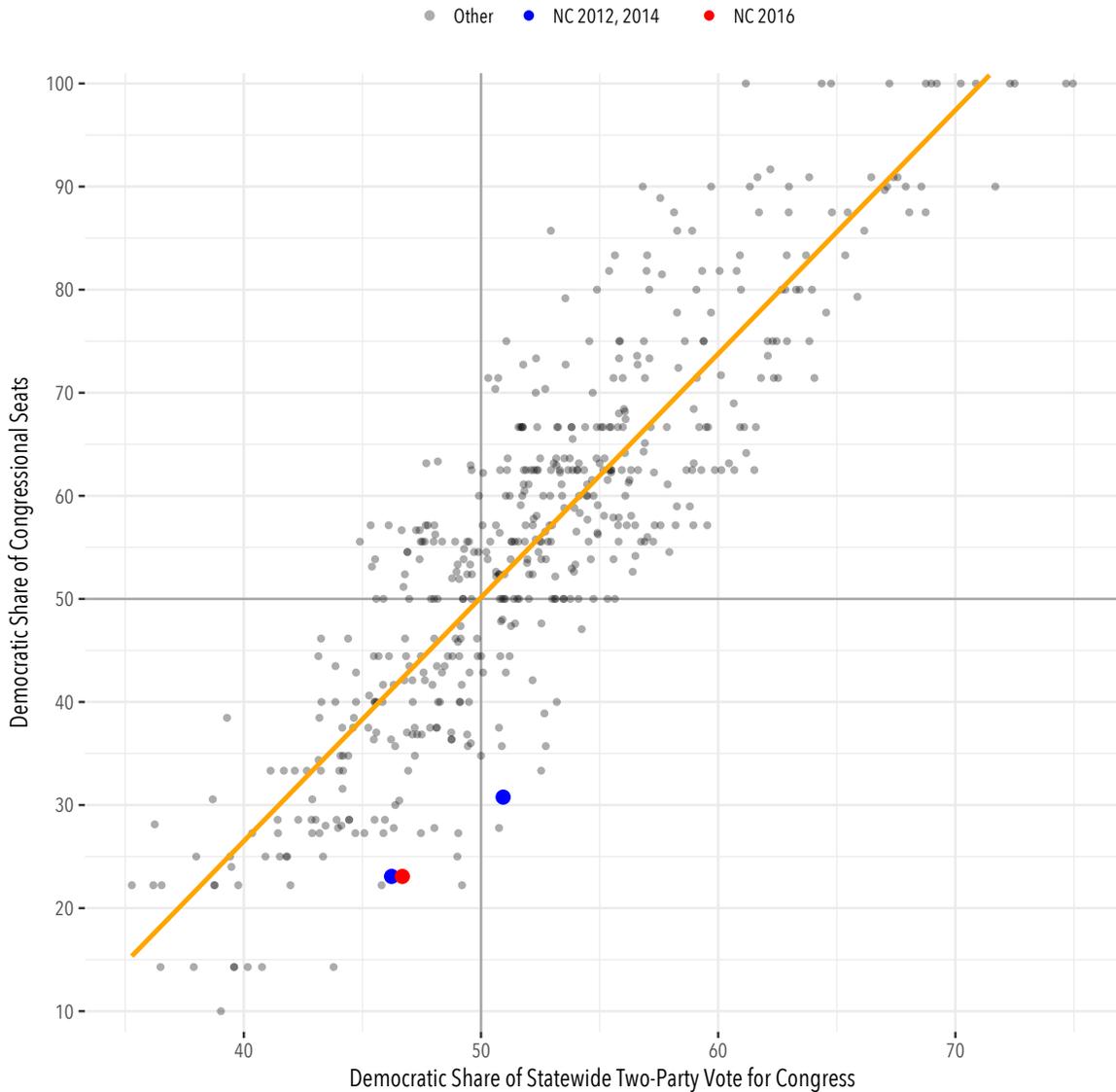


Figure 12: Seats and Votes in Congressional elections, 1972-2016. Each plotted point corresponds to a particular state in a particular election. The Democratic share of the statewide, two-party vote (after any imputations for uncontested seats) is plotted on the horizontal axis and the percentage of the states' CDs won by Democratic candidates is plotted on the vertical axis. The analysis covers states with seven or more CDs. The orange line is a linear regression that is not constrained to run through the 50-50 point; instead, the fact that the regression line *does* run through 50-50 point reflects the fact that averaged over many districting plans and many elections over some 50 years of American political history, if a party wins 50% of the statewide, two-party vote in a Congressional election, it generally wins half of the seats in that state.

as to when the efficiency gap gets sufficiently large to be causing at least a one seat disparity relative to the long-run, historical mapping of state-wide votes into seats won. I consider the point at which the efficiency gap is associated with a *half* seat surplus/deficit, which is the point at which the surplus/deficit is closer to one seat than no seats.

In so doing, it is worth remembering that the analysis here pools over states with differing numbers of Congressional districts. I have omitted states with fewer than 7 CDs, but even so, the analysis spans Alabama, Colorado (2002-2016) and South Carolina (2012-2016) and other states with just seven CDs, through to California with 53 seats (2002-2016). Accordingly, I disaggregate the data into six bins: states with 7 or 8 CDs, 9 or 10 CDs, 11 to 15 CDs, 16 to 20 CDs, 21 to 30 CDs and 31 to 53 CDs.

In each of these bins, I examine what values of the efficiency gap are associated with discrepancies from the historical votes-seats relationship. In general, I expect a positive correlation between the efficiency gap and discrepancies relative to the long-run historical relationship between votes and seats, and indeed, this is confirmed in Figure 13.

Table 2 presents the results of this analysis. For states with small numbers of CDs, an efficiency gap of about $-.07$ or $-.08$ is associated with a Democratic seat deficit of approximately $1/2$ a seat in the corresponding districting cycle; an efficiency gap larger than this $1/2$ seat threshold is more likely to generate a one seat deficit (surplus) than no deficit (or surplus). This threshold is roughly the same irrespective of whether one is considering a Democratic half-seat deficit or surplus. Larger values of the efficiency gap are associated with a one seat deficit or surplus: about $-.16$ for a one-seat Democratic deficit, about $.14$ for a one seat Democratic surplus.

As states' Congressional delegations get larger, the efficiency gap values associated with a $1/2$ seat or a one seat deficit/surplus start to get smaller. For large delegations (above 20 seats), a one seat departure from the historical mapping between vote shares and seat shares (Figure 12) is associated with a $-.06$ efficiency gap (Democratic deficit) or a $.02$ efficiency gap (Democratic surplus).

A simple summary of the estimates presented in Table 2 is that for states with delegations with 15 or fewer members, an efficiency gap of $\pm .08$ means it is more likely that a seat changes hands than not. The corresponding number is $\pm .05$ for

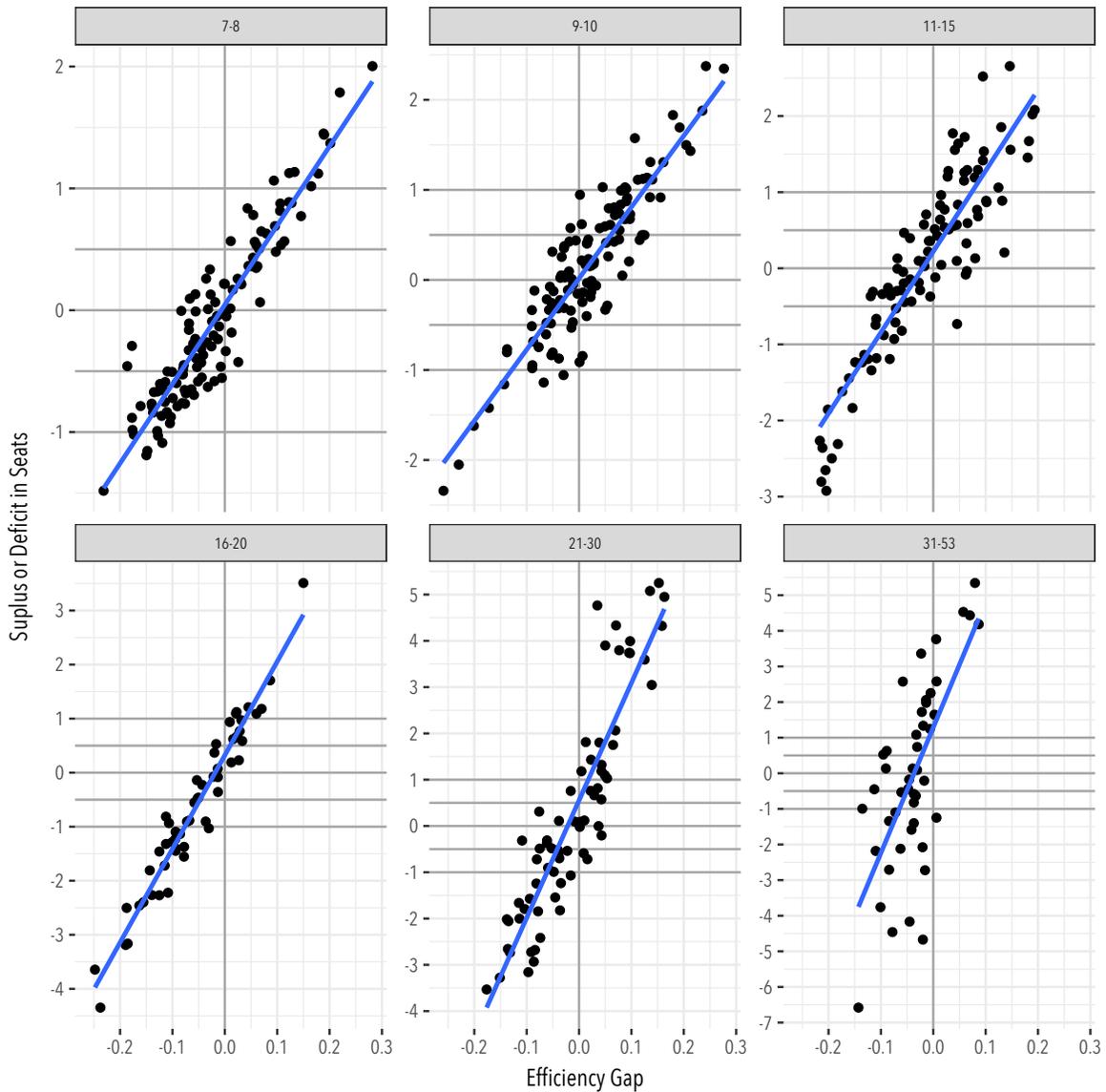


Figure 13: Surplus seats and the efficiency gap, 1972-2016, by number of seats in each state. The surplus seats measure is defined as the difference between the percentage of seats won by the Democrats in a given election and the seats predicted given the historical, regression relationship shown in Figure 12. The blue line in each panel is a smoothing spline fit as to minimize cross-validated prediction error (Wood, 2006).

# CDs	Seat Deficit/Surplus			
	-1	-0.5	0.5	1
7-8	-.17 [-.18, -.15]	-.08 [-.09, -.07]	.07 [.06, .08]	.14 [.13, .15]
9-10	-.12 [-.14, -.11]	-.06 [-.08, -.05]	.06 [.05, .07]	.12 [.11, .13]
11-15	-.11 [-.12, -.10]	-.08 [-.09, -.07]	.02 [.00, .03]	.07 [.06, .08]
16-20	-.08 [-.08, -.07]	-.05 [-.06, -.04]	.01 [.00, .02]	.04 [.03, .05]
21-30	-.06 [-.07, -.05]	-.04 [-.04, -.03]	.00 [-.00, .01]	.02 [.01, .03]
31-53	-.06 [-.08, -.05]	-.05 [-.06, -.04]	-.02 [-.03, -.01]	-.01 [-.02, .00]

Table 2: Efficiency gap scores associated with indicated Congressional seat deficit/surplus. Columns refer to Democratic seat deficit/surplus; rows correspond to the number of CDs in a state. A surplus or a deficit in seats is defined as deviation from the historical relationship between seat shares and vote shares shown in Figure 12.

states with more than 15 CDs. These values are conservative in the sense that it is often the case that smaller values of the efficiency gap are associated with a seat changing hands. Erring on the side of larger values of the efficiency gap has the effect of reducing the number of plans that would be recommended for judicial scrutiny.

12 Predictive performance of 1st efficiency gap observed under a plan

I now investigate the prognostic properties of the first *EG* observed under a districting plan. In each instance the test is whether the magnitude (or absolute value) of the first *EG* observed under a plan exceeds a given threshold value EG^* . The outcome of interest is whether the average of the plan’s remaining *EG* scores has the same sign as the *EG* from the first election. That is, does a positive (or negative) initial *EG* score accurately predict that the remainder-of-plan average *EG* will also be positive (or negative). Critically, does the predictive utility of the

1st *EG* observed under a plan vary with the magnitude of that 1st *EG* score?

The classification or prediction task here can be summarized as follows:

Test	Actual	
	Positive	Negative
Positive	True Positive	False Positive
Negative	False Negative	True Negative

Note that in the discussion that follows the “test” is whether the 1st election *EG* score exceeds a particular magnitude or threshold. The “actual” outcome is whether the remainder-of-plan average *EG* under that districting plan has the same sign as the 1st election *EG* score.

I compute and plot a number of quantities from this exercise. First, I note the “detection prevalence rate” or the “test positive” rate, the proportion of districting plans for which the value of the efficiency gap in the first election under the plan exceeds a given threshold. In the left-hand panel of the top row of Figures 14 and 15 I refer to this quantity as the “proportion flagged” or $\Pr(\text{flagged})$.

The prognostic measures I rely on are conventional measures of predictive or classification accuracy used throughout the quantitative sciences:

1. *sensitivity*, or true positive rate (TPR): the proportion of positives that test positive (“flagged”), $TP/(TP + FN)$, labelled as $\Pr(\text{flagged}|\text{positive})$ in 2nd panel, top row of Figures 14 and 15.
2. *specificity*, or true negative rate (TNR): the proportion of negatives that test negative, $TN/(TN + FP)$, labelled as $\Pr(\neg\text{flagged}|\text{negative})$ in the 3rd panel, top row of Figures 14 and 15, “ \neg ” being a conventional shorthand for negation.
3. *precision*, the proportion of cases testing positive (“flagged”) that are actually positive, $TP/(TP + FP)$, labelled as $\Pr(\text{positive}|\text{flagged})$ in the right hand panel of the top row of Figures 14 and 15.
4. *accuracy*, the proportion of cases that are true positives or true negatives, $(TP + TN)/(TP + FP + FN + TN)$, labelled as $\Pr(\text{correct diagnosis})$ in the left hand panel of the bottom row of Figures 14 and 15.

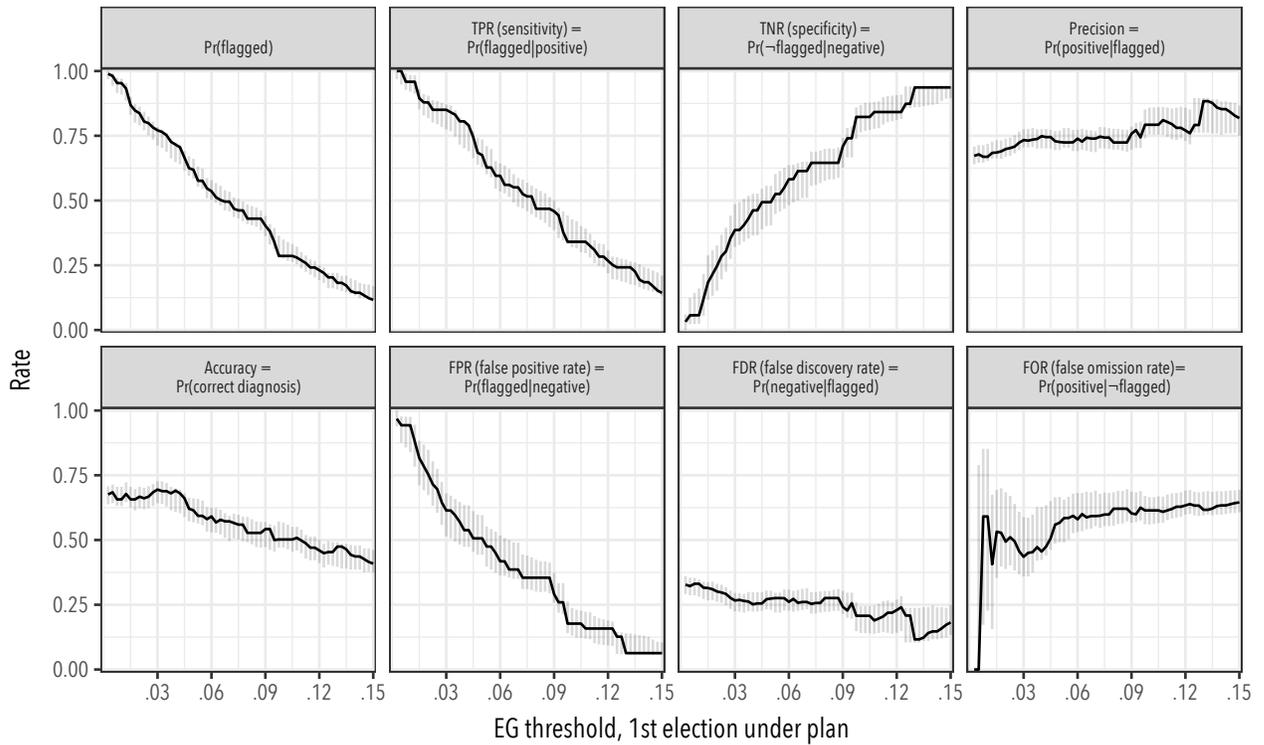


Figure 14: Prognostic performance measures, first efficiency gap under a districting plan more extreme than threshold (horizontal axis) as a predictor of whether the remainder-of-plan average efficiency gap recorded under the districting plan has the same sign as the first efficiency gap. Vertical lines indicate 95% confidence intervals. Analysis restricted to plans with three or more elections.

5. the *false positive rate* (FPR); proportion of negative cases that test positive, 1 minus the specificity or $FP/(TN + FP)$, labelled as $\Pr(\text{flagged} \mid \text{negative})$ in the 2nd panel, bottom row of Figures 14 and 15.
6. the *false discovery rate* (FDR); the proportion of cases testing positive that are actually negative, $FP/(TP + FP)$, labelled as $\Pr(\text{negative} \mid \text{flagged})$ in the 3rd panel, bottom row of Figures 14 and 15. Note that this quantity is one minus the precision.
7. the *false omission rate*; the proportion of cases that test negative that are actually positive, $FN/(FN + TN)$, labelled as $\Pr(\text{positive} \mid \neg \text{flagged})$ in Figures 14 and 15.

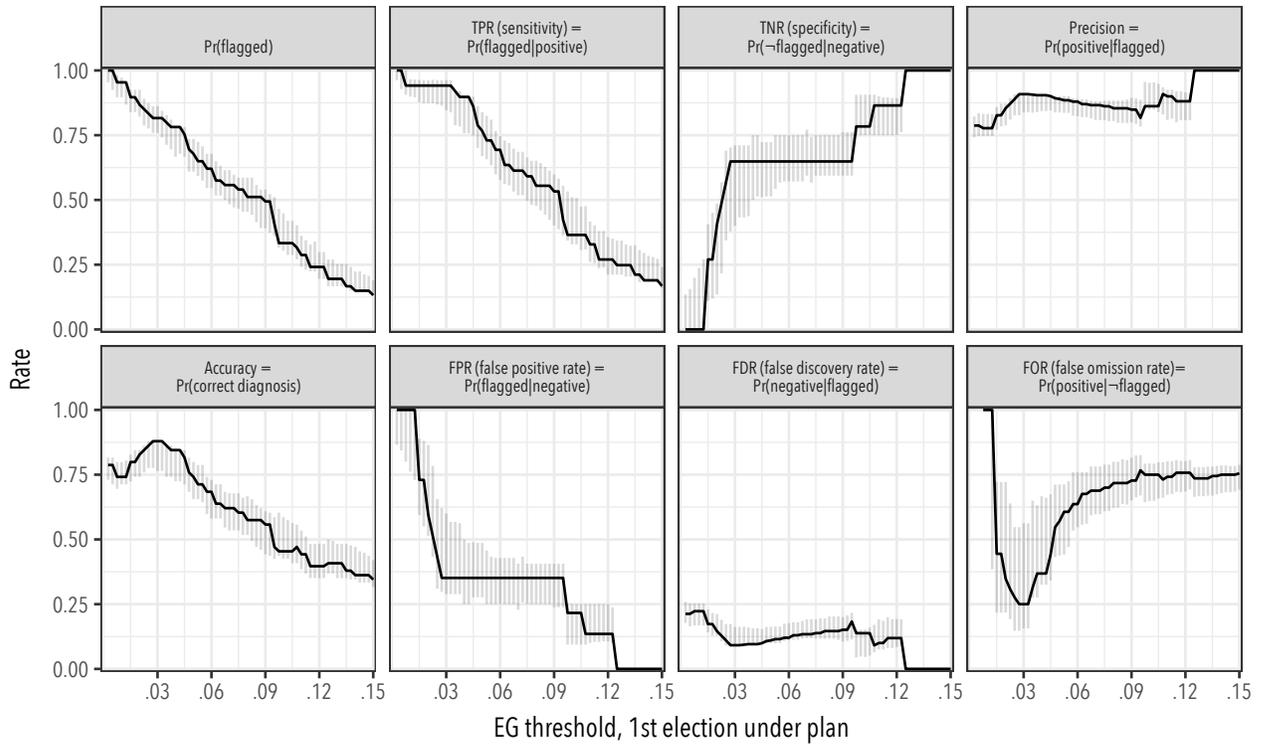


Figure 15: Prognostic performance measures, first efficiency gap under a districting plan more extreme than threshold (horizontal axis) as a predictor of whether the remainder-of-plan average efficiency gap recorded under the districting plan has the same sign as the first efficiency gap. Vertical lines indicate 95% confidence intervals. Analysis restricted to plans with three or more elections. The rates in this figure are from plans enacted since 2000.

Figure 14 displays these various rates based on all plans with three or more elections over the entire 1972-2016 period. Figure 15 displays results based on plans with three or more elections enacted since 2000.

A relatively large proportion of plans have large 1st election *EG* scores. As many as 23% of plans begin life with a 1st election *EG* of .12 in magnitude or greater. Of these 23% of plans, 87.5% go on to have a remainder-of-plan average efficiency gap of the same sign as the 1st election *EG*, an indication of the *reliability* of the 1st election *EG*. Generally, the precision of a prognostic test based on the 1st election *EG* is high, approaching 90% once the 1st election *EG* is greater than .03 in magnitude.

A more stringent threshold — a 1st election *EG* of at least .15 in magnitude — is tripped by fewer plans, about 12.5% of plans. At this threshold the precision of the test criterion remains high (or conversely, the false discovery rate stays low), but the false omission rate has climbed to 76%; i.e., the threshold is so stringent that only one out of every four plans with a remaining plan-average *EG* with the same sign as the 1st *EG* is being flagged by the test. The overall accuracy of the test falls to around 32% if one were to adopt a very stringent threshold such as $|1stEG| > .15$.

Note that the false positive rate (FPR) continues to fall as the test threshold is made more stringent: at a threshold of .12, only 12.5% of plans that do *not* have a remainder-of-plan average *EG* with the same sign as the 1st election *EG* are flagged. If the test criterion was $|1stEG| > .12$, then (a) 23% of plans would trip that threshold, (b) with a false positive rate of about 12.5% and (c) a false discovery rate of 12.5% also.

Districting plans enacted since 2000 see larger and more durable efficiency gap scores, such that the “signal” in the 1st election *EG* is more reliable, relative to plans from the 1970s-1990s. An almost identical proportion of post-2000 plans (Figure 15) trip the 1st *EG* threshold of +/- .12 (25%) as in the analysis of 1972-2016 plans (Figure 14).

But a compelling, distinctive feature of plans enacted since 2000 is how few of them “reverse course” after the 1st election. The false discovery rates in Figure 15 are all zero once the 1st election *EG* is .03 or greater in magnitude. That is, in the post-2000 era, if a plan begins life with a 1st election $EG < -.03$ its remainder-of-plan average *EG* will be negative, or positive if the 1st election $EG > .03$.

Similarly, the false positive rate is zero once beyond a 1st election *EG* of .03 (or .055, taking into account uncertainty in the *EG* estimates stemming from imputations for uncontested districts). Likewise, the precision of the test criterion is 100% once the threshold exceeds these levels. Figure 15 also makes clear that setting the threshold at higher levels doesn't risk "false alarms", but rather, false omissions.

For the post-2000 era, there is very little risk of flagging a plan that will go on to have a remainder-of-plan average efficiency gap that contradicts the "signal" about the direction of partisan advantage in the 1st election's efficiency gap. This means that the operative questions would seem to be (a) the volume of plans being flagged for scrutiny; and (b) the magnitude of the remainder-of-plan average efficiency gaps likely to follow given the 1st election *EG*.

Here I draw on the analysis provided in section 11, which supplies guidance in setting an efficiency gap threshold, helping us understand how values of the efficiency gap translate into a tangible outcome such as a change in the seats won for a given level of the statewide vote.

In utilizing the results of Table 2 I (a) focus on the 1/2 seat threshold; and (b) treat the parties symmetrically, taking the larger in magnitude of the efficiency gap scores associated with a 1/2 seat surplus or deficit. That is, the efficiency gap scores I would use as thresholds are: $\pm .08$ for states with delegations with 15 or fewer members, and $\pm .05$ for states with larger delegations.

With this analysis in hand, I now examine which values of the efficiency gap in the 1st election under a plan are predictive of plan-average efficiency gap scores at these threshold values.

12.1 Regression relationship between 1st *EG* and remainder-of-plan average *EG*

Figure 16 shows the relationship between first-election efficiency gaps and average efficiency gaps observed over the remainder of the plan, for the districting plans analyzed here. I restrict this analysis to plans with at least three elections, so that the average *EG* computed net of the 1st election spans at least two elections, leaving 108 plans in this analysis. Figure 17 repeats this analysis for plans enacted since 2000, with 44 plans with three more elections.

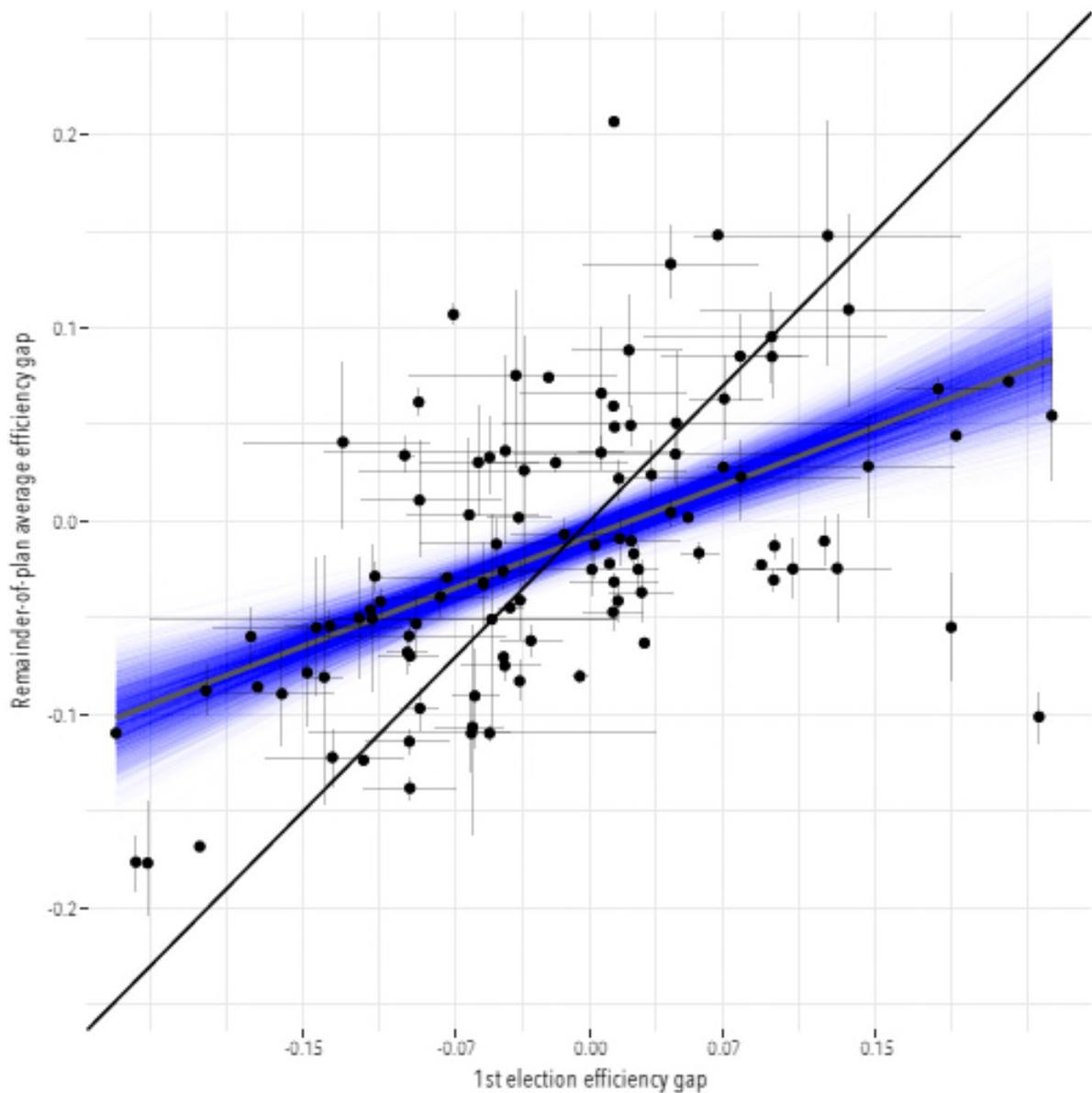


Figure 16: Scatterplot of first-election efficiency gap scores (horizontal axis) and remainder-of-plan average efficiency gap (vertical axis). The diagonal black line is a 45-degree line; the data would lie on this line if first-election efficiency gaps coincided with remainder-of-plan average efficiency gaps. The blue lines are linear regressions, which vary because the underlying data are subject to uncertainty stemming from imputations for uncontested districts. Vertical and horizontal lines extending from each data point cover 95% confidence intervals in either direction, summarizing the uncertainty in both first-election *EG* and remainder-of-plan average *EG* given the imputations for uncontested districts. Analysis restricted to plans with at least three elections. The *EG* in North Carolina in 2016 is -0.194.

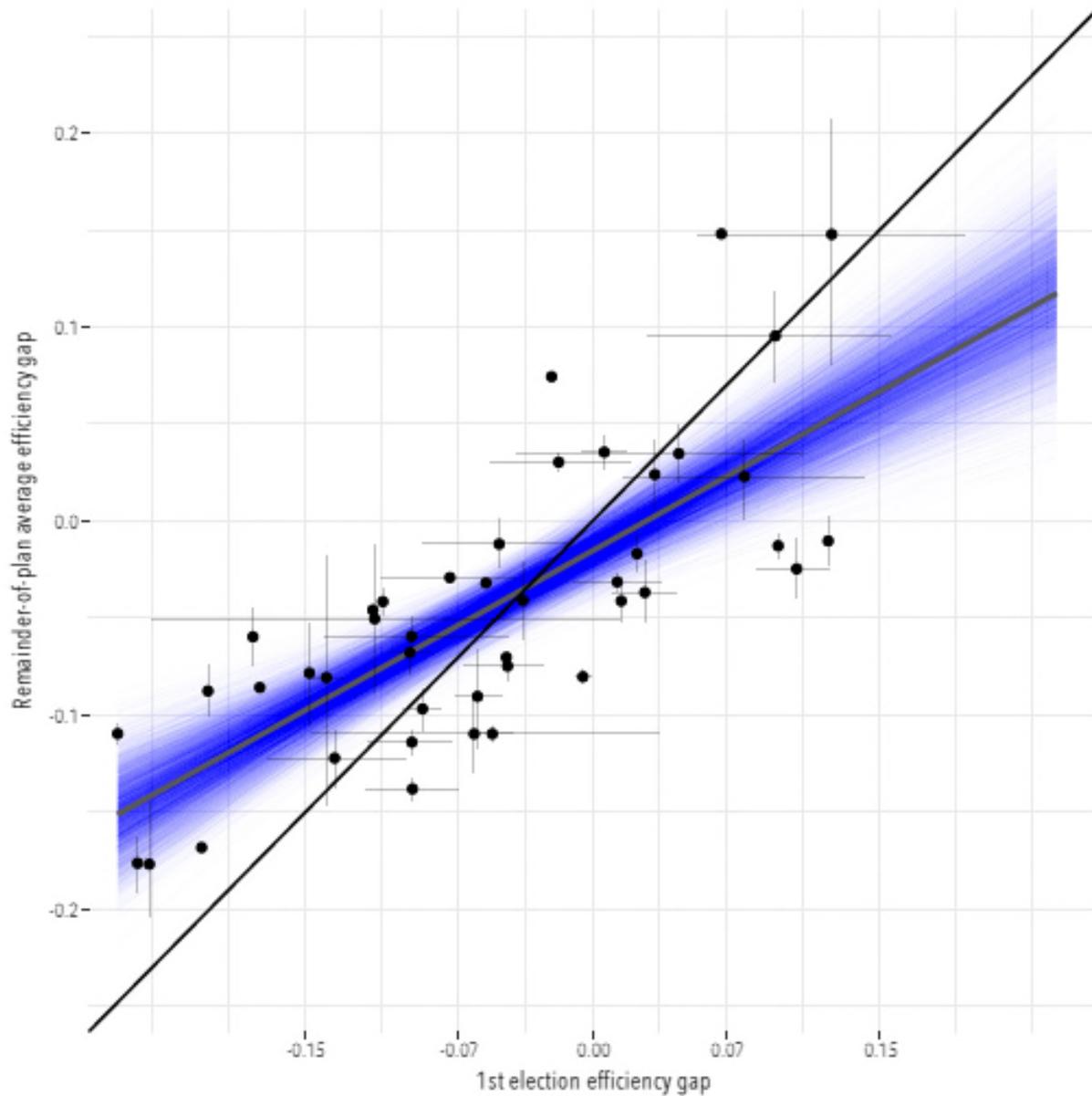


Figure 17: Scatterplot of first-election efficiency gap scores (horizontal axis) and remainder-of-plan average efficiency gap (vertical axis). The diagonal black line is a 45-degree line; the data would lie on this line if first-election efficiency gaps coincided with remainder-of-plan average efficiency gaps. The blue lines are linear regressions, which vary because the underlying data are subject to uncertainty stemming from imputations for uncontested districts. Vertical and horizontal lines extending from each data point cover 95% confidence intervals in either direction, summarizing the uncertainty in both first-election *EG* and remainder-of-plan average *EG* given the imputations for uncontested districts. Analysis restricted to plans with at least three elections, enacted after 2000. The *EG* in North Carolina in 2016 is -0.194.

The black diagonal line on each graph is a 45-degree line: if the relationship between first-election *EG* and remainder-of-plan average *EG* were perfect, the data would all lie on this line. Even given the uncertainty that accompanies *EG* measures due to uncontestedness, the relationship between first-election *EG* and remainder-of-plan average *EG* is quite strong. The correlation between 1st election *EG* scores and the remainder-of-plan average *EG* is .54 (95% CI .50 to .57). In the post-2000 era, this correlation is .74 (95% CI .70 to .78).

In the particular case of North Carolina, in 2016 the efficiency gap is -0.194. The analysis of historical data discussed above — and graphed in Figure 16 — forecasts that the remainder-of-plan average *EG* for this plan will be -0.08 (95% CI -0.21 to 0.04). Although there is uncertainty as to the eventual, remainder-of-plan average *EG*, I am highly confident that the plan used for North Carolina's 2016 Congressional elections — if left undisturbed — will produce a negative, pro-Republican, average efficiency gap. Figure 18 summarizes this uncertainty revealing that the probability of a positive (pro-Democratic) remainder-of-plan average *EG* is 9.5%.

If these calculations are based on the relationship between 1st election efficiency gaps and remainder-of-plan average efficiency gaps in the post-2000 era (Figure 17), then given the *EG* in 2016 in North Carolina, the forecast remainder-of-plan average *EG* is -.12 (95% CI -.23 to -.02). Using the post-2000 data, the probability that the plan used for North Carolina's 2016 Congressional elections — if left undisturbed — will ultimately turn out to have a positive, pro-Democratic remainder-of-plan average efficiency gap is less than 2% (see Figure 19).

12.2 1st election efficiency gap and thresholds

Earlier I suggested using an efficiency gap of .08 as a threshold value in states with relatively small numbers of Congressional seats (15 or fewer members). Now I ask what 1st election efficiency gap is associated with a remainder-of-plan average efficiency gap of .08 or greater in magnitude.

The regression analysis shown in Figure 17 suggests that in the post-2000 era, a districting plan will tend to have a remainder-of-plan average efficiency gap below -.08 if its 1st *EG* is -.10 or lower (indicative of advantage for Republicans).

Prob (avg EG < 0 | 1st EG = -.19) = .9032

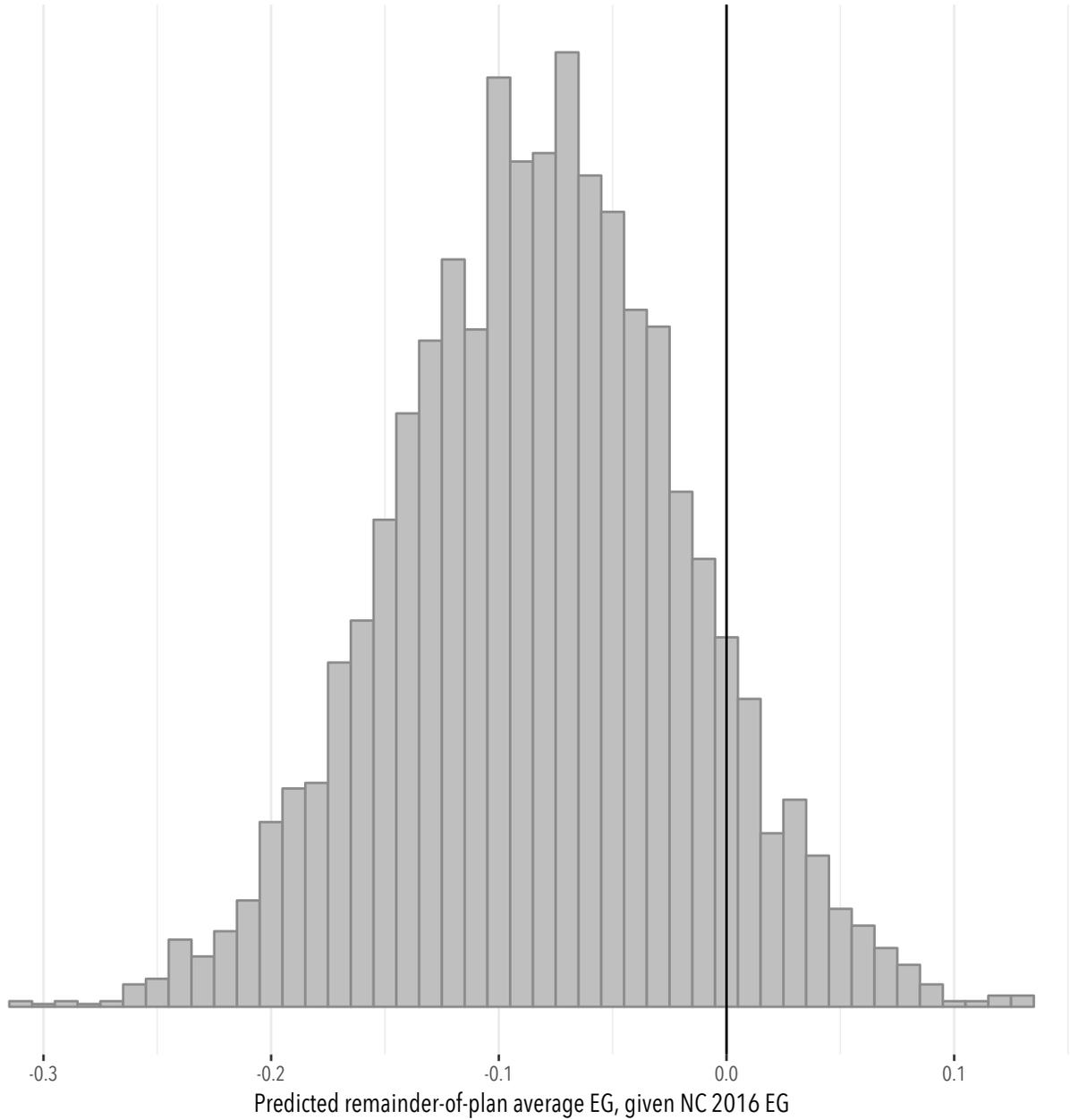


Figure 18: Histogram summarizing predictions as to the remainder-of-plan average efficiency gap expected from the current North Carolina districting plan, based on the historical, regression relationship between 1st election *EG* and remainder-of-plan average *EG* displayed in Figure 16.

Prob (avg EG < 0 | 1st EG = -.19) = .9862

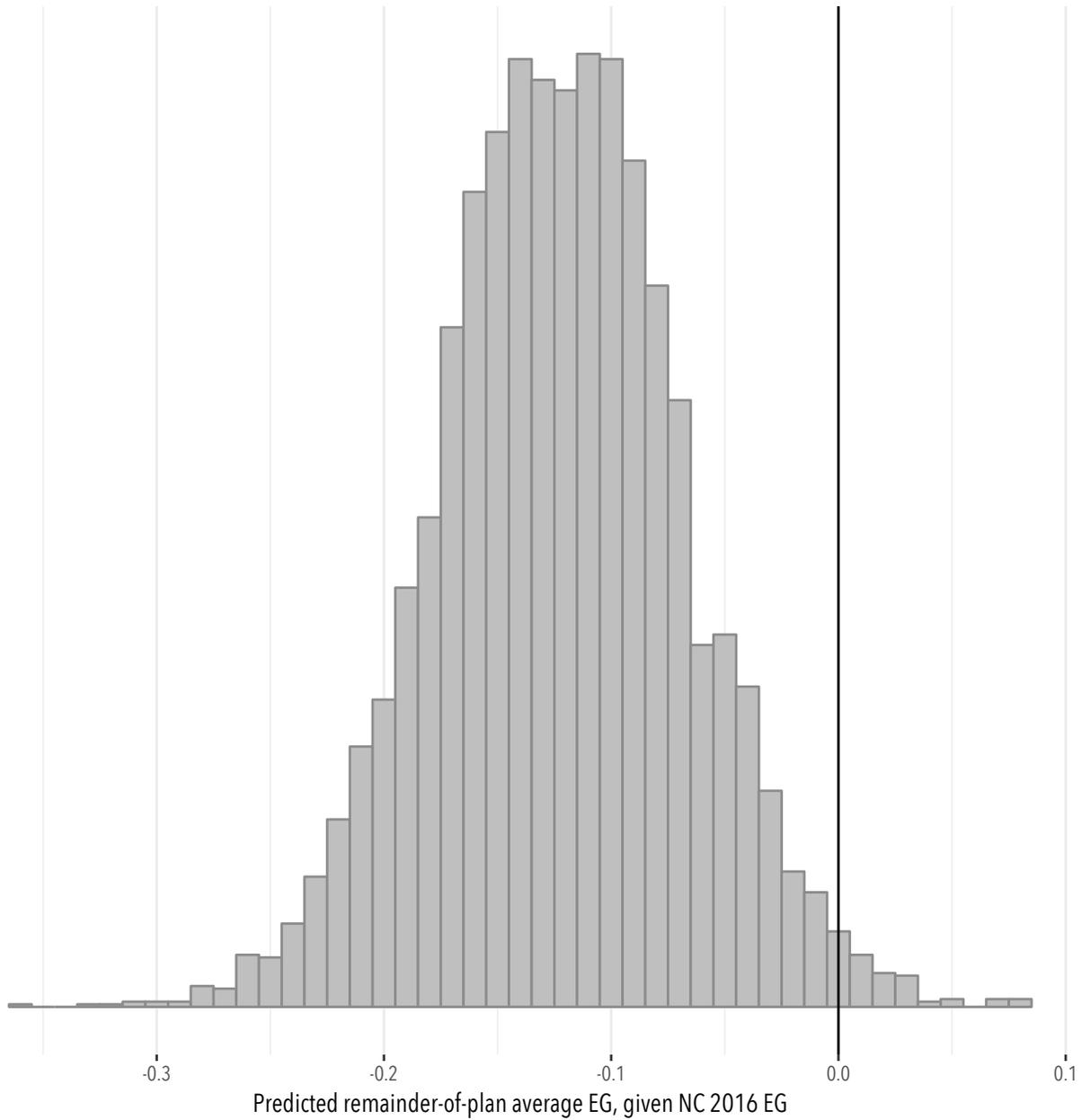


Figure 19: Histogram summarizing predictions as to the remainder-of-plan average efficiency gap for the current North Carolina districting plan, based on the historical, regression relationship between 1st election *EG* and remainder-of-plan average *EG*, subset to plans enacted since 2000, displayed in Figure 17.

A remainder-of-plan average efficiency gap above .08 in the post-2000 era is associated with a 1st election *EG* of .14 (indicative of advantage for Democrats). I suggest a compromise between the -.10 threshold for plans appearing to favor Republicans and the .14 threshold for plans appearing to favor Democrats, setting the 1st election, efficiency gap threshold for states with relatively small delegations at $\pm .12$.

For states with larger Congressional delegations, I seek the 1st election efficiency gap score that is associated with a remainder-of-plan average efficiency gap of $\pm .05$. In the post-2000 era, the regression analysis in Figure 17 shows that a 1st election efficiency gap of -.06 is associated with a remainder-of-plan average efficiency gap of -.05. For plans exhibiting Democratic advantage, a 1st election efficiency gap of .09 is associated with a remainder-of-plan average efficiency gap of .05. This suggests using $\pm .075$ as the 1st election efficiency gap threshold for states with more than 15 CDs; it would seem only fair that any threshold that would flag an apparently pro-Republican plan for scrutiny should also flag an apparently pro-Democratic plan for scrutiny.

These proposed thresholds are set using only the post-2000 data, and would be slightly different if the entire dataset were used. Employing only the post-2000 data is appropriate here given the advances in redistricting technology and the increases in the durability of the efficiency gap apparent in the post-2000 data.

12.3 Summary

A summary of the efficiency gap thresholds I recommend and their properties is presented in Table 3. To restate the key steps in the argument and supporting findings from the data:

1. In section 11 I determined the values of the efficiency gap that are associated with politically meaningful departures from the long-run relationship between vote shares and seat shares in state-level Congressional elections (see Figure 12 and 13).
2. One-half of a seat is the point at which a change in the allocation of seats is more likely than no change, given a particular split of the two-party vote.

3. In states with a relatively small number of Congressional districts (7 to 14), the value of the efficiency gap associated with at least a half-seat departure from the long-run relationship between vote shares and seat shares is $\pm.08$.
4. In states with a relatively large number of Congressional districts (15 or more), the value of the efficiency gap associated with at least a half-seat departure from the long-run relationship between vote shares and seat shares is $\pm.05$.
5. I then determined the value of the efficiency gap observed in the 1st election under a districting plan that is associated with a remainder-of-plan average efficiency gap that is at least as large as the thresholds defined above (section 12.2). These 1st election efficiency gap values differ between the parties. Hence I propose compromise values of $\pm.12$ in states with 7-14 Congressional districts and $\pm.075$ in states with 15 or more Congressional districts.
6. In states with 7-14 Congressional districts, 9 out of 30 plans enacted in the post-2000 era trip the compromise 1st election *EG* threshold of $\pm.12$. Just three of the nine plans go on to have a remainder-of-plan average efficiency gap smaller than the $\pm.08$ threshold.
7. In states with 15 or more Congressional districts, 8 out of 14 plans enacted in the post-2000 era trip the compromise 1st election threshold of $\pm.075$. All of these eight plans go on to have a remainder-of-plan average efficiency gap beyond the $\pm.05$ threshold.

13 Sensitivity to perturbations in election outcomes

How sensitive is the efficiency gap to reasonable swings in vote shares? I investigate the behavior of the efficiency gap when we perturb election outcomes, mimicking “uniform swing” across the jurisdiction. That is, a given election produces a set of vote shares across districts. A new, hypothetical election is considered in which all vote shares move up or down by a predetermined quantity (i.e., the “swing”). Since all districts move by the same amount, this technique is

	Number of CDs	
	7-14 CDs	≥ 15 CDs
$EG \Rightarrow \geq .5$ seat deviation from historical norm	$\pm .08$	$\pm .05$
1st election EG such that remainder-of-plan average EG exceeds threshold:		
Democratic advantage	.14	.09
Republican advantage	-.10	-.06
Compromise	$\pm .12$	$\pm .075$
Test positive rate, post-2000	9/30 (30%)	8/14 (57%)
False discovery rate, post-2000	3/9 (33%)	0/8 (0%)

Table 3: Rationale and summary of efficiency gap thresholds and their properties. Analysis restricted to plans in place for at least three Congressional elections.

known as uniform swing. In real-world elections swings are never uniform, and so this method is widely considered to be a simplification. On the other hand, modeling or predicting swing district by district is quite difficult, especially where there are not useful predictors of district-specific swings.

I perform the following exercise with elections held since the 2010 round of redistricting (i.e., generated under districting plans that are currently in effect). For each election, I simulate a series of uniform swings, evenly spaced between -10% to +10%, a quite large set of swings by the standards of state-level swings in Congressional elections. For instance, swings in the Democratic share of the statewide two-party vote in North Carolina Congressional elections from 1972 to 2016 are estimated to range between -9.1 percentage points between 2008 and 2010 and +8.9 percentage points between 1972 and 1974.

At each level of uniform swing, I record the perturbed vote shares and the seat shares that result from the perturbed votes. Note that a seat changes hands if the addition of the assumed level of uniform swing pushes that seat's Democratic two-party vote share to the other side of 50%. At each level of simulated uniform swing I recompute the efficiency gap.

I then examine how much the efficiency gap measures — generated under different levels of uniform swing — depart from the efficiency gap observed under the actual election. In particular, if relatively small changes in swing produce large changes in the *EG*, one might be concerned as to the stability and reliability of the efficiency gap as a characterisation of a districting plan. Keep in mind that this exercise keeps the districting plan as it is and merely shifts vote shares up and down over a range of hypothetical levels of statewide swing.

The top row of Figure 20 displays correlations between actual efficiency gaps and simulated efficiency gaps, under different hypothetical levels of uniform swing (horizontal axis), with separate panels for low, medium and high values of actual efficiency gaps. Note that when uniform swing is zero, the simulated efficiency gaps correspond to the actual efficiency gaps, and so the correlation between the two sets of efficiency gaps is exactly 1.0. Under larger levels of uniform swing, the correlation between observed and simulated efficiency gaps diminishes.

Small efficiency gaps (less than .04 in absolute value) are less resistant to perturbations from uniform swing. At high levels of uniform swing and for small, actual efficiency gaps, the correlation between actual efficiency gaps and the sim-

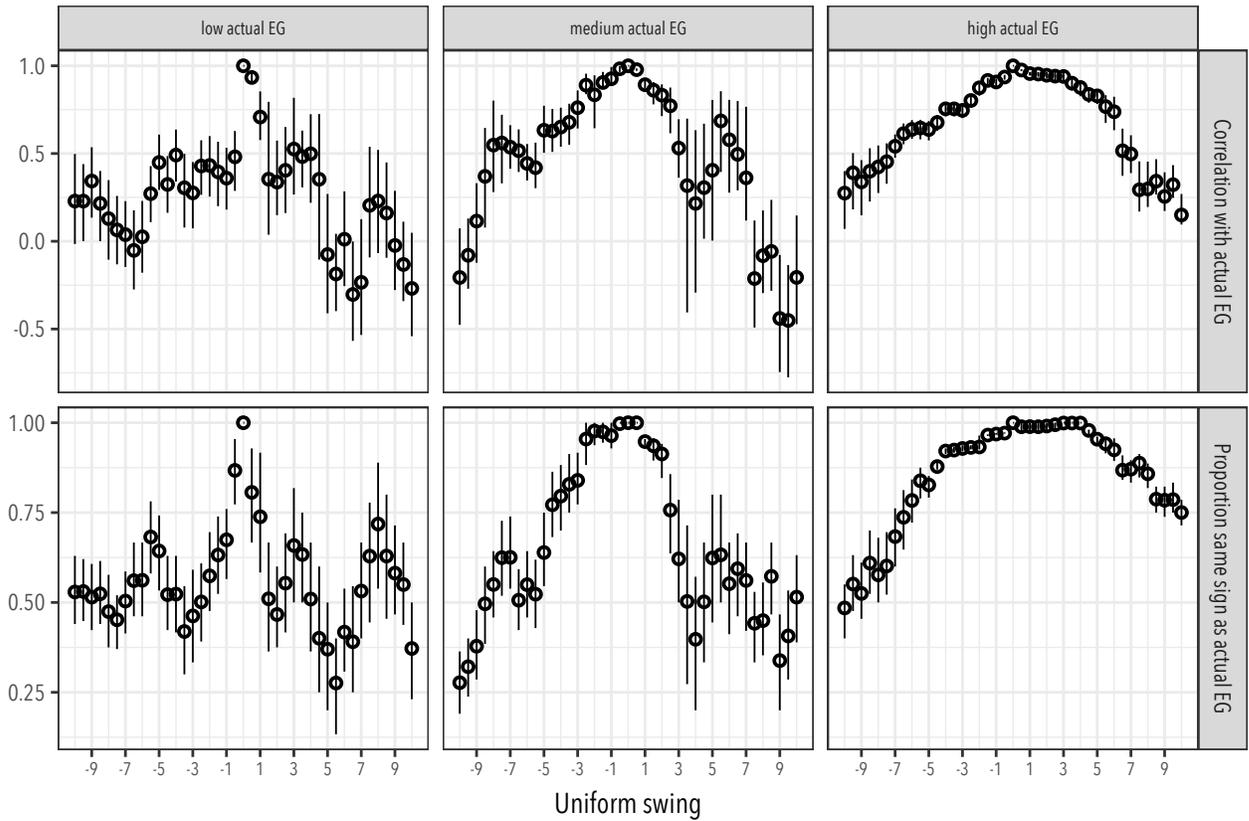


Figure 20: Correlation between actual efficiency gaps and simulated efficiency gaps (top row) and proportion of simulated efficiency gaps with same sign as actual efficiency gaps (bottom row), by hypothetical levels of uniform swing (horizontal axis). Vertical lines are 95% confidence intervals. The three columns correspond to actual efficiency gaps that are low in magnitude (less than .04 in absolute value; left column), medium (.04 to .09 in absolute value, medium column) and high (above .09 in absolute value, right column). When uniform swing is zero, the simulated efficiency gaps correspond to the actual efficiency gaps, and so the correlation between the two sets of efficiency gaps is exactly 1.0 and 100% of the simulated efficiency gaps have the same sign as the actual efficiency gaps.

ulated efficiency gaps approaches zero. Larger values of the efficiency gap are much more robust to perturbations from uniform swing. In fact, for large actual efficiency gaps (greater than .09 in magnitude), the correlation between actual and simulated efficiency gaps stays quite large over the entire range of simulated levels of uniform swing considered here (top right panel of Figure 20).

The bottom row of Figure 20 displays the proportion of simulated efficiency gaps that have the same sign as the actual efficiency gaps, under a range of hypothetical levels of uniform swing (horizontal axis), again with separate panels for low, medium and high values of actual efficiency gaps. Again, note that small efficiency gaps — less than .04 in magnitude and hence relatively close to zero — are reasonably likely to flip sign under moderate to large values of hypothetical uniform swing. About half of these small efficiency gap estimate flip sign when the corresponding election result is perturbed with reasonably large state-wide swings one way or the other. But large efficiency gaps — those in the top tercile in absolute value — show great resistance to sign flips even in face of moderate or even large hypothetical state-wide swings (lower right panel of Figure 20). Barely any of the large efficiency gaps flip sign when uniform swings are below 2.5 percentage points and just a small proportion flip sign even when perturbed by larger statewide swings. Twenty-one percent of actual efficiency gaps greater than .09 in magnitude flip sign when exposed to a large hypothetical statewide swing of five percentage points towards Republicans. Only 7% of efficiency gaps flip sign under a five point swing towards Democrats.

13.1 Sensitivity of the North Carolina efficiency gap to perturbations

I focus on the robustness of the efficiency gap observed in North Carolina in 2016 in Figure 21, which graphs the relationship between efficiency gaps and the assumed levels of uniform swing. As in the analysis described in the preceding paragraphs, vote shares recorded in 2016 in North Carolina's 13 CDs are all shifted by the same amount (the same level of uniform swing), shown on the horizontal axis. At each level of uniform swing, the shifted vote shares and the party winning each seat are recorded, which in turn generate a corresponding efficiency gap (vertical axis).

The actual 2016 outcome corresponds to no perturbation at all (uniform

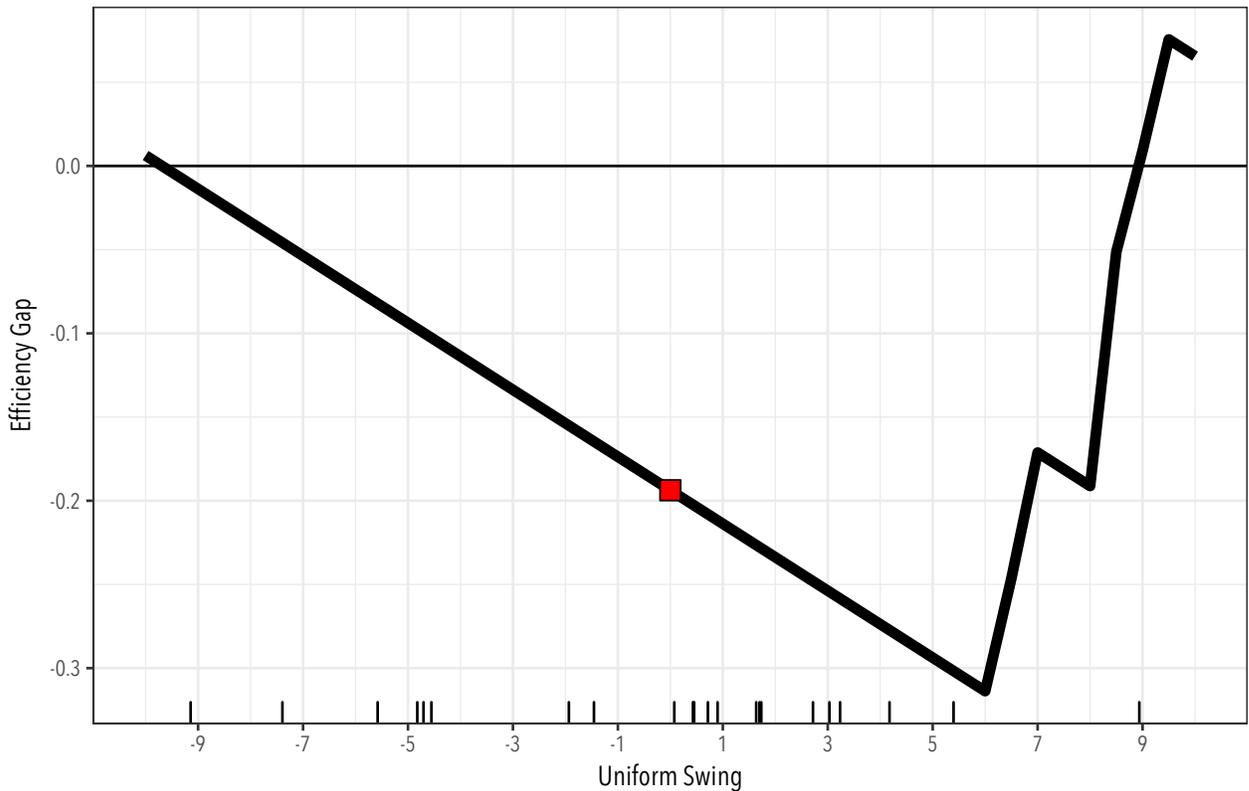


Figure 21: North Carolina efficiency gap scores generated by perturbing the actual 2016 result by varying degrees of uniform swing. The red square indicates the observed efficiency gap for North Carolina in 2016. Tick marks on the horizontal axis indicate swings in North Carolina Congressional elections 1972-2016.

swing of zero) and the observed level of the efficiency gap is plotted with a red square. Tick marks on the horizontal axis indicate swings in North Carolina Congressional elections 1972-2016, giving some guidance as to which values of uniform swing are more typical than others. Step-like changes in the efficiency gap result when the level of uniform swing is large enough to change the outcome of a seat.

The 2016 value of the efficiency gap is not just large, but quite robust to even large changes in the 2016 outcome. Shifting North Carolina’s 2016 Congressional election results in a more Democratic direction (rightward movement along the horizontal axis in Figure 21) results in the efficiency gap becoming even more negative. This is because the districting plan in North Carolina has few marginal seats, a by-product, if not a hallmark, of a partisan gerrymander. If Democrats

obtained a statewide, uniform swing of even six points — taking Democratic share of the two-party vote to 52.7% — no seats would change hands relative to the actual 2016 results. The most marginal of the 10 seats won by Republicans — District 13 — was decided 56.1% (R) to 43.9% (D) and would produce the same outcome even if the state swung by six points towards the Democrats. Under this scenario, the efficiency gap would grow in magnitude, to -.31.

Conversely, the efficiency gap continues to be large and negative (indicative of an advantage for Republicans) if the observed 2016 results are hypothetically shifted towards greater Republican vote share. Even after applying the largest, pro-Republican swing observed in the 1972-2016 era — the nine point swing towards Republicans in the 2010 Congressional election — the efficiency gap remains negative.

Note that the 2016 election outcome — featuring a Democratic vote share of 46.7% — represents a relatively good Republican year relative to North Carolina’s recent Congressional elections. Swings in a Democratic direction are more likely than further pro-Republican swings, resulting in even larger efficiency gaps unless the pro-Democratic swing exceeded six percentage points.

13.2 Robustness of the efficiency gap further indicates the severity of the underlying partisan advantage

The robustness of the efficiency gap observed in North Carolina in 2016 reflects the extent to which the current districting plan advantages Republicans. Democratic votes are “packed” in North Carolina, so much so that in 2016, the most marginal of the three seats won by a Democrat (District 12) was decided 67% (D) to 33% (R). North Carolina would have to swing by *seventeen points* in order for Republicans to pick up another seat, a swing almost twice as large as the largest swing seen in the last 45 years of North Carolina’s history.

If Republicans were to pick up ten or eleven additional points of vote statewide, taking them to 63% or 64% of the statewide vote — the 10-3 split among North Carolina’s 13 CDs starts to look reasonable, a reasonably fair reflection of what would be an extremely lopsided split in the statewide vote. It is a measure of the extent to which the current plan advantages Republicans that it would take this much swing *towards* Republicans to rationalize the current plan, taking the

Republican share of the statewide vote for Congress to levels 9 points above the high-water mark of 55.8% obtained in 1994. Note also that for more reasonable or typical values of swing (say, five points towards Republicans), the efficiency gap would remain below $-.10$, a value that is large relative to the efficiency gaps observed in the multi-state historical analysis presented above. Swings towards Democrats would see the efficiency gap get even larger, becoming as large as $-.31$ under a swing of six points.

14 Comparison with partisan bias

Finally, as a validation exercise, I compare my measures of the efficiency gap with measures of partisan bias (introduced earlier in section 5). Partisan bias is widely accepted as a measure of the symmetry and hence fairness (or unfairness) of a districting plan. The criticism of partisan bias is that it turns on what is almost always a counter-factual or hypothetical scenario of an even 50-50 split in the statewide two-party vote.

This criticism notwithstanding, support for the validity of the efficiency gap follows to the extent that the efficiency gap and the partisan bias measure are positively correlated, in the circumstances in which partisan bias is acknowledged to be a reasonable measure, i.e., competitive elections, with statewide splits of the two-party vote that are close to the stylized 50-50 split contemplated by the partisan bias measure.

Figure 22 presents two comparisons of partisan bias and the efficiency gap: one for elections that are not competitive (a statewide split of the two-party vote more lopsided than 52.5/47.5) and another for elections that are decided by a statewide split close to 50-50 (closer than 52.5/47.5). Clearly, when elections are close, partisan bias and the efficiency gap are highly correlated, evidence of the validity of the efficiency gap measure.

Conversely, partisan bias and the efficiency gap display almost no correlation when elections are not close. This suggests that for elections that are not close — where the election outcome is some distance from the counter-factual of 50-50 election contemplated by partisan bias — partisan bias should be not relied on a measure of the fairness of a districting plan.

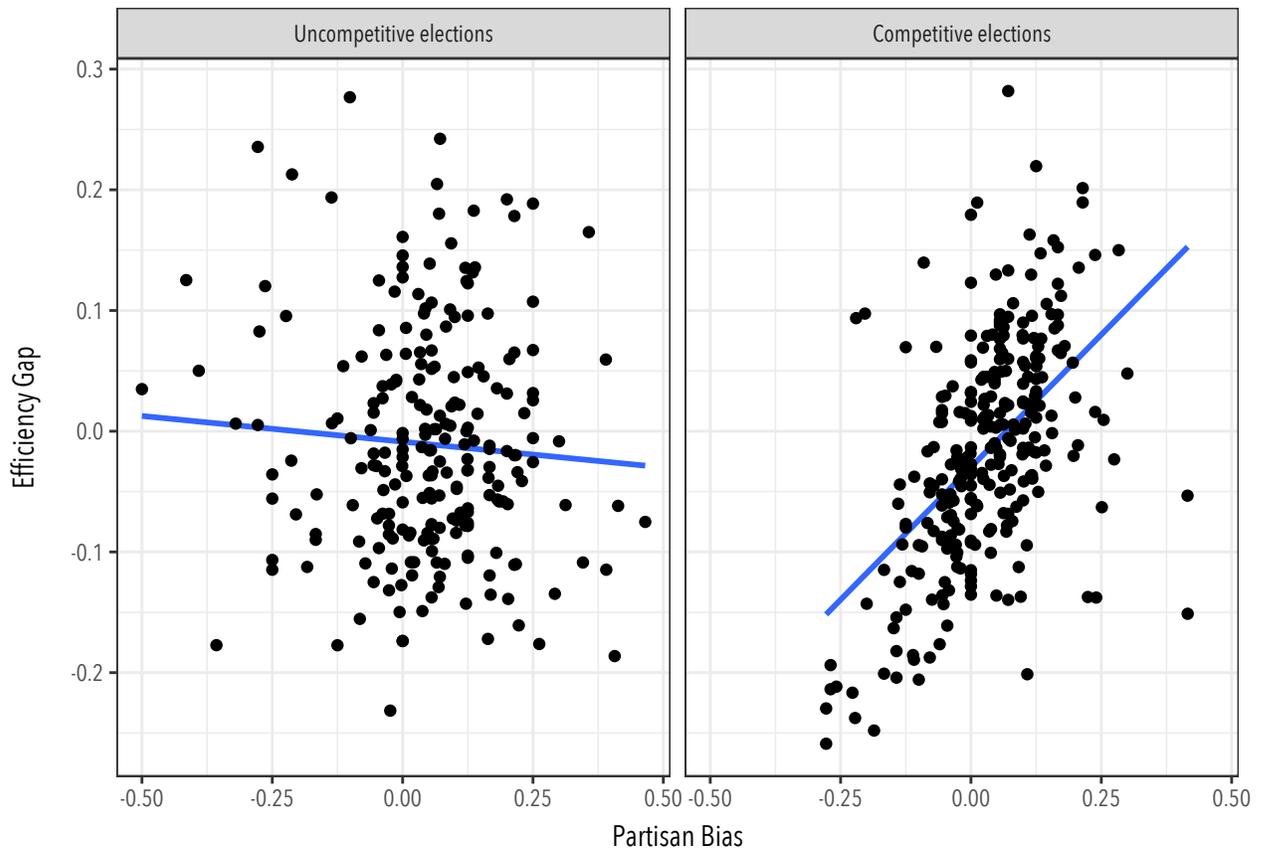


Figure 22: Partisan bias and the efficiency gap compared, competitive elections (closer than 52.5/47.5) and non-competitive elections.

15 Conclusion: the North Carolina plan

North Carolina has had one Congressional election under the current districting plan, in 2016. Two Congressional elections were held under the preceding districting plan, in 2012 and 2014. That districting plan bears many similarities with the current plan. These three most recent Congressional elections in North Carolina have had zero (2012 and 2016) or at most one uncontested district (2014), meaning that the estimates of the efficiency gap scores generated for North Carolina are accompanied by no or little uncertainty due to imputations for uncontested seats.

In 2012, Democratic candidates for Congress won 50.9% of the two-party vote for Congress in North Carolina; they won 4 out of the state's 13 seats, or 30.8%. In 2014, Democratic candidates for Congress won 46.2% of the two-party vote for Congress (95% CI 45.4% to 47.1%, reflecting uncertainty stemming from the imputations for missing data). Democrats won 3 out of the 13 seats (23.1%). In 2016, Democratic candidates for Congress won 46.7% of the two-party vote for Congress. They again won 3 out of the 13 seats. The efficiency gaps associated with each of these elections are large: $-.214$ in 2012, $-.211$ in 2014 (95% CI $-.229$ to $-.195$) and $-.194$ in 2016.

These large, negative estimates of the efficiency gap — and the large disparities between vote shares and seat shares in North Carolina Congressional elections — are driven by the same phenomenon: a systematic advantage for Republican candidates in the districting plans used in these North Carolina Congressional elections.

The negative *EG* estimates generated in these last three elections in North Carolina are unusual relative to North Carolina's political history (see Figure 23), and when compared with efficiency gap scores from Congressional elections over 40 years and many states (see Figure 24). In particular (see Table 4), the 2012 *EG* estimate for North Carolina is:

- the largest *EG* estimate North Carolina has produced over the 44 year period spanned by this analysis (1972-2016);
- the 12th largest *EG* estimate by magnitude (95% CI 12th to 17th) out of 512 *EG* estimates produced in my analysis;

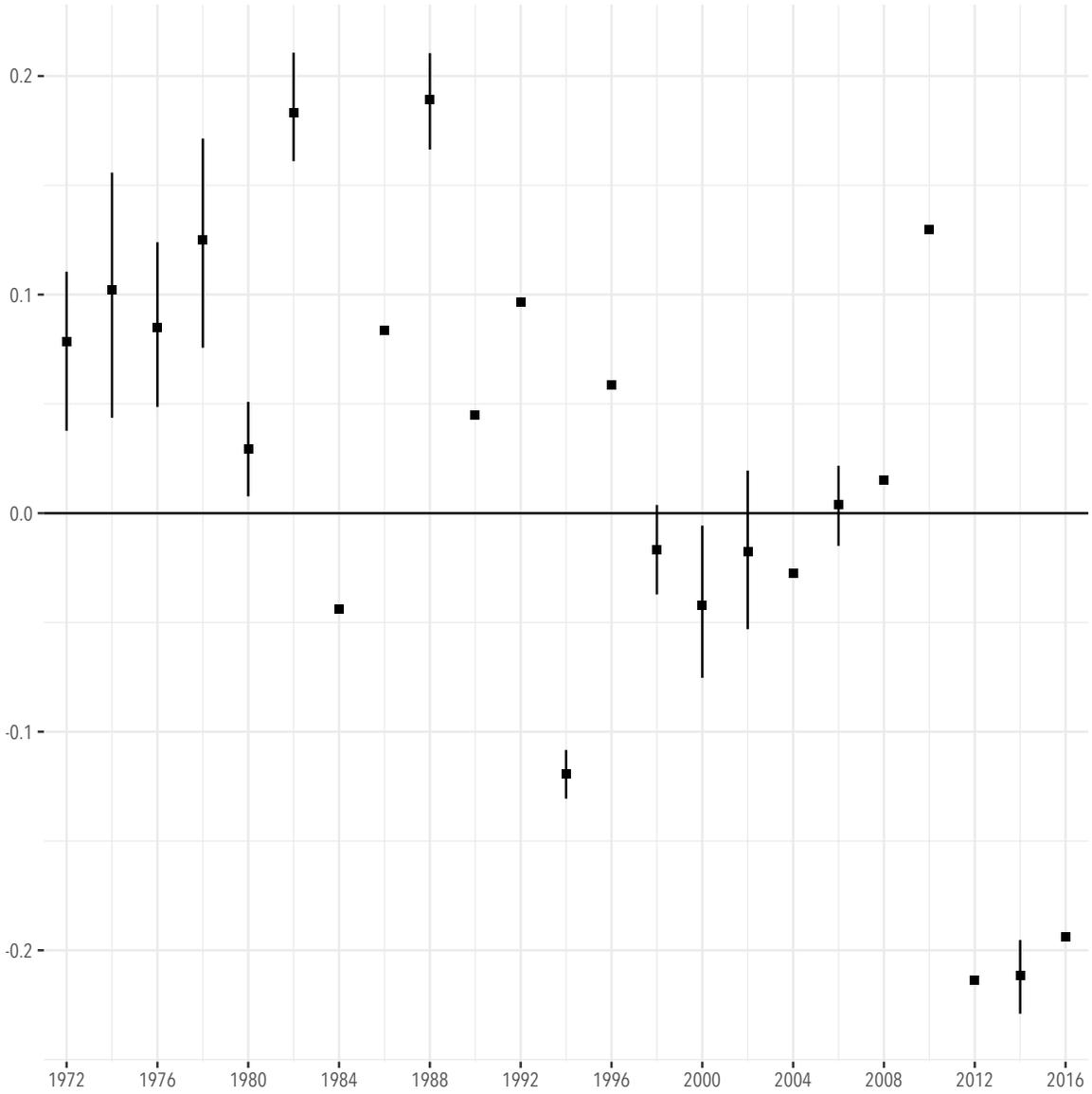


Figure 23: History of efficiency gap estimates in North Carolina, 1972-2016. Vertical lines indicate 95% credible intervals, reflecting uncertainty stemming from imputations for uncontested seats.

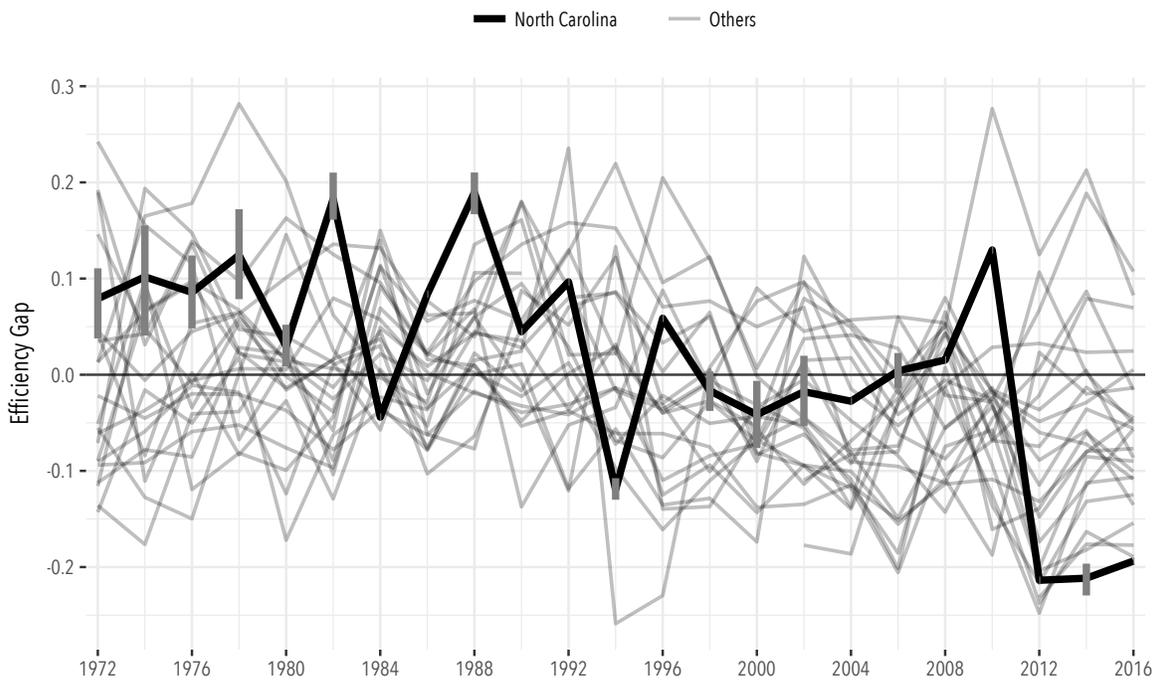


Figure 24: History of efficiency gap estimates in all states, with North Carolina highlighted, 1972-2016. Vertical lines indicate 95% credible intervals, reflecting uncertainty stemming from imputations for uncontested seats.

	2012	2014	2016
Efficiency gap	-0.21	-0.21	-0.19
	[-0.21, -0.21]	[-0.23, -0.20]	[-0.19, -0.19]
Overall rank	12	14	21
	[12, 17]	[9, 22]	[19, 26]
Rank post 2010	5	7	10
	[4, 7]	[3, 9]	[9, 12]
Rank, pro-Repub	7	8	13
	[6, 10]	[5, 13]	[12, 17]
Rank, pro-Repub post 2010	5	6	9
	[4, 6]	[2, 8]	[9, 11]

Table 4: Efficiency gap estimates and rankings, North Carolina 2012, 2014 and 2016. 95% credible intervals in brackets.

- the 5th largest *EG* estimate since 2010, by magnitude (95% CI 4th to 7th); and
- the 7th largest *EG* estimate indicative of Republican advantage (*EG* estimates with a negative sign, 95% CI 6th to 10th).

The 2016 efficiency gap score of -0.194 is slightly smaller than the 2012 and 2014 efficiency gap estimates in North Carolina. The 2016 score is (see Table 4):

- the 21st largest *EG* estimate by magnitude (95% CI 19th to 26th) out of 512 *EG* estimates produced in my analysis;
- the 10th largest *EG* estimate since 2010, by magnitude (95% CI 9th to 12th); and
- the 13th largest *EG* estimate indicative of Republican advantage (*EG* estimates with a negative sign, 95% CI 12th to 17th).

The jump from the *EG* values being recorded towards the end of the previous districting plan in North Carolina (2002-2010) to the 2012 and 2014 values strongly suggests that the districting plan adopted prior to the 2012 Congressional election is the driver of the change, systematically degrading the efficiency with which Democratic votes translate into Democratic seats in North Carolina. This accords with a more general pattern of (a) the correlation between partisan

control of redistricting and the sign and magnitude of the resulting efficiency gaps; and (b) more plans being drawn under Republican control in recent decades.

The historical analysis reported above supports the proposition that North Carolina's *EG* scores are likely to endure over the course of the plan. Few states ever record *EG* scores as large as those observed in North Carolina. Indeed, there is virtually no precedent for the lopsided three election sequence of efficiency gaps generated in North Carolina in 2012, 2014 and 2016 in the data I analyze here (1972-2016). Analysis of the trajectories of efficiency gaps over the lives of redistricting plans strongly suggests that when a districting plan has an initial value as large and as negative as the one observed in North Carolina in 2016, it will continue to produce large, negative efficiency gaps (if left undisturbed), generating seat tallies for Democrats well below those that would be generated from a neutral districting plan.

A handwritten signature in black ink, appearing to read "Simon Jackman". The signature is fluid and cursive, with a long horizontal stroke at the end.

Simon Jackman
March 1, 2017

References

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- Niemi, Richard G. and Simon Jackman. 1991. "Bias and Responsiveness in State Legislative Districting." *Legislative Studies Quarterly* 16:183–202.
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URL: <http://mcmc-jags.sourceforge.net>
- Stephanopoulos, Nicholas O. and Eric M. McGhee. 2015. "Partisan Gerrymandering and the Efficiency Gap." *University of Chicago Law Review* 82:831–900.
- Tufte, Edward R. 1973. "The Relationship Between Seats and Votes in Two-Party Systems." *American Political Science Review* 67:540–554.
- Wood, Simon N. 2006. *Generalized Additive Models: An Introduction with R*. Boca Raton, Florida: Chapman and Hall/CRC.

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EDUCATION

UNIVERSITY OF ROCHESTER Rochester, New York.
Ph.D. in Political Science, 1995. In residence 1989-1991.

PRINCETON UNIVERSITY Princeton, New Jersey.
Visiting doctoral student, Woodrow Wilson School of International and Public Affairs,
1991-94.

UNIVERSITY OF QUEENSLAND St. Lucia, Queensland, Australia.
Bachelor of Arts (with first class Honours in Government) December 1988.

FACULTY APPOINTMENTS

UNITED STATES STUDIES CENTRE, UNIVERSITY OF SYDNEY Sydney, Australia
Professor of Political Science and Chief Executive Officer. April 2016-present.

STANFORD UNIVERSITY Stanford, California.
Professor, Department of Political Science and (by courtesy) Department of Statistics,
September 2007 - 2016.

Associate Professor, Department of Political Science and (by courtesy) Department of
Statistics, September 2002 - August 2007.

Assistant Professor, Department of Political Science, July 1996 - August 2002.

UNITED STATES STUDIES CENTRE, UNIVERSITY OF SYDNEY Sydney, Australia.
Visiting Professor, September 2008-August 2009; June 2010 - June 2013.

UNIVERSITY OF CHICAGO Chicago, Illinois.
Assistant Professor, Department of Political Science, July 1994 to June 1996.

ACADEMIC AND PROFESSIONAL LEADERSHIP

AMERICAN POLITICAL SCIENCE ASSOCIATION
Program chair 2014, Annual Meeting, with Melanie Manion.

AMERICAN NATIONAL ELECTION STUDIES, PRINCIPAL INVESTIGATOR. With Gary Segura (Stanford) and Vince Hutchings and Ted Brader (Univ. Michigan), a principal investigator of the single largest political science research project funded by the National Science Foundation (approximately USD \$10M per 4 year election cycle). Responsible for authoring, fielding and delivering multiple surveys of the American electorate over the 2012 and 2016 election cycles. Responsible to a 25 person Board of Advisors; managing seven project personnel. See <http://www.electionstudies.org>.

SOCIETY FOR POLITICAL METHODOLOGY, PRESIDENT, 2003-05. Led the 2nd largest organized group of the American Political Science Association, with over 900 dues-paying members, a quarterly journal (*Political Analysis*), newsletter (*The Political Methodologist*) and annual conference (over 120 attendees, hosted by Stanford University in 2004). See <http://polmeth.wustl.edu>.

PUBLICATIONS: BOOKS

Bayesian Analysis for the Social Sciences. Wiley: New York. 2009. 600 pages. [Wiley](#); [Amazon](#).

PUBLICATIONS: ARTICLES IN REFEREED JOURNALS

A30. “The Predictive Power of Uniform Swing” *PS: Political Science and Politics*. 2014. [47\(2\):317-321](#).

A29. with Larry Bartels. “A Generational Model of Political Learning”. *Electoral Studies*. 2014. [33: 7-18](#).

A28. with Shanto Iyengar, Solomon Messing, Nicholas Valentino et al. “Do Attitudes About Immigration Predict Willingness to Admit Individual Immigrants?: A Cross-National Test of the Person-Positivity Bias.” *Public Opinion Quarterly*. 2013. [77\(3\): 641-655](#).

A27. with Lynn Vavreck. “Primary Politics: Race, Gender and Age in the 2008 Democratic Primary” *Journal of Elections, Public Opinion and Parties*. 2010. [V20\(2\): 153-186](#).

A26. with Joshua Clinton. “To Simulate or NOMINATE?” *Legislative Studies Quarterly*. 2009. [V34\(4\): 593-621](#).

A25. with Christian Kleiber and Achim Zeileis. “Regression Models for Count Data in R” *Journal of Statistical Software*. 2008. [27\(8\)](#).

A24. with Matt Levendusky and Jeremy Pope. “Measuring District Preferences with Implications for the Study of U.S. Elections” *Journal of Politics*. 2008. [V70\(3\) :736-753](#).

A23. with Shawn Treier. “Democracy as a Latent Variable”. *American Journal of Political Science*. 2008. [V52\(1\): 201-217](#). Winner, Gregory M. Luebbert Prize, Best Article in Comparative Politics Published in 2008 or 2009, American Political Science Association, Organized Section in Comparative Politics.

A22. with Paul M. Sniderman. “The Limits of Deliberative Discussion: A Model of Everyday Political Argument” *Journal of Politics*. 2006. [V68\(2\): 272-283](#). Winner, Best article in the *Journal of Politics* for 2006, Southern Political Science Association.

A21. “Pooling the Polls Over An Election Campaign”. *Australian Journal of Political Science*. 2005. [40\(4\):499-517](#).

A20. with Joshua Clinton and Doug Rivers. “ ‘The Most Liberal Senator’? Analyzing and Interpreting Congressional Roll Calls” *PS: Political Science and Politics*. 2004. [37\(4\):805-811](#). Reprinted in David A. Rochefort (ed.) 2005. *Quantitative Methods in*

Practice: Readings from PS. CQ Press: Washington, DC. pp:104-117.

A19. “What Do We Learn from Graduate Admissions Committees?: A Multiple-Rater, Latent Variable Model, with Incomplete Discrete and Continuous Indicators.” *Political Analysis*. 2004. 12(4): 400-424.

A18. with Joshua Clinton and Doug Rivers. “The Statistical Analysis of Roll Call Data.” *American Political Science Review*. 2004. 98(2):355-370.

A17. “Bayesian Analysis for Political Research” *Annual Reviews of Political Science*. 2004. 7:483-505.

A16. with D. Sunshine Hillygus. “Voter Decision-Making in Election 2000: Campaign Effects, Partisan Activation, and the Clinton Legacy” *American Journal of Political Science*. 2003. 47(4):583-596.

A15. “Multidimensional Analysis of Roll Call Data via Bayesian Simulation: Identification, Estimation, Inference and Model Checking” 2001. *Political Analysis*. 9(3):227-241.

A14. “Estimation and Inference are ‘Missing Data’ Problems: Unifying Social Science Statistics via Bayesian Simulation.” 2000. *Political Analysis*. 8(4):307--332.

A13. “Estimation and Inference via Bayesian Simulation: an introduction to Markov Chain Monte Carlo.” 2000. *American Journal of Political Science*. 44(2):375-404.

A12. “Non-Compulsory Voting in Australia?: what surveys can (and can’t) tell us.” 1999. *Electoral Studies*. 18(1):29-48.

A11. “Correcting Surveys for Non-Response and Measurement Error using Auxiliary Information.” 1999. *Electoral Studies*. 18:7-28.

A10. “Pauline Hanson, the Mainstream, and Political Elites: the place of race in Australian political ideology.” *Australian Journal of Political Science*. 1998. 33:167-186.

A9. with Neal Beck. “Beyond Linearity By Default: Generalized Additive Models.” *American Journal of Political Science*. 1998. 42:596-627.

A8. “Some More of All That: a Reply to Charnock.” *Australian Journal of Political Science*. 1995. 30:347-55.

A7. with Gary Marks. “Forecasting Australian Elections: 1993 And All That.” *Australian Journal of Political Science*. 1994. 29:277-91.

A6. with Bruce Western. “Bayesian Inference for Comparative Research.” *American Political Science Review*. 1994. 88(2):412-23.

A5. “Measuring Electoral Bias: Australia, 1949-1993.” *British Journal of Political Science*. 1994. 24:319-57.

A4. “Split Parties Finish Last: Preferences, Pluralities and the 1957 Queensland election.” *Australian Journal of Political Science*. 1992. 27:434-48.

A3. with Francis Vella. “Electoral Redistricting and Endogenous Partisan Control.”

Political Analysis. 1991. 3:155-71.

A2. with Richard G. Niemi. "Bias and Responsiveness in State Legislative Districting." *Legislative Studies Quarterly*. 1991. 16(2):183-202.

A1. with Richard G. Niemi and Laura R. Winsky. "Candidacies and Competitiveness in Multimember Districts." *Legislative Studies Quarterly*. 1991. 16(1):91-110.

PUBLICATIONS: BOOK CHAPTERS

C13. "Polarization in Less than Thirty Seconds: Continuous Monitoring of Voter Response to Campaign Advertising" (with Shanto Iyengar and Kyu Hahn). Dan Schill, Rita Kirk and Amy E. Jasperson (eds). 2017. *Political Communication in Real Time: Theoretical and Applied Research Approaches*. Routledge: New York.

C12. "Cosmopolitanism" (with Lynn Vavreck). Paul Sniderman and Ben Highton (eds). 2011. *Facing the Challenge of Democracy: Explorations in the Analysis of Public Opinion and Political Participation*. Princeton University Press: Princeton, New Jersey.

C11. "Inference, Classical and Bayesian". Badie, Bertrand, Dirk Berg-Schlosser and Leonardo Morlino (eds). 2011. *International Encyclopedia of Political Science*. Thousand Oaks, California: Sage.

C10. "Measurement". Janet Box-Steffensmeier, Henry Brady and David Collier (eds). 2008. *The Oxford Handbook of Political Methodology*. Oxford: Oxford University Press.

C9. "Bayesian Analysis". Kurian, George T. (ed). *Encyclopedia of Political Science*. 2010. Washington, D.C.: Congressional Quarterly Press.

C8. "Incumbency Advantage and Candidate Quality". Simms, Marian and John Warhurst (eds). 2005. *Mortgage Nation: the 2004 Australian Election*. Perth, Western Australia: API Network. pp: 335-347.

C7. "Bayes Factors", "Bayes Theorem", "Generalized Additive Models" and "Generalized Least Squares". In Michael Lewis-Beck, Alan E. Bryman, and Tim Futing Liao (eds). *SAGE Encyclopedia of Social Science Research Methods*. 2003. Thousand Oaks, California: Sage Publications.

C6. "Political Parties and Electoral Behaviour" In Ian McAllister, Steve Dowrick and Riaz Hassan (eds.) *The Cambridge Handbook of the Social Sciences in Australia*. 2003. Cambridge University Press: Cambridge. pp: 266-286.

C5. with Paul M. Sniderman. "Pluralistic Intolerance, Political Culture and Democratic Theory." In Gérard Grunberg, Nonna Mayer and Paul M. Sniderman (eds.), *Democracy Under Strain (La démocratie à l'épreuve: une nouvelle approche de l'opinion des Français)*. 2002. Presses de Sciences Po: Paris.

C4. with Paul M. Sniderman. "Democratic Discussion: The Role of Reasons and Political Sophistication in Political Argument." In Gérard Grunberg, Nonna Mayer and Paul M. Sniderman (eds.), *Democracy Under Strain (La démocratie à l'épreuve: une nouvelle approche de l'opinion des Français)*. 2002. Presses de Sciences Po: Paris.

- C3.** with Paul M. Sniderman. “The Institutional Organization of Choice Spaces: a political conception of political psychology.” In Kristen Monroe (ed.), *Political Psychology*. 2002. Lawrence Erlbaum: Mahway, New Jersey. pp: 209-224.
- C2.** “Compulsory Voting”, *International Encyclopedia of the Social and Behavioral Sciences*. 2001. Elsevier: Oxford, UK.
- C1.** “Liberalism, Public Opinion, and their Critics: some lessons for defending science.” *Annals of the New York Academy of Sciences*, 1996, 775:346-368. Reprinted in Paul R. Gross and Norman Levitt (eds.), *The Flight from Science and Reason*. 1997. Johns Hopkins University Press: Baltimore, MD. pp:346-368.

OTHER PUBLICATIONS

- O13.** with Micah Altman. “Nineteen ways of looking at statistical software”. *Journal of Statistical Software*. 2011. V42(1).
- O12.** with Peter Brent. “A Shrinking Australian Electoral Roll?” *Democratic Audit of Australia*. 2008.
- O11.** “Data from the Web into R”. *The Political Methodologist* (Newsletter of the Political Methodology Section of the American Political Science Association). 2007. V14(2): 11-15.
- O10.** “Out of Step or Out of Office? (or just a bad election for Republicans)” www.Pollster.com. November 11, 2006.
- O9.** “A Methodological Education in Four Parts (Part III)”. *The Political Methodologist* (Newsletter of the Political Methodology Section of the American Political Science Association). 2004. 12(2):6-11.
- O8.** “R for the Political Methodologist”. *The Political Methodologist* (Newsletter of the Political Methodology Section of the American Political Science Association). 2003. 11(2):20-22.
- O7.** “President Bush, the Public and the 2002 Elections” (with Richard A. Brody). *The Polling Report*. September 2, 2002. V18(17).
- O6.** “Understanding Statistics and Statisticians (a review essay)” *The Political Methodologist* (Newsletter of the Political Methodology Section of the American Political Science Association). 2002. 10(2):19-20.
- O5.** “Calculating and Plotting Confidence Intervals” *The Political Methodologist* (Newsletter of the Political Methodology Section of the American Political Science Association). 1999. 9(1):14-15.
- O4.** “Political Elites and the Mainstream.” *Labor Herald* (newspaper of the Australian Labor Party). May 1997. p3.
- O3.** “Rats and Representation: the Colston defection.” *Current Affairs Bulletin* V73(3). October/November 1996. 23-26.
- O2.** “GAUSS and S-PLUS: a comparison.” *The Political Methodologist* (Newsletter

of the Political Methodology Section of the American Political Science Association). 1994. 6(1):8-13.

01. "Graduate Study in Political Science in the United States." *APSA Newsletter* (Newsletter of the Australasian Political Studies Association). 1992. 59:2-3.

AWARDS AND FELLOWSHIPS

AMERICAN ACADEMY OF ARTS AND SCIENCES Cambridge, Massachusetts
Elected as a Fellow of the Academy, April 2013.

COMPARATIVE POLITICS SECTION, AMERICAN POLITICAL SCIENCE ASSOCIATION
Gregory M. Luebbert Prize, Best Article in Comparative Politics Published in 2008 or 2009, for "Democracy as a Latent Variable" (with Shawn Treier, listed above as peer-refereed article A23).

SOUTHERN POLITICAL SCIENCE ASSOCIATION
The *Journal of Politics* 2006 Best Paper Award, for "The Limits of Deliberative Discussion: A Model of Everyday Political Arguments" (with Paul M. Sniderman; listed above as peer-refereed article A22).

UNIVERSITY OF SYDNEY Sydney, Australia
New South Wales Residency Expatriate Researchers Award, University of Sydney, the New South Wales Department of Education and the Commonwealth Scientific and Industrial Research Organisation. Support for a 12 week visit to the School of Economics and Political Science, University of Sydney, July-September, 2007.

STANFORD UNIVERSITY Stanford, California.
Dean's Award for Distinguished Teaching, School of Humanities and Science, 2000/01.

STANFORD UNIVERSITY Stanford, California.
Victoria Schuck Faculty Scholar, School of Humanities and Sciences. September 2000 to September 2003.

AUSTRALIAN NATIONAL UNIVERSITY Canberra, Australia.
Research Fellow, Department of Political Science and Reshaping Australian Institutions Project, Division of Economics and Politics, Research School of the Social Sciences, August 1996 to September 1997.

PRINCETON UNIVERSITY Princeton, New Jersey.
visiting student, 1991-94, supported by fellowship from Woodrow Wilson School of Public and International Affairs.

RESEARCH GRANTS

NATIONAL SCIENCE FOUNDATION Washington, DC
“The American National Election Studies (ANES), 2010-2013”, with Gary Segura and Vince Hutchings. Award date: January 22, 2010. SES-0937715.

NATIONAL SCIENCE FOUNDATION Washington, DC
“The Politics of Identity and Democratic Values”, with Paul M. Sniderman. June 2001-June 2003.

OFFICE OF TECHNOLOGY LICENSING RESEARCH INITIATIVES Stanford University
“The New Political Methodology: Analysis and Inference via Visualization and Simulation”. June 2000 - June 2002.

NATIONAL SCIENCE FOUNDATION Washington, DC
“Democracy, Toleration, and the Strains of French Politics”, with Paul M. Sniderman. Jan 1999-Jan 2002.

SIQSS Stanford University
Stanford Institute for the Quantitative Study of Society. “Individual Responses to the Lewinsky Affair” (with Richard A. Brody) December 1998-June 1999.

NATIONAL SCIENCE FOUNDATION Washington, DC
“The American National Election Studies (ANES), 2010-2013”, with Gary Segura and Vince Hutchings. Award date: January 22, 2010. SES-0937715.

NATIONAL SCIENCE FOUNDATION Washington, DC
“The Politics of Identity and Democratic Values”, with Paul M. Sniderman. June 2001-June 2003.

OFFICE OF TECHNOLOGY LICENSING RESEARCH INITIATIVES Stanford University
“The New Political Methodology: Analysis and Inference via Visualization and Simulation”. June 2000 - June 2002.

NATIONAL SCIENCE FOUNDATION Washington, DC
“Democracy, Toleration, and the Strains of French Politics”, with Paul M. Sniderman. Jan 1999-Jan 2002.

SIQSS Stanford University
Stanford Institute for the Quantitative Study of Society. “Individual Responses to the Lewinsky Affair” (with Richard A. Brody) December 1998-June 1999.

CONSULTING THE CAMPAIGN LEGAL CENTER Washington, DC
Expert report and testimony for plaintiffs, regarding the districting plan used in Wisconsin’s state legislative elections; 2015-2016, *Whitford v Gill*.

THE GUARDIAN AUSTRALIA, MAY 2013 - SEPTEMBER 2013. Statistical consulting, poll analysis, and commentary, for the 2013 Australian Federal election.

HUFFINGTON POST, 2012-2014. Tracking and forecasting public opinion, voting intentions, over the 2012 presidential election campaign and beyond.

FEDERAL COMMUNICATIONS COMMISSION, 2010-11. Assessing how features of media market (concentration of ownership, number of media outlets) have measurable impacts on public opinion, political engagement and political participation. Merging survey data with characteristics of media-markets; utilized Bayesian hierarchical modeling to assess relationships between media-market characteristics and micro-level public opinion.

POLITICAL INSTABILITY TASK FORCE, 2008. An initiative of the U.S. government. <http://globalpolicy.gmu.edu/pitf>. Developing reliable indicators for forecasting political instability, quantitative assessments of risk of state failure.

“THE BULLETIN” (AUSTRALIAN CONSOLIDATED PRESS), 2007. Tracking public opinion over the Australian 2007 election campaign, integrating betting markets forecasts with polling data, commentary and analysis for the *The Bulletin* magazine and on-line outlets.

INTEGRATED MEDIA MEASUREMENT, INC. 2006-07. Since acquired by Arbitron. Developed tools for the analysis and visualization of media viewing data.

VIRGINIA MODELING ANALYSIS AND SIMULATION CENTER, 2004-2006. Quantitative analysis of social networks.

INTERSURVEY (NOW KNOWLEDGE NETWORKS, INC.) 2000-2001. Authoring and fielding political tracking polls via the Internet, developing statistical algorithms for tracking and forecasting public opinion.

EDITORIAL SERVICE

ANNUAL REVIEW OF POLITICAL SCIENCE, 2005-2013. Associate Editor of an annual monograph reviewing recent research and controversies in political science, published by Annual Reviews (Palo Alto, California); <http://www.annualreviews.org/loi/polisci>.

POLITICAL ANALYSIS, 2010-PRESENT. Associate Editor of specialist journal on development and application of statistical methods in political science contexts. Published by Oxford University Press; <http://pan.oxfordjournals.org>.

EDITORIAL BOARD SERVICE. *American Political Science Review* (current); *American Journal of Political Science*, *Journal of Politics*, *Electoral Studies*, *Australian Journal of Political Science* (current), *Public Opinion Quarterly* (current); *Political Analysis*.

RECENT INVITED LECTURES, SEMINARS AND WORKSHOPS

RESEARCH TRIANGLE INSTITUTE
Durham, North Carolina
“Bayesian Analysis for the Social Sciences”. August 2013.

NUFFIELD COLLEGE, OXFORD Oxford, United Kingdom
“Data Analysis and Inference for Experiments”. Two, three day series of lectures and workshops. July 2013.

TEDx SYDNEY Sydney, Australia
“Politics and the Data Revolution”. May 2013.

INTERNATIONAL POLITICAL SCIENCE ASSOCIATION São Paulo, Brazil
February 2013, week-long series of lectures and workshops, empirical studies of legislative politics.

UNIVERSITY OF TORONTO Toronto, Canada
January 2013. “The Unremarkable Re-election of Barack Obama.”

LAW SCHOOL, STANFORD UNIVERSITY 2012 Conference on Empirical Legal Studies
November 2012, Introduction to Bayesian inference.

EXPERIMENTS IN GOVERNANCE AND POLITICS Stanford, California
November 2012. Roundtable on pre-registration of research designs.

STANFORD ALUMNI ASSOCIATION Stanford in Washington, Washington D.C.
October 2012. “Understanding the 2012 Election.”

TECHNISCHE UNIVERSITÄT DRESDEN Dresden, Germany
Keynote speaker, “Measurement in the Social Sciences”, Symposium on “The Quality of Measurement: Validity, Reliability and its Ramifications for Multivariate Modeling in the Social Sciences.” September 2012.

EXETER UNIVERSITY APSA 2012 meetings, New Orleans
August 2012, Short course on comparative studies of elections and electoral behavior.

UNITED STATES STUDIES CENTRE Sydney, Australia
June 2012, “Small state bias in the U.S. Senate.”

BUSINESS ANALYTICS GROUP, SCHOOL OF BUSINESS, UNIVERSITY OF SYDNEY Sydney, Australia
June 2012, “How Does Obama Match Up?” (with Lynn Vavreck)

UNIVERSITAT AUTÒNOMA DE BARCELONA Barcelona, Spain
June 2012, “Change (or not much of it): dynamics of public opinion in the 2008 U.S. presidential election campaign.”

DEPARTMENT OF STATISTICS, STANFORD UNIVERSITY Stanford, California
May 2012, “How Does Obama Match Up?” (with Lynn Vavreck)

INTERNATIONAL POLITICAL SCIENCE ASSOCIATION São Paulo, Brazil
February 2012, week-long series of lectures and workshops, a practical introduction to Bayesian statistical analysis.

UNITED STATES STUDIES CENTRE Sydney, Australia
July 2011, week-long series of lectures and workshops, introduction to regression analysis in the social sciences (with Bruce Western), part of the SSMART seminars.

UNIVERSITY OF GEORGIA Athens, Georgia
May 2011, “How Does Obama Match Up?” (with Lynn Vavreck)

NEW YORK UNIVERSITY New York, New York
May 2011, “How Does Obama Match Up?” (with Lynn Vavreck)

PRINCETON UNIVERSITY Princeton, New Jersey
April 2011, “Validating Reports of Voter Registration and Turnout in CCAP” (with Lynn Vavreck)

VANDERBILT UNIVERSITY Nashville, Tennessee
February 2011, “Cosmopolitanism” (with Lynn Vavreck)

TEXAS A&M UNIVERSITY College Station, Texas
January 2011, week-long series of lectures and workshops on Bayesian statistical analysis in the social sciences.

FONDACIÓN JUAN MARCH Madrid, Spain
November 2010, week-long series of lectures and workshops on Bayesian statistical analysis in the social sciences.

UNIVERSITY OF ESSEX Wivenhoe Park, England
August 2010, week-long series of lectures and workshops on Bayesian statistical analysis in the social sciences, part of the Essex Summer School in Quantitative Methods in the Social Sciences.

Earlier invited lectures, seminars and workshops: Yale University (March 2004; March 2009), Harvard University (February 2004; June 2008; December 2008), University of Minnesota, Minneapolis (November 1998), University of California, Berkeley (April 1998), University of California, Davis (February 2003), University of California, Los Angeles (April 2000, April 2009), University of California, San Diego (November 1997; April 1998; February 2001), University of California, Santa Barbara (May 2000; May 2003), University of Iowa (October 2006), Old Dominion University (January 2005), New York University (May 2002; November 2005), Nuffield College, Oxford University (November 2007); University of Pittsburgh (March 2002), Pennsylvania State University (April 2005), Princeton University (September 2000, December 2001, May 2008, March 2009), University of Houston (February 2001), Stanford University Statistics Department (May 2001, April 2005, April 2009, May 2012), Texas A&M University (February 1996), Tel Aviv University (November 1994), University of Washington (April 2002), University of Queensland (October 1992, May 1997), University of Sydney (July 2007, July 2009, June 2010, June 2012) and the Australian Parliamentary Library (Parliament House, Canberra, September 1997).

CONTRIBUTED SOFTWARE

`pssc1`: a package of classes and methods for R developed in the Political Science Computational Laboratory, Stanford University, developed with the assistance of Christina Maimone and Alex Tahk. Last updated on the Comprehensive R Archive Network: March 2011.

TEACHING

AMERICAN POLITICS Political parties, elections, campaigns, electoral behavior and political participation, public opinion.

POLITICAL METHODOLOGY Scope and methods of political science, mathematics for social scientists, foundations and history of statistical inference, introductory through advanced econometrics and data analysis, Bayesian approaches to econometrics and statistics, models for measurement of political phenomena, statistical computing and graphical displays of data.

COMPARATIVE POLITICS: Democratic political systems (constitutions, electoral systems), comparative electoral behavior and public opinion, comparative political economy, research methods for comparative politics.

SERVICE TO THE PROFESSION

Program chair designate, American Political Science Association, 2014.

Principal Investigator, American National Election Studies, 2009-2013. With Gary Segura and Vince Hutchings.

Reviewer, National Research Council, Report on “Non-Response in Social Science Data Collection: A Research Agenda”, August 2012.

International Academic Advisory Board, United States Studies Centre, University of Sydney, 2010-present.

Chair, Emerging Scholar Award Committee, Society for Political Methodology, 2011-2012.

American Political Science Association, James Madison Award Committee, 2011.

American Political Science Association, Task Force on Democracy Audits and Governmental Indicators, 2010-2011.

Program Committee, Annual Summer Meeting of the Society for Political Methodology, 2009.

Distinguished Career Achievement Award Committee, Society for Political Methodology, 2008.

Chair, Distinguished Career Achievement Award Committee, Society for Political Methodology, 2007.

Editorial Board, *Political Science Network*, 2007-present.

Program Committee, UseR! Conference, Vienna, Austria, June 2006.

Program Committee, Annual Summer Meetings of the Political Methodology Section of the American Political Science Association, 2004.

President, Society for Political Methodology and the Political Methodology Section of the American Political Science Association, 9/2003-9/2005.

Program Committee, Annual Summer Meetings of the Political Methodology Section of the American Political Science Association, 2003.

Board of Overseers, American National Election Studies, September 2002 - October 2005.

Faculty Associate, Empirical Implications of Theoretical Models Workshop, Washington University, St Louis: 2002, 2003, 2005, 2006, 2007, 2009, 2010.

Vice-President, Political Methodology Section of the American Political Science Association, 2001-2003

Chair, program committee, Annual Summer Meetings of the Political Methodology Section of the American Political Science Association, 2002

referee, National Science Foundation.

lecturer, "Bayesian Modeling for the Social Sciences" at the 1998, 1999, 2000 and 2001 ICPSR Summer Schools in Quantitative Methods, Hubert M. Blalock Memorial Lecture Series: Advanced Topics in Social Research -- Frontiers of Quantitative Methods

lecturer, short course in Bayesian statistics at the Annual Meetings of the American Political Science Association, Boston, Massachusetts, September 2, 1998.

selection committee, 1998 summer meetings of the Political Methodology Society, University of California, San Diego

contributor, *The Political Methodologist*, Newsletter of the Methodology Section of the American Political Science Association.

contributor, *Newsletter* of the Australasian Political Studies Association.

DEPARTMENT AND UNIVERSITY SERVICE

Graduate Admissions Committee, Department of Statistics 2012-13.

Director, Stanford Center for American Democracy.

Graduate Admissions Committee, Department of Political Science, 2011-12.

Director, Method of Analysis Program in the Social Sciences, 2007-2010.

Chair, American Politics Search Committee, Department of Political Science, 2007/08.

Chair, Graduate Admissions Committee, Department of Political Science, 2006/07, 2007/08.

Method of Analysis in the Social Sciences, Steering Committee, 2005-07.

University Committee on Academic Computing and Information Systems, 2004-07.
Curriculum Committee, 2002-2006.
Chair, Political Methodology Search Committee, 2002/03.
Chair, Dean's Committee on Social Science Computing, 2002-03.
Committee on Social Science Computing, 2001/02
Political Methodology Search Committee, 2001/02
Graduate Admissions Committee, 1997/98, 1998/99, 1999/00.
Field Convener, American Politics 1997/98, 1998/99.
Field Convener, Political Methodology, 1999/2000.
American Politics Search Committee, 1998/99.
Science, Technology and Society Search Committee, 1998/99.
Convener, Honors College, September 1999.
International Relations Search Committee, 1999/2000.
Computer Network Administrator, Department of Political Science, 1999/2000.
Ad-hoc Committee on the Department's move to Encina Hall, 1999/2000.
Co-Convenor, Department Speaker Series, 1999/2000.
Presentation to the Senate Committee on Academic Computing and Information Systems, February 26, 2001.
Presentations to Stanford Alumni Associations, Los Gatos (September 2000), Monterey Bay (October 2000), Sydney, Australia (January 2001), Boston, MA (October 2010).
Graduate Admissions and In-Residence Student Evaluation Committee, Department of Political Science, University of Chicago, 1994/95, 1995/96.