IN THE UNITED STATES DISTRICT COURT
FOR THE WESTERN DISTRICT OF WISCONSIN

WILLIAM WHITFORD, ROGER ANCLAM, EMILY BUNTING, MARY LYNNE DONOHUE, HELEN HARRIS, WAYNE JENSEN, WENDY SUE JOHNSON, JANET MITCHELL, ALLISON SEATON, JAMES SEATON, JEROME WALLACE, and DONALD WINTER,	)	No. 15-cv-421-bbc
Plaintiffs,	)	
v.	)	
GERALD C. NICHOL, THOMAS BARLAND, JOHN FRANKE, HAROLD V. FROEHLICH, KEVIN J. KENNEDY, ELSA LAMELAS, and TIMOTHY VOCKE,	)	
Defendants.	)	

DECLARATION OF SIMON DAVID JACKMAN

I, Simon David Jackman, pursuant to 28 U.S.C. § 1746, hereby declare as follows:

1. I am one of Plaintiffs' expert witnesses in the above captioned action. I make this declaration based on my personal knowledge and in support of the Plaintiffs' Opposition to the Defendants' Motion for Summary Judgment.

2. Attached as Exhibit A is a true and correct copy of the Expert Report I submitted in this case. This report is a true and correct statement of my opinions and conclusions, applying the principles of my academic discipline and scholarship in the field to a reasonable degree of scientific certainty.

3. Attached as Exhibit B is a true and correct copy of my curriculum vitae.

4. Attached as Exhibit C is a true and correct copy of the Rebuttal Report I submitted in this case. This report is a true and correct statement of my opinions and conclusions, applying the principles of my academic discipline and scholarship in the field to a reasonable degree of scientific certainty.

5. Attached as Exhibit D is a true and correct copy of a document I created called "Sensitivity of the Efficiency Gap to Uniform Swing," setting out the calculations I relied on in drawing my conclusions in my Rebuttal Report (Exhibit B).

6. Attached as Exhibit E is a true and correct table showing the Michigan Senate Results for 1994, 1996, 1998, 2002, 2006, 2008, 2010, and 2014, drawn from the Klarner database that I relied on in preparing my expert reports in this case.

7. Attached as Exhibit F is a true and correct table showing party control of redistricting, that I relied on in preparing my Rebuttal Report, provided to me by counsel.

8. Attached as Exhibit G is a true and correct copy of an article I relied on in drafting my expert reports: Eric McGhee, *Measuring Partisan Bias in Single-Member District Electoral Systems*, 39 Legis. Stud. Q. 55 (2014).

9. Attached as Exhibit H is a true and correct copy of an article I relied on in drafting my expert reports: Benjamin Fifield et al., *A New Automated Redistricting Simulator Using Markov Chain Monte Carlo* (2015).

10. Attached as Exhibit I is a true and correct copy of an article I relied on in drafting my expert reports: Andrew Gelman & Gary King, *Estimating the Consequences of Electoral Redistricting,* 85 J. Am. Stat. Ass'n 274 (1990).

11. Attached as Exhibit J is a true and correct copy of a book introduction I read as background for my expert reports: Gary W. Cox & Jonathan N. Katz, *Elbridge Gerry's Salamander* 3-11 (2002).

12. Attached as Exhibit K is a true and correct copy of an article I read as background for my expert reports: Brue E. Cain, *Assessing the Partisan Bias Effects of Redistricting*, 79 Am. Pol. Sci. Rev. 320 (1985).

I declare under penalty of perjury that the foregoing is true and correct.

Dated this 22nd day of January, 2016.

SIMON DAVID JACKMAN

Assessing the Current Wisconsin State Legislative Districting Plan

Simon Jackman

July 7, 2015

Contents

1 Introduction

My name is Simon Jackman. I am currently a Professor of Political Science at Stanford University, and, by courtesy, a Professor of Statistics. I joined the Stanford faculty in 1996. I teach classes on American politics and statistical methods in the social sciences.

I have been asked by counsel representing the plaintiffs in this lawsuit (the "Plaintiffs") to analyze relevant data and provide expert opinions in the case titled above. More specifically, I have been asked

- to determine if the current Wisconsin legislative districting plan constitutes a partisan gerrymander;
- to explain a summary measure of a districting plan known as "the efficiency gap" (Stephanopolous and McGhee, 2015), what it measures, how it is calculated, and to assess how well it measures partisan gerrymandering;
- to compare the efficiency gap to extant summary measures of districting plans such as partisan bias;
- to analyze data from state legislative 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 suggest a threshold or other measure that can be used to determine if a districting plan is an extreme partisan gerrymander;
- to describe how the efficiency gap for the Wisconsin districting plan compares to the values of the efficiency gap observed in recent decades elsewhere in the United States;
- to describe where the efficiency gap for the current Wisconsin districting plan lies in comparison with the threshold for determining if a districting plan constitutes an extreme partisan gerrymander.

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 state legislative elections, 1967 to the present available from the Inter-University Consortium for Political and Social Research (ICPSR study number 34297); I use a release of the data updated through 2014, maintained by Karl Klarner (Indiana State University and Harvard University).
- presidential election returns, 2000-2012, aggregated to state legislative districts.

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 am being compensated at a rate of $250 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 doesn't 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. *EG* can be computed directly from a given election's results, without recourse to extensive statistical modeling or assumptions about counter-factual 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 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 we'd expect 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 $EG > 0$ the converse.

4. A districting plan in which *EG* is consistently observed to be positive is evidence that the plan embodies a pro-Democratic gerrymander; the magnitudes of the *EG* measures speak to the severity of the gerrymander. Conversely, a districting plan with consistently negative values of the efficiency gap is consistent with the plan embodying a pro-Republican gerrymander.

5. **Performance of the efficiency gap in 786 state legislative elections.** My analysis of 786 state legislative elections (1972-2014) examines properties of the efficiency gap. *EG* is estimated with some uncertainty in the presence of uncontested districts (and uncontested districts are quite prevalent in state legislative elections), but this source of uncertainty is small relative to differences in the *EG* across states and across districting plans.

6. **Stability of the efficiency gap.** *EG* is stable in pairs of temporally adjacent elections held under the same districting plan. In 580 pairs of consecutive

EG measures, the probability that each *EG* measure has the same sign is 74%. In 141 districting plans with three or more elections, 35% have a better than 95% probability of *EG* being negative or positive for the entire duration of the plan; in about half of the districting plans the probability that *EG* doesn't change sign is above 75%.

7. **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, *none* were recorded after 2000.

8. **The current Wisconsin state legislative districting plan** (the "Current Wisconsin Plan"). In Wisconsin in 2012, the average Democratic share of district-level, two-party vote (V) is estimated to be 51.4% ($\pm$0.6, the uncertainty stemming from imputations for uncontested seats); recall that Obama won 53.5% of the two-party presidential vote in Wisconsin in 2012. Yet Democrats won only 39 seats in the 99 seat legislature (S = 39.4%), making Wisconsin one of 7 states in 2012 where we estimate $V > 50\%$ but $S < 50\%$. In Wisconsin in 2014, V is estimated to be 48.0% ($\pm$0.8) and Democrats won 36 of 99 seats (S = 36.4%).

9. Accordingly, Wisconsin's *EG* measures in 2012 and 2014 are large and negative: -.13 and -.10 (to two digits of precision). The 2012 estimate is the largest *EG* estimate in Wisconsin over the 42 year period spanned by this analysis (1972-2014).

10. Among 79 *EG* measures generated from state legislative elections after the 2010 round of redistricting, Wisconsin's *EG* scores rank 9th (2012, 95% CI 4 to 13) and 18th (2014, 95% CI 14 to 21). Among 786 *EG* measures in the 1972-2014 analysis, the magnitude of Wisconsin's 2012 *EG* measure is surpassed by only 27 (3.4%) other cases.

11. Analysis of efficiency gaps measures in the post-1990 era indicates that conditional on the magnitude of the Wisconsin 2012 efficiency gap (the first election under the Current Wisconsin Plan), there is a 100% probability

that *all subsequent elections* held under that plan will also have efficiency gaps disadvantageous to Democrats.

12. **The Current Wisconsin Plan presents overwhelming evidence of being a pro-Republican gerrymander**. In the entire set of 786 state legislative elections and their accompanying *EG* measures, there are *no precedents* prior to this cycle in which a districting plan generates an initial two-election sequence of *EG* scores that are each as large as those observed in *WI*.

13. The Current Wisconsin Plan is generating *EG* measures that make it *extremely likely* that it has a systematic, historically large and enduring, pro-Republican advantage in the translation of votes into seats in Wisconsin's state legislative elections.

14. **An actionable threshold based on the efficiency gap.** Historical analysis of the relationship between the first *EG* measure we observe under a new districting plan and the subsequent *EG* measures lets us assess the extent to which that first *EG* estimate is a *reliable* indicators of a *durable* and hence *systematic* feature of the plan. In turn, this let us assess the *confidence* associated with a range of possible *actionable EG thresholds*.

15. My analysis suggests that *EG* greater than .07 in absolute value be used as an actionable threshold. Relatively few plans produce a first election with an *EG* measure in excess of this threshold, and of those that do, the historical analysis suggests that most go on to produce a sequence of *EG* estimates indicative of systematic, partisan advantage consistent with the first election *EG* estimates, At the 0.07 threshold, 95% of plans would be either (a) undisturbed by the courts, or (b) struck down because we are sufficiently confident that the plan, if left undisturbed, would go on to produce a one-sided sequence of *EG* estimates, consistent with the plan being a partisan gerrymander. In short, our "confidence level" in the 0.07 threshold is 95%.

16. **The Current Wisconsin Plan is generating estimates of the efficiency gap far in excess of this proposed, actionable threshold**. In 2012 elections to the Wisconsin state legislature, the efficiency gap is estimated to be -.13; in

2014, the efficiency gap is estimated to be -.10. Both measures are separately well beyond the conservative .07 threshold suggested by the analysis of efficiency gap measures observed from 1972 to the present.

A vivid, graphical summary of my analysis appears in Figure 1, showing the average value of the efficiency gap in 206 districting plans, spanning 41 states and 786 state legislative elections from 1972 to 2014. The Current Wisconsin Plan has been in place for two elections (2012 and 2014), with an average efficiency gap of -.115. 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. Note that *only four other districting plans have lower average efficiency gap scores than the Current Wisconsin Plan*, and these are also from the post-2010 round of redistricting. That is, Wisconsin's current plan is generating the 5th lowest average efficiency gap observed in over 200 other districting plans used in state legislative elections throughout the United States over the last 40 years. The analysis I report here documents why the efficiency gap is a valid and reliable measure of partian gerrymandering and why are confident that the current Wisconsin plan exceeds even a conservative definition of partisan gerrymandering.

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).

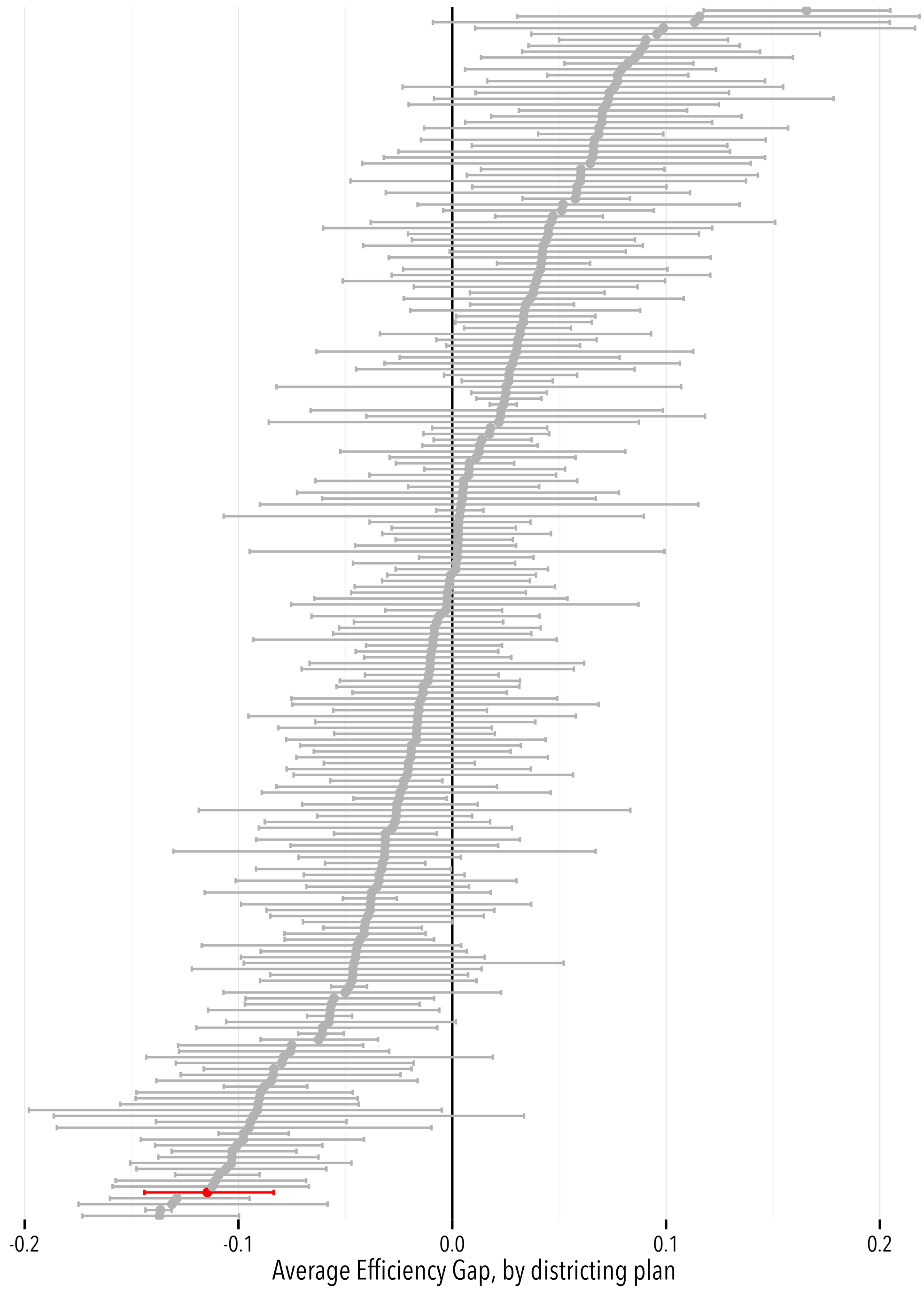

Figure 1: Average efficiency gap score, 206 districting plans, 1972-2014. Plans have been sorted from low average *EG* scores to high. Horizontal lines cover 95% confidence intervals. Negative efficiency gap scores are plans that disadvantage Democrats; positive efficiency gap scores favor Democrats. The Current Wisconsin Plan is shown in red. See also Figure 36.

There may be elections where a party wins a majority of seats (and control of the jurisdiction's legislature) despite not winning a majority of votes: $S > .5$ while $V < .5$ and vice-versa. In fact, there are numerous instances of mismatches between the party winning the statewide vote and the party controlling the state legislature in recent decades. I estimate that since 1972 there have been 63 cases of Democrats winning a majority of the vote in state legislative elections, while not winning a majority of the seats, and 23 cases of the reverse phenomenon, where Democrats won a majority of the seats with less than 50% of the statewide, two-party vote.

Geographic clustering of partisans is typically a prerequisite for partisan gerrymandering. This is nothing other than partisan "packing": a gerrymandered districting plan creates a relatively small number of districts that have unusually large proportions of partisans from party *B*. The geographic concentration of party *B* partisans might make creating these districts a straightforward task. In other districts in the jurisdiction, party *B* supporters never (or seldom) constitute a majority (or a plurality), making those districts "safe" for party *A*. This districting plan helps ensure party *A* wins a majority of seats even though party *B* has a majority of support across the jurisdiction, or at the very least, the districting plan helps ensures that party *A*'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 *A* to "more efficiently" translate its votes into seats, relative to the way the plan translates party *B*'s votes into seats. This nomenclature is telling, as we will see when we consider the *efficiency gap* measure, below.

Assessing the partisan fairness of a districting plan is fundamentally about measuring 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, crossing the $(V, S) = (.5, .5)$ point: i.e., under PR, $S = V$ and a party that wins 50% of the vote will be allocated 50% of the seats. Absent a deterministic allocation rule like pure PR, seats-votes curves are most usefully thought of in probabilistic terms, due to the fact that there are many possible configurations of district-specific outcomes corresponding to a given jurisdiction-wide V, and hence uncertainty — represented by a probability *distribution* — over possible values of S given V.

In single-member, simple plurality (SMSP) systems, we often see non-linear, "S"-shaped seats-votes curves. With an approximately symmetric mix of districts (in terms of partisan leanings), large changes in seat shares (S) can result from relatively small changes in votes shares (V) at the middle of the distribution of district types. This presumes a districting plan such that both parties have a small number of "strongholds," with extremely large changes in vote shares needed to threaten these districts, and so the seats-votes curve tends to "flatten out" as jurisdiction-wide vote share (V) takes on relatively large or small values. Other shapes are possible too: e.g., bipartisan, incumbent-protection plans generate seats-votes curves that are largely flat for most values of V, save for the constraint that the curve run through the points $(V, S) = (0, 0)$ and $(1, 1)$; i.e., relatively large movements in V generates relatively little change in seats shares.

[1]The curve labeled "Cube Law" in Figure 2 is generated assuming that $S/(1-S) = [V/(1-V)]^3$, an approximation for the lack of proportionality we observe in single-member district systems, though hardly a "law."

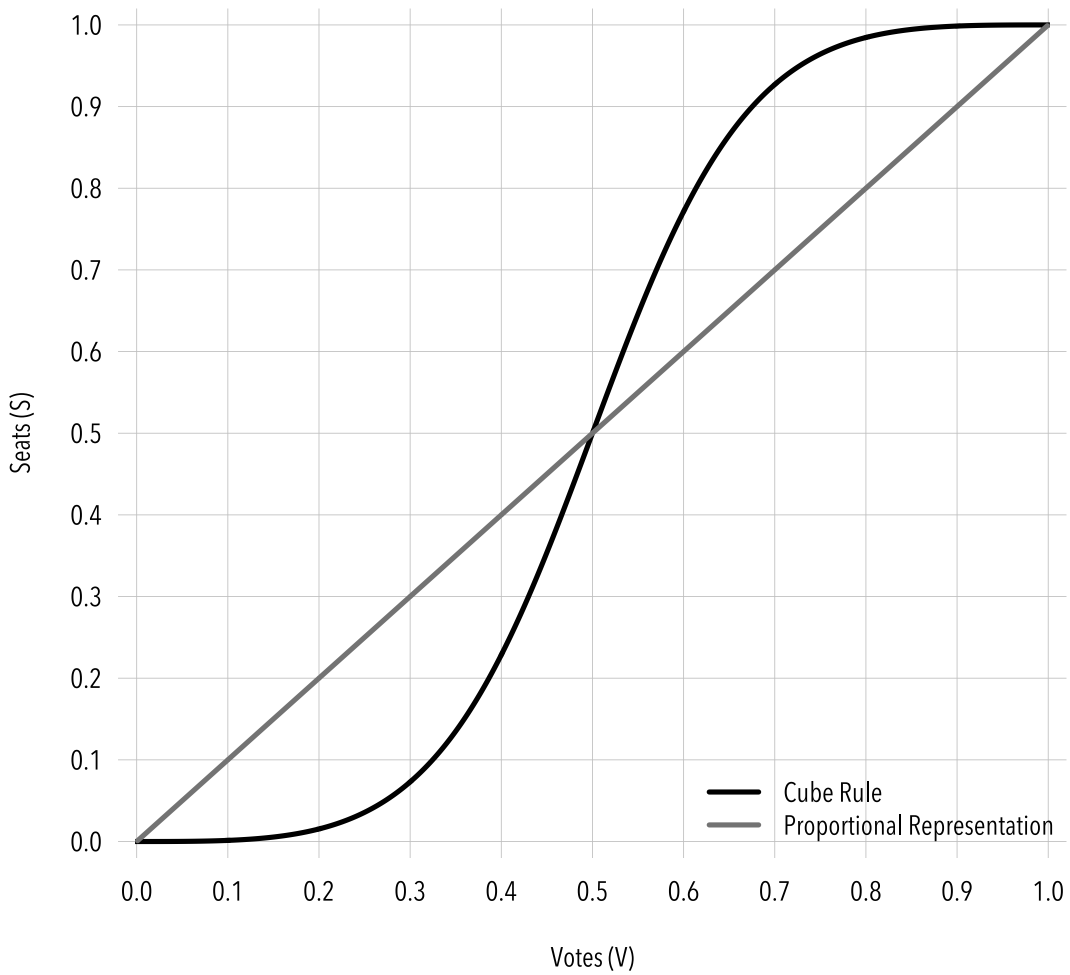

Figure 2: Two Theoretical Seats-Votes Curves

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 $V = .5$. Formally, symmetry of the seats-vote curve is the condition 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. 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* then 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 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 redistricting plan before the plan can be properly assessed. Indeed, few plans stay intact long enough to permit reliable analysis in

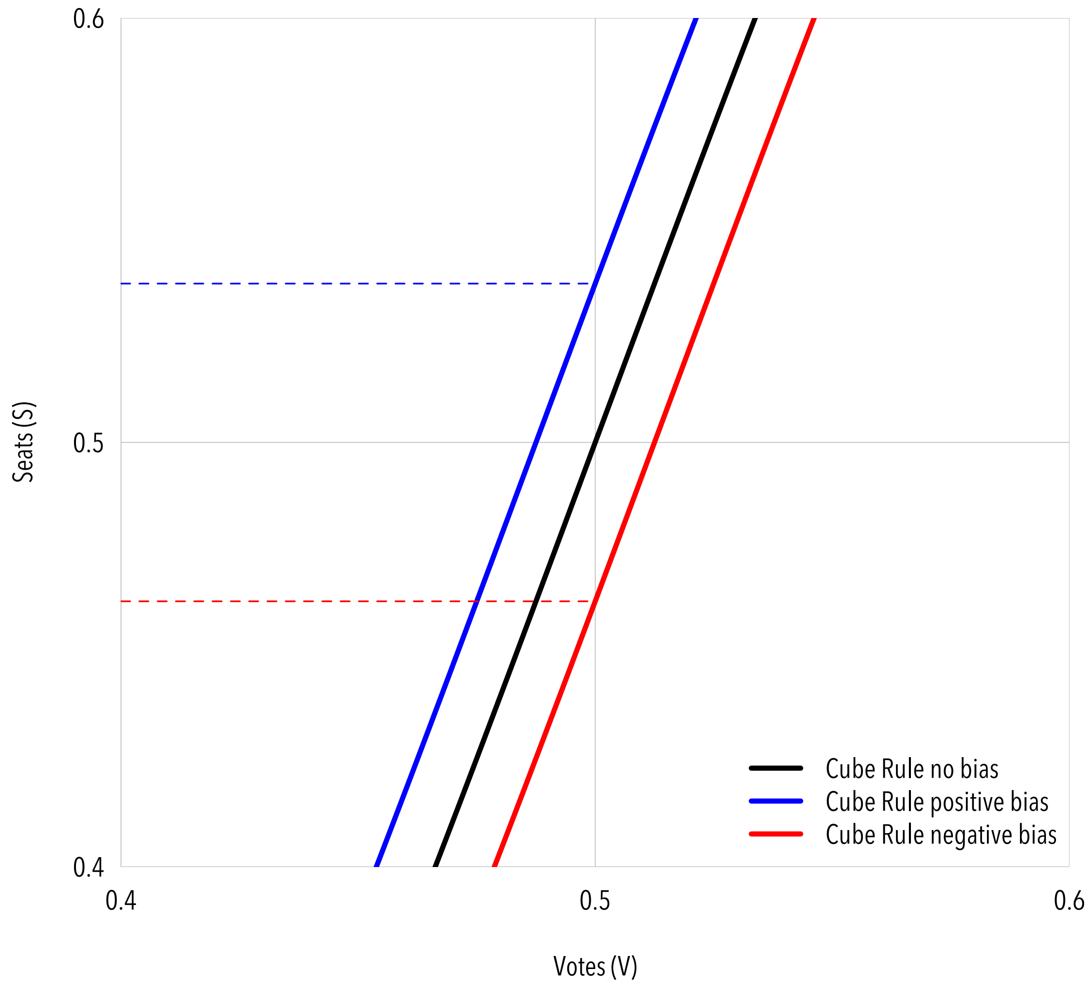

Figure 3: Theoretical seats-votes curves, with different levels of partisan bias. This graph is "zoomed in" on the region $V \in [.4, .6]$ and $S \in [.4, .6]$; the seats-votes "curves" are approximately linear in this region.

this way. State-level plans in the United States might generate as many five elections between decennial censuses. Accordingly, many uses of the "multi-year" method pool multiple plans and/or across jurisdictions, so as to estimate average 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 and so partisan bias can't be estimated without further assumptions. Absent *any* actual elections under the plan, we 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. 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); 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 we can define V 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 we might observe 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 with which we can estimate 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 we must contemplate (when assessing the partisan bias of a districting plan) becomes all the more speculative.

In no small measure this is a marketing failure, of sorts. Partisan bias (at least under the uniform swing assumption) is essentially a measure of skew or asymmetry in *actual* vote shares. Partisan bias garners great rhetorical and normative appeal by directing attention to what happens at $V = .5$; it seems only “fair” that if a party wins 50% or more of the vote it should expect to win a majority of the districts.

Yet this distracts us from the fact that *asymmetry* in the distribution of vote shares across districts is the key, operative feature of a districting plan, and the extent to which it advantages one party or the other. Critically, we need not make appeals to counter-factual, hypothetical elections in order to assess this asymmetry.

6 The Efficiency Gap

The efficiency gap (EG) is also an asymmetry measure, as we see below. 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.

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 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: any districts with $v_i < .5$ generate no seats.

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 $v_i > .5$) and a larger number of districts that disperse votes through districts won by party B (“cracking” with $v_i < .5$). 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 also closely tied to asymmetry in the distribution of v_i.

Some notation will help make the point more clearly. If $v_i > .5$ 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.

6.1 The efficiency gap when districts are of equal size

Under the assumption of equally sized districts McGhee (2014, 80) re-expresses the efficiency gap as:

$$EG = S - .5 - 2(V - .5) \tag{1}$$

recalling that $S = n^{-1} \sum_{i=1}^{n} s_i$ is the proportion of seats won by party A and $V = n^{-1} \sum_{i=1}^{n} v_i$ is the proportion of votes won by party A.

The assumption of equally-sized districts is especially helpful for the analysis reported below, since the calculation of EG in a given election then reduces to using the jurisdiction-level quantities S and V as in equation 1. For the analysis of historical election results reported below, it isn't possible to obtain measures of district populations, meaning that we really have no option other than to rely on the jurisdiction-level quantities S and V when estimating the EG.

I operationalize V as the average (over districts) of the Democratic share of the two-party vote, in seats won by either a Democratic or Republican candidate;

this set of seats includes uncontested seats, where I will use imputation procedures to estimate two-party vote share. If districts are of equal size (and ignoring seats won by independents and minor party candidates) then this average over districts will correspond to the Democratic share of the state-wide, two-party vote.

6.2 The seats-vote curve when the efficiency gap is zero

This simple expression for the efficiency gap implies that *if the efficiency gap is zero*, we obtain a particular type of seats-votes curve, shown in Figure 4:

1. the seats-votes curve runs through the 50-50 point. If the jurisdiction wide vote is split 50-50 between party A and party B then with an efficiency gap of zero, $S = .5$.

2. conditional on $V = .5$ (an even split of the vote), the efficiency gap is the same as partisan bias: $V = .5 \iff EG = S - .5$, the seat share for party A in excess of 50%. That is, the efficiency gap reduces to partisan bias *under the counter-factual scenario* $V = .5$ that the partisan bias measure requires us to contemplate. On the other hand, the efficiency gap is not premised on that counter-factual holding, or any other counter-factual for that matter; the efficiency gap summarizes the distribution of observed district-level vote shares v_i.

3. the seats-votes curve is linear through the 50-50 point with a slope of 2. That is, with $EG = 0$, $S = 2V - .5$. Or, with a zero efficiency gap, each additional percentage point of vote share for party A generates *two* additional percentage points of seat share. A zero efficiency gap does not imply proportional representation (a seats-votes that is simply a 45 degree line).

4. a party winning 25% or less of the jurisdiction-wide vote should win zero seats under a plan with a zero efficiency gap; a party winning 75% or more of the jurisdiction-wide vote should win all of the seats under a plan with a zero efficiency gap. This is a consequence of the "2-to-1" seats/vote ratio and the symmetry implied by a zero efficiency gap. A party that wins an extremely low share of the vote ($V < .25$) can only be winning any seats if it enjoys an efficiency advantage over its opponent.

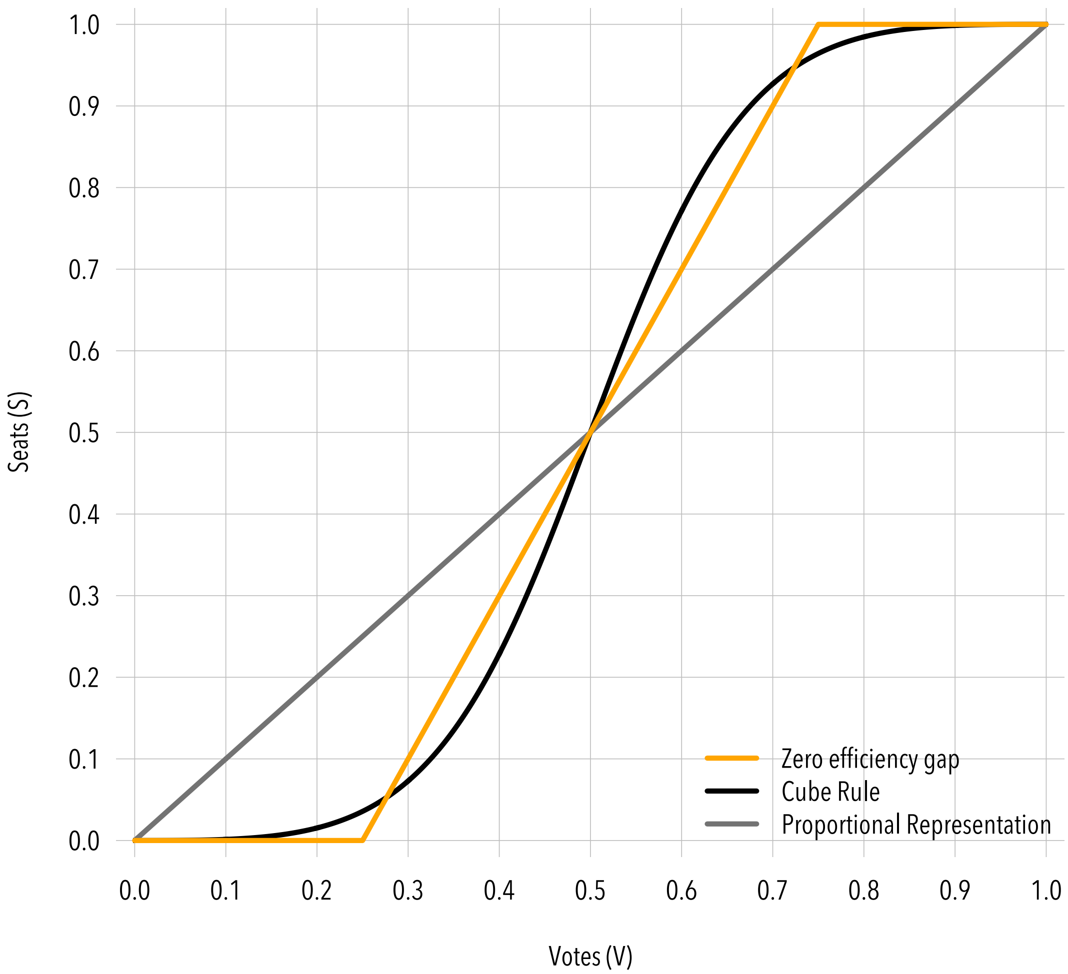

Figure 4: Theoretical seats-votes curves. The $EG = 0$ curve implies that (a) a party winning less than $V = .25$ jurisdiction-wide should not win any seats; (b) symmetrically, a party winning more than $V = .75$ jurisdistion-wide should win all the seats; and (c) the relationship between seat shares S and vote shares V over the interval $V \in [.25, .75]$ is a linear function with slope two (i.e., for every one percentage point gain in vote share, seat share should go up by two percentage points).

Moreover, the efficiency gap is trivial to compute once we have V and S for a given election. We don't need a sequence of elections under a plan in order to compute EG, nor do we need to anchor ourselves to a counter-factual scenario such as $V = .5$ as we do when computing partisan bias. For any given observed V, the hypothesis of zero efficiency gap tells us what level of S to expect.

6.3 The efficiency gap as an excess seats measure

In this sense the efficiency gap can be interpreted even more simply as an "excess seats" measure. Recall that $EG = 0 \iff S = 2V - .5$. In a given election we observe $EG = S - .5 - 2(V - .5)$. The efficiency gap can be computed by noting how far the observed S lies above or below the orange line in Figure 4.

A positive EG means "excess" seats for party A relative to a zero efficiency gap standard given the observed V in that election; conversely, a negative EG mean a deficit in seats for party A relative to a zero efficiency gap standard given the observed V.

7 State legislative elections, 1972-2014

We estimate the efficiency gap in state legislative elections over a large set of states and districting plans, covering the period 1972 to 2014. We begin the analysis in 1972 for two primary reasons: (a) state legislative election returns are harder to acquire prior to the mid-1960s, and not part of the large, canonical data collection we rely on (see below); and (b) districting plans and sequences of elections from 1972 onwards can be reasonably considered to be from the post-malapportionment era.

For each election we recover an estimate of the efficiency gap based on the election results actually observed in that election. To do this, I compute two quantities for each election:

1. V, the statewide share of the two-party vote for Democratic candidates, formed by averaging the district-level election results v_i (the Democratic share of the two-party vote in district i) in seats won by major party candidates, including uncontested seats, and

2. *S*, the Democratic share of seats won by major parties.

Recall that these quantities are the inputs required when computing the efficiency gap (equation 1).

The analysis that follows relies on a data set widely used in political science and freely available from the Inter-University Consortium for Political and Social Research (ICPSR study number 34297). The release of the data I utilize covers state legislative election results from 1967 to 2014, updated by Karl Klarner (Indiana State University and Harvard University). I subset the original data set to general election results since 1972 in states whose lower houses are elected via single-member districts, or where single-member districts are the norm. Multi-member districts "with positions" are treated as if they are single-member districts.

Figure 5 provides a graphical depiction of the elections that satisfy the selection criteria described above.

- Arizona, Idaho, Louisiana, Maryland, Nebraska, New Hampshire, New Jersey, North Dakota and South Dakota all drop out of the analysis entirely, because of exceedingly high rates of uncontested races, using multi-member districts, non-partisan elections, or the use of a run-off system (Louisiana).

- Alaska, Hawaii, Illinois, Indiana, Kentucky, Maine, Minnesota, Montana, North Carolina, Vermont, Virginia, West Virginia and Wyoming do not supply data over the entire 1972-2014 span; this is sometimes due to earlier elections being subject to exceedingly high rates of uncontestedness, the use of multi-member districts or non-partisan elections.

- Alabama and Mississippi have four-year terms in their lower houses, contributing data at only half the rate of the vast bulk of states with two-year legislative terms.

- Twenty-three states supply data every two years from 1972 to 2014, including Michigan and Wisconsin.

- Data is more abundant in recent decades. For the period 2000 to 2014, 41 states contribute data to the analysis at two or four year intervals.

In summary, the data available for analysis span 83,269 district-level state legislative contests, from 786 elections across 41 states.

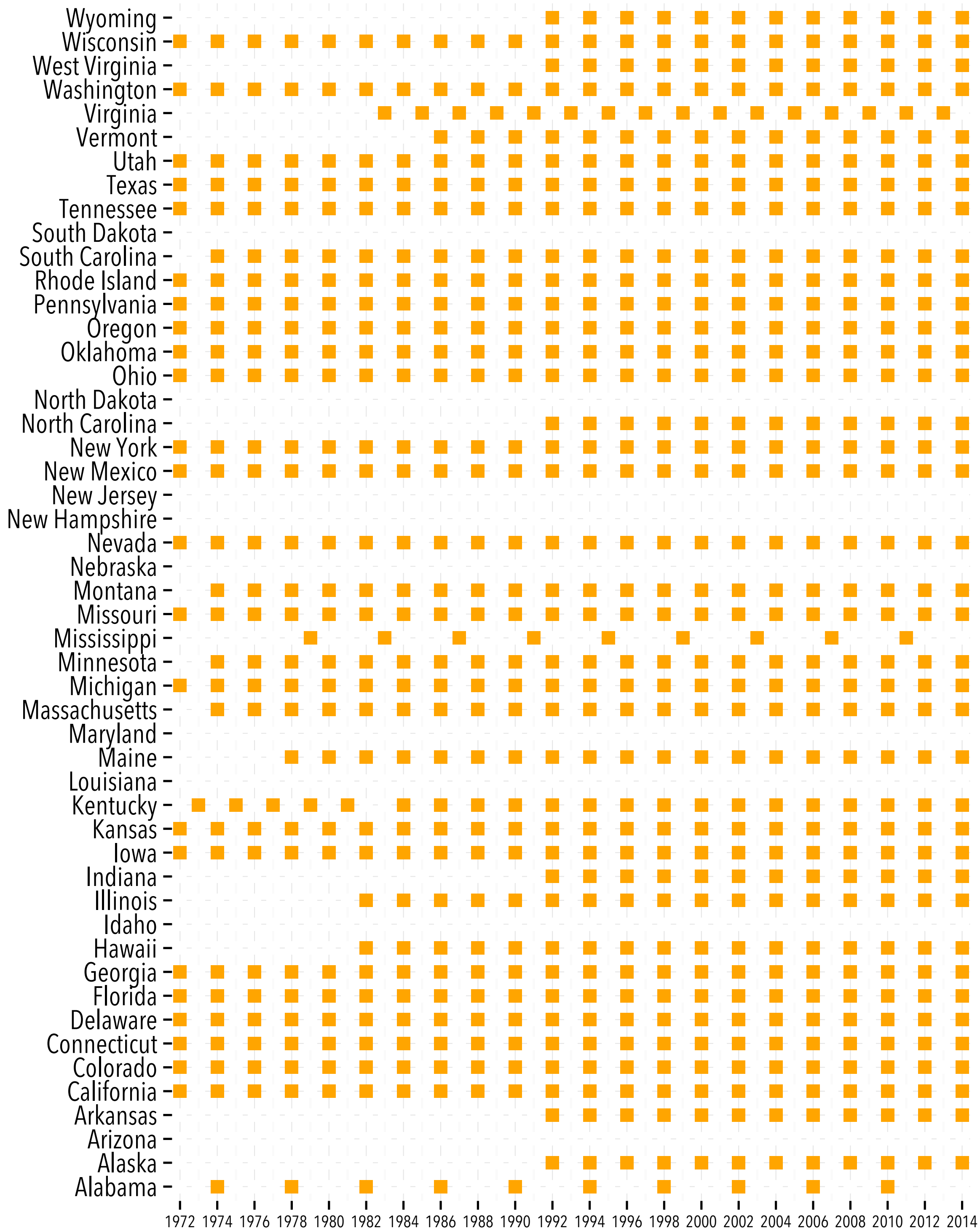

Figure 5: 786 state legislative elections available for analysis, 1972-2014, by state.

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* do we observe *within* districting plans, versus variation in the *EG between* districting plans.

To the extent that the *EG* is a feature of a districting plan per se, we should observe a small amount of within-plan variation relative to between plan variation. To perform this analysis we must group sequences of elections within states by the districting plan in place at the time.

Stephanopolous and McGhee (2015) provide a unique identifier for the districting plan in place for each state legislative election, for which I adopt here.

Figure 6 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 we see just one election under a plan: examples include Alabama 1982, California, Hawaii 1982, Tennessee 1982, Ohio 1992, South Carolina 1992, North Carolina 2002, and South Carolina 2002.

Alaska, Kentucky, Pennsylvania and Texas held just one election under their respective districting plans adopted after the 2010 Census. In each of those states a different plan was in place for 2014 state legislative elections. Alabama's state legislature has a four year term and we observe only the 2014 election under its post-2010 plan. The last election from Mississippi was in 2011 and was held under the plan in place for its 2003 and 2007 elections.

7.2 Uncontested races

Uncontested races are common in state legislative elections, and are even the norm in some states. For 38.7% of the district-level results in this analysis, it isn't possible to directly compute a two-party vote share (v_i), either because the seat was uncontested or not contested by both a Democratic and Republican candidate, or (in a tiny handful of cases) the data are missing.

In some states, for some elections, the proportion of uncontested races is so high that we drop the election from the analysis. As noted earlier, examples

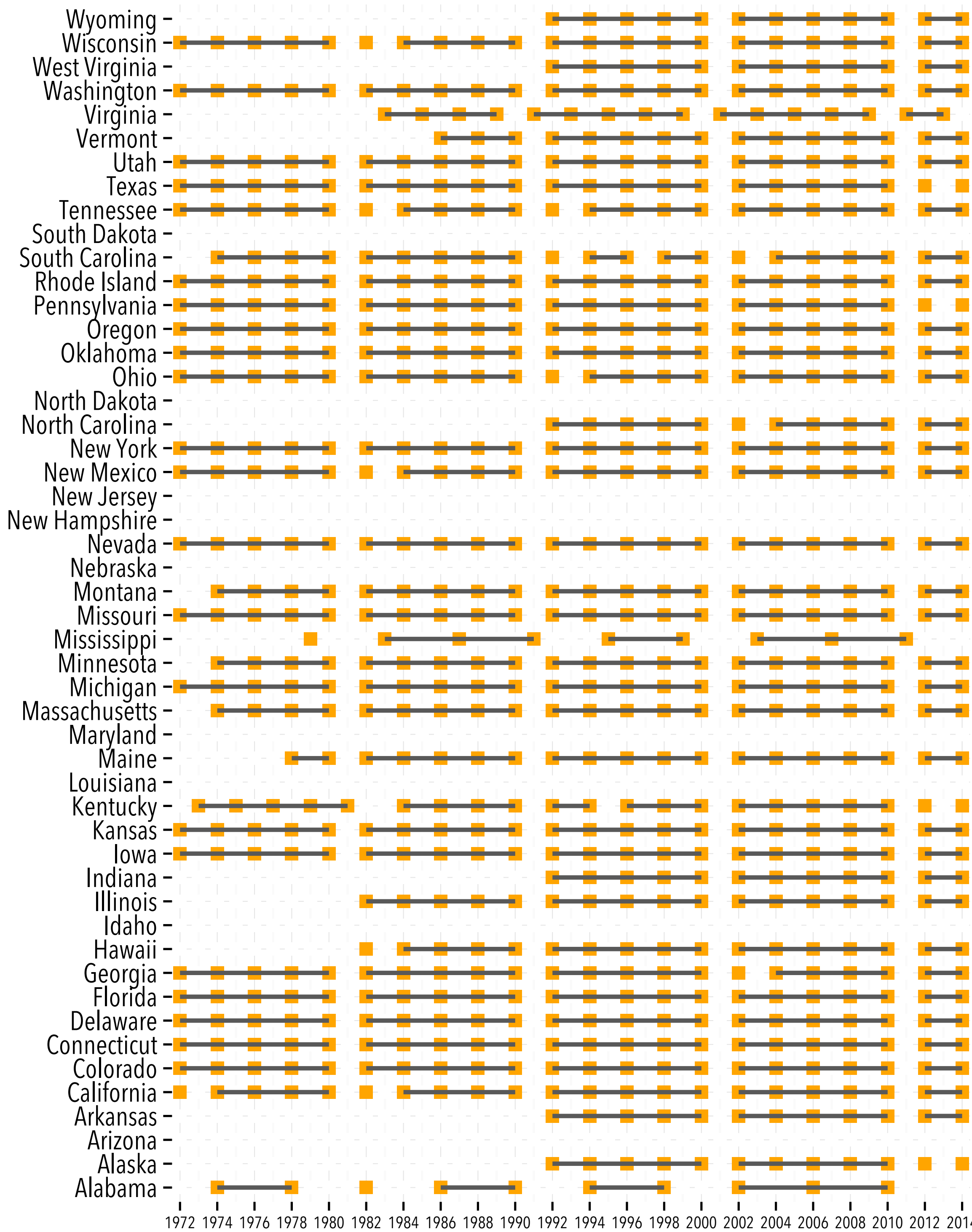

Figure 6: 786 state legislative elections available for analysis, 1972-2014, by state, grouped by districting plan (horizontal line).

include Arkansas elections prior to 1992 and South Carolina in 1972.

Even with these elections dropped from the analysis, the extent of uncontestedness in the remaining set of state legislative election results is too large to be ignored. Of the remaining elections, 31% have missing two-party results in at least half of the districts.

A graphical summary of the prevalence of uncontested districts appears in Figure 7, showing the percentage of districts without Democratic and Republican vote counts, by election and by state. Uncontested races are the norm in a number of Southern states: e.g., Georgia, South Carolina, Mississippi, Arkansas, Texas, Alabama, Virginia, Kentucky and Tennessee record rates of uncontestedness that seldom, if ever, drop below 50% for the period covered by this analysis. Wyoming also records a high proportion of districts that do not have Democratic versus Republican contests. States that lean Democratic also have high levels of uncontestedness too: see Rhode Island, Massachusetts, Illinois and, in recent decades, Pennsylvania.

Michigan and Minnesota are among the states with the lowest levels of uncontested districts in their state legislative elections. Over the set of 786 state legislative elections we examine, there are just *three* instances of elections with Democrats and Republicans running candidates in every district: Michigan supplies two of these cases (2014 and 1996) and Minnesota the other (2008).

8 Imputations for Uncontested Races

Stephanopolous and McGhee (2015) note the prevalence of uncontested races and report using a statistical model to impute vote shares to 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

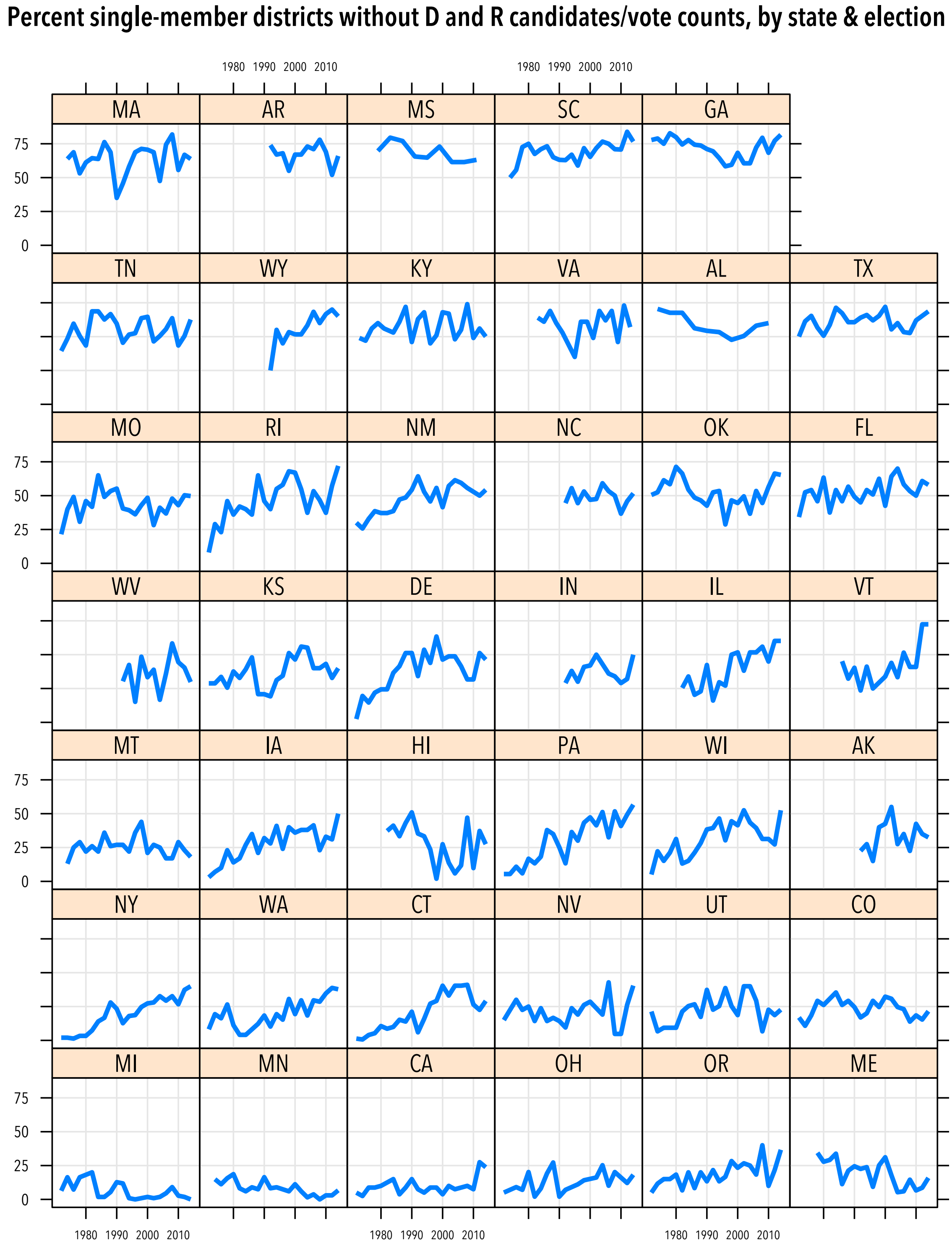

Figure 7: Percentage of districts missing two-party vote shares, by election, in 786 state legislative elections, 1972-2014. Missing data is almost always due to districts being uncontested by both major parties.

vote share in state legislative districts (v_i).

8.1 Imputation model 1: presidential vote shares

The first imputation model relies on presidential election returns reported at the level of state legislative districts. Presidential election returns are excellent predictors of state legislative election outcomes and observed even when state legislative elections are uncontested. I fit a series of linear regressions of v_i on the Democratic share of the two-party vote for president in district i, as recorded in the most temporally-proximate presidential election for which data is available and for which the current election's districting plan was in place; separate slopes and intercepts are estimated depending on the incumbency status of district i (Democratic, Open/Other, Republican).

The model also embodies the following assumptions in generating imputations for unobserved vote shares in uncontested districts. In districts where a Republican incumbent ran unopposed, we assume that the Democratic share of the two-party vote would have been less than 50%; conversely, where Democratic incumbents ran unopposed, we assume that the Democratic share of the vote would have been greater than 50%.

In most states the analysis predicts 2014 and 2012 state legislative election results v_i using 2012 presidential vote shares; 2006, 2008 and 2010 v_i is regressed on 2008 presidential vote shares, and so on. Some care is needed matching state and presidential election results in states that hold their state legislative elections in odd-numbered years, or where redistricting intervenes. In a small number of cases, presidential election returns are not available, or are recorded with district identifiers that can't be matched in the state legislative elections data. We lack data on presidential election results by state legislative district prior to 2000, so 1992 is the earliest election with which we can match state legislative election results to presidential election results at the district level.

The imputation model generally fits well. Across the 447 elections, the median r^2 statistic is 0.82. The cases fitting less well include Vermont in 2012 ($r^2 = 0.29$), with relatively few contested seats and multi-member districts with positions.

We examine the performance of the imputation model in a series of graphs, below, for six sets of elections: Wisconsin in 2012 and 2014, Michigan in 2014

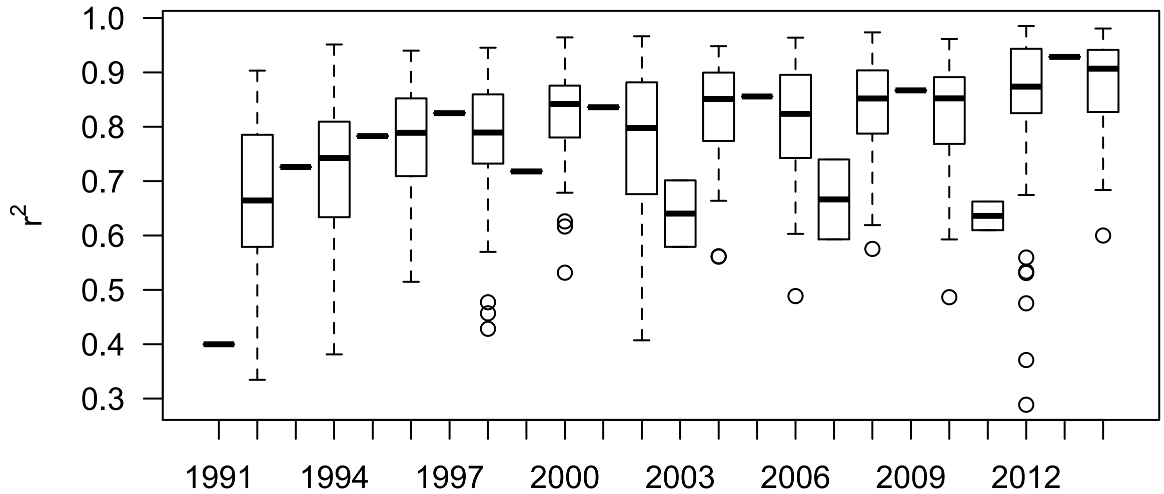

Figure 8: Distribution of r^2 statistics, regressions of Democratic share of two-party vote in state legislative election outcomes on Democratic share of the two-party for president.

(with no uncontested districts), South Carolina in 2012 (with the highest proportion of uncontested seats in the 2012 data), Virginia in 2013 and Wyoming in 2012 (the latter two generating extremely large, negative values of the efficiency gap). Vertical lines indicate 95% confidence intervals around imputed values for the Democratic share of the two-party vote in state legislative elections (vertical axis). Separate slopes and intercepts are fit for each incumbency type. Note also that the imputed data almost always lie on the regression lines.

Imputations for uncontested districts are accompanied by uncertainty. Although the imputation models generally fit well, like any realistic model they provides less than a perfect fit to the data. Note too that in any given election, there is only a finite amount of data and hence a limit to the precision with which we can make inferences about unobserved vote shares based on the relationship between observed vote shares and presidential vote shares.

Uncertainty in the imputations for v in uncontested districts generates uncertainty in "downstream" quantities of interest such as statewide Democratic vote share V and the efficiency gap measure EG. This is key, given the fact that uncontestedness is so pervasive in these data. We want any conclusions about the efficiency gap's properties or inferences about particular levels of the efficiency gap to reflect the uncertainty resulting from imputing vote shares in uncontested districts.

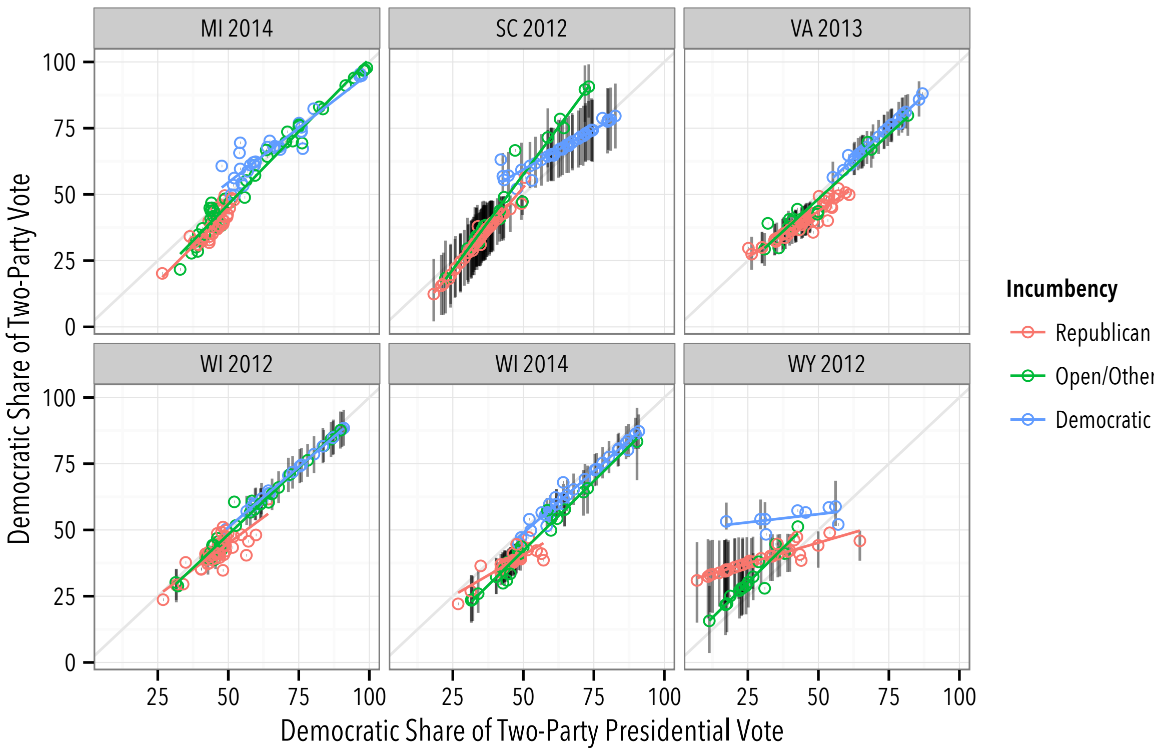

Figure 9: Regression model for imputing unobserved vote shares in 6 selected elections. Vertical lines indicate 95% confidence intervals around imputed values for the Democratic share of the two-party vote in state legislative elections (vertical axis). Separate slopes and intercepts are fit for each incumbency type. Note also that the imputed data almost always lie on the regression lines.

8.2 Imputation model 2

We rely on imputations based on presidential election returns when they are available. But presidential vote isn't always available at the level of state legislative districts (not before 1992, in this analysis). To handle these cases, we rely on a second imputation procedure, one that models sequences of election results observed under a redistricting plan, interpolating unobserved Democratic vote shares given (1) previous and future results for a given district; (2) statewide swing in a given state election; and (3) change in the incumbency status of a given district. This model also embodies the assumption that unobserved vote shares would nonetheless be consistent with what we *did* observe in a given seat: where a Democrat wins in an uncontested district, any imputation for v in that district must lie above 50%, and where a Republican wins an uncontested district, any imputation for v must lie below 50%.

8.3 Combining the two sets of imputations

We now have two sets of imputations for uncontested districts: (1) using presidential vote as a basis for imputation, where available (447 state legislative elections from 1992 to 2014); and (2) the imputation model that relies on the trajectory of district results over the history of a districting plan, including incumbency and estimates of swing, which supplies imputations for uncontested districts in all years.

When there are no uncontested districts, obviously the two imputations must agree, for the trivial reason that are no imputations to perform. As the number of uncontested districts rises, the imputations from the two models have room to diverge. Where the two sets of imputations are available for a given election (elections where presidential vote shares by state legislative districts are available) we generally see a high level of agreement between the two methods.

The two sets of imputations for V correlate at .99. With only a few exceptions (see Figure 10), the discrepancies are generally small relative to the uncertainty in the imputations themselves. As the proportion of districts with missing data increases, clearly the scope for divergence between the two models increases.

To re-iterate, we prefer the imputations from "Model 1" based on the regressions utilizing presidential vote shares in state legislative districts, and use them whenever available (i.e., for most states in the analysis, the period 1992-2014). We only rely on "Model 2" when presidential vote shares are not available. We model the difference between the two sets of imputations, adjusting the "Model 2" imputations of V to better match what we have obtained from "Model 1", had the necessary presidential vote shares by state legislative district been available.

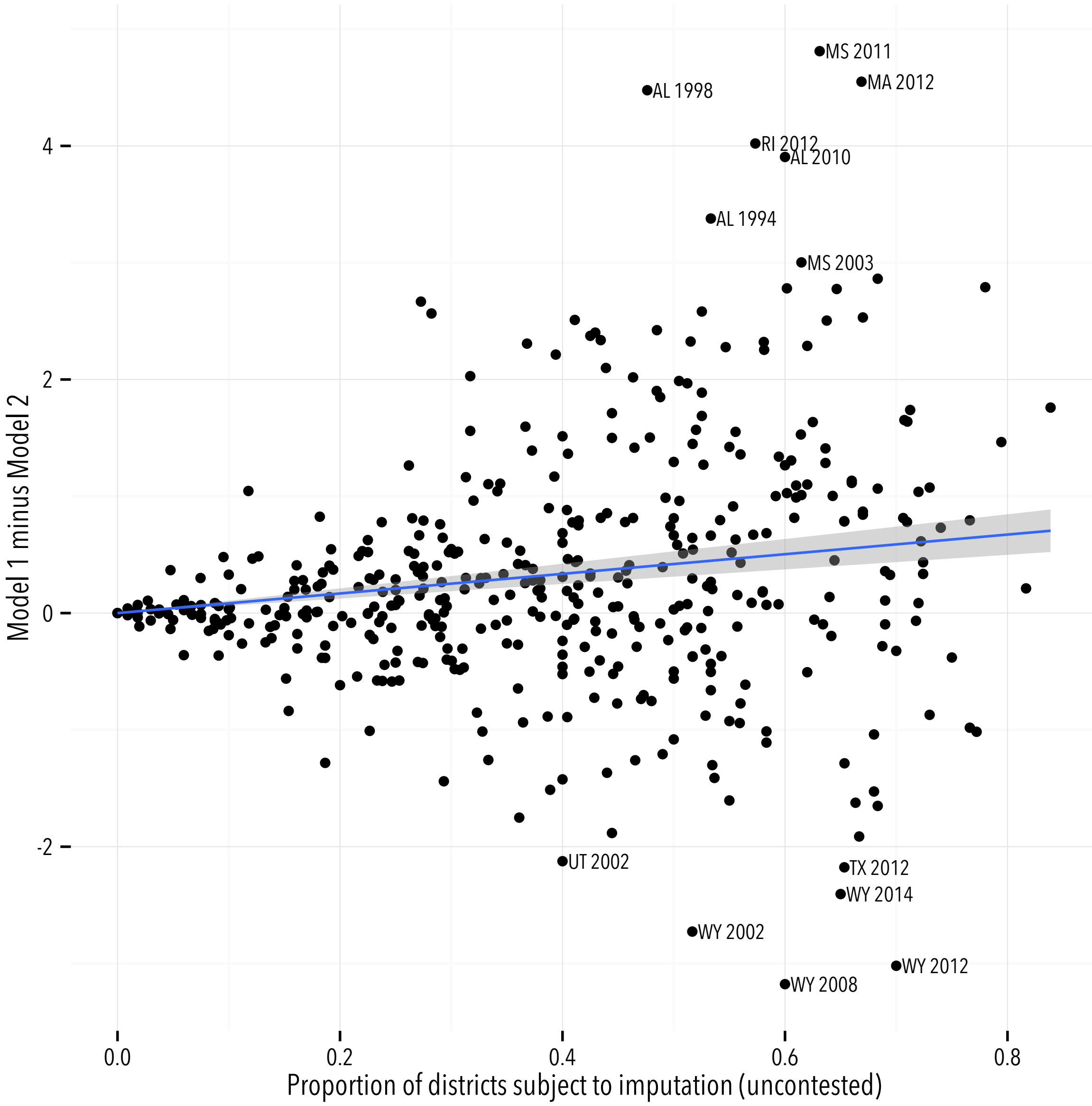

Figure 10: Difference between imputations for V by proportion of uncontested seats. The fitted regression line is constrained to respect the constraint that the imputations must coincide when there are no uncontested seats.

8.4 Seat and vote shares in 786 state legislative elections

After imputations for missing data, each election generates a seats-votes (V, S) pair. In Figure 11 we plot *all* of the V and S combinations over the 786 state elections in the analysis. We also overlay the seats-vote curve corresponding to an efficiency gap of zero. This provides us with a crude, visual sense of how often we see large departures from the zero EG benchmark.

The horizontal lines around each plotted point show the uncertainty associated with each estimate of V (statewide, Democratic, two-party vote share), given the imputations made for uncontested and missing district-level vote shares. Uncontested seats do not generate uncertainty with respect to the party winning the seat, and so the resulting uncertainty is with respect to vote shares, on the horizontal axis in Figure 11.

The efficiency gap in each election is the vertical displacement of each plotted (V, S) point from the orange, zero-efficiency gap line in Figure 11. Uncertainty as to the horizontal co-ordinate V (due to imputations for uncontested races) generates uncertainty in determining how far each point lies above or below the orange, zero efficiency gap benchmark.

9 The efficiency gap, by state and election

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

We have 786 efficiency gap measures in 41 states, spanning 43 election years. These are computed by substituting each state election's estimate of V and the corresponding, observed seat share S into equation 1.

Figure 12 shows the efficiency gap estimates for each state election, grouped by state and ordered by year; vertical lines indicate 95% credible intervals arising from the fact that the imputation model for uncontested seats induces uncertainty in V and any quantity depending on V such as EG (recall equation 1). In many cases the uncertainty in EG stemming from imputation for uncontested seats is small relative to variation in EG both between and within districting plans.

We observe considerable variation in the EG estimates across states and elections. Some highlights:

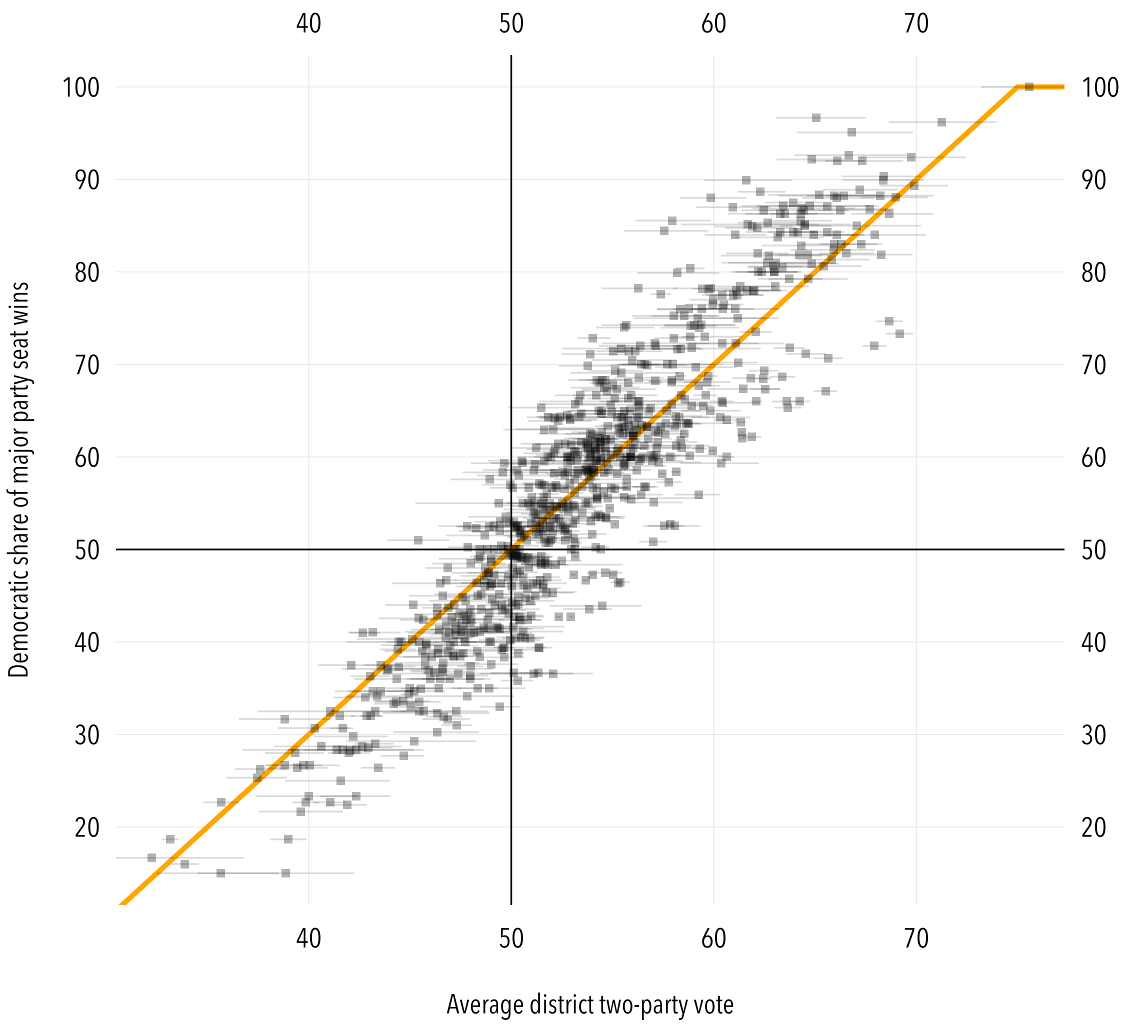

Figure 11: Democratic seat shares (S) and vote shares (V) in 786 state legislative elections, 1972-2014, in 41 states. Seat shares are defined with respect to single-member districts won by either a Republican or a Democratic candidate, including uncontested districts. Vote shares are defined as the average of district-level, Democratic share of the two-party vote, in the same set of districts used in defining seat shares. Horizontal lines indicate 95% credible intervals with respect to V, due to uncertainty arising from imputations for district-level vote shares in uncontested seats. The orange line shows the seats-votes relationship we expect if the efficiency gap were zero. Elections below the orange line have $EG < 0$ (Democratic disadvantage); points above the orange line have $EG > 0$ (Democratic advantage).

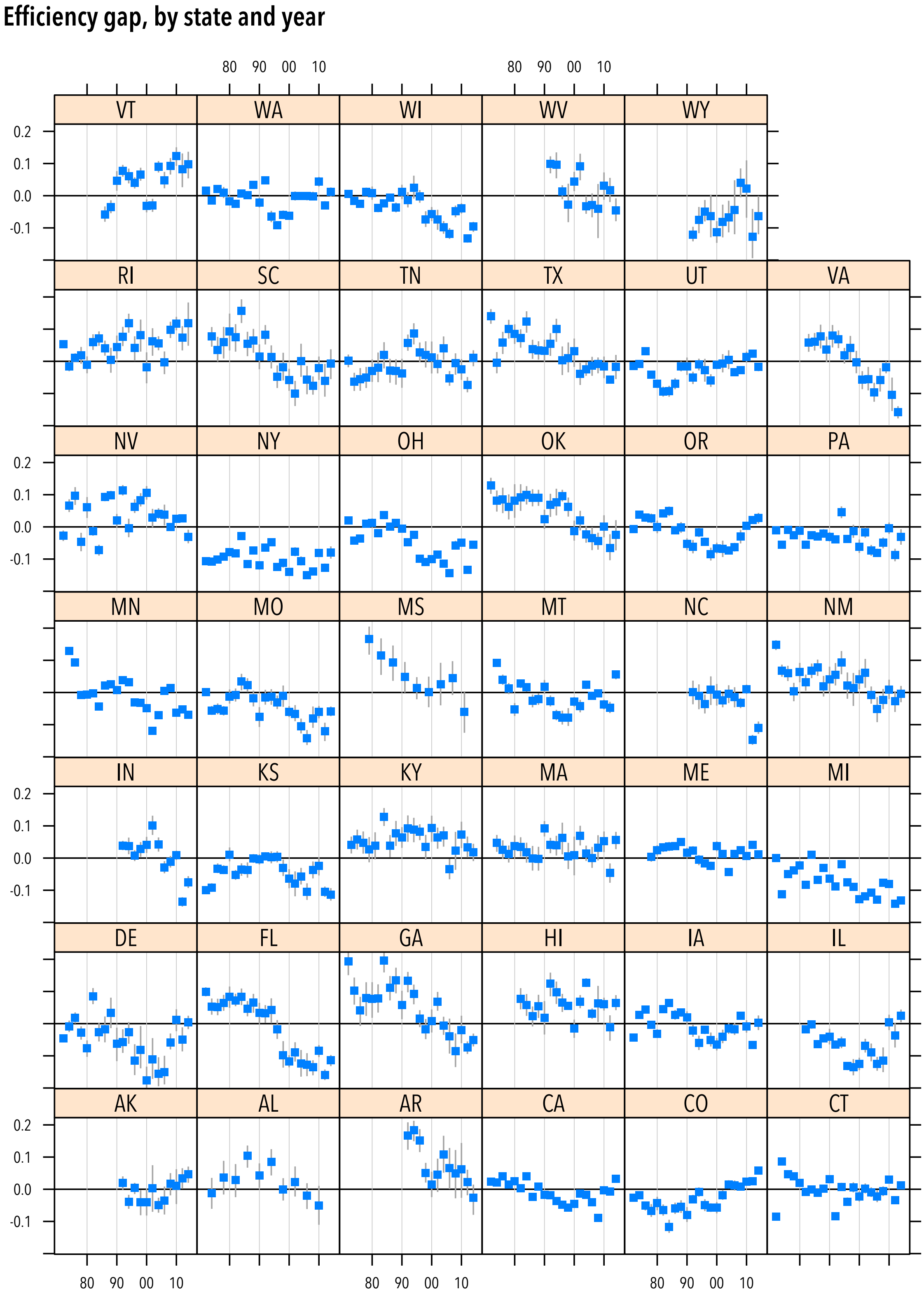

Figure 12: Efficiency gap estimates in 786 state legislative elections, 1972-2014. Vertical lines cover 95% credible intervals.

1. estimates of *EG* range from −0.18 to 0.20 with an average value of −0.005.

2. The lowest value, −0.18 is from Delaware in 2000. There were 19 uncontested seats in the election to the 41 seat state legislature. Democrats won 15 seats ($S = 15/41 = 36.6\%$). I estimate V to be 52.1%. Via equation 1, this generates $EG = -0.18$. Considerable uncertainty accompanies this estimate, given the large number of uncontested seats. The 95% credible interval for V is $\pm$ 2.03 percentage points, and the 95% credible interval for the accompanying *EG* estimate is $\pm$ 0.04.

3. The highest value of *EG* is 0.20 is from Georgia in 1984. There were 140 uncontested seats in the election to the 180 seat state legislature. Democrats won 154 seats ($S = 154/180 = 85.6\%$). I estimate V to be 57.9%. Again, using equation 1, this generates $EG = 0.2$. Considerable uncertainty also accompanies this estimate, given the large number of uncontested seats. The 95% credible interval for V is $\pm$ 1.89 percentage points, and the 95% credible interval for the accompanying *EG* estimate is $\pm$ 0.04. Figure 13 contrasts the seats and votes recorded in Georgia against those for the entire data set, putting Georgia's large *EG* estimates in context.

4. New York has the lowest median *EG* estimates, ranging from -.15 (2006) to -.028 (1984). Statewide V ranges from 53.7% to 69.2%, but Democrats only win 70 (1972) to 112 (2012) seats in the 150 seat state legislature, so S ranges from .47 to .75, considerably below that we'd expect to see given the vote shares recorded by Democrats if the efficiency gap were zero. See Figure 15.

5. Arkansas has the highest median *EG* score by state, .10; see Figure 14.

6. Connecticut has the median, within-state median *EG* score of approximately zero; Figure 16 shows Connecticut's seats and votes have generally stayed close to the $EG = 0$ benchmark.

7. Michigan has the third lowest median *EG* scores by state, surpassed only by New York and Wyoming. Michigan's *EG* scores range from -.14 (2012) to .01 (1984). V ranges from 50.3% to 60.6%, a figure we estimate confidently given low and occasionally even zero levels of uncontested districts

in Michigan state legislative elections. Yet *S* ranges from 42.7% (Democrats won 47 out of 110 seats in 2002, 2010 and 2014) to 63.6% (Democrats won 70 out of 110 seats in 1978). See Figure 17.

8. Wisconsin's *EG* estimates range from -.14 (2012) to .02 (1994). Although the *EG* estimates for WI are not very large relative to other states in other years, Wisconsin has recorded an unbroken run of negative *EG* estimates from 1998 to 2014 and records two very large estimates of the efficiency gap in elections held under its current plan: -.13 (2012) and -.10 (2014). In short, Democrats are underperforming in state legislative elections in Wisconsin, winning fewer seats than a zero efficiency gap benchmark would imply, given, their statewide level of support. See Figure 18.

9.1 Are efficiency gap estimates statistically significant?

Recall that $EG < 0$ means that Democrats are disadvantaged, with relatively more wasted votes than Republicans; conversely $EG > 0$ means that Democrats are the beneficiaries of an efficiency gap, in that Democrats have fewer wasted votes than Republicans. But *EG* does vary from election to election, even with the same districting plan in place and *EG* is almost always not measured perfectly, but is estimated with imputations for uncontested seats.

In Figure 19 we plot the imprecision of each efficiency gap estimate (the half-width of its 95% credible interval) against the estimated *EG* value itself. Points lying inside the cones have *EG* estimates that are small relative to their credible intervals, such that we would not distinguish them from zero at conventional levels of statistical significance. Not all *EG* estimates can be distinguished from zero at conventional levels of statistical significance, nor should they. But many estimates of the *EG* *are* unambiguously non-zero. Critically, the two most recent Wisconsin *EG* estimates (-.13 in 2012, -.10 in 2014) are clearly non-negative, lying far away from the "cone of ambiguity" shown in Figure 19; the 95% credible interval for the 2012 estimates runs from -.146 to -.121 and from -.113 to -.081 for the 2014 estimate.

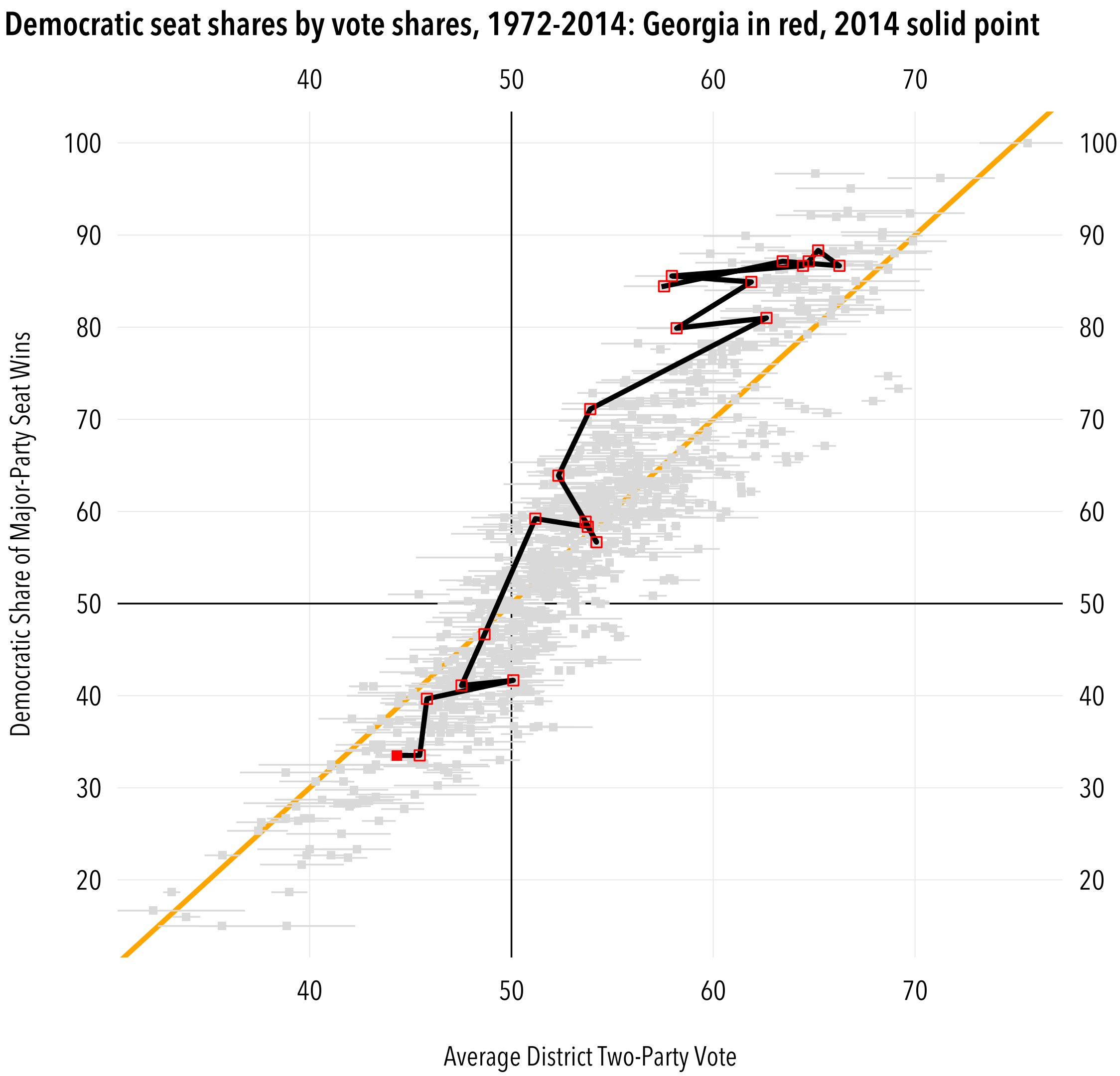

Figure 13: Georgia, Democratic seat share and average district two-party vote share, 1972-2014. Orange line shows the seats-votes curve if the efficiency gap were zero; the efficiency gap in any election is the vertical distance from the corresponding data point to the orange line. Gray points indicate elections from other states and elections (1972-2014). Horizontal lines cover a 95% credible interval for Democratic average district two-party vote share, given imputations in uncontested districts.

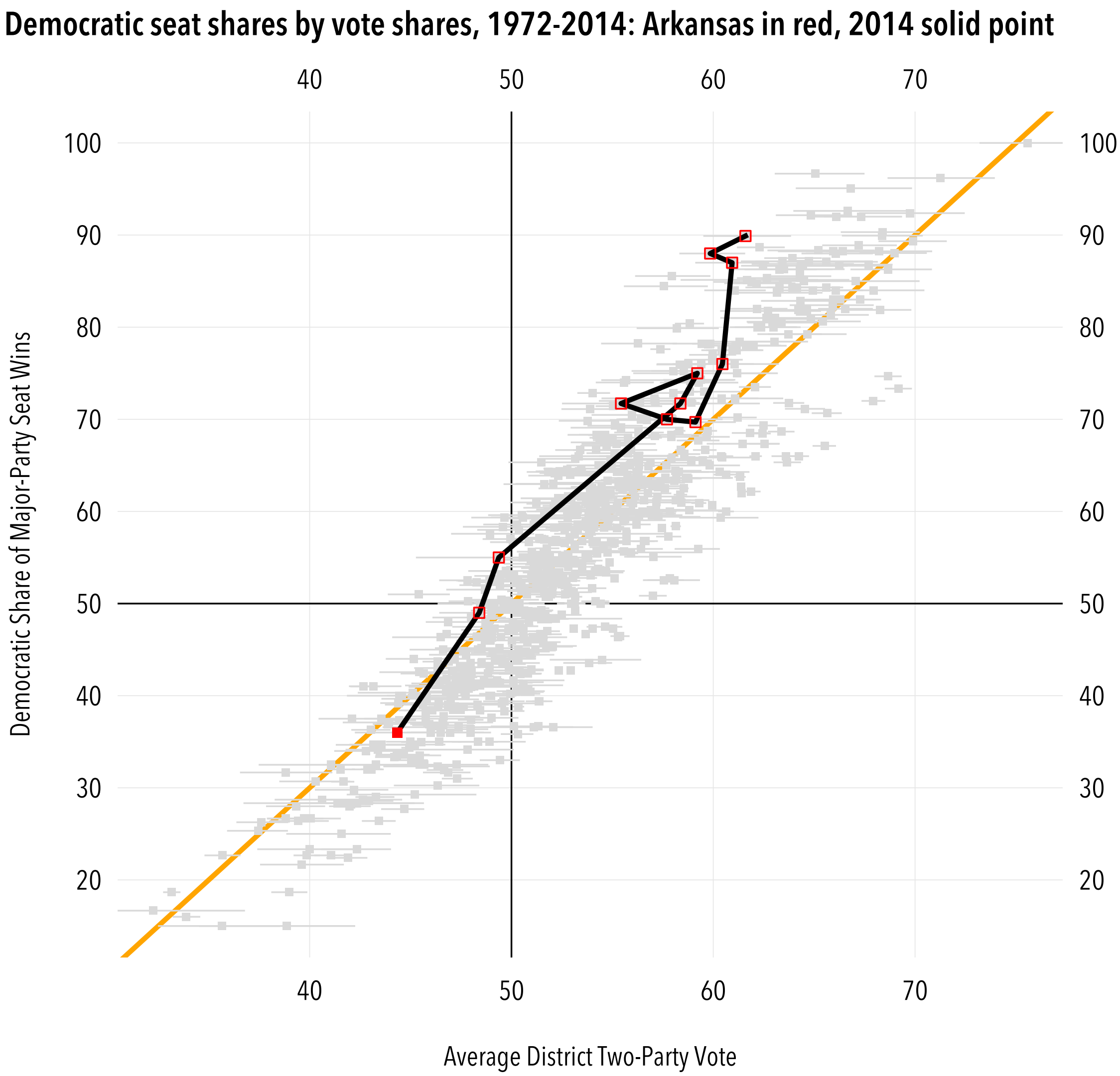

Figure 14: Arkansas, Democratic seat share and average district two-party vote share, 1992-2014. Orange line shows the seats-votes curve if the efficiency gap were zero; the efficiency gap in any election is the vertical distance from the corresponding data point to the orange line. Gray points indicate elections from other states and elections (1972-2014). Horizontal lines cover a 95% credible interval for Democratic average district two-party vote share, given imputations in uncontested districts.

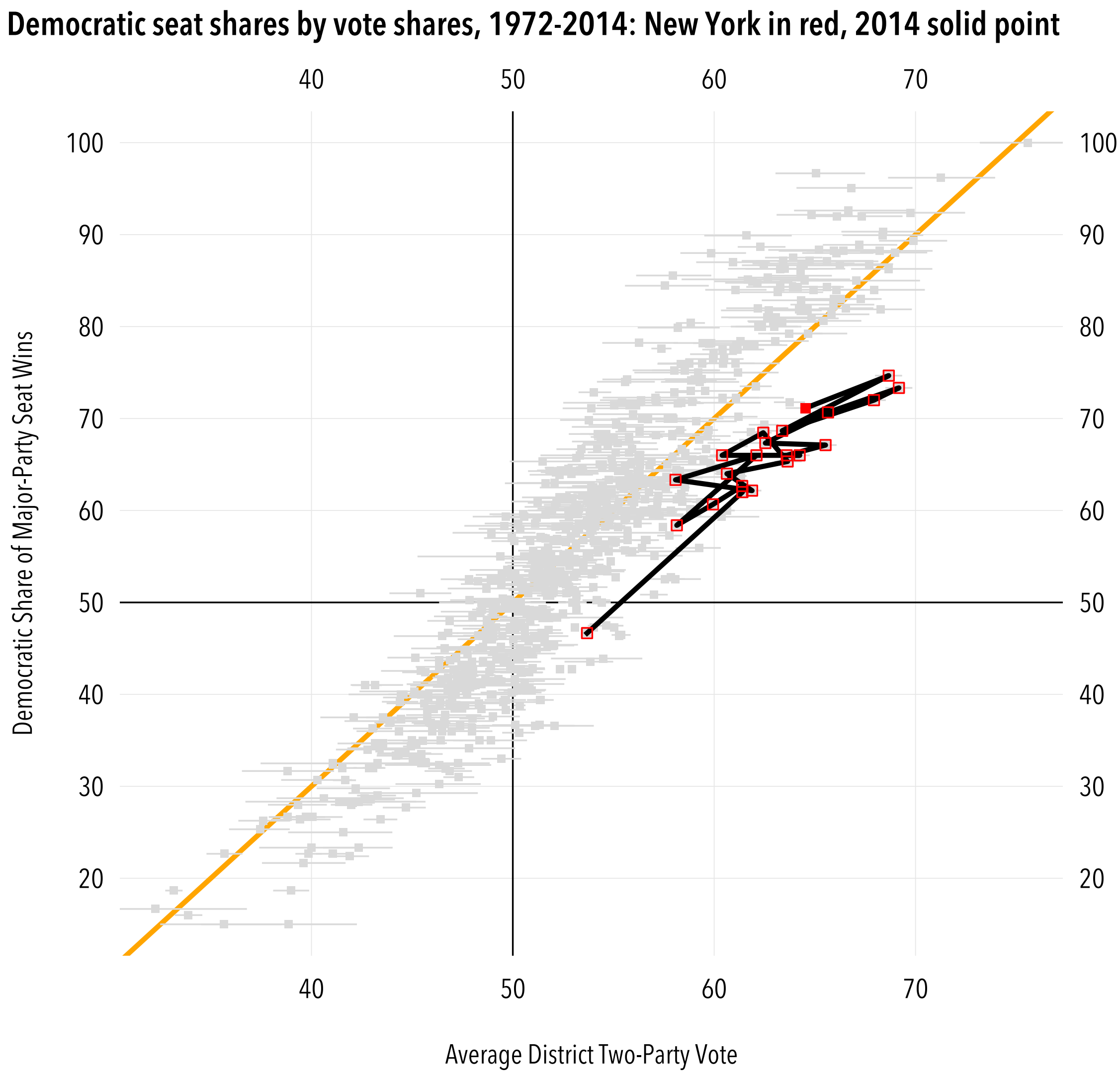

Figure 15: New York, Democratic seat share and average district two-party vote share, 1972-2014. Orange line shows the seats-votes curve if the efficiency gap were zero; the efficiency gap in any election is the vertical distance from the corresponding data point to the orange line. Gray points indicate elections from other states and elections (1972-2014). Horizontal lines cover a 95% credible interval for Democratic average district two-party vote share, given imputations in uncontested districts.

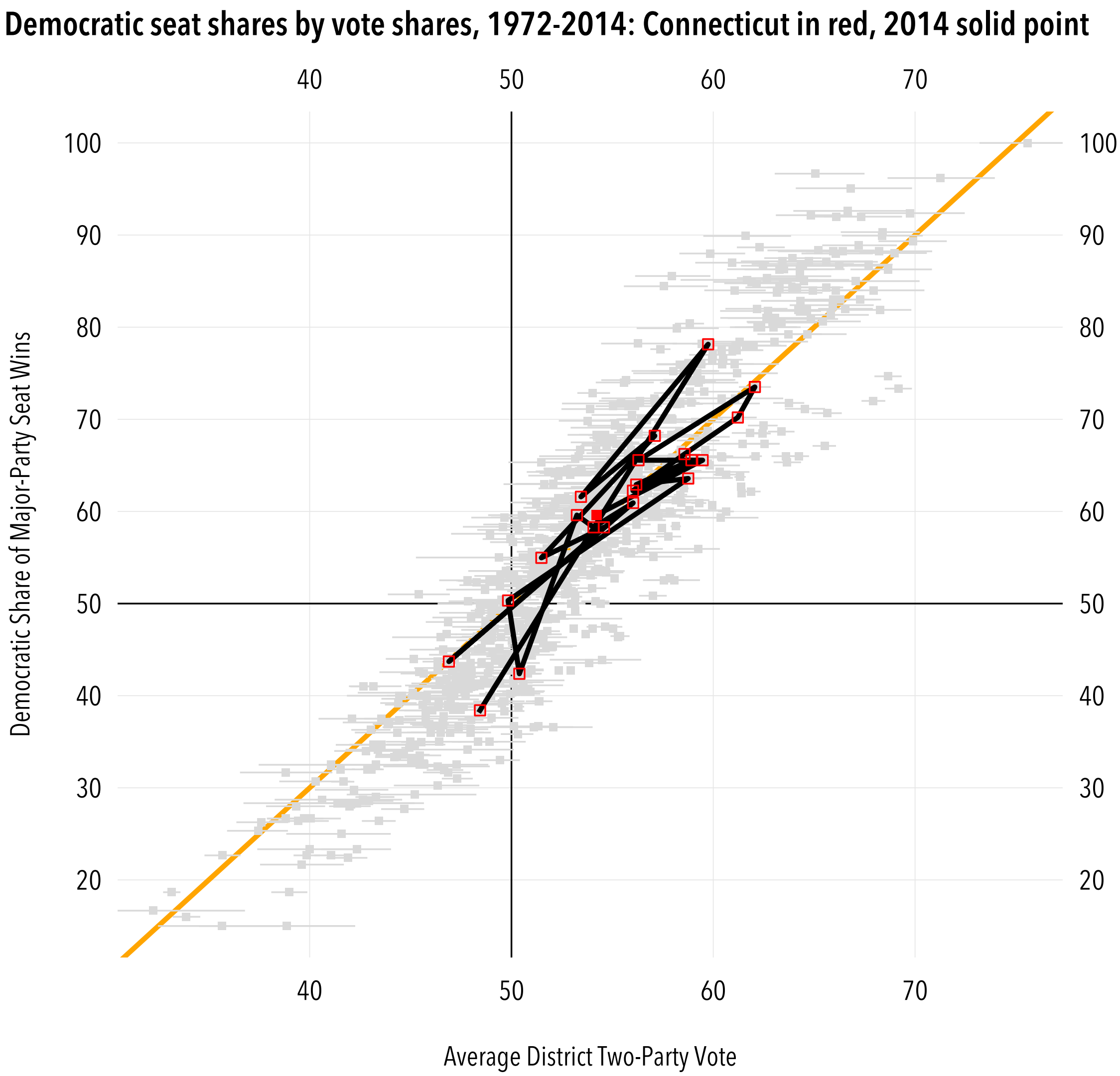

Figure 16: Connecticut, Democratic seat share and average district two-party vote share, 1972-2014. Orange line shows the seats-votes curve if the efficiency gap were zero; the efficiency gap in any election is the vertical distance from the corresponding data point to the orange line. Gray points indicate elections from other states and elections (1972-2014). Horizontal lines cover a 95% credible interval for Democratic average district two-party vote share, given imputations in uncontested districts.

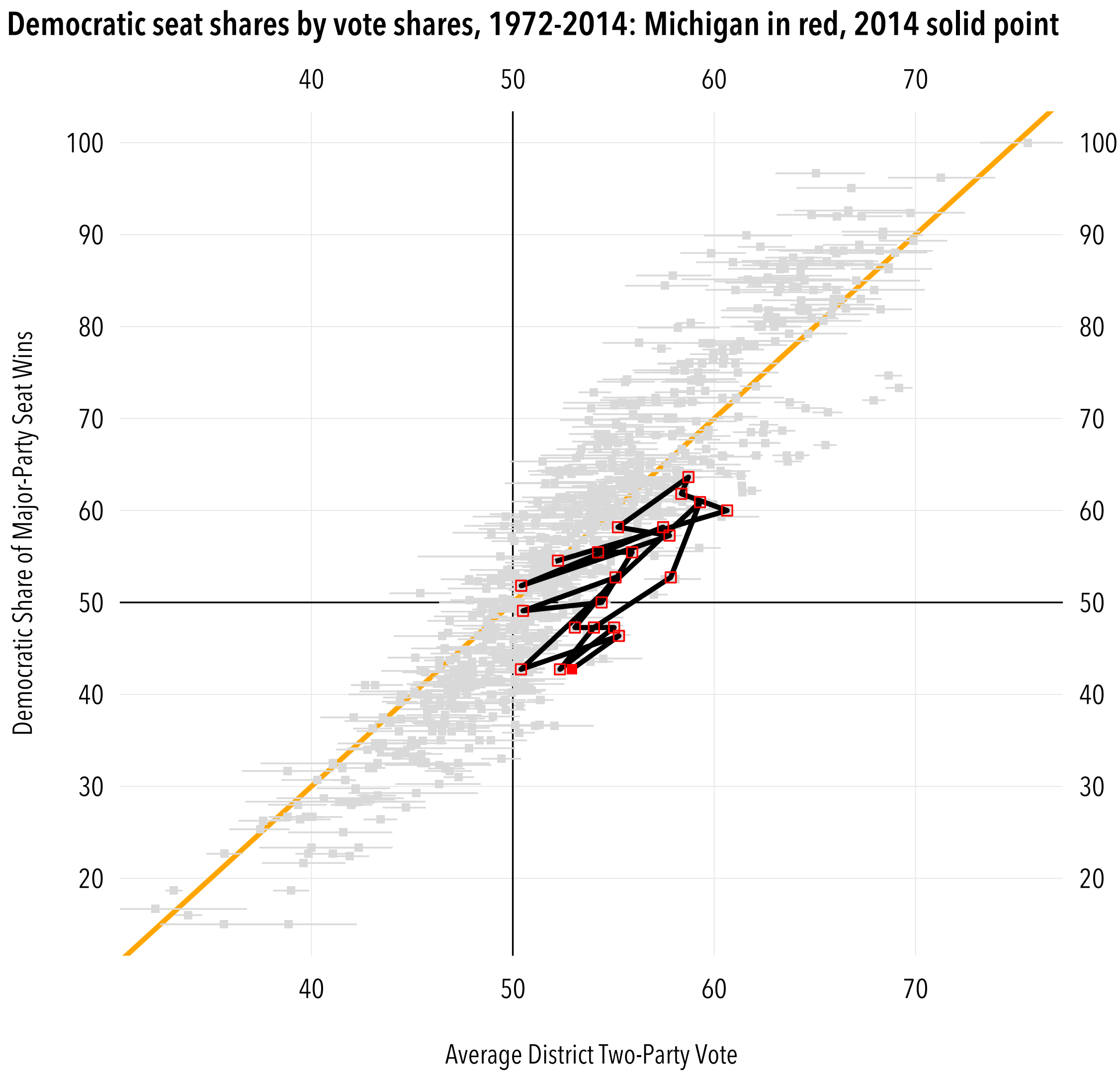

Figure 17: Michigan, Democratic seat share and average district two-party vote share, 1972-2014. Orange line shows the seats-votes curve if the efficiency gap were zero; the efficiency gap in any election is the vertical distance from the corresponding data point to the orange line. Gray points indicate elections from other states and elections (1972-2014). Horizontal lines cover a 95% credible interval for Democratic average district two-party vote share, given imputations in uncontested districts.

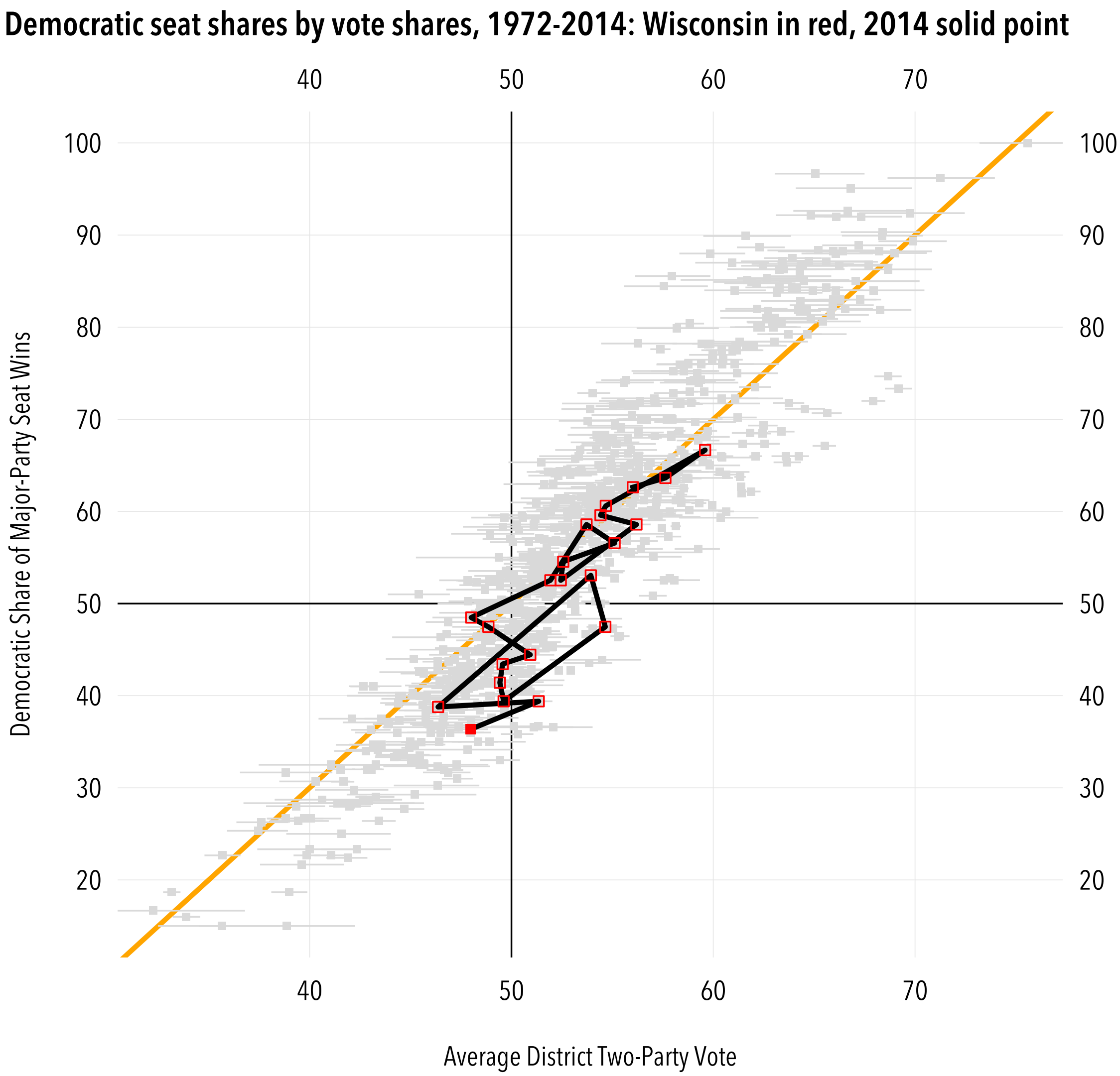

Figure 18: Wisconsin, Democratic seat share and average district two-party vote share, 1972-2014. Orange line shows the seats-votes curve if the efficiency gap were zero; the efficiency gap in any election is the vertical distance from the corresponding data point to the orange line. Gray points indicate elections from other states and elections (1972-2014). Horizontal lines cover a 95% credible interval for Democratic average district two-party vote share, given imputations in uncontested districts.

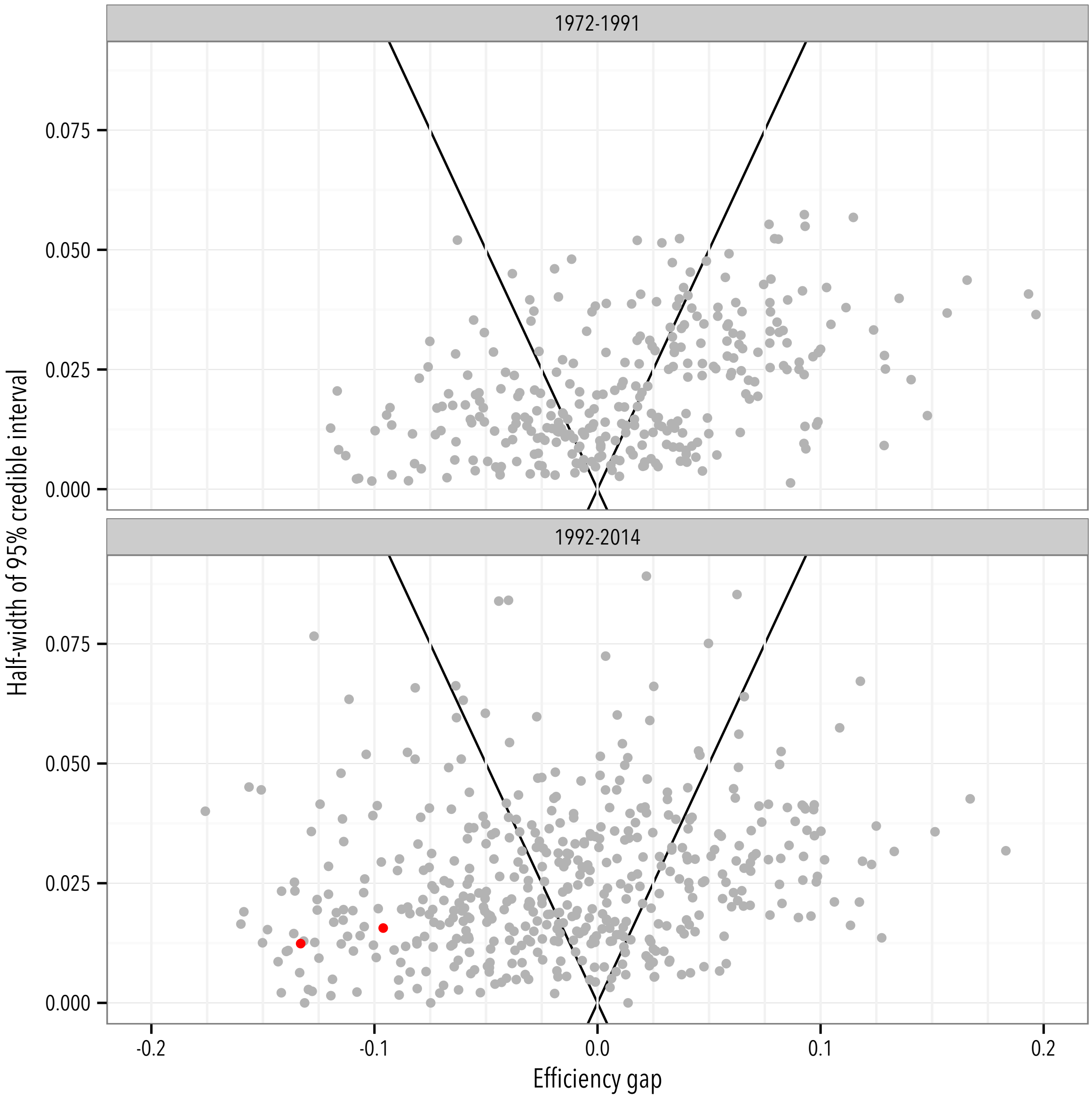

Figure 19: Uncertainty in the efficiency gap, against the *EG* estimate itself. The vertical axis is the half-width of the 95% credible interval for each *EG* estimate (plotted against the horizontal axis); points lying inside the cones have *EG* estimates that are small relative to their credible intervals, such that we would not distinguish them from zero at conventional levels of statistical significance. *EG* estimates from Wisconsin in 2012 and 2014 are shown as red points in the lower panel. Note the greater prevalence of large, negative and precisely estimated *EG* measures in recent decades.

9.2 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 redistricting plans. If recent decades have generally seen smaller values of the efficiency gap relative to past decades, then this might be informative as to how we should assess contemporary districting plans and their corresponding values of the *EG*.

Figure 20 plots *EG* estimates over time, overlaying estimates of the smoothed, weighted quantiles (25th, 50th and 75th) of the *EG* measures (the weights capture the uncertainty accompanying each estimate of the *EG*). The distribution of *EG* measures in the 1970s and 1980s appeared to slightly favor Democrats; about two-thirds 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 (Republican efficiency advantage over Democrats); see Figure 21.

There is some evidence that the 2010 round of redistricting has generated an increase in the magnitude of the efficiency gap in state legislative elections. For most of the period under study, there seems to be no distinct trend in the magnitudes of the efficiency gap over time; see Figure 22. The median, absolute value of the efficiency gap has stayed around 0.04 over much of the period spanned by this analysis; elections since 2010 are producing higher levels of *EG* in magnitude.

It is also interesting to note that the estimate of the 75th percentile of the distribution of *EG* magnitudes jumps markedly after 2010, suggesting that districting plans enacted after the 2010 census are systematically more gerrymandered than in previous decades. Of the almost 800 *EG* estimates in the analysis, spanning 42 years of elections, the largest, negative estimates (an efficiency gap disadvantaging Democrats) are more likely to be recorded in the short series of elections after 2010. These include Alabama in 2014 (-.18), Florida in 2012 (-.16), Virginia in 2013 (-.16), North Carolina in 2012 (-.15) and Michigan in 2012 (-.14); these five elections are among the 10 least favorable to Democrats we observe in the entire set of elections. Among the 10 most pro-Democratic *EG* scores, *none* were recorded after 2000. The most favorable election to Democrats in terms of *EG* since 2010 is the 2014 election in Rhode Island (*EG* = .12), which is only the 20th largest (pro-Democratic) *EG* in the entire analysis.

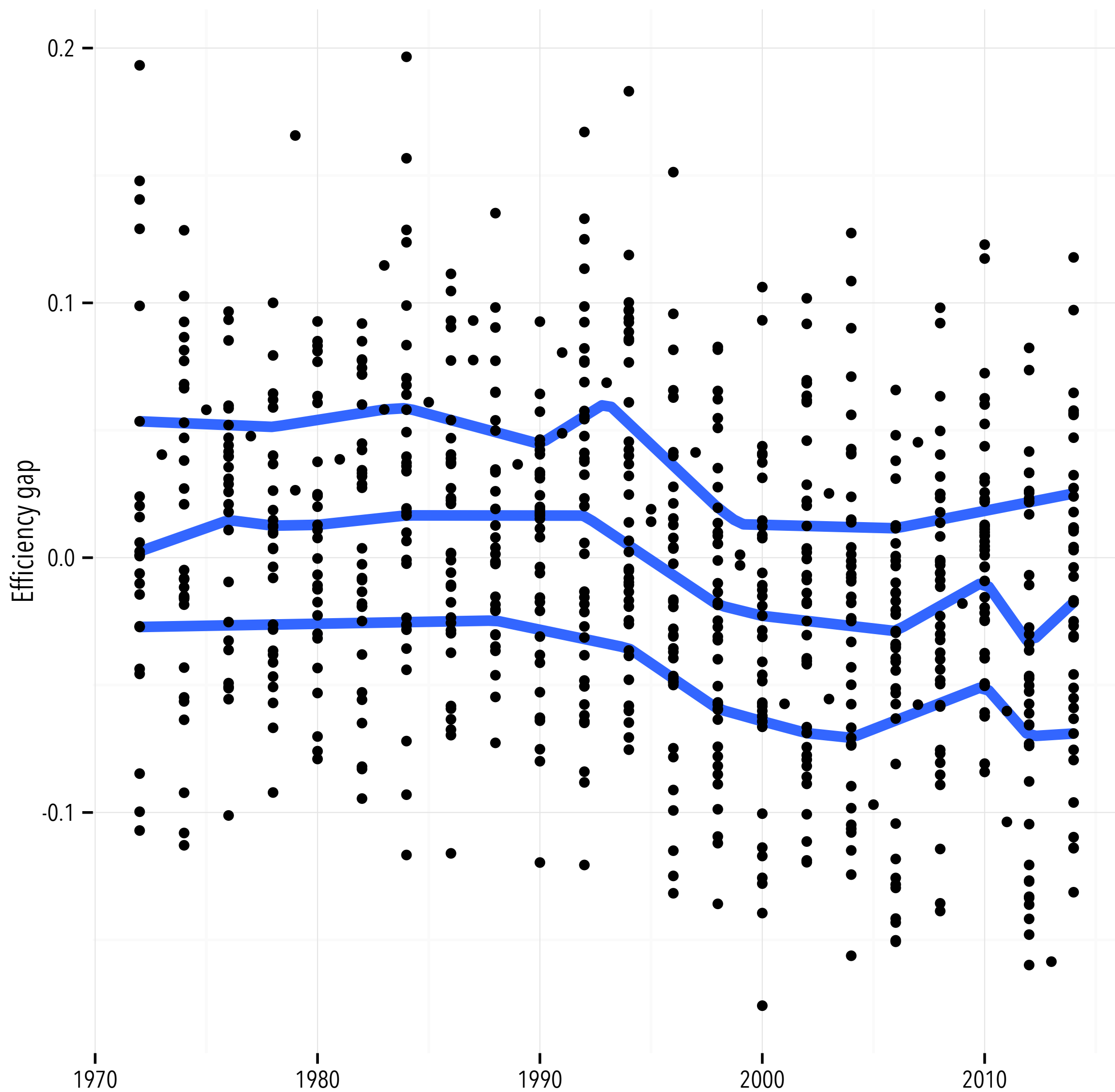

Figure 20: Efficiency gap estimates, over time. The lines are smoothed estimates of the 25th, 50th and 75th quantiles of the efficiency gap measures, weighted by the precision of each *EG* measure.

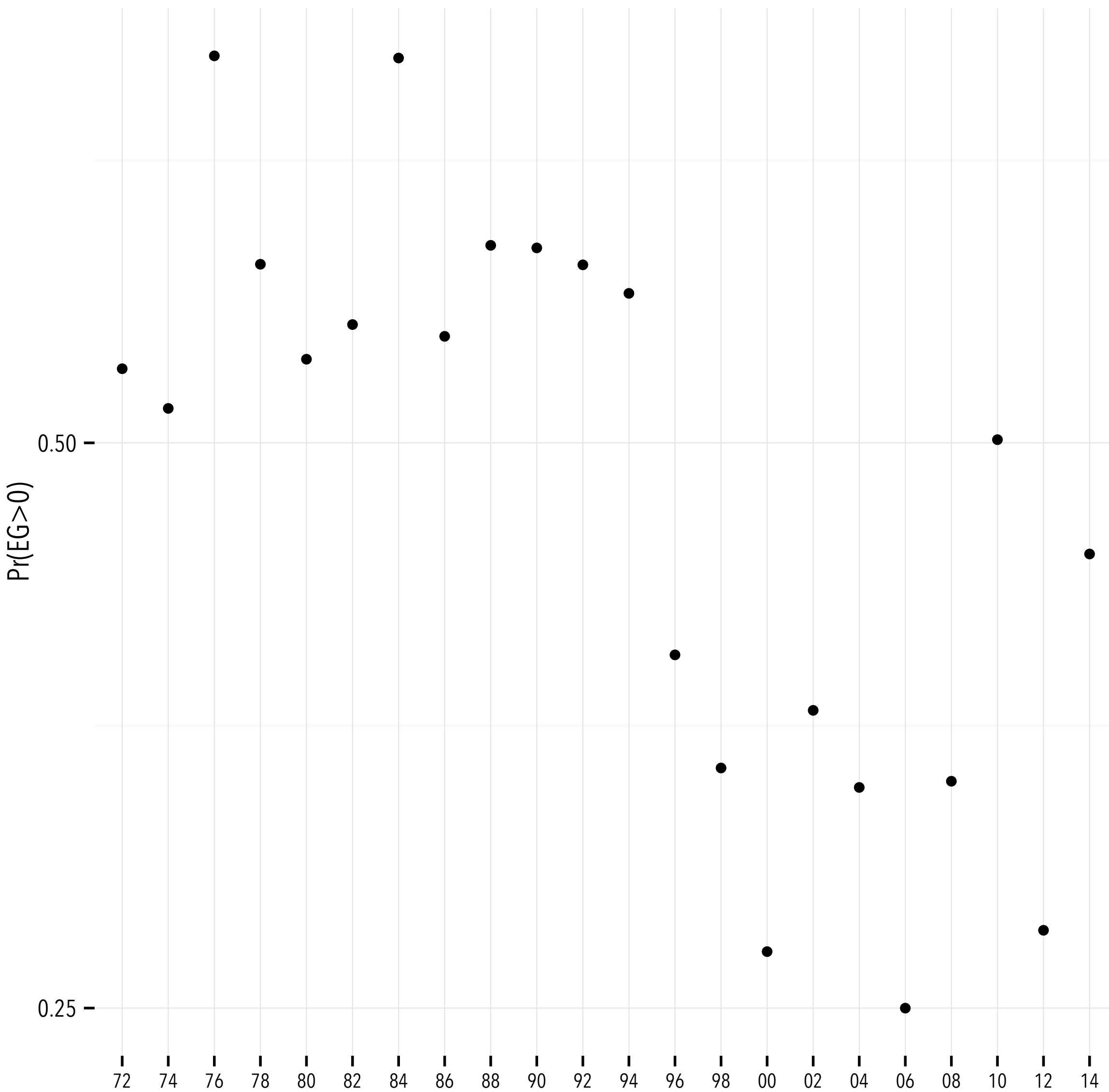

Figure 21: Proportion of efficiency gap measures that are positive, by two year intervals.

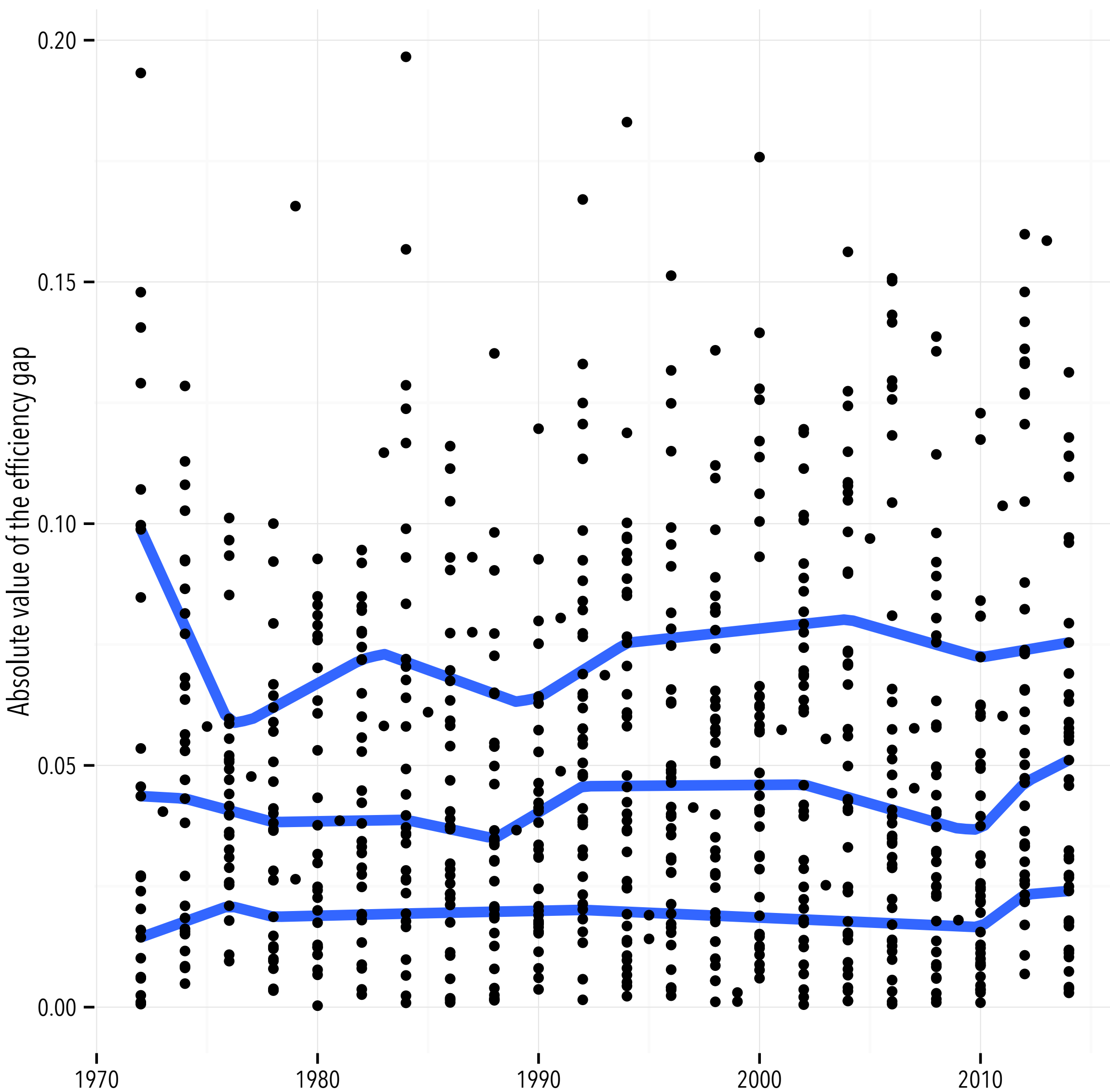

Figure 22: Absolute value of efficiency gap measures, over time. The lines are smoothed estimates of the 25th, 50th and 75th quantiles of the absolute value of the efficiency gap measure, weighted by the precision of each *EG* measure.

9.3 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 these election-specific factors will contribute to election-to-election variation in the efficiency gap.

Precisely because we expect a reasonable degree of election-to-election variation in the efficiency gap, we assess the magnitude of this "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*.

About 76% of the variation in the *EG* estimates is between-plan variation. The *EG* measure does vary election-to-election, but there is a moderate to strong "plan-specific" component to variation in the *EG* scores. We conclude that the efficiency gap *is* measuring an enduring feature of a districting plan.

We examine some particular districting plans. The 786 elections in this analysis span 150 districting plans. For plans with more than one election, we compute the standard deviation of the sequence of election-specific *EG* measures observed under the plan. These standard deviations range from .011 (Kentucky's plan in place for just two elections in 1992 and 1994, or Indiana's plan 1992-2000) to .079 (Delaware's plan between 2002 and 2010).

A highly variable plan: Deleware 2002-2010. Figure 23 shows the seats, votes and *EG* estimates produced under the Delaware 2002-2010 plan. This is among the most variable plans we observe with respect to the *EG* measure. An efficiency gap running against the Democrats for 2002, 2004 and 2006 (the latter election saw Democrats win only 18 seats out of 41 with 54.5% of the state wide vote) falls to a small gap in 2008 ($V = 0.584, S = 25/41 = .61, EG = -0.058$) and

Delaware ends the decade with a positive efficiency gap in 2010. The Democratic district-average two-party vote share fell to $V = 0.561$ in 2010, but translated into $S = 26/41 = .63, EG = 0.012$.

A plan with moderate variability in the EG. The median, within-plan standard deviation of the *EG* is about .03. This roughly corresponds to the within-plan standard deviation of the *EG* observed under the plan in place for five Wisconsin state legislative elections 1992-2000, presented in Figure 24. This was a plan that generated relatively small values of *EG* that alternated sign over the life of the plan: negative in 1992, positive in 1994 and 1996, and negative in 1998 and 2000.

A low variance plan, Indiana 1992-2000. See Figure 25. The *EG* measures recorded under this plan are all relatively small and positive, ranging from 0.008 to 0.041 and correspond to an interesting period in Indiana state politics. Democrats won 55 of the 100 seats in the Indiana state house in the 1992 election with what I estimate to be just over 50% of the district-average vote (29 of 100 seats were uncontested). Democratic vote share fell to about 45% in the 1994 election (38 uncontested seats), and Democrats lost control of the legislature. The 1996 election resulted in a 50-50 split in the legislature. Democrats won legislative majorities in the 1998 and 2000 elections, while the last election might have been won by Democrats with just less than 50% of the district-vote; I estimate $V = 0.495 \pm .012$ and $EG = 0.041$.

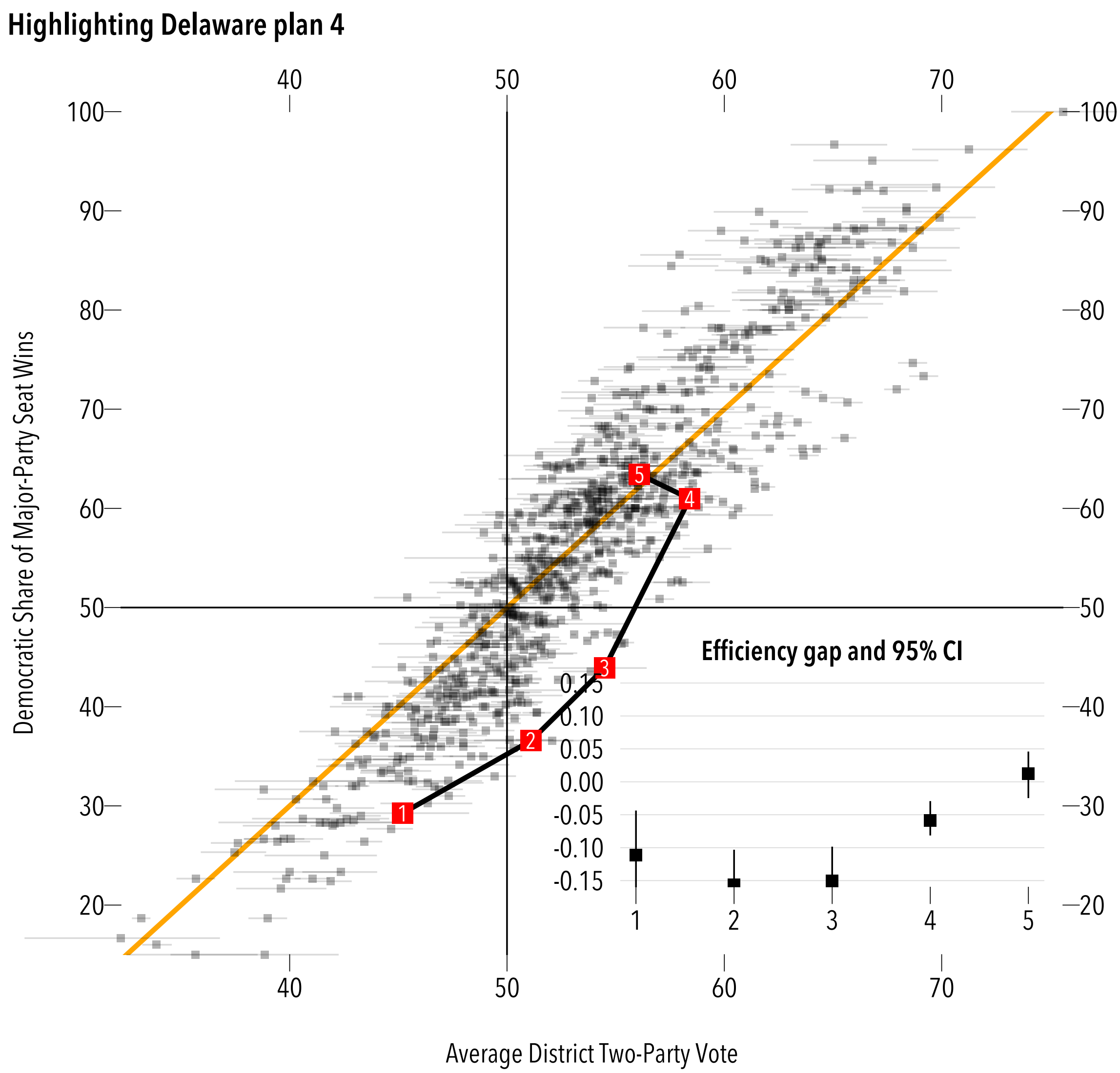

Figure 23: Seats, votes and the efficiency gap recorded under the Delaware plan, 2002-2010. Orange line shows the seats-votes curve if the efficiency gap were zero; the efficiency gap in any election is the vertical distance from the corresponding data point to the orange line. Gray points indicate elections from other states and elections (1972-2014). Horizontal lines cover a 95% credible interval for Democratic average district two-party vote share, given imputations in uncontested districts. The inset in the lower right shows the sequence of efficiency gap measures recorded under the plan; vertical lines are 95% credible intervals.

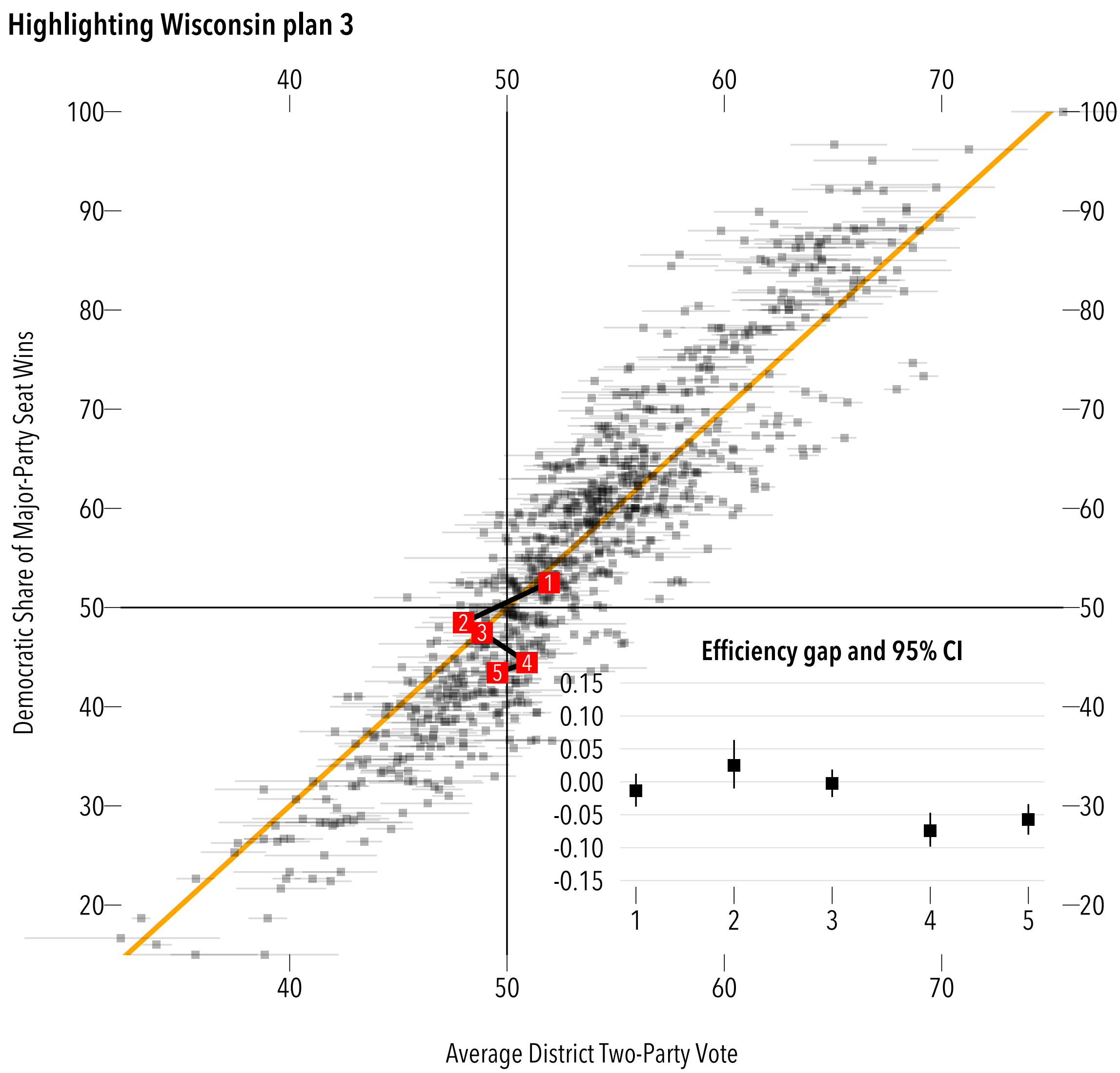

Figure 24: Seats, votes and the efficiency gap recorded under the Wisconsin plan, 1992-2000. Orange line shows the seats-votes curve if the efficiency gap were zero; the efficiency gap in any election is the vertical distance from the corresponding data point to the orange line. Gray points indicate elections from other states and elections (1972-2014). Horizontal lines cover a 95% credible interval for Democratic average district two-party vote share, given imputations in uncontested districts. The inset in the lower right shows the sequence of efficiency gap measures recorded under the plan; vertical lines are 95% credible intervals.

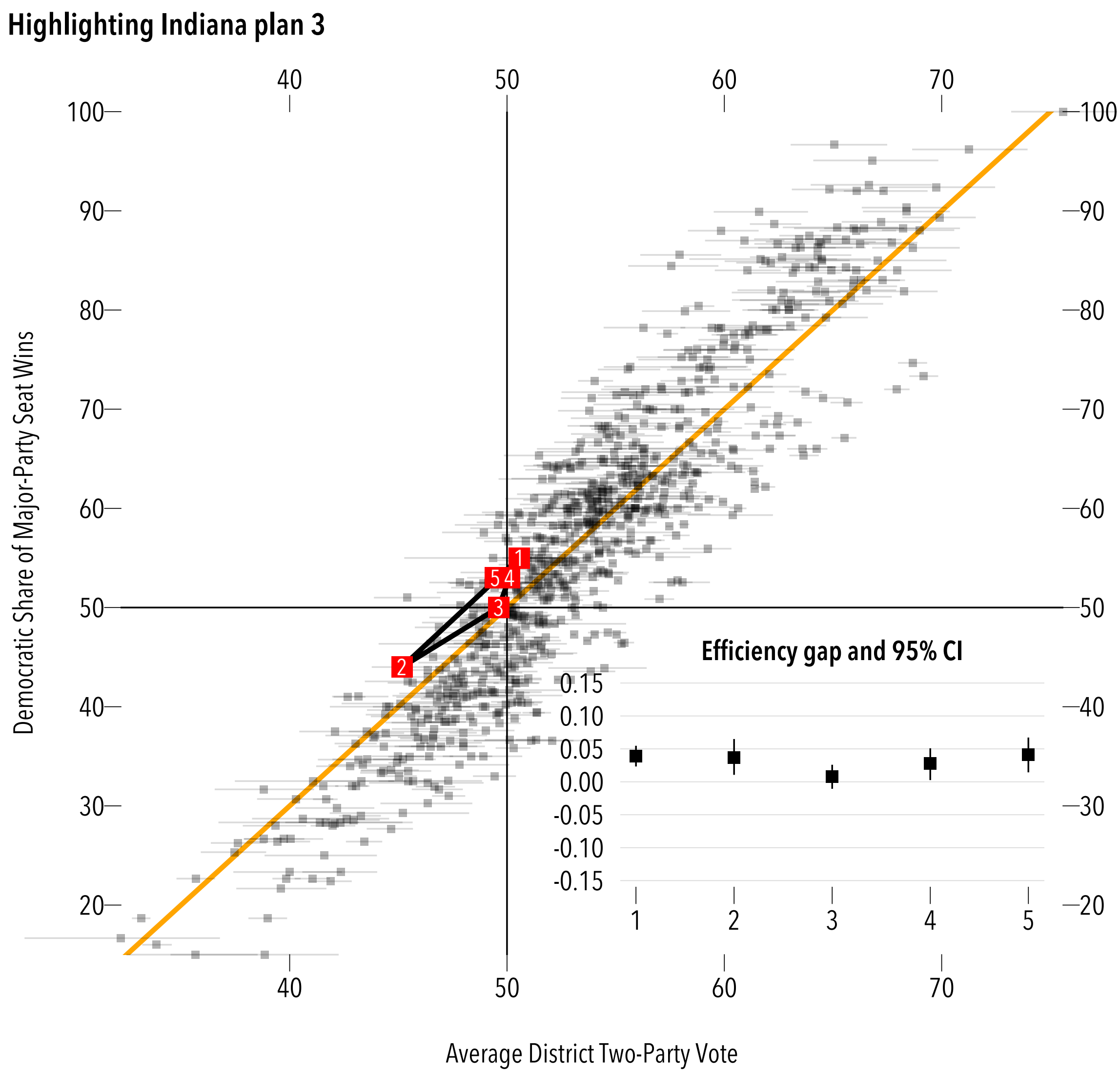

Figure 25: Seats, votes and the efficiency gap recorded under the Indiana plan, 1992-2000. Orange line shows the seats-votes curve if the efficiency gap were zero; the efficiency gap in any election is the vertical distance from the corresponding data point to the orange line. Gray points indicate elections from other states and elections (1972-2014). Horizontal lines cover a 95% credible interval for Democratic average district two-party vote share, given imputations in uncontested districts. The inset in the lower right shows the sequence of efficiency gap measures recorded under the plan; vertical lines are 95% credible intervals.

9.4 How often does the efficiency gap change sign?

Having observed a particular value of *EG*, how confident are we that:

- the *EG* measure is distinguishable from zero at conventional levels of statistical significance? That is, how sure are we as to the sign of any particular *EG* estimate? We addressed this question in section 9.1.
- it will be followed by one or more estimates of *EG* that are of the same sign?
- over the life of a districting plan, *EG* remains on one side of zero or the other?

The latter two questions are key. It is especially important that we assess the *durability* of the sign of the *EG* measure under a districting plan, if we seek to assert that a districting plan is a partisan gerrymander. We will see that *magnitude* and *durability* of the efficiency gap go together: *large* values of the efficiency gap don't seem to be capricious, but likely to be repeated over the life of a districting plan, consistent with partisan disadvantage being a systematic feature of the plan.

We begin this part of the analysis by considering temporally adjacent *pairs* of *EG* estimates. Can we be confident that these have the same sign? In general, yes. Of the full set of 786 elections for which we compute an efficiency gap estimate, 580 are temporally adjacent, within state and districting plan. Figure 26 shows that we usually see efficiency gap measures with the same sign; this probability exceeds 90% for almost half of the temporally adjacent pairs of efficiency gap measures. Averaged over all pairs, this "same sign" probability is 74%. While the efficiency gap does vary election to election, these fluctuations are not so large that the *sign* of the efficiency gap is likely to change election to election.

What about over the life of an entire redistricting plan? How likely is it that the efficiency gap retains the same sign over, say, three to five elections in a given state, taking into account election-to-election variation *and* uncertainty arising from the imputation procedures used for uncontested districts?

We have 141 plans that supply three or more elections with estimate of the efficiency gap. Of these, 17 plans are *utterly unambiguous* with respect to the sign of the efficiency gap estimates recorded over the life of the plan: for each of these plans we estimate the probability that the *EG* has the same sign over the life of the plan to be 100%. These plans are listed below in Table 1.

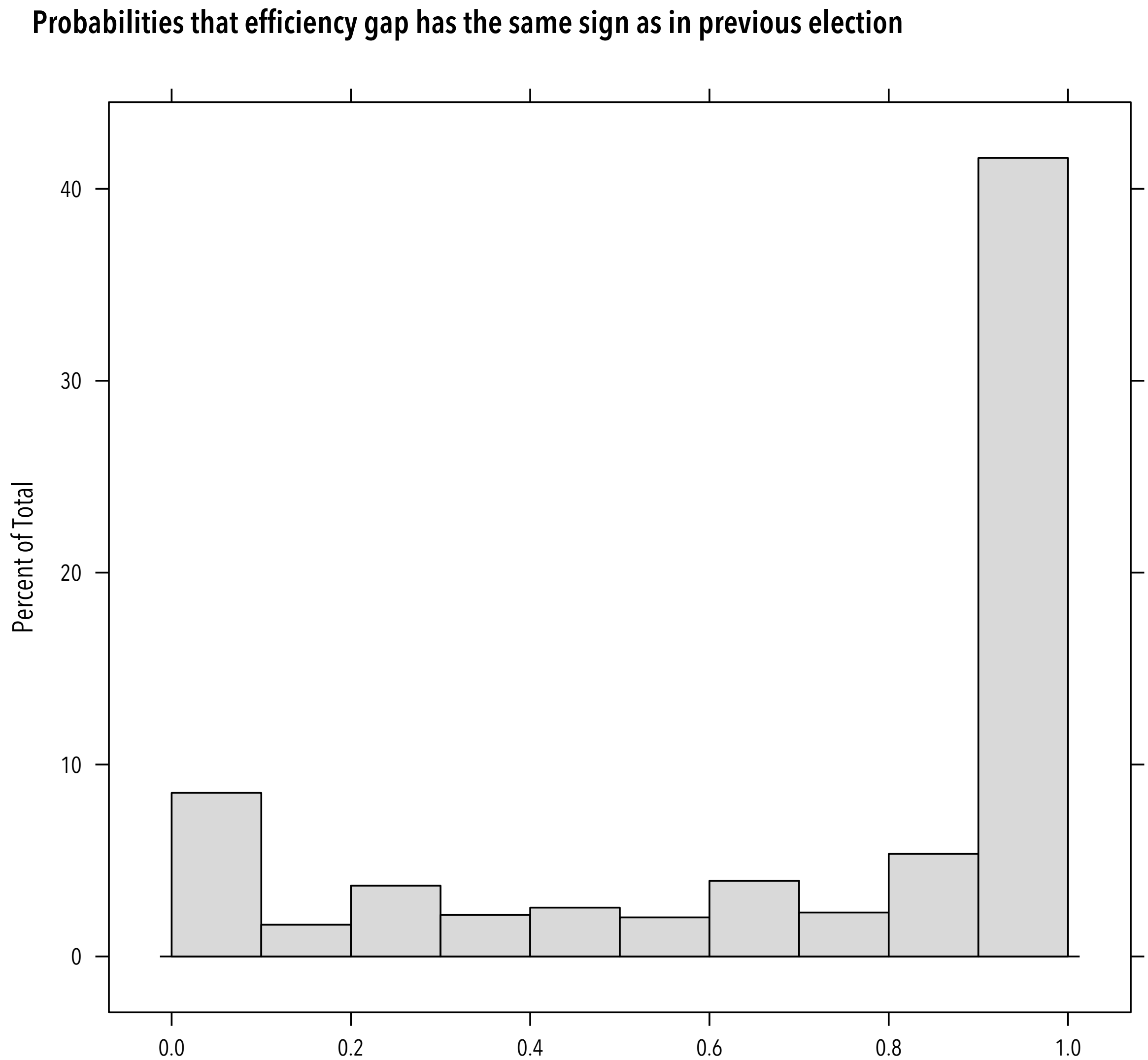

Figure 26: Stability in 580 successive pairs of efficiency gap measures

State	Plan	Start	End	EG avg	EG min	EG max
Florida	4	2002	2010	-0.112	-0.136	-0.084
New York	4	2002	2010	-0.111	-0.150	-0.078
Illinois	3	1992	2000	-0.103	-0.136	-0.058
Michigan	4	2002	2010	-0.103	-0.130	-0.077
New York	3	1992	2000	-0.098	-0.139	-0.048
New York	1	1972	1980	-0.097	-0.108	-0.079
Missouri	4	2002	2010	-0.091	-0.142	-0.061
Ohio	4	2002	2010	-0.090	-0.143	-0.049
New York	2	1982	1990	-0.084	-0.120	-0.028
Ohio	3	1994	2000	-0.083	-0.109	-0.025
Michigan	3	1992	2000	-0.080	-0.128	-0.019
Wisconsin	4	2002	2010	-0.076	-0.118	-0.039
Colorado	2	1982	1990	-0.075	-0.117	-0.055
Colorado	1	1972	1980	-0.041	-0.067	-0.018
California	3	1992	2000	-0.041	-0.057	-0.018
Pennsylvania	2	1982	1990	-0.033	-0.056	-0.020
Florida	1	1972	1980	0.070	0.052	0.099

Table 1: Plans with no doubt as to the sign of the efficiency gap over the life of the plan (3+ elections).

Interestingly, these plans with an utterly unambiguous history of one-sided *EG* measures are almost all plans with efficiency gaps that are disadvantagous to Democrats. Michigan's 2002-2010 plan is on this list, as is the plan in place in Wisconsin 2002-2010 (average *EG* of -.076).

We examine this probability of "3+ consecutive *EG* measures with the same sign" for all of the plans with 3 or more elections in this analysis. 35% of 141 plans with 3 or more elections have at least a 95% probability of recording plans with *EG* measures with the same sign. If we relax this threshold to 75%, then 46% of plans with 3 or more elections exhibit *EG* measures with the same sign. Again, there is a reasonable amount of within-plan movement in *EG*, but in a large proportion of plans the efficiency gap appears to be a stable attribute of the plan.

10 A threshold for the efficiency gap

We now turn to the question of what might determine a threshold for determining if the EG is a *large and enduring* characteristic of a plan. We pose the problem as follows:

> for a given threshold $EG^* > 0$, what is the probability that having observed a value of $EG \geq EG^*$ we then see $EG < 0$ in the remainder of the plan?

To answer this we compute

- if (and optionally, when) a plan has $EG \geq EG^*$;
- conditional on seeing $EG \geq EG^*$, do we also observe $EG < 0$ (a sign flip) in the same districting plan?

For $EG < 0$, the computations are reversed: conditional on seeing $EG < EG^*$, do we also see $EG > 0$ under the same plan?

Figure 27 displays two proportions, plotted against a series of potential thresholds on the horizontal axis. The two plotted proportions are:

- the proportion of plans in which we observe an EG more extreme than the specified threshold EG^* (on the horizontal axis);
- among the plans that trip the specified threshold, the proportion in which we see a EG in the same plan with a different sign to EG^*.

Plans with at least one election with $|EG| > .07$ are reasonably common: over the entire set of plans analyzed here — and again, with the uncertainty in EG estimates taken into account — there is about a 20% chance that a plan will have at least one election with $|EG| < .07$.

Observing $EG > .07$ is not a particularly informative signal with respect to the other elections in the plan. Conditional on observing an election with $EG > .07$ (an efficency gap favoring Democrats), there is an a 45% chance that *under the same plan* we will observe $EG < 0$. That is, making an inference about a plan on the basis of one election with $EG > .07$ would be quite risky. Estimates

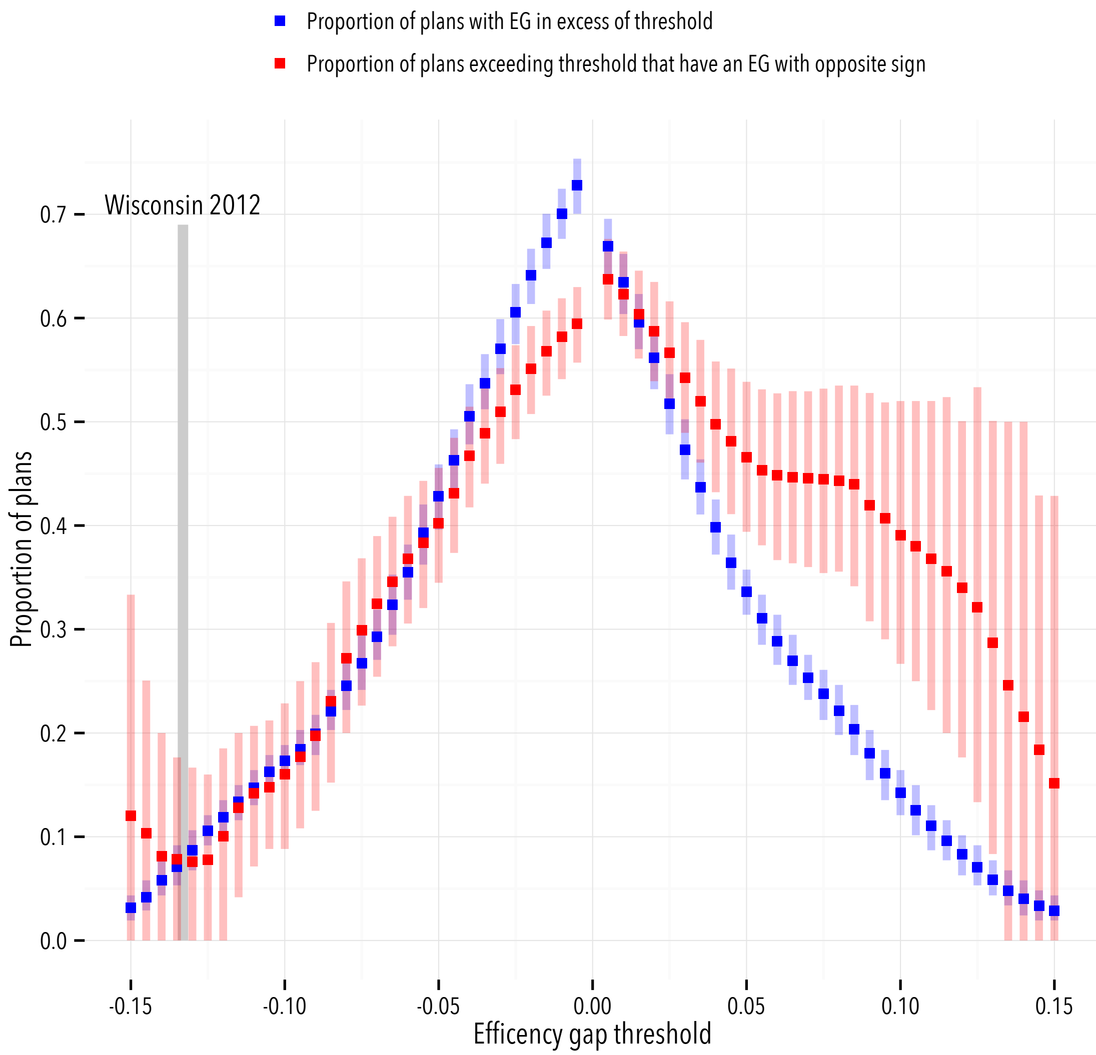

Figure 27: Proportion of plans that (a) record an efficiency gap measure at least as extreme as the value on the horizontal axis; and (b) conditional on at least one election with *EG* in excess of this threshold (not necessarily the first election), the proportion of plans where there is another election in the plan with an *EG* of the opposite sign.

of the “sign flip” rate conditional on a plan generating a relatively large, pro-Democratic *EG* estimates are quite unreliable because there are so few plans generating large, pro-Democratic *EG* estimates to begin with; note the confidence intervals on the “sign flip” rate get very wide as the data become more scarce on the right hand side of the graph.

This finding is not symmetric. The “signal” $EG < -.07$ (an efficiency gap disadvantageous to Democrats) is much more informative about other elections in the plan than the opposite signal $EG > .10$ (a pro-Democratic efficiency gap). If any single election in the plan has $EG < -.07$ then the probability that *all* elections in the plan have $EG < 0$ is about .80. That is, there is a smaller degree of within-plan volatility in plans that disadvantage Democrats. Observing a relatively low value of the *EG* such as $EG < -.07$ is much more presumptive of a systematic and enduring feature of a redistricting plan than the opposite signal $EG > .07$. Efficiency gap measures that appear to indicate a disadvantage for Democrats are thus more reliable signals about the respective districting plan than efficiency gap measures indicating an advantage for Democrats.

We repeat this previous exercise, but restricting attention to more recent elections and plans, with the results displayed in Figure 28. Again we see that plans with pro-Democratic *EG* measures are quite likely to also generate an election with $EG < 0$; and again, note that estimates of the “sign flip” rate are quite unreliable because there are so few plans generating large, pro-Democratic *EG* estimates to begin with.

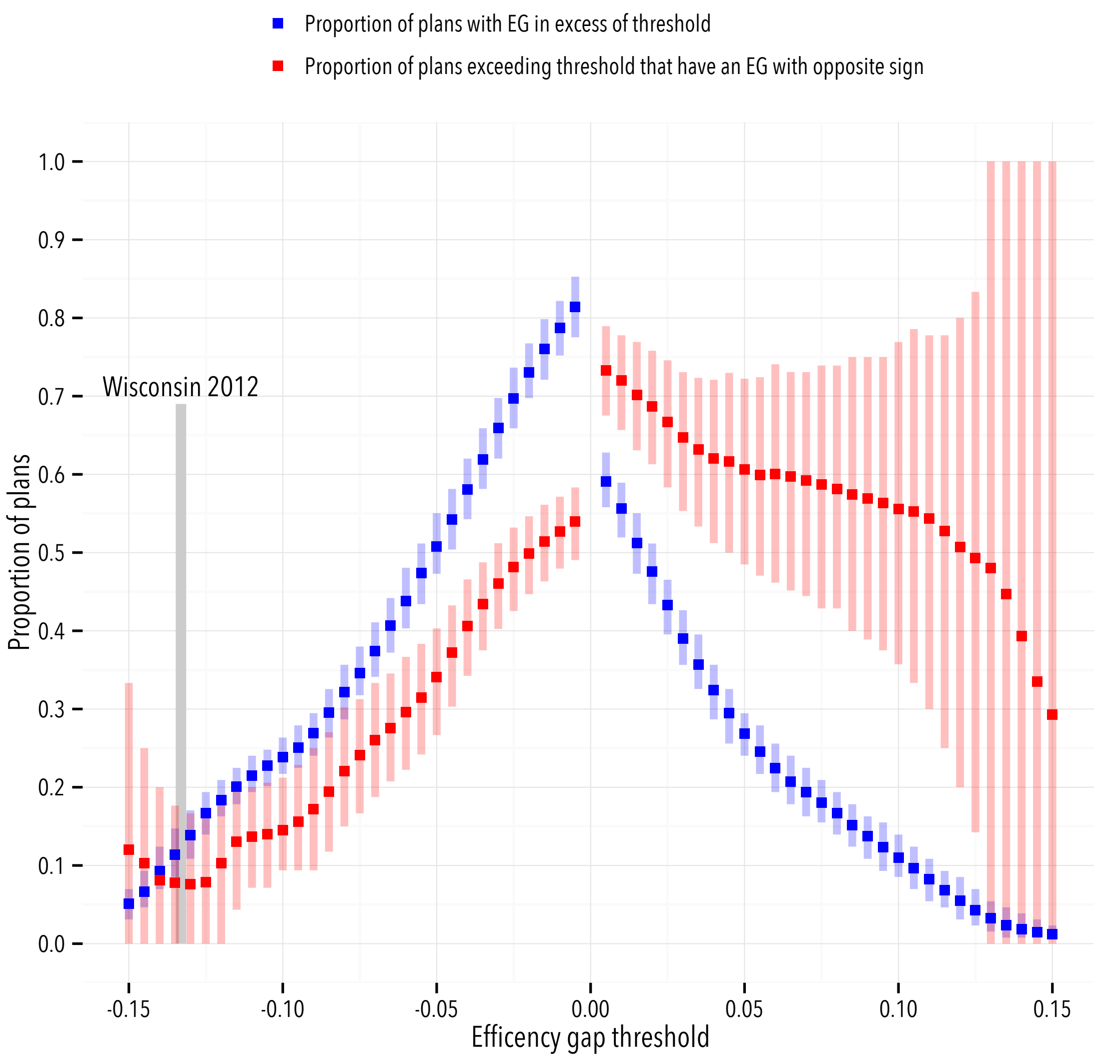

Figure 28: Proportion of plans in which (a) the efficiency gap measure is at least as extreme as the value on the horizontal axis; and (b) of these plans with at least one election with *EG* in excess of this threshold (not necessarily the first election), the proportion of plans in which there is another election in the plan with an *EG* of the *opposite* sign. Analysis of state legislative elections in 129 plans, 1991-present.

10.1 Conditioning on the first election in a districting plan

We also compute this probability of a sign flip in *EG* conditional on the magnitude of the *EG* observed with the *first* election under a districting plan. We perform this analysis twice: (1) for all elections in the data set and (2) for elections held under plans adopted in 1991 or later.

Figures 29 and 30 display the results of these analyses. First, over the full set of data (Figure 29) we observe a roughly symmetric set of *EG* scores in the first election under a plan. But we seldom see plans in the 1990s or later that commence with a large, pro-Democratic efficiency gap; the probability of a first election having $EG > .10$ is zero and the probability of a first election having $EG > .05$ (historically, not a large *EG*) is only about 11%. Negative efficiency gaps (not favoring Democrats) are much more likely under the first election in the post-1990 plans: almost 40% of plans open with $EG < -.05$ and about 20% of plans open with $EG < -.10$.

As noted earlier, pro-Democratic efficiency gaps seem much more fleeting than pro-Republican efficiency gaps. Conditional on a pro-Republican estimate of $EG > 0$ in the first election under a plan, the probability of seeing *EG* change sign over the life of the plan is almost always around 40% (1972-2014, Figure 29) or 50% (1991-present, Figure 30).

A very different conclusion holds if the first election observed under a plan indicates a sizeable efficiency gap working to disadvantage Democrats. In fact, the more negative the initial *EG* observed under a plan, the more confident we can be that we will continue to observe $EG < 0$ over the sequence of elections to follow under the plan. Conditional on a first election with $EG < -.10$, the probability of *all subsequent* efficiency gaps being negative is about 85%. Indeed, it is more likely than not that if the first election has $EG < 0$ (no matter how small), then so too will all subsequent elections (a 60% chance of this event).

Note that the Current Wisconsin Plan opens with $EG = -.13$ in the 2012 election. Analysis of efficiency gap measures in the post-1990 era (Figure 30) indicates that conditional on an *EG* measure of this size and sign, there is a 100% probability that *all subsequent elections* held under that plan will also have efficiency gaps disadvantageous to Democrats. That is, in the post-1990 era, if a plan's first election yields $EG \leq -.13$, we *never* see a subsequent election under that plan yielding a pro-Democratic efficiency gap. In short, a signal such as

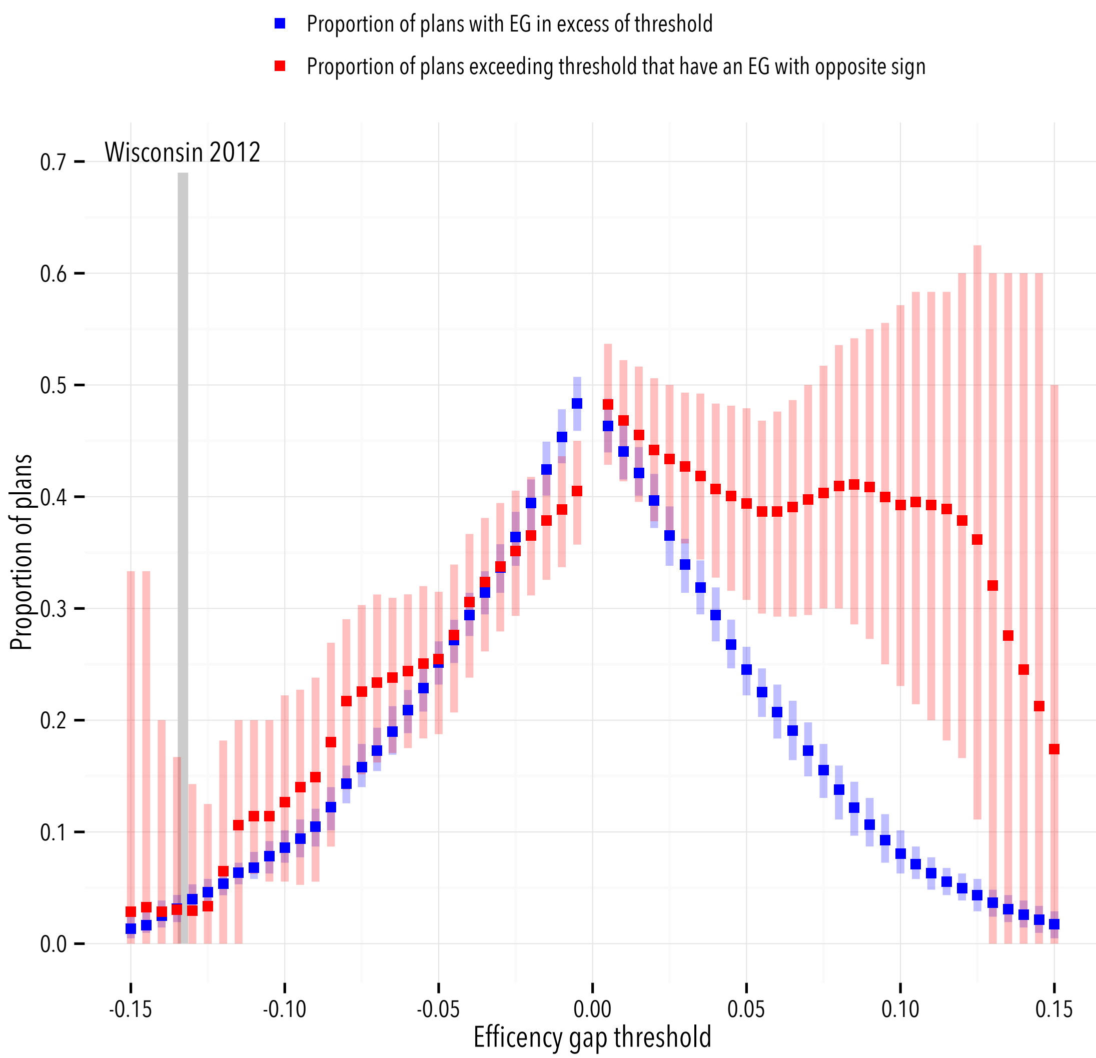

Figure 29: Proportion of plans in which the *first election* (a) has an efficiency gap measure at least as extreme as the value on the horizontal axis; and (b) conditional on the first election having an *EG* in excess of this threshold, the proportion of those plans in which a *subsequent election* has an *EG* of the *opposite* sign. Analysis of all state legislative elections in all plans with more than one election, 1972-present.

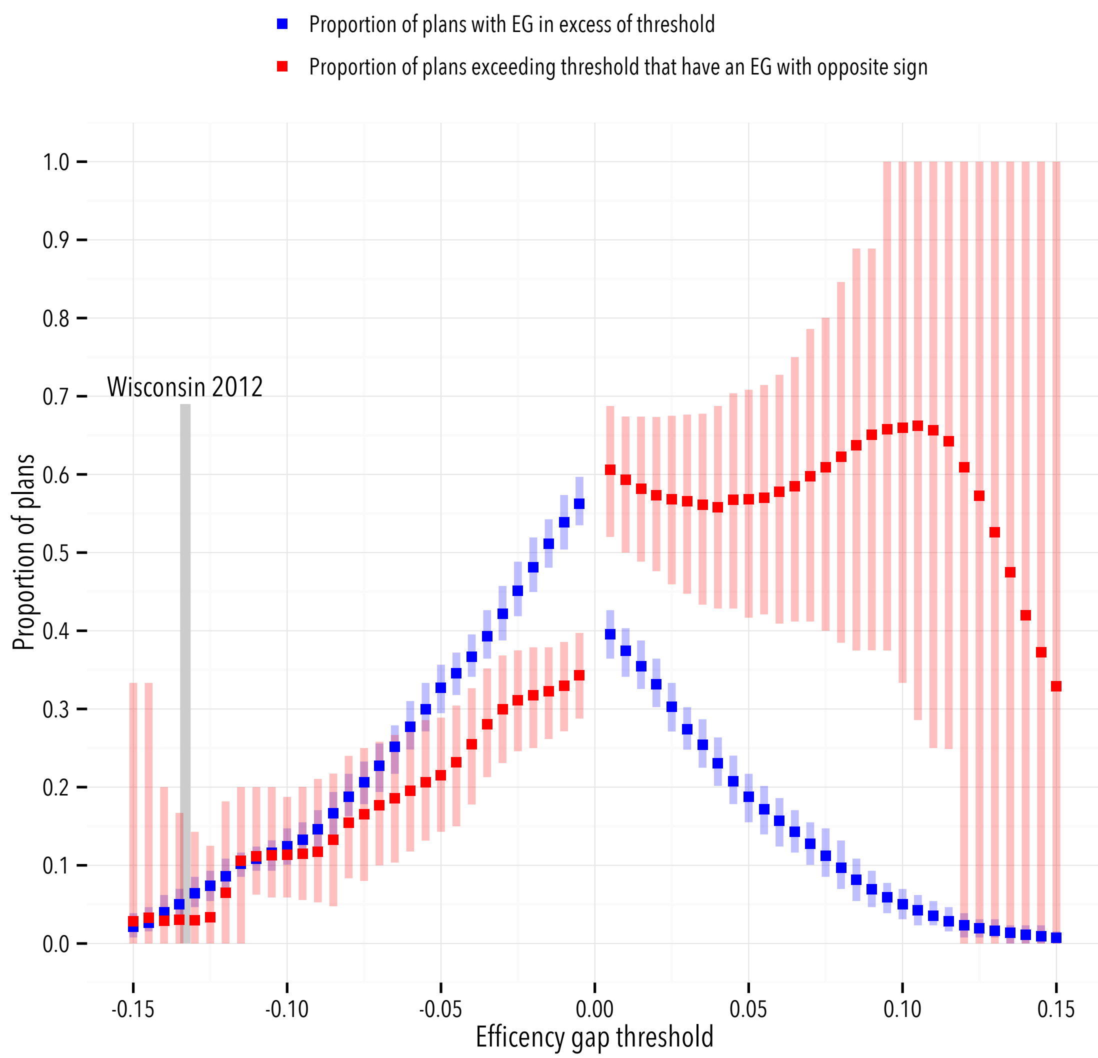

Figure 30: Proportion of plans in which the *first election* (a) has an efficiency gap measure at least as extreme as the value on the horizontal axis; (b) conditional on the first election having an *EG* in excess of this threshold, the proportion of those plans in which a *subsequent election* has an *EG* of the *opposite* sign. Analysis of state legislative elections in 129 plans, 1991-present.

$EG \leq -.13$ is extremely reliable with respect to the districting plan that generated it, at least given the post-1990 record.

10.2 Conditioning on the first two elections in a districting plan

The difficulty with conditioning on the first two elections of a districting plan is that the data start to thin out. In the entire data set there simply aren't many districting plans that equal or surpass the two, relatively large values of *EG* observed in Wisconsin in the first two elections of the current plan. Indeed, the only cases with a similar history of *EG* measures like Wisconsin's in 2012 and 2014 are contemporaneous cases: Florida, Michigan, and North Carolina in 2012 and 2014.

We relax the threshold of what counts as a similar case to encompass plans whose first two efficiency gap measures are within 75% of the magnitude of Wisconsin's 2012 and 2014 *EG* measures; we now pick up 11 roughly comparable cases, 4 of which date from earlier decades. Again, this is testament to how recent decades have seen an increase in the prevalence of larger, negative values of the efficiency gap.

For the four prior cases we plot the sequence of *EG* estimates in Figure 31. With the exception of the last election in the highly unusual Delaware sequence (among the most volatile observed in the data set; see section 9.3), the other proximate cases all go on to record efficiency gap measures that are below zero over the balance of the plan. We stress that four cases doesn't provide much basis for comparison, but this only speaks to the fact that the sequence of two large, negative values of the efficiency gap in Wisconsin in 2012 and 2014 are virtually without historical precedent. We have little guidence from the historical record as to what to expect given an opening sequence of *EG* measures like the ones observed in Wisconsin. But the little evidence we do have suggests that a stream of similarly sized, negative values of the efficiency gap are quite likely over the balance of the districting plan.

10.3 An actionable EG threshold?

We now consider a more general question: what is an actionable threshold for the efficiency gap?

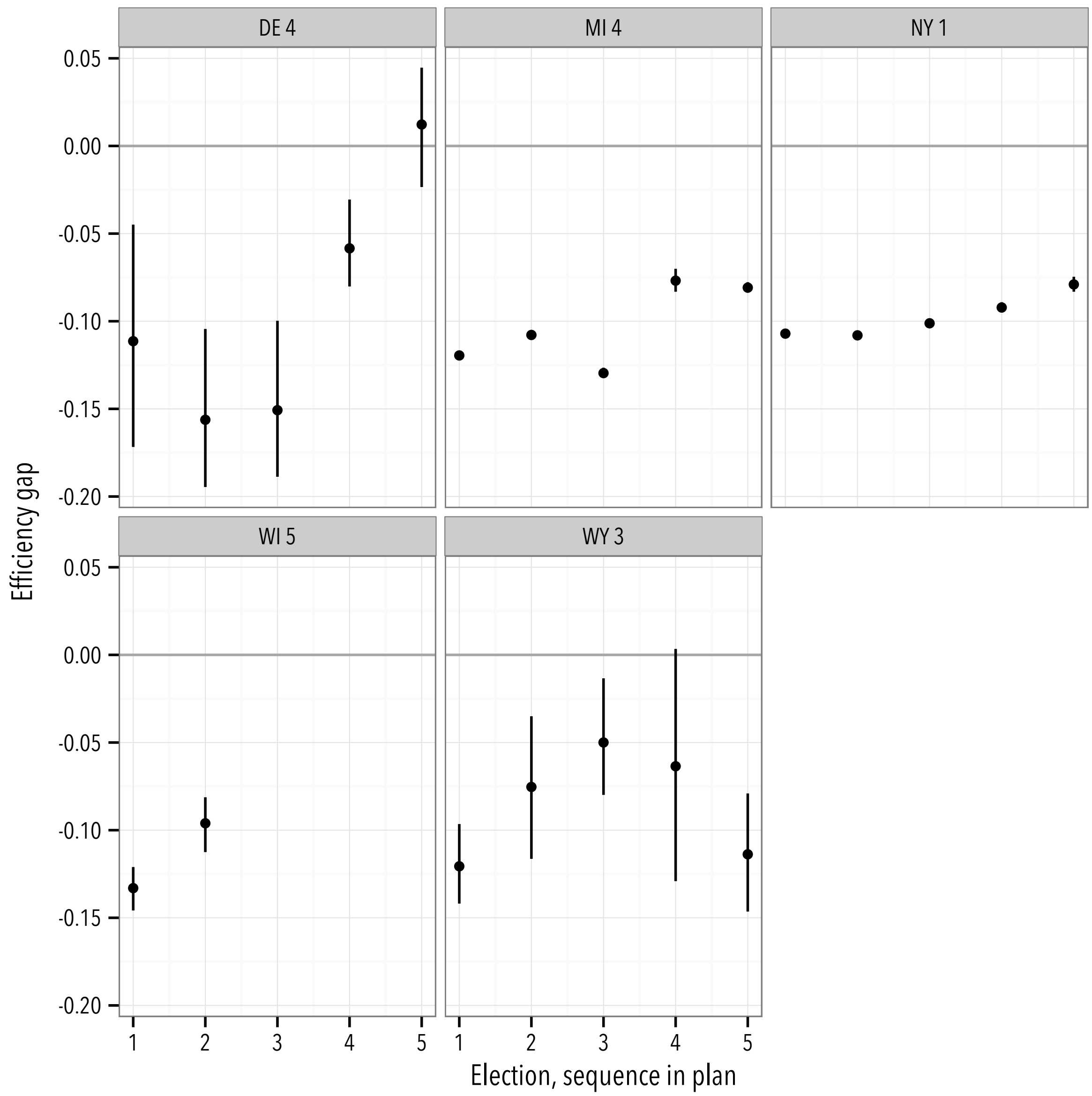

Figure 31: Sequence of *EG* estimates observed over the life of districting plans, for pre-2010 plans with first two *EG* scores within 75% of the magnitude of the *EG* scores observed in Wisconsin in 2012 and 2014.

First, recall that relatively small *EG* estimates are likely to be swamped by their estimation uncertainty, depending on the proportion of uncontested districts in the given election and the statistical procedures. In every instance though, this is an empirical question; at least in the approach I present here, each *EG* estimate I generate is accompanied with uncertainty bounds, letting us assess the *probability* that a given estimate is positive or negative. Figure 19 provides a summary of the relationship between the size of the *EG* estimate and the "statistical significance" of the estimate (in the sense that the 95% credible interval for each estimate does not overlap zero).

Second, the distribution of *EG* statistics in the 1972-2014 period is roughly symmetric around zero. Reference to this empirical distribution might also be helpful in setting actionable thresholds, and answering the question "is the *EG* measure at issues large relative to those observed in the previous 40 years of state legislative elections?" Double digit *EG* measures (-.10 or below; .10 or above) are pushing out into the extremes of the observed distribution of *EG* estimates: *EG* estimates of this magnitude are comfortably past the question of "statistical significance." Just 15% of the 786 *EG* measures generated in this analysis are below -.07; fewer than 12% are greater than .07.

We do need to be careful when making these kinds of *relative* assessments about the magnitude of the efficiency gap. If pro-Republican gerrymandering is widespread, then it will be less unusual to see a large, negative *EG* estimate, at least contemporaneously; in fact this appears to the case in the post-2010 set of elections, where the longer-term distinctiveness of the Wisconsin numbers is matched and in some cases exceeded by other states also recording unusually large, negative *EG* estimates (e.g., Florida, Michigan, Virginia and North Carolina). This speaks to the utility of the longer-term, historical analysis in both Stephanopolous and McGhee (2015) and in this report. It it is important to remember that $EG = 0$ corresponds to a partisan symmetry in wasted vote rates; we should be wary of arguments that would lead us to tolerate small to moderate levels of the efficiency gap because they appear to be the norm in some period of time, or in some set of jurisdictions.

In any litigation, much will turn on the question of *durability* in the efficiency gap, and this concern motivates much of the preceeding analysis. We cannot wait until three, four, or more elections have transpired under a plan in order to assess its properties. Courts will be asked to assess a plan based on only one *EG* estimate, or two. Analysis of the sort I provide here will be informative in these cases, assessing whether the estimate is so large that the historical record suggests that the first election's *EG* estimate is a reliable indicator as an enduring feature of the plan, and not an election-specific aberration.

10.4 Confidence in a given threshold

Figures 32 and 33 present my estimate of a "confidence rate" associated with a range of possible "actionabale thresholds" for the efficiency gap. These figures essentially re-package the information shown in Figures 29 and 30. Suppose a court rejects or amends every plan with a first election *EG* more extreme (further away from zero) than the proposed threshold shown on the horizontal axis of these graphs. A certain number of plans fail to trip this threshold, and so are upheld by the courts if they are challenged. Of those that do trip the threshold and are rejected by a court, what is our confidence that the plan, if left undisturbed, would go on to produce a sequence of *EG* measures that lie on the same side of zero as the threshold? Combining these two proportions gives us an overall confidence measure associated with a particular threshold.

This analysis points to a benchmark of about -.06 or -.07 as the actionable threshold given a first election with $EG < 0$ (Democratic disadvantage) or .08 or .09 when we observe $EG > 0$ in the first election under a redistricting plan (Democratic advantage); the asymmetry here reflects the fact that districting plans evincing apparent Democratic advantages are not as durable or as common (in recent decades) as plans presenting evidence of pro-Republican gerrymanders. At these proposed benchmarks the overall confidence rates are estimated to be 95%, with this confidence rate corresponding to a benchmark used widely in statistical decision-making in many fields of science.

Figures 32 and 33 also highlight that $EG < -.07$ or $EG > .07$ would be an extremely conservative threshold. On the pro-Democratic side, $EG > .07$ is a rare event. Districting plans unfavorable to Democrats, with $EG < -.07$ are not unusual; about 10% of post-1990 plans generate *EG* measures below -.07; the proportion of these plans that then record a sign flip is only about 10%; see Figure 30. If the presumption was that any plan with a first election showing

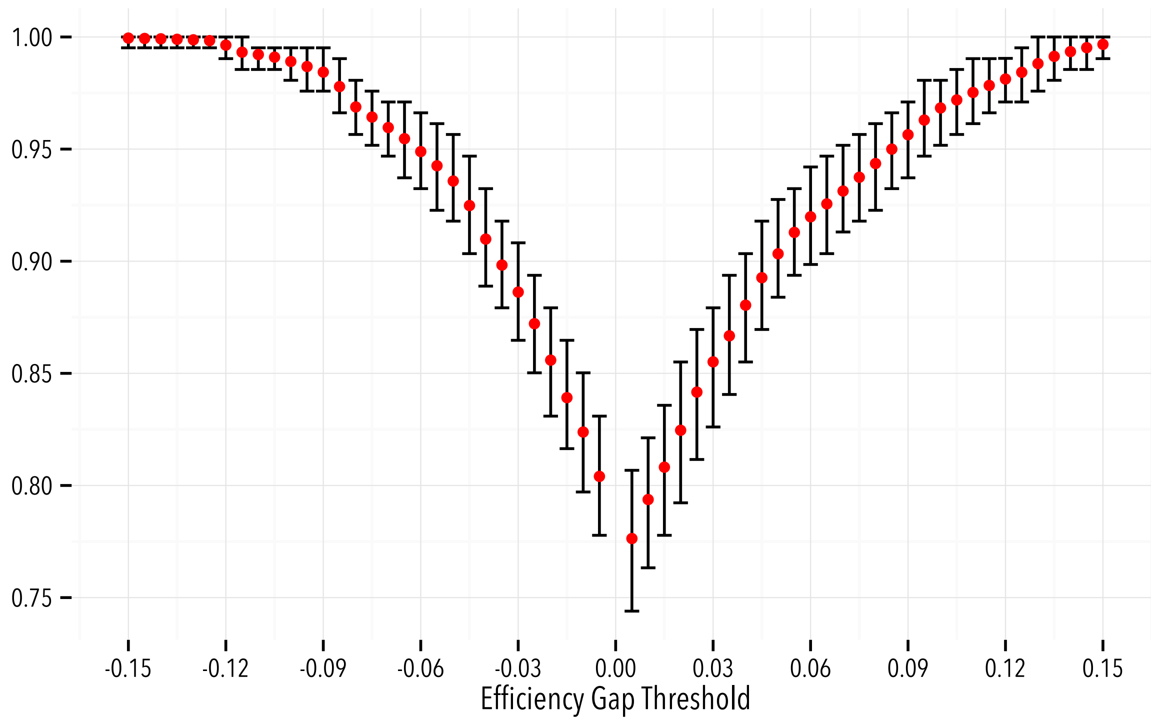

Figure 32: Proportion of plans being either (a) undisturbed or (b) if left undisturbed, would continue to produce one-sided partisan advantage (no sign change in subsequent *EG* measures), as a function of the proposed "first election," efficiency gap threshold (horizontal axis), based on analysis of all multi-election districting plans, 1972-2014. The proportion on the vertical axis is thus interpretable as the "confidence level" associated with intervention at a given first election, *EG* threshold. Vertical lines indicate 95% credible intervals.

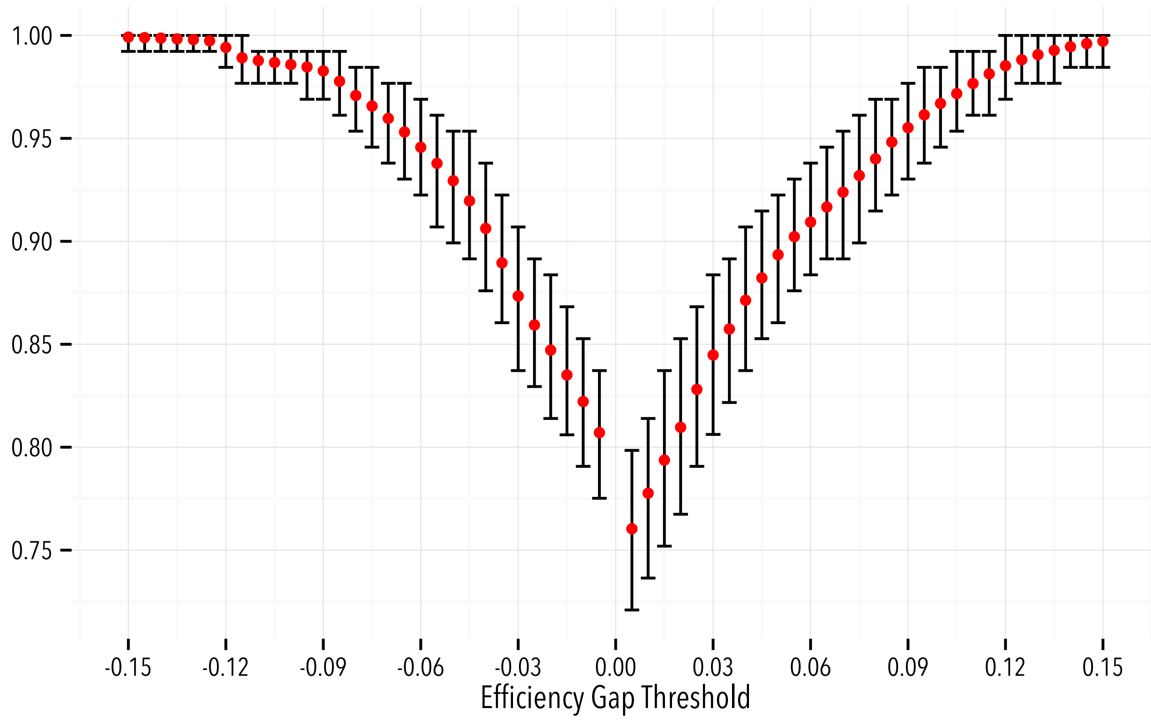

Figure 33: Proportion of plans being either (a) undisturbed or (b) if left undisturbed, would continue to produce one-sided partisan advantage (no sign change in subsequent *EG* measures), as a function of the efficiency gap threshold (horizontal axis), based on analysis of post-1990 plans and elections. The proportion on the vertical axis is thus interpretable as the "confidence level" associated with intervention at a given first election, *EG* threshold. Vertical lines indicate 95% credible intervals.

$EG < -.07$ would be rejected, then we'd be "wrong" to do so in about 10% of those cases (in the sense that if left in place, the plan would go on to produce at least one election with $EG > 0$). The total error rate in this case would be *1% of all plans*. Equivalently, 99% of all plans would be either left undisturbed or appropriately struck down or amended by a court, given the historical relationship between "first election" *EG* measures and the sequence of *EG* measures that follow.

11 Conclusion: the Wisconsin plan

Wisconsin has had two elections for its legislature under the plan currently in place, in 2012 and 2014. Both elections were subject to considerable rates of uncontestedness (27 of 99 seats in 2012 and 52 of 99 seats in 2014), but these rates are hardly unusual; Wisconsin's rates of uncontested districts in these two elections are low to moderate compared to other states. We use the relationship between state legislative election results and presidential election results in state legislative districts (and incumbency) to impute two-party vote shares in uncontested seats (see section 7.2). With a complete set of vote shares, we then compute average district-level Democratic two-party vote share (V) and note the share of seats (contested and uncontested) won by Democratic candidates (S).

In Wisconsin in 2012, and after imputations for uncontested seats, V is estimated to be 51.4% (± 0.6); recall that Obama won 53.5% of the two-party presidential vote in Wisconsin in 2012. Yet Democrats won only 39 seats in the 99 seat legislature ($S = 39.4\%$), making Wisconsin one of 7 states in 2012 where we estimate $V > 50\%$ but $S < 50\%$ and where Democrats failed to win a majority of legislative seats despite $V > 50$ (the other states are Florida, Iowa, Michigan, North Carolina and Pennsylvania). In 2014, V is estimated to be 48.0% (± 0.8) and Democrats won 36 of 99 seats ($S = 36.4\%$).

This provides the raw ingredients for computing the efficiency gap (*EG*) for these two elections (recalling equation 1). Repeating these calculations across a large set of state elections provides a basis for assessing whether the efficiency gap estimates for Wisconsin in 2012 and 2014 are noteworthy.

Wisconsin's efficiency gap measures in 2012 and 2014 are -.13 and -.10 (to two digits of precision). These negative estimates indicate the disparity between

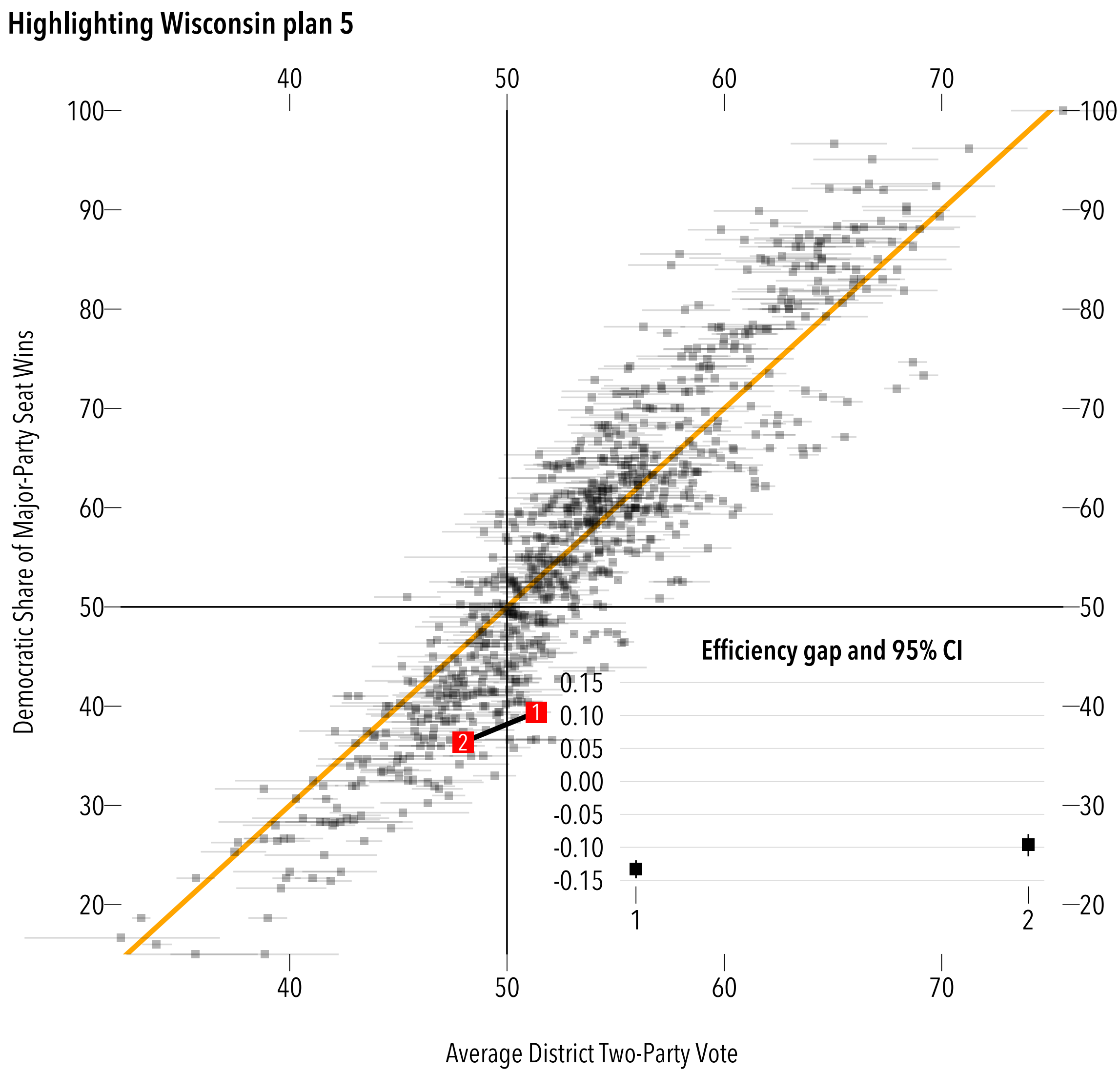

Figure 34: Seats, votes and the efficiency gap recorded under the Wisconsin plan, 2012 and 2014. Orange line shows the seats-votes curve if the efficiency gap were zero; the efficiency gap in any election is the vertical distance from the corresponding data point to the orange line. Gray points indicate elections from other states and elections (1972-2014). Horizontal lines cover a 95% credible interval for Democratic average district two-party vote share, given imputations in uncontested districts. The inset in the lower right shows the sequence of efficiency gap measures recorded under the plan; vertical lines are 95% credible intervals.

vote shares and seat shares in these elections, which in turn, is consistent with partisan gerrymandering. The negative *EG* estimates generated in 2012 and 2014 are unusual relative to Wisconsin's political history (see Figure 35). The 2012 estimate is the largest *EG* estimate in Wisconsin over the 42 year period spanned by this analysis (1972-2014); the 2014 estimate is the fourth largest (behind 2012, 2006 and 2004, although it is essentially indistinguishable from the 2004 estimate). The jump from the *EG* values being recorded towards the end of the previous districting plan in Wisconsin (2002-2010) to the 2012 and 2014 values strongly suggests that the districting plan adopted in 2011 is a driver of the change, systematically degrading the efficiency with which Democratic votes translate into Democratic seats in the Wisconsin state legislature.

Wisconsin's 2012 and 2014 *EG* estimates are also large relative to the *EG* scores being generated contemporaneously in other state legislative elections. Figure 36 shows *EG* estimates recorded under plans in place since the post-2010 census round of redistricting; the *EG* estimates are grouped by state and ordered, with Wisconsin highlighted. We have 78 *EG* scores from elections held since the last round of redistricting. Among these 79 scores, Wisconsin's *EG* scores rank eigth (2012, 95% CI 3 to 12) and seventeenth (2014, 95% CI 13 to 20).

The historical analysis reported above supports the proposition that Wisconsin'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 Wisconsin; indeed, there is virtually no precedent for the lop-sided, two election sequence of *EG* scores generated in Wisconsin in 2012 and 2014 in the data I analyze here (1972-2014). The closest historical analogs suggest that a districting plan that generates an opening, two-election sequence of *EG* scores like those from Wisconsin will continue to do so, generating seat shares for Democrats that are well below those we would expect from a neutral plan.

The Current Wisconsin Plan is generating estimates of the efficiency gap far in excess of the proposed, actionable threshold (see section 10). In 2012 elections to the Wisconsin state legislature, the efficiency gap is estimated to be -.13; in 2014, the efficiency gap is estimated to be -.10. Both measures are separately well beyond the conservative .07 threshold suggested by the analysis of efficiency gap measures observed from 1972 to the present.

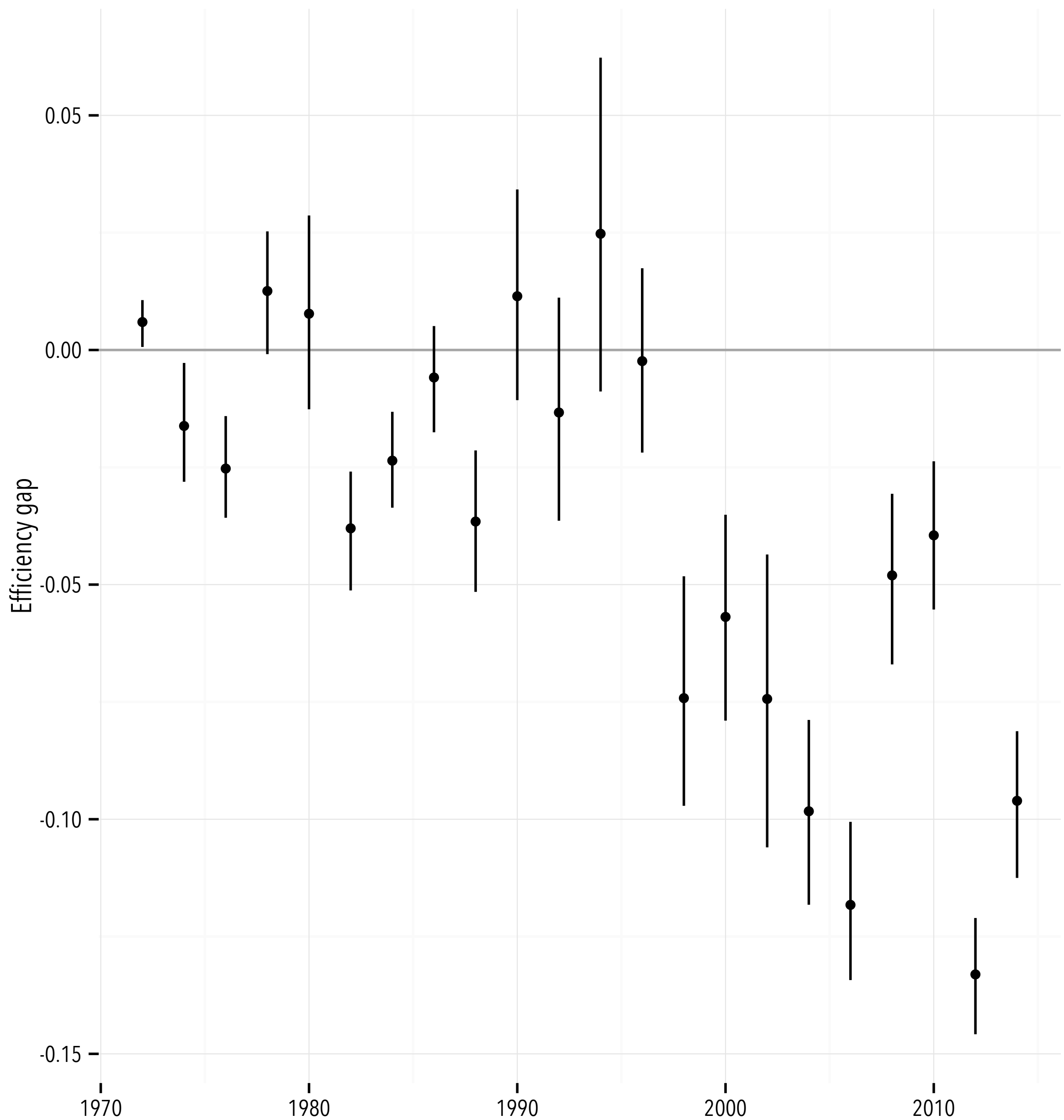

Figure 35: History of efficiency gap estimates in Wisconsin, 1972-2014. Vertical lines indicate 95% credible intervals.

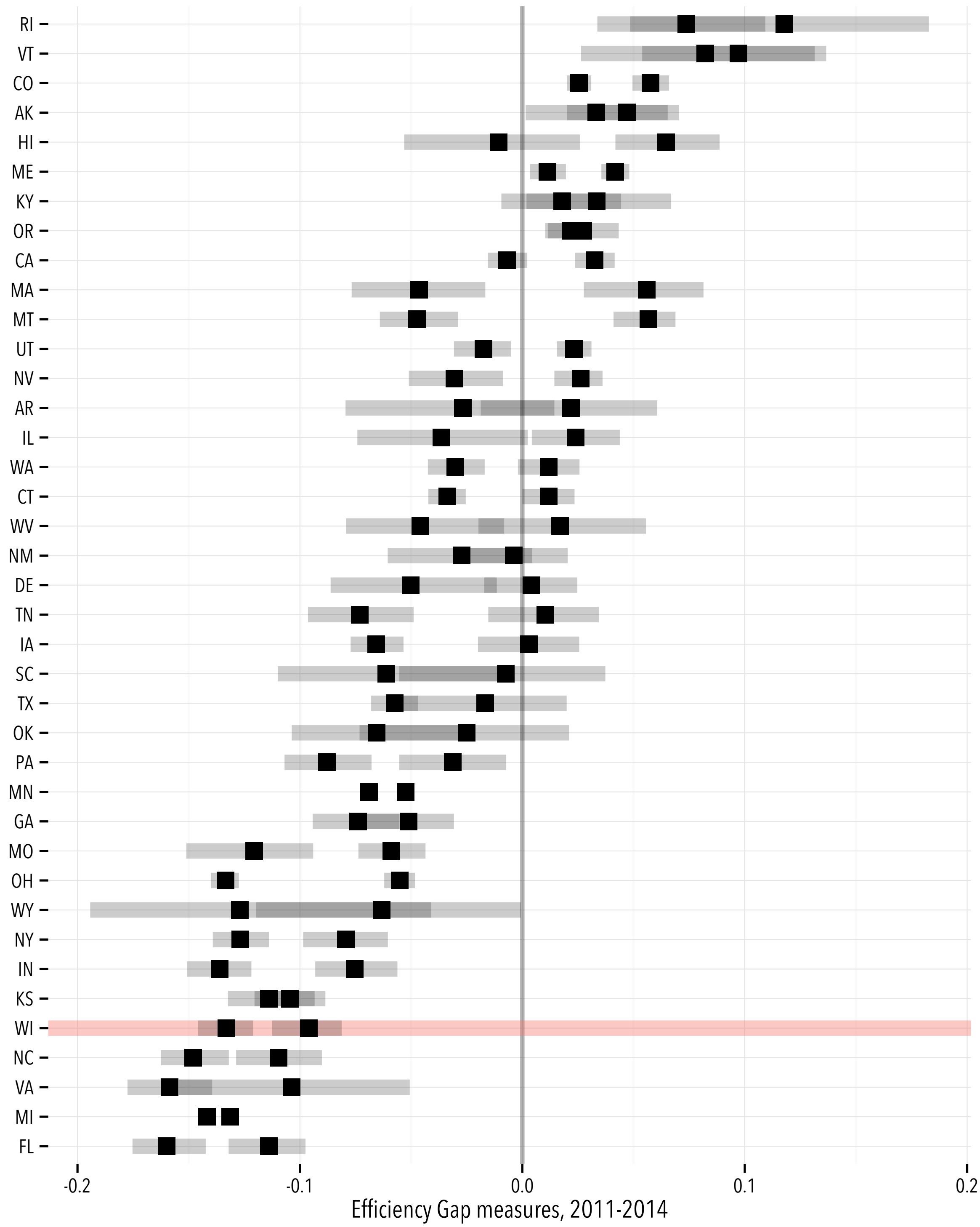

Figure 36: *EG* estimates in 2012 and 2014, grouped by state and ordered. Horizontal bars indicate 95% credible intervals.

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Gelman, Andrew and Gary King. 1990. "Estimating the Consequences of Electoral Redistricting." *Journal of the American Statistical Association* 85:274–82.

Kendall, M. G. and A. Stuart. 1950. "The Law of Cubic Proportion in Election Results." *The British Journal of Sociology* 1(3):183–196.

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Niemi, Richard G. and Simon Jackman. 1991. "Bias and Responsiveness in State Legislative Districting." *Legislative Studies Quarterly* 16:183–202.

Stephanopolous, Nicholas O. and Eric M. McGhee. 2015. "Partisan Gerrymandering and the Efficiency Gap." *University of Chicago Law Review* 82. Forthcoming.

Tufte, Edward R. 1973. "The Relationship Between Seats and Votes in Two-Party Systems." *American Political Science Review* 67:540–554.

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FACULTY APPOINTMENTS

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

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.

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.

ACADEMIC AND PROFESSIONAL LEADERSHIP

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

AMERICAN NATIONAL ELECTION STUDIES 2009-2013, PRINCIPAL INVESTIGATOR. With Gary Segura (Stanford) and Vincent Hutchings (Univ. Michigan), principal investigator of the single largest political science research project funded by the National Science Foundation ($11M). Responsible for authoring, fielding and delivering multiple surveys of the American electorate over the 2012 election cycle, including a 2 wave, 140 minute, face-to-face, nationally-representative survey of over 2,000 respondents. Responsible to a 25 person Board of Advisors; managing seven project personnel. See http://www.electionstudies.org.

Co-operative Campaign Analysis Project, 2007-2008, Principal Investigator. With Lynn Vavreck (UCLA), authored and fielded a 6 wave panel study of the American electorate over the primary and general election campaign for the 2008 U.S. presidential election, spanning 20K respondents. The study drew together 27 teams of researchers from around the United States and Europe, raising $650K; the surveys were administered on-line by Polimetrix, Inc (Palo Alto, California).

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.

Director of Graduate Studies, 2003-2006. Overseeing the PhD program in the Department of Political Science at Stanford, regularly ranked in the top 3 political science PhD programs in the United States. Full-time staff assistant and $1.5M budget for post-graduate tuition and stipends; managing curriculum development, policies and procedures, academic assessments of students' progress towards degree, disciplinary matters.

AWARDS, HONORS & FELLOWSHIPS

American Academy of Arts and Sciences — Cambridge, Massachusetts
Elected as a Fellow of the Academy, 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.

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

PUBLICATIONS: BOOKS

Bayesian Analysis for the Social Sciences. Wiley: New York. 2009. 600 pages. Wiley; Amazon.

PUBLICATIONS: ARTICLES IN REFEREED JOURNALS

A30. with John Ahlquist and Kenneth Mayer. "Alien Abduction and Voter Impersonation in the 2012 U.S. General Election: evidence from a survey list experiment". *Election Law Journal*. Forthcoming.

A29. with Larry Bartels. "A Generational Model of Political Learning". *Electoral Studies*. 2014. 33 (March): 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. "All that glitters: the betting markets and the 2013 Australian Federal election". Johnson, Carol and John Warhurst (eds). *The 2013 Australian Federal election.* 2014. Australian National University Press: Canberra.

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 democratie a lèpreuve: une nouvelle approche de l'opinion des Francais)*. 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 democratie a lèpreuve: une nouvelle approche de l'opinion des Francais)*. 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. Elesvier: 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

O14. "The Predictive Power of Uniform Swing" *PS: Political Science and Politics*. 2014. 47(2): 317-321.

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.

07. "President Bush, the Public and the 2002 Elections" (with Richard A. Brody). *The Polling Report*. September 2, 2002. V18(17).

06. "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.

05. "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.

04. "Political Elites and the Mainstream." *Labor Herald* (newspaper of the Australian Labor Party). May 1997. p3.

03. "Rats and Representation: the Colston defection." *Current Affairs Bulletin* V73(3). October/November 1996. 23-26.

02. "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.

RECENT CONSULTING

FACEBOOK. APRIL 2014 - PRESENT. Design and analysis of surveys.

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

HUFFINGTON POST, 2012-2013. Tracking and forecasting public opinion, voting intentions leading up to the 2012 presidential election campaign.

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.

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

ASIAN POLITICAL METHODOLOGY CONFERENCE — Taipei, Taiwan
January 2015.

ACSPRI SOCIAL SCIENCE METHODOLOGY CONFERENCE — Sydney, Australia
"Survey research in the 21st century: challenges, opportunities and open questions." December 2014.

AUSTRALIAN POLITICAL STUDIES ASSOCIATION CONFERENCE — Sydney, Australia
"All that glitters: the predictive power of betting markets in the 2013 Australian Federal election." October 1, 2014.

SOCIETY FOR POLITICAL METHODOLOGY — University of Georgia, Athens, Georgia
"Why does the American National Election Study Overestimate Voter Turnout?" (with Bradley T. Spahn). July 2014.

MUNK SCHOOL OF GLOBAL AFFAIRS, UNIVERSITY OF TORONTO — Toronto, Canada
"Data Analysis and Inference for Experiments". Three day series of lectures and workshops. May 2014.

MASSACHUSETTS INSTITUTE OF TECHNOLOGY — Cambridge, Massachusetts
"Three challenges for survey research: lessons from the 2012 ANES." May 2014.

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". 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."

Technishce 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."

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

`pscl`: 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.

SERVICE TO THE PROFESSION

Program chair (with Melanie Manion), Annual Meeting of the 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, 2014-15.

Masters Program Admission Committee, Department of Statistics, 2013-14.

Director, Stanford Center for American Democracy, 2009-present.

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.

Rebuttal Report

Simon Jackman

December 21, 2015

Introduction

In this rebuttal report, I respond to criticisms made by Sean P. Trende and Professor Nicholas Goedert in their respective expert reports. I also conduct new empirical analyses further confirming the validity of the efficiency gap as a measure of partisan gerrymandering and the reasonableness of the proposed 0.07 threshold. More specifically, my principal contributions are the following:

- *First*, I respond to Goedert's various critiques of the efficiency gap and of the proposed efficiency gap threshold. Among other things, he misunderstands the relevance of efficiency gap data, cherry-picks information from my initial report while ignoring its broader context, and wrongly claims that plaintiffs' test would mandate "hyper-responsiveness" or prevent states from pursuing goals such as competitiveness or proportional representation.

- *Second*, I calculate several widely accepted prognostic measures—all based on the rates of true positives, false positives, true negatives, and false negatives—with respect to the odds of a district plan's efficiency gap changing signs over the plan's lifetime given a certain efficiency gap value in the plan's first election. Based on these measures, I conclude that the proposed 0.07 threshold is highly conservative. In fact, this threshold *sacrifices* some accuracy (which would be maximized at a lower threshold) in order to reduce the proportion of false positives.

- *Third*, I calculate the same prognostic measures with respect to the odds of a district plan's *average* efficiency gap, over its lifetime, having a different sign than that observed in the first election under a plan, given a certain efficiency gap value in this first election. Under this method, the proposed 0.07 threshold appears even more conservative, driving down the share of false positives to below 5%.

- *Fourth*, I compare the values of the efficiency gap in the *first* election under a plan and *on average* over the plan's lifetime. This relationship is impressively tight (r^2=0.73), indicating that a plan's initial bias is a very good predictor of its overall lifetime bias. For Act 43, this analysis allows us to predict that it will *average* a pro-Republican efficiency gap of almost 10% over the 2010 cycle as a whole.

- *Fifth*, I examine to what extent changes in party control over redistricting are responsible for the pro-Republican trend in the efficiency gap since the 1990s. In the current cycle, about *four times* more state house plans were designed by Republicans in full control of state government than in the 1990s. Had the distribution of party control over redistricting remained unchanged, essentially *all* of the pro-Republican movement in the efficiency gap over the last two decades would not have occurred. It is thus changes in party control, and *not* changes in the country's political geography, that primarily account for Republicans' growing redistricting advantage over the last generation.

- *Sixth*, I address recent work by Chen and Rodden (2013), cited by both Trende and Goedert for the proposition that Republicans enjoy a natural geographic advantage over Democrats. Chen and Rodden's simulated maps are not *lawful* because they ignore the Voting Rights Act and state redistricting criteria; they are based on presidential election results rather than more relevant state legislative election results; they do not constitute a representative sample of the entire plan solution space; and they are contradicted by other recent work (Fryer & Holden 2011) finding that randomly drawn plans *reduce* bias and *increase* electoral responsiveness.
- *Lastly*, I comment on Trende's analysis of particular state legislative and congressional plans. This analysis is marked by conceptual and methodological errors severe enough to render it useless. For example, Trende ignores two of the three prongs of plaintiffs' proposed test; he calculates congressional efficiency gaps without converting them from percentage points to House seats and for House delegations too small to generate reliable estimates; and he simply *substitutes* presidential election results for congressional election results whenever the latter are missing due to uncontested races. None of this work meets accepted standards of social science rigor.

1 Responses to Goedert's criticisms

In his report, Goedert offers several critiques of the efficiency gap and of the 0.07 threshold I recommended in my initial report, based primarily on the alleged instability of the efficiency gap. None of these critiques have merit. In this section, I respond to Goedert's points relying only on the analysis of my initial report and on the existing literature. My new empirical analyses appear in subsequent sections.

First, Goedert appears to believe that a plan's efficiency gap is only relevant to the extent that it sheds light on the partisan intent (or lack thereof) underlying the plan. He writes that "such intent cannot be inferred" from a large efficiency gap, that "a durable bias . . . is not even a sign of deliberate partisan intent," and that the "efficiency gap [is] a standard to measure partisan intent" (pp. 11, 13, 19). But this is not at all the legal function of the efficiency gap in plaintiffs' proposed test. Rather, partisan intent is its own independent inquiry, and the efficiency gap then comes into play at the *second* stage of the test, to determine if a plan's electoral *consequences* are sufficiently severe that it should be deemed presumptively unconstitutional. To put it simply, the efficiency gap is plaintiffs' measure of partisan *effect*, not of partisan *intent*. Goedert's misunderstanding of this basic point infects all of his discussion.

Second, Goedert observes that of *all* plans, anytime in the decade, with a *pro-Democratic* efficiency gap of greater than 0.07, a substantial proportion of them switch signs over their lifetimes (p. 11). In making this observation, Goedert cherry-picks a single bit of data from my initial report, and an irrelevant piece of data at that. This fact is irrelevant because it applies to plans no matter when their elections were held, while the appropriate universe for plaintiffs, defendants, and courts is limited to the *first* elections held under plans. It is the first elections that typically will be used in litigation, given Justice Kennedy's admonition in *Vieth* that plans should not be struck down based on a "hypothetical state of affairs," but rather "if and when the feared inequity arose" (*Vieth v. Jubelirer* (2004), p. 420). And the fact is misleading because it applies only to pro-Democratic efficiency gaps above 0.07, and not to the larger set of pro-Republican efficiency gaps above this threshold.

If we consider only plans that exhibit a pro-Democratic efficiency gap above 0.07 in their *first* elections, the probability that they will switch signs over their lifetimes drops by about five percentage points (Jackman Report, p. 61). And if we then turn to plans that exhibit a *pro-Republican* efficiency gap above 0.07 in their first elections—a more sizeable set, for which more accurate estimates are possible—this probability drops all the way to about 15% (Jackman Report, p. 61). In other words, of plans that open with large pro-Republican efficiency gaps, close to 85% of them continue to favor Republicans in every election for the remainder of the cycle. *This* is the most pertinent data point in my report, not the one cherry-picked by Goedert, and it reveals the persistence of many gerrymanders.

Third, Goedert discusses *congressional* district plans throughout his report, even though this case is exclusively about state legislative redistricting (pp. 7-8, 10, 12, 20). In doing so, he makes some of the same errors as does Trende: namely, not converting the efficiency gap from percentage points to House seats, and improperly handling uncontested races (in his case, by not adjusting for the uncontestedness *at all*, and simply treating the races as if all of the vote went to one party and none to the other). I discuss these errors in more detail later in this report.

Fourth, Goedert claims that it is "arbitrary" to focus on the first election after redistricting, and that doing so "biases toward a finding of *EG* durability" by ignoring wave elections (p. 14). As noted above, the first election after redistricting is the critical one for purposes of litigation, since under *Vieth*, it is after this election that a lawsuit will typically commence and have to be decided by the courts. Later elections are largely irrelevant for litigation purposes, since it is unreasonable to expect suits to be brought six or eight or even ten years into a cycle. Moreover, my analysis in no way ignored wave elections; to the contrary, I determined the odds that a plan's efficiency gap would switch signs by examining *all* elections held under the plan, waves and non-waves alike. If anything, the fact that most wave elections over the last forty years have not taken place in the first election after redistricting biases *against* a finding of durability, since these elections may well cause the efficiency gap to flip signs.

Fifth, Goedert is wrong that an efficiency gap of zero represents "'hyper-responsive' representation" (p. 2). In fact, as he has recognized in his own prior work, an efficiency gap of zero corresponds almost exactly to the responsiveness actually displayed by American elections over the course of the twentieth century, under which "a 1% increase in vote share will produce about a 2% increase in seat share" (Goedert 2014, p. 3). Indeed, this correspondence is one of the efficiency gap's most attractive properties, and it explains why Goedert himself calculated a quantity nearly identical to the efficiency gap in his work (Goedert 2014; Goedert 2015).

And sixth, Goedert is wrong as well that plaintiffs' proposed test might discourage states from pursuing worthwhile goals such as competitiveness or proportional representation (pp. 6-10). If a state's aim in redrawing districts was to make them more competitive or to produce more proportional representation, then the partisan intent required by the first prong of plaintiffs' test would not be present. Even if partisan intent were somehow found, the state would likely be able to show that its plan's large efficiency gap was necessitated by its pursuit of competitiveness or proportional representation. And in any event, competitiveness and proportional representation are extremely rare objectives in American redistricting. Only *one* state, Arizona, has a competitiveness requirement, and not a *single* state has a proportional representation criterion. (And needless to say, line-drawers do not tend to seek out either of these goals on their own.)

2 Reliability of a district plan's first efficiency gap

Having rebutted Goedert's criticisms using preexisting data, I now provide further analysis of the reliability of the first efficiency gap (*EG*) observed in the life of a district plan. This played a key role in the determination of the threshold *EG* value in my initial report. In that report, I focused on the probability of a "sign-flip": that is, given the magnitude of the efficiency gap observed in the first election under a district plan, what can we infer about the likelihood that all subsequent efficiency gaps observed under that plan will have the same sign as that from the first election.

Under this approach, just one election that produces an efficiency gap with a different sign from the efficiency gap in the first election will generate a "failure," in the sense we would say that the plan has generated an efficiency gap that conflicts with that from the first election. In short, the "constant sign" analysis in my original report considers the most extreme set of efficiency gap estimates produced under a plan and insists that they have the same sign. In this sense, the "constant sign" analysis I performed is a quite stringent and conservative test of what we can or ought to infer from the efficiency gap observed in the first election under the district plan. Another approach would be to inquire as to the *average* efficiency gap over the life of the district plan. A summary statistic such as the average is—by definition—less sensitive to extreme values. At the same time—and again, by definition—the average measures central tendency or typicality, and is the most widely used summary statistic in existence. I thus consider how well the first *EG* observed under a district plan predicts the average *EG* observed over the life of the plan.

But I first provide some additional analysis of the prognostic properties of the first efficiency gap observed under a district plan. In each instance the test is whether the first *EG* observed under a plan exceeds a given threshold value. The outcome of interest is whether the plan's remaining efficiency gaps have the same sign as the *EG* from the first election. For purposes of this exercise, plans are classified as "positive" (all *EG* scores under the plan have the same sign) or "negative" (*EG* scores differ in sign). With these definitions in place, we can then classify plans according to the accuracy of the prediction implicit in the first *EG* observed under the plan:

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

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

1. sensitivity, or the *true positive rate*: proportion of positives that test positive, TP/(TP + FN)

2. specificity, or the *true negative rate*: proportion of negatives that test negative, TN/(TN + FP)

3. *balanced accuracy*, the average of the sensitivity and the specificity
4. *accuracy*, the proportion of cases that are true positives or true negatives, (TP + TN)/(TP + FP + FN + TN).
5. the *false positive rate*; proportion of negative cases that test positive, 1 minus the specificity or FP/(TN + FP).
6. the *false discovery rate*; proportion of cases testing positive that are actually negative, FP/(TP + FP).
7. the *false omission rate*; proportion of cases that test negative that are actually positive, FN/(FN + TN).

Figure 1 shows how these prognostic performance indicators vary as a function of the absolute *EG* threshold (on the horizontal axis in the figure). That is, as we move to the right in each panel of the graph, the test is becoming increasingly stringent: larger absolute values of the efficiency gap in the first election under a district plan are required to trip the increasingly higher threshold. When the threshold is set to zero, all plans trip the threshold (all first-election *EG*s are greater than zero in magnitude, by definition) and so all cases test positive; in this case the sensitivity is 1, while conversely the specificity is 0 and the false positive rate is 1 (all negatives test positive).

The test has better properties as the threshold grows, with the accuracy measures maximized around absolute values of .03 to .04. Yet accuracy is not all in this context. The rate of false positives is quite high at thresholds where the accuracy is high, as is the false discovery rate. At a threshold of .03, for example, over half of plans that would go on to exhibit sign flips in their *EG*s would test positive and be flagged for inspection; of the plans selected for scrutiny, more than a third would turn out to have *EG* sign flips over the life of the plan. The .07 threshold is thus a conservative standard, the point at which the rate of false positives is becoming reasonably low (25%), without letting the false omission rate go above 50%.

It is worth noting the weight being put on false discoveries or false alarms versus the weight on false omissions in this context, which in turn reflects the conservatism and caution of the thinking underlying the .07 threshold. We propose accepting *twice* the rate of false omissions (plans that should have been scrutinized but were not) than the rate of false discoveries (plans that would be flagged for scrutiny given the *EG* observed in the first election, but would then go on to display sign flips). To reiterate: the proposed standard for judicial scrutiny is cautious and conservative, erring on the side of letting even durably skewed plans stand.

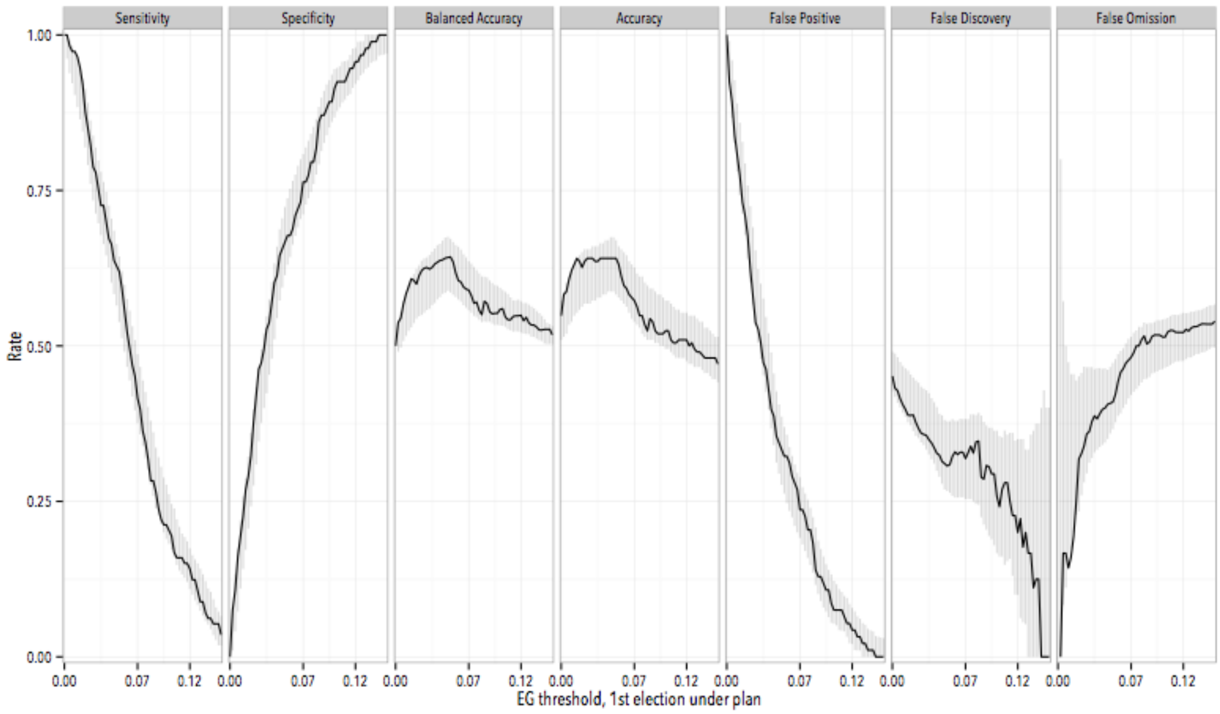

Figure 1: Prognostic performance measures, first efficiency gap under a district plan more extreme than threshold (horizontal axis) as a predictor of whether the subsequent efficiency gaps recorded under the district plan all have the same sign as the first efficiency gap. Vertical lines indicate 95% confidence intervals. Analysis spans all state legislative elections and district plans as per my initial report, 1972-2014.

Figure 2 repeats this analysis, but only considering the performance of *negative* values of the first-election efficiency gap threshold, consistent with Republican advantage (and more relevant to the Wisconsin plan at issue). Here the threshold becomes less stringent as we move across the horizontal axis from left to right, from larger negative thresholds to closer to zero at the right hand edge of each panel. With a large negative threshold (left hand edge of each panel), almost all plans test negative and so the sensitivity is close to zero, the specificity is 1, and the false positive rate is zero. The accuracy measures increase as the threshold becomes less stringent, attaining maxima in the range -.05 to -.02. Again—and consistent with the cautious approach we take—we emphasize that accuracy is not the sole criterion we use to evaluate a decision rule. At low values of the threshold, where accuracy is maximized, the false positive and false discovery rates are relatively high. On the other hand, at the proposed threshold value of -.07, the false positive rate is under 10% (fewer than 10% of plans with efficiency gaps changing signs would be scrutinized), and the false omission rate is about 35% (close to

35% of plans would not be flagged despite having *EG*s of the same sign over their lifetimes). The proposed threshold again errs on the side of restraint, tolerating a higher rate of false omissions than false discoveries.

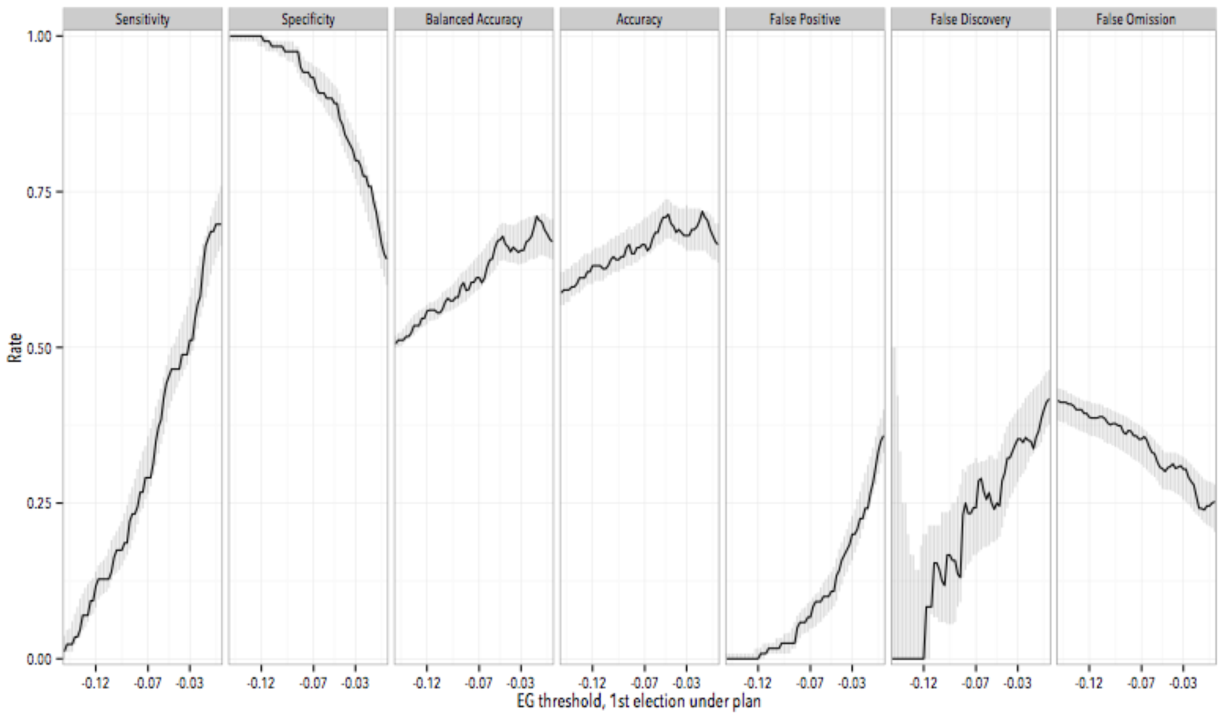

Figure 2: Prognostic performance measures, first efficiency gap under a district plan more extreme than threshold (horizontal axis) as a predictor of whether the subsequent efficiency gaps recorded under the district plan all have the same sign as the first efficiency gap. Vertical lines indicate 95% confidence intervals. Analysis examines negative, first-election threshold values of the efficiency gap, consistent with Republican advantage.

Figure 3 presents the corresponding analysis of *positive* values of the first-election *EG* threshold, consistent with Democratic advantage. Here the proposed threshold becomes more stringent as we move to the right of each panel, in the sense that fewer plans trip the threshold. At high values of the threshold (the right hand edge of each panel), no plans trip the threshold and all are classified as "negatives," leading to a specificity of 1, and false positive and false discovery rates of zero. Once again, accuracy is maximized at a less stringent threshold than the proposed .07 standard, around .03. The false positive rate is much lower at the proposed threshold of .07 than at the accuracy-maximizing threshold of .03. Note that the false discovery rates are moderately large but unstable and estimated with considerable imprecision; this is because there are so few plans exhibiting high (pro-Democratic) levels of *EG* in their first election. Moreover, of the few plans that do trip a given pro-Democratic threshold in their first election, it is reasonably likely that they will record efficiency gaps that will change sign over the life of the plan; this sign-flip or "false discovery" probability is about 35% at the proposed threshold of .07.

Comparing the analyses in Figures 2 and 3, we see an asymmetry in the results. The .07 threshold is more permissive with respect to plans that begin life exhibiting Democratic advantage than it is for plans that initially exhibit Republican advantage. At a +/- .07 threshold, the false discovery rate for plans initially exhibiting Republican advantage is under 10%, but around 35% for plans initially exhibiting Democratic advantage. As Figure 3 shows, it is difficult to find a threshold for apparently pro-Democratic plans that drives the false discovery rate to reliably low levels, if only because the historical record has relatively few instances of these types. We also note that the .07 threshold generates false omission rates of about 30% for both sets of plans.

Because the preceding discussion is somewhat technical, it is worth restating its principal conclusion: It is that an efficiency gap threshold of 0.07 is quite conservative, in that it sacrifices some accuracy (which would be maximized at a threshold of around 0.03) in order to drive down the false positive and false discovery rates. At a threshold of 0.07, in fact, the false positive and false discovery rates are about *half* of the false omission rate, indicating that there are about twice as many plans that are *not* being flagged even though their *EG* signs would remain one-sided throughout the cycle, than there are plans that *are* being flagged even though their *EG* signs would flip. This is further powerful confirmation of the reasonableness of the 0.07 efficiency gap threshold.

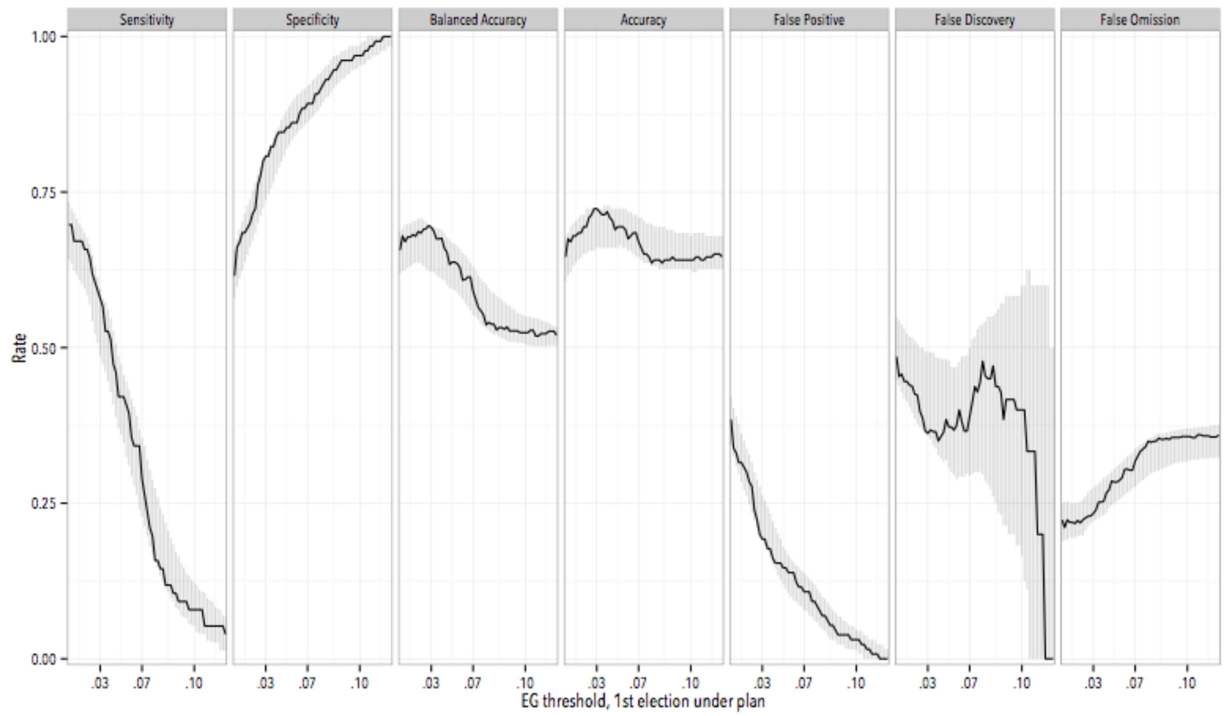

Figure 3: Prognostic performance measures, first efficiency gap under a district plan more extreme than threshold (horizontal axis) as a predictor of whether the subsequent efficiency gaps recorded under the district plan all have the same sign as the first efficiency gap. Vertical lines indicate 95% confidence intervals. Analysis examines positive, first-election threshold values of the efficiency gap, consistent with Democratic advantage.

3 First-election efficiency gap reliability with respect to the plan-average efficiency gap sign

Next we consider a slightly different kind of test; given that the first election under a district plan produces a value of the efficiency gap above or below a given threshold, how likely is it that the *average* value of the efficiency gap produced over the life of the plan lies on the same side of zero as that of the first election? Recall that the sign of the efficiency gap speaks to the corresponding direction of partisan advantage ($EG < 0$ is consistent with Republican advantage; conversely for $EG > 0$). We expect that this will be a less strenuous test than asking if *any* EG has an opposite sign to the first EG observed under a district plan.

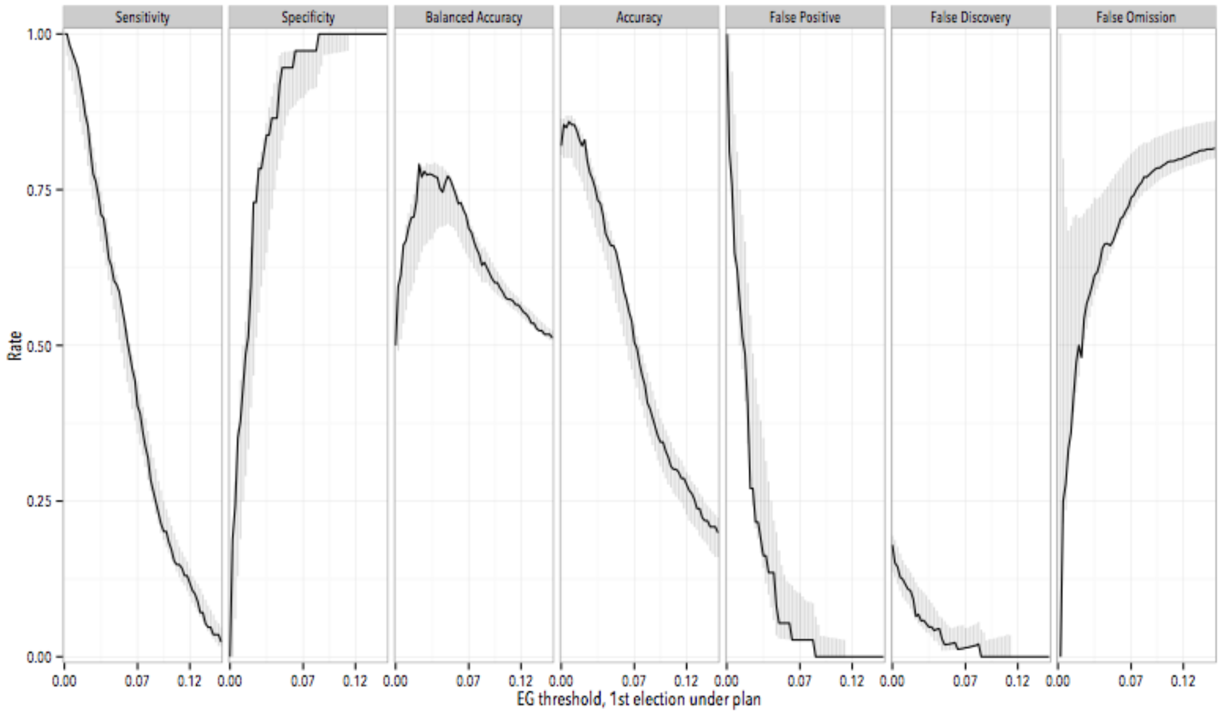

Figure 4: Prognostic performance measures, first efficiency gap under a district plan more extreme than threshold (horizontal axis) as a predictor of whether the average efficiency gap recorded under the district plan has the same sign as the first efficiency gap. Vertical lines indicate 95% confidence intervals. Analysis spans all state legislative elections and district plans as per my initial report, 1972-2014.

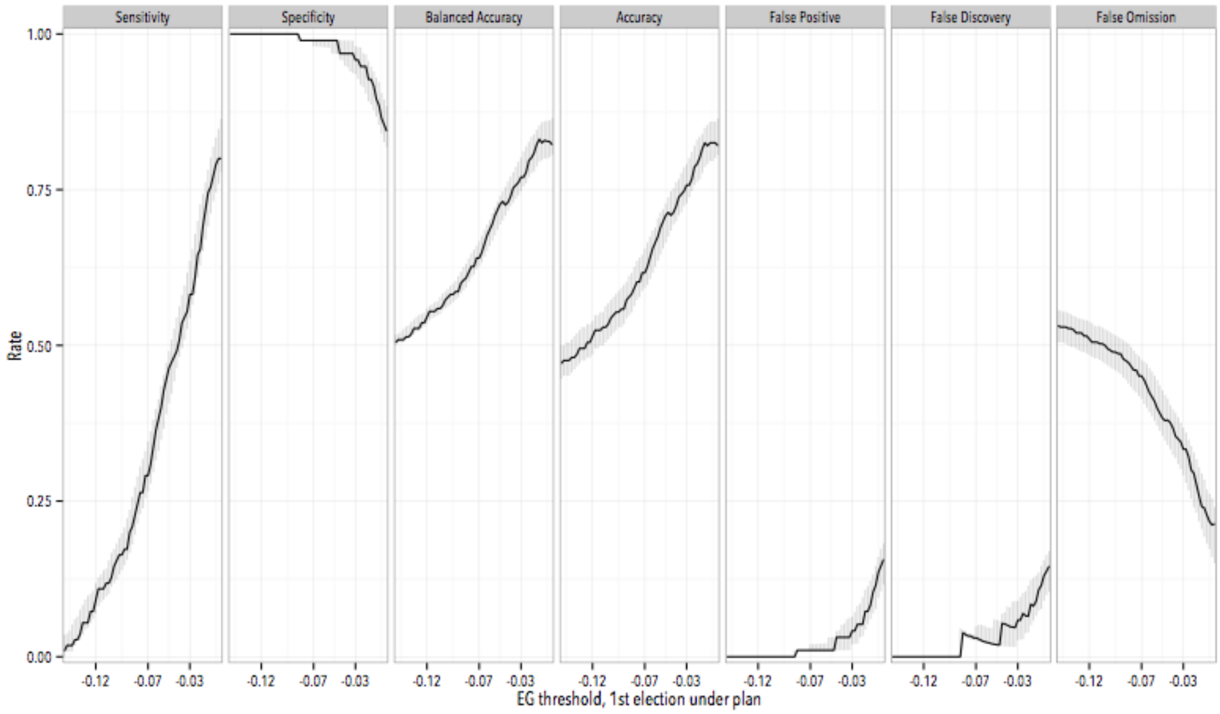

Figure 5: Prognostic performance measures, first efficiency gap under a district plan more extreme than threshold (horizontal axis) as a predictor of whether the average efficiency gap recorded under the district plan has the same sign as the first efficiency gap. Vertical lines indicate 95% confidence intervals. Analysis examines negative, first-election threshold values of the efficiency gap, consistent with Republican advantage.

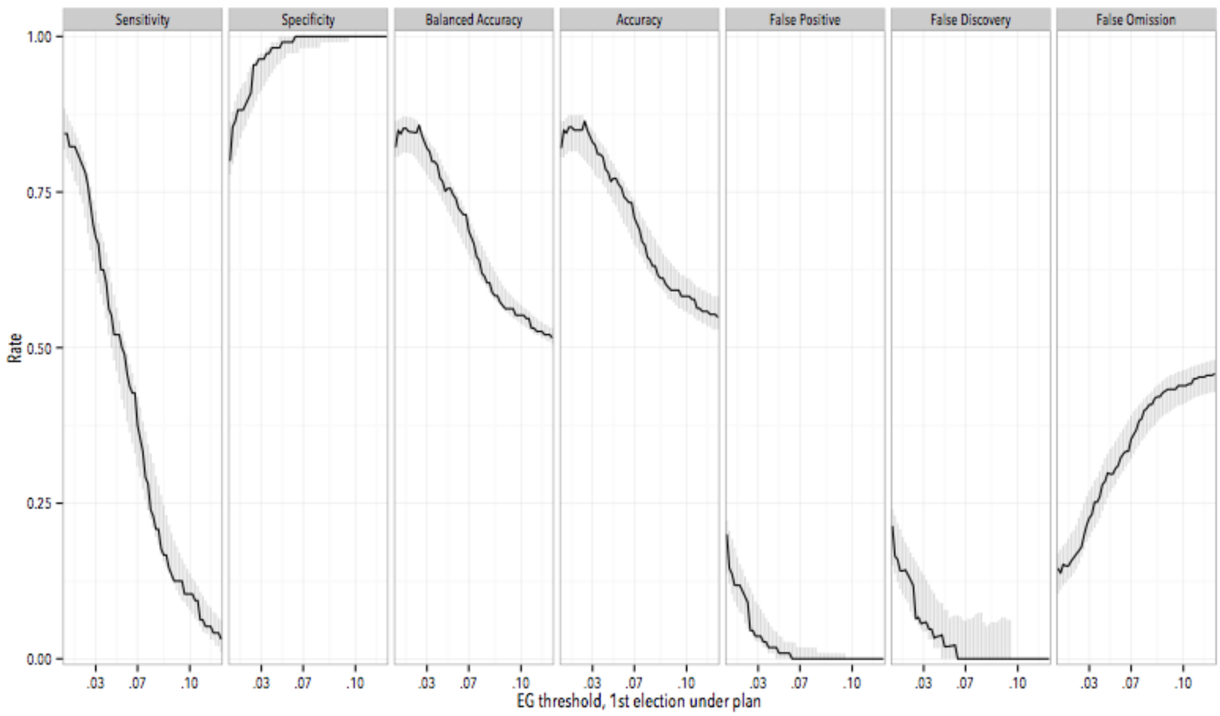

Figure 6: Prognostic performance measures, first efficiency gap under a district plan more extreme than threshold (horizontal axis) as a predictor of whether the average efficiency gap recorded under the district plan has the same sign as the first efficiency gap. Vertical lines indicate 95% confidence intervals. Analysis examines positive, first-election threshold values of the efficiency gap, consistent with Democratic advantage.

Figures 4, 5 and 6 show the prognostic performance of the first-election *EG* with respect to the sign of the corresponding plan's average *EG*, looking at the absolute value of the first-election *EG* (Figure 4), negative first-election efficiency gaps (Figure 5) and positive first-election efficiency gaps (Figure 6). The first thing to observe is the generally superior prognostic performance when it comes to forecasting the sign of the *plan-average* efficiency gap, relative to the prognostic performance with respect to *all* of the plan's efficiency gaps having the same sign. As anticipated, the former is better predicted by the plan's first-election efficiency gap than the latter. Second, the accuracy-versus-caution tradeoff noted earlier is also apparent. The proposed threshold of +/- 0.07 trades away accuracy for very low false positive and false discovery rates, below 5%, at the cost of higher false omission rates, a pattern we observed earlier. Finally, note that at the proposed threshold of +/- 0.07, almost one-half of all plans with a negative (pro-Republican) average *EG* would *not* be candidates for scrutiny (right-hand panel of Figure 5); about one-third of plans with a positive (pro-Democratic) average *EG* also would not trigger the threshold for scrutiny.

4 Relationship between the first-election efficiency gap and the plan-average efficiency gap

I next present analysis on a related issue, the relationship between the magnitudes of the *first* efficiency gap observed under a plan and the *average* efficiency gap we observe over the life of the plan. Does a larger or smaller first-election efficiency gap portend anything for the average value of the efficiency gap generated over the life of a district plan?

Clearly the first value of the efficiency gap and the plan-average efficiency gap are related; the former contributes to the calculation of the latter, and after the first election under a district plan we observe at most four more elections under the plan (given elections every two years in most states and redistricting once a decade). Accordingly we expect a positive correlation between the two quantities. The interesting empirical question—and one with considerable substantive implications for the issue at hand—is *how strong* the relationship is between the first-election efficiency gap and the corresponding plan-average efficiency gap. This speaks to the reliability of the first-election *EG* measure as a predictor of *EG* over the life of the plan.

Figure 7 shows the relationship between the first-election *EG* and the average *EG* observed over the entire plan. Note that we restrict this analysis to plans with at least three elections, so that the first election does not unduly contribute to the calculation of the average; this restriction has the consequence of omitting elections from the most recent round of redistricting after the 2010 Census, which have contributed at most two elections. The black diagonal line on the graph is a 45-degree line: if the relationship between first-election *EG* and plan-average *EG* were perfect, the data would all lie on this line. Instead we see a classic "regression-to-the-mean" pattern, with a positive regression slope of less than one (as indeed we should, given that the first-election *EG* on the horizontal axis contributes to the average plotted on the vertical axis). But the relationship here is especially strong. The variation in plan-average efficiency gaps explained by this regression is quite large, about 73%; after taking into account the uncertainty in the *EG* scores (stemming from the imputation procedures used for uncontested districts; see my initial report) a 95% confidence interval on the variance explained measure ranges from 67% to 74% (the uncertainty has the consequence of tending to make the regression fit slightly less well). That is, even given the uncertainty that accompanies *EG* measures due to uncontestedness, the relationship between first-election *EG* and plan-average *EG* is quite strong.

In particular, at the threshold values of +/- 0.07 there is very little doubt as to the plan-average value of the efficiency gap. The historical relationship between first-election *EG* and plan-average *EG* shown in Figure 7 indicates that a first-election *EG* of -.07 is typically associated with a plan-average *EG* of about -0.053 (95% CI -0.111 to 0.004); the probability that the resulting, expected plan-average *EG* is negative is 96.5%. Conditional on a first-election *EG* of .07 we typically see a plan-average *EG* of about 0.037 (95% CI -0.021 to 0.093); the probability that the resulting, expected plan-average *EG* is positive is 89.8%. This constitutes additional, powerful evidence that (a) first-election *EG* estimates are predictive with respect to the *EG* estimates that will be observed over the life of the plan; and (b) the threshold values of +/- 0.07 are conservative, generating high-confidence predictions as to the behavior of the district plan in successive elections.

In the particular case of Wisconsin in 2012—the first election under the plan in question—I estimated the efficiency gap to be -0.133 (95% CI -0.146 to -0.121). The analysis of historical data discussed above—and graphed in Figure 7—indicates that the plan-average *EG* for this plan will be -0.095 (95% CI -0.152 to -0.032)[1], a quite large value by historical standards, placing the current Wisconsin district plan among the five to ten most disadvantageous district plans for Democrats in the data available for analysis. The probability that the Wisconsin plan—if left undisturbed—will turn out to have a positive, pro-Democratic, average efficiency gap is for all practical purposes zero (less than 0.1%).

1 It is also worth stressing that the confidence interval is computed so as to take into account uncertainty from all known sources: in the underlying efficiency gap scores themselves, the fact that the 2012 *EG* scores for Wisconsin are large by historical standards, and in the regression relationship between first-election *EG* and plan-average *EG*.

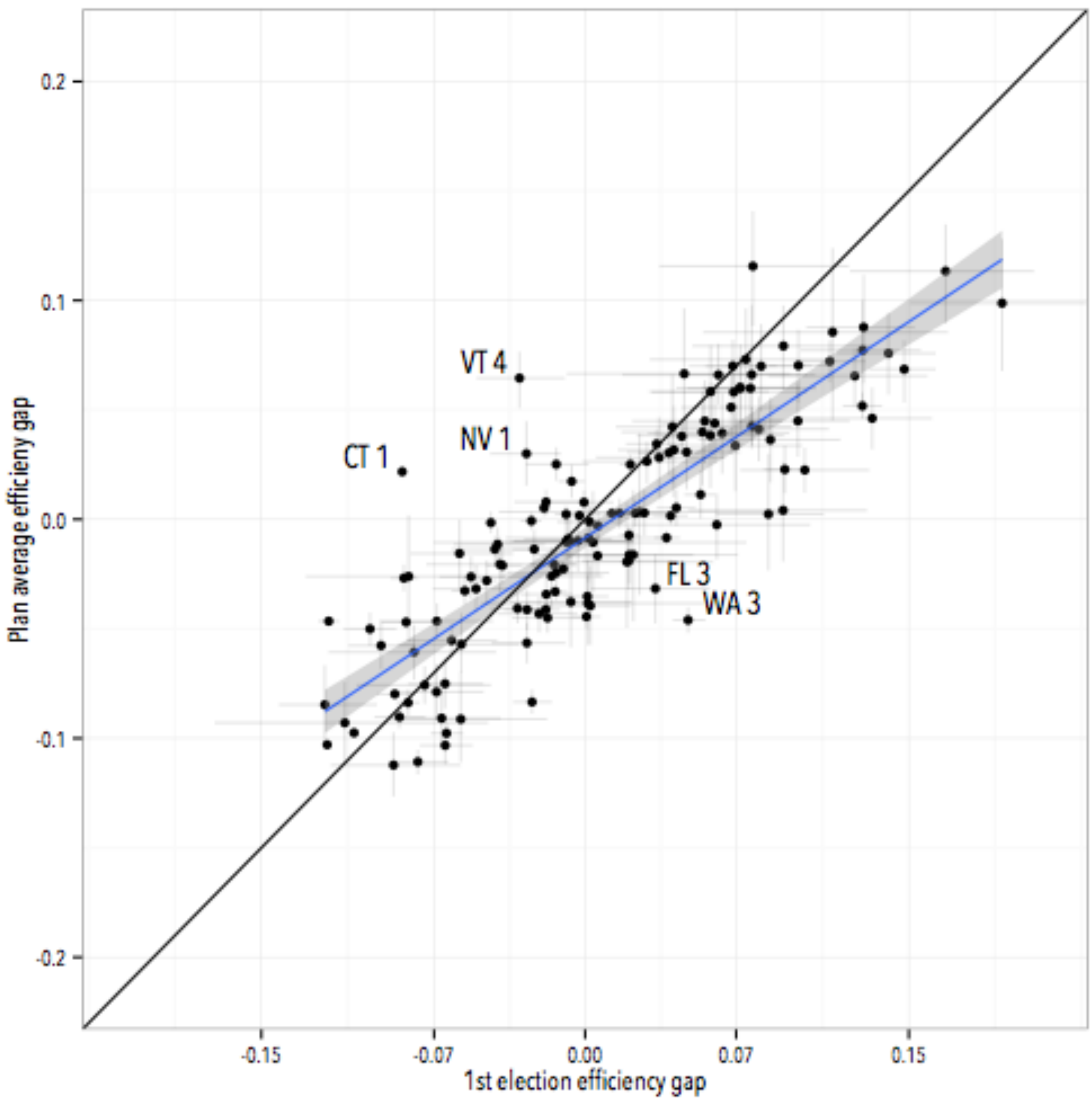

Figure 7: Scatterplot of first-election efficiency gap scores (horizontal axis) and plan-average efficiency gap scores (vertical axis). The diagonal black line is a 45-degree line; the data would lie on this line if first-election efficiency gaps coincided with plan-average efficiency gaps. The solid blue line is a linear regression with slope .64 (95% CI 0.57 to 0.72); the shaded region around the blue line is a 95% confidence interval for the regression line. 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 plan-average *EG*, stemming from imputations for uncontested districts. Outliers are labeled (state, plan). Analysis restricted to plans with at least three elections (1972-2010), omitting plans adopted after the 2010 Census. The first-election *EG* for the current Wisconsin plan is -0.133 (95% CI -0.146 to -0.121).

5 Party control as an explanation for change in the efficiency gap

Both Trende and Goedert point out that, on average, state house plans have exhibited pro-Republican efficiency gaps in recent years (Trende, paragraphs 129-30; Goedert p. 19). They then argue that this pro-Republican mean is attributable to a natural pro-Republican political geography in many states. However, as I found in my initial report, the *overall* efficiency gap average, over the entire 1972-2014 period, is very close to zero (Jackman Report, p. 35, 45, 57). There is thus no sign of a natural pro-Republican advantage in the dataset as a whole, nor any evidence (despite Trende and Goedert's unsupported assertions to the contrary) that states' political geography is changing in ways that favor Republicans.

In fact, the one historical change that *is* undeniable is the trend toward unified Republican control over redistricting. As Figure 8 displays, only about 10% of all state house plans were designed by Republicans in full control of the state government in the 1990s, compared to about 30% by Democrats in full control and about 60% by another institution (divided government, a commission, or a court). But in the 2000s, Republicans were fully responsible for slightly *more* plans than were Democrats (about 20% versus about 15%). And in the 2010s, the partisan gap jumped again, to about *40%* of plans designed entirely by Republicans, versus less than 20% designed entirely by Democrats.

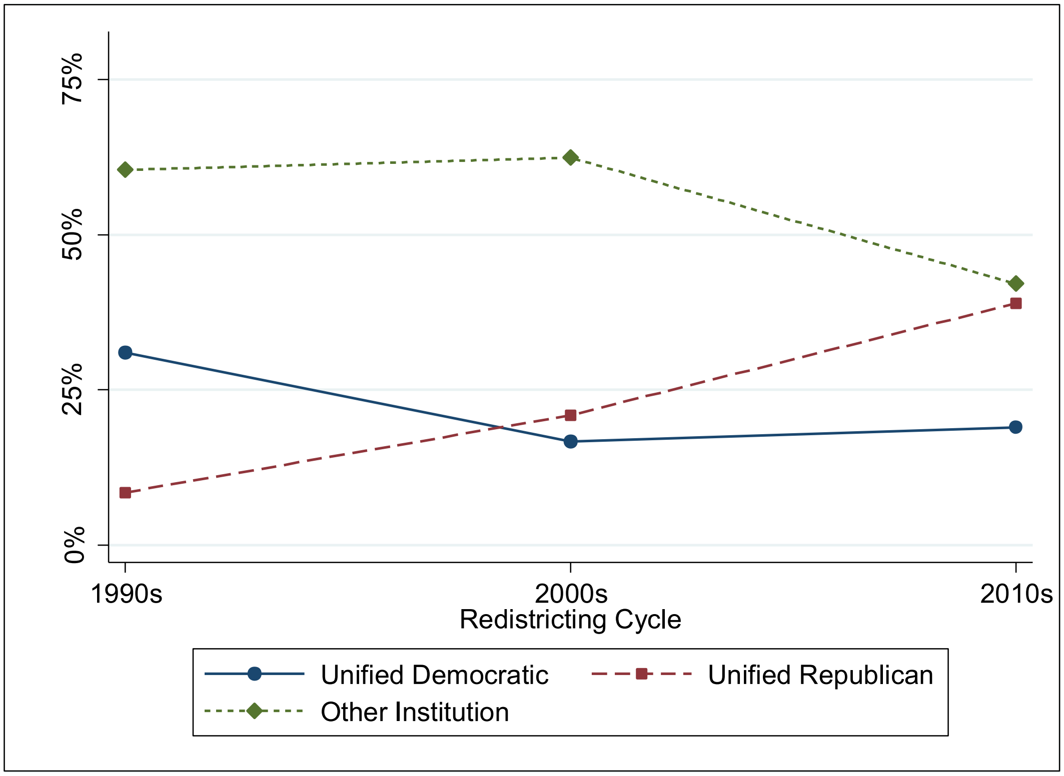

Figure 8: Share of all state house plans, by cycle, designed by Democrats in unified control of state government, by Republicans in unified control of state government, or by another institution (divided state government, commission, or court).

To determine the impact of this change in party control on the change in the efficiency gap over the last generation, I carry out three regressions, one for the 1990 redistricting cycle, one for the 2000 cycle, and one for the 2010 cycle. In each case, state house plans' efficiency gaps are the dependent variable, and unified Democratic control over redistricting and unified Republican control over redistricting are the independent variables. (The omitted category is any other institution responsible for redistricting, such as divided government, a court, or a commission.) Figure 9 then displays the *actual* average efficiency gap for each cycle, as well as the *predicted* average efficiency gap if the distribution of party control over redistricting had remained unchanged since the 1990s.

As is evident from the chart, state house plans' average efficiency gap in the 2000 cycle would have been substantially less pro-Republican (by about 0.5 percentage points) had Republicans not gained control of more state governments in this cycle relative to the 1990s. And in the current cycle, *all* of the efficiency gap's movement in a Republican direction would have been erased had the distribution of party control over redistricting not changed since the 1990s. That is, if the same distribution of party control had existed in this cycle as in the 1990s, state house plans' average efficiency gap would have been very close to zero, not over 3% in a Republican direction. Accordingly, it is the change in party control that appears to account for essentially all of the pro-Republican trend in the efficiency gap over the past two decades—and not, as claimed by Trende and Goedert, a dramatic alteration of the country's political geography.

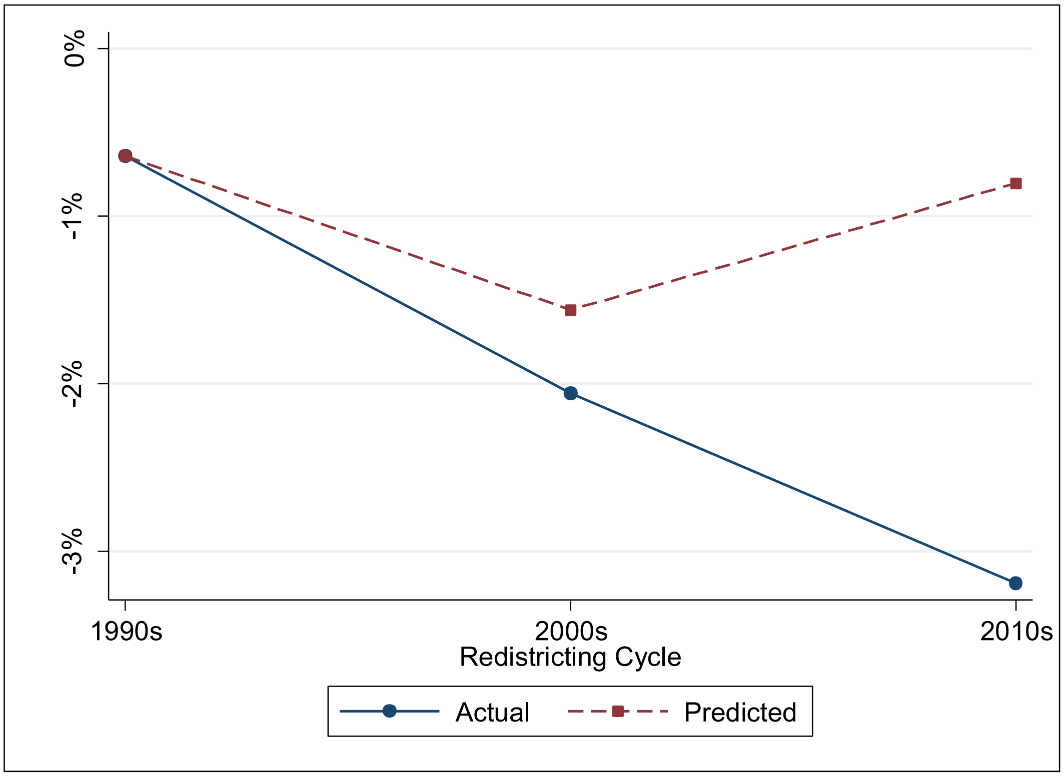

Figure 9: Actual and predicted values of state house plans' average efficiency gaps by cycle. Predicted values calculated assuming that the 1990s distribution of party control over redistricting remained constant in subsequent cycles.

6 Response to the Chen and Rodden map simulations

Both Trende and Goedert cite a recent article by Chen and Rodden (2013) that purports to find, based on simulations of hypothetical district maps, that random redistricting would benefit Republicans because of their more efficient spatial allocation (Trende, paragraphs 89, 126; Goedert, pp. 13, 18, 21). While I respect Chen and Rodden's contribution, there are several issues with their work that make it inapplicable here.

First, Chen and Rodden do not even attempt to simulate *lawful* plans. Rather, they simulate plans "using only the traditional districting criteria of equal apportionment and geographic contiguity and compactness" (Chen and Rodden, 248). They do not take into account Section 2 of the Voting Rights Act, which often requires majority-minority districts to be constructed. They also do not take into account Section 5 of the VRA, which until 2013 meant that existing majority-minority districts could not be eliminated in certain states. And they do not take into account state-level criteria such as respect for political subdivisions and respect for communities of interest, which are in effect in a majority of states (NCSL 2010, pp. 125-27).

Second, Chen and Rodden only use *presidential* election results in their analysis, but these outcomes may diverge from *state legislative* election results due to voter roll-off as well as voter preferences that vary by election level. As Stephanopoulos and McGhee have noted, "If certain voters consistently support Republicans at the presidential level and Democrats at the legislative level, then presidential data may produce more pro-Republican estimates than legislative data" (Stephanopoulos & McGhee, 870). In fact, this is exactly what seems to be occurring; at the congressional level, efficiency gaps are about 6% more Republican when they are calculating using presidential data than when they are computed on the basis of congressional election results.

Third, Chen and Rodden's simulated maps do not constitute a representative sample of the entire plan solution space. Their simulation algorithm has "no theoretical justification," is "best described as ad-hoc," and is not "designed to yield a representative sample of redistricting plans" (Fifield et al. 2015, pp. 2-3; Altman & McDonald 2010, p. 108). The explanation for this lack of representativeness is highly technical and involves the details of the particular simulation approach adopted by Chen and Rodden. But its implication is clear: that no conclusions can yet be drawn about the partisan consequences of randomly drawn maps.

Lastly, Chen and Rodden's results are directly contradicted by Fryer and Holden, who also simulated contiguous, compact, and equipopulous districts for multiple states. Unlike Chen and Rodden, Fryer and Holden found that, "[u]nder maximally compact districting, measures of Bias are slightly *smaller* in all states except [one]" (Fryer & Holden 2011, p. 514). Fryer and Holden also found that "[i]n terms of responsiveness . . . there are large and statistically significant" *increases* in all states, sometimes on the order of a fivefold rise (p. 514). Their analysis thus leads to the opposite inference from Chen and Rodden's: that randomly drawn contiguous and compact districts favor *neither* party and substantially boost electoral responsiveness.

7 Trende's analysis of particular plans

Trende devotes a large portion of his report (paragraphs 106-31) to analyzing the efficiency gaps of particular state legislative and congressional plans. He first examines a set of seventeen state legislative plans that had efficiency gaps favoring the same party over their entire lifespans, arguing that not all of these plans were gerrymanders (paragraphs 106-14). He then cites a series of congressional plans, some of which he claims had large efficiency gaps despite not being gerrymanders, and others of which allegedly had small efficiency gaps despite being gerrymanders (paragraphs 115-24). All of this analysis is riddled with conceptual and methodological errors that, in my judgment, renders it unreliable and unhelpful to the court.

Beginning with the set of seventeen state legislative plans that had efficiency gaps of the same sign throughout their lifespans, Trende asserts that they "would be included in the definition of a gerrymander," and are a "list of gerrymandered states" (paragraphs 109-10). But neither plaintiffs nor I argue that these plans should have been held unconstitutional. That is, neither plaintiffs nor I argue that these plans were designed with partisan intent (the first element of plaintiffs' proposed test), that their initial efficiency gaps exceeded a reasonable threshold (the second element), or that their efficiency gaps could have been avoided (the third element). To the contrary, I simply included these plans in my report to illuminate historical cases in which the efficiency gap's direction did not change over the course of a decade. I never stated or implied that these plans should have been deemed unlawful.

However, if we focus on the plans among the seventeen that likely *would* have failed plaintiffs' proposed test (at least the first two elements), we see that both the test and the efficiency gap perform exceptionally well. Five of the seventeen plans featured unified control by a single party over redistricting (from which, like Goedert (2014) and Goedert (2015), we can infer partisan intent) as well as an initial efficiency gap above 7% (the threshold I recommended in my initial report): Florida in the 1970s, Florida in the 2000s, Michigan in the 2000s, New York in the 1970s, and Ohio in the 2000s. Assuming that these plans' large efficiency gaps were avoidable (a granular inquiry that cannot be carried out here), it would have been quite reasonable for all of these maps to attract heightened judicial scrutiny. In particular:

- Florida's plan in the 1970s was designed exclusively by Democrats, opened with a 9.9% pro-Democratic efficiency gap, averaged a 7.0% pro-Democratic efficiency gap over its lifespan, and never once favored Republicans.

- Florida's plan in the 2000s was designed exclusively by Republicans, opened with a 8.9% pro-Republican efficiency gap, averaged a 11.2% pro-Republican efficiency gap over its lifespan, and never once favored Democrats.
- Michigan's plan in the 2000s was designed exclusively by Republicans, opened with a 12.0% pro-Republican efficiency gap, averaged a 10.3% pro-Republican efficiency gap over its lifespan, and never once favored Democrats.
- New York's plan in the 1970s was designed exclusively by Republicans, opened with a 10.7% pro-Republican efficiency gap, averaged a 9.7% pro-Republican efficiency gap over its lifespan, and never once favored Democrats.
- Ohio's plan in the 2000s was designed exclusively by Republicans, opened with a 8.6% pro-Republican efficiency gap, averaged a 9.0% pro-Republican efficiency gap over its lifespan, and never once favored Democrats.

Accordingly, we see that if my report's set of seventeen plans is analyzed properly, the opposite conclusion emerges from the one advocated by Trende. Only a subset of the seventeen plans likely would have failed plaintiffs' proposed test. But *every member* of this subset turns out to have been an exceptionally severe and durable gerrymander, featuring a very large and consistent efficiency gap over its lifespan. These are *precisely* the historical cases in which judicial intervention may have been advisable.

After commenting on these seventeen state legislative plans, Trende discusses a series of *congressional* plans, all from the 2000 and 2010 redistricting cycles. These congressional plans are entirely irrelevant to this case, which deals only with state legislative redistricting. Neither in their complaint nor in their subsequent filings do plaintiffs ever argue that their approach should be applied to congressional plans. And neither Mayer nor I provide any empirical analysis of congressional plans. In my initial report, in particular, I examined state legislative plans from 1972 to the present, but no congressional plans at all.

This state legislative focus has two explanations. First, and more importantly, each congressional delegation is *not* a legislative chamber in its own right, but rather a portion (often a very small portion) of the U.S. House of Representatives. Methods applicable to entire chambers cannot simply be transferred wholesale to delegations that make up only fractions of Congress. Second, most congressional delegations have many fewer seats than most state houses. The efficiency gap becomes lumpier when there are fewer seats, because each seat accounts for a larger proportion of the seat total, and the efficiency gap thus shifts more as each seat changes hands. This lumpiness is entirely avoided when state legislative plans, which typically have dozens or even hundreds of districts, are at issue.

For these reasons, Stephanopoulos and McGhee make two adjustments when analyzing congressional plans in their work on the efficiency gap. First, they convert the efficiency gap from percentage points to *seats* by multiplying the raw efficiency gap by each state's number of congressional districts. As they explain their method, "What matters in congressional plans is their impact on the total number of *seats* held by each party at the national level. Conversely, state houses are self-contained bodies of varying sizes, for which *seat shares* reveal the scale of parties' advantages and enable temporal and spatial comparability" (Stephanopoulos & McGhee, 869). Second, they only calculate efficiency gaps for states with at least eight congressional districts. Efficiency gaps are lumpier for states with fewer than eight districts, and additionally, congressional "redistricting in smaller states has only a minor influence on the national balance of power" (Stephanopoulos & McGhee, 868).

In his report, Trende fails to make either of these necessary adjustments when examining congressional plans. That is, he does not convert the efficiency gap from percentage points to seats, and he calculates the efficiency gap for small congressional delegations with fewer than eight seats. There is no authority in the literature for his methodological choices, and he is unable to cite any. And his flawed methods have serious substantive consequences that render his results entirely untrustworthy.

Take Trende's failure to convert the efficiency gap from percentage points to House seats. He claims that Alabama's congressional plan had an efficiency gap of -12.5% in 2002, that Arizona's congressional plan had an efficiency gap of 16% in 2012, that Colorado's congressional plan had an efficiency gap of -9% in 2002 and -10% in 2012, that Illinois's congressional plan had an efficiency gap of -9% in 2002, and that Iowa's congressional plan had an efficiency gap of -20% in 2002—all above my suggested 7% threshold for state legislative plans (paragraphs 115-16, 118-19, 121-22). But when converted to seats, *all* of these efficiency gaps become quite small, lower in all cases than the two-seat threshold proposed in the literature for congressional plans (Stephanopoulos & McGhee, 887-88). Specifically, using Trende's own calculations—which, as I discuss below, are incorrect in any event—Alabama had an efficiency gap of -0.9 seats in 2002, Arizona had an efficiency gap of 1.4 seats in 2012, Colorado had an efficiency gap of -0.6 seats in 2002 and -0.7 seats in 2012, Illinois had an efficiency gap of -1.7 seats in 2002, and Iowa had an efficiency gap of -1.0 seats in 2002. *None* of these scores are high enough to rise to presumptive unlawfulness under the literature's suggested two-seat threshold, meaning that we come to exactly the *opposite* conclusion as Trende after making the necessary adjustment.

Next take Trende's consideration of Alabama's congressional plan in 2002 (which had seven districts), Iowa's congressional plan in 2002 (five districts), and Colorado's congressional plans in 2002 and 2012 (seven districts each) (paragraphs 115-16, 119, 122). All four of these plans have fewer than eight districts, and so, based on the literature, should not be included in any efficiency gap analysis because of the measure's lumpiness when applied to so few seats. Trende nowhere acknowledges this limitation, and indeed appears unaware of its existence.

Moreover, Trende's study of congressional plans is marred by two further flaws, one conceptual and the other methodological. The conceptual defect is that, as in his earlier discussion of state legislative plans, he assumes that a large efficiency gap is all that is necessary to render a plan unconstitutional. He writes that efficiency gaps of -12.5%, -9%, -9%, -20%, and 16% "would invite court scrutiny as a Republican gerrymander" or "would invite court scrutiny as a Democratic gerrymander" (paragraphs 115, 116, 118, 119, 121, 122). But again, this is not plaintiffs' proposed test. A large efficiency gap is only a single prong of the test, and does not result in a verdict of unconstitutionality unless it is paired with a finding of partisan intent *and* a finding that it could have been avoided. Trende entirely overlooks these other elements.

The methodological defect is that whenever there were uncontested congressional races, Trende simply *substituted* presidential election results for the missing congressional results. As he put it in his deposition, he "used presidential results" and "imputed those results to the congressional races" whenever the races were uncontested (Trende deposition, p. 83). This is an exceptionally crude method that is guaranteed to produce errors, both because there is voter roll-off from the presidential to the congressional level and because voters may have different presidential and congressional preferences. Of course, presidential results can be used as the *inputs* to a regression model that *predicts* the outcomes of uncontested congressional races. Indeed, this is the preferred approach in the literature, and the approach I employed in my initial report. But presidential results cannot simply be plugged in without any adjustment, and no competent social scientist would have done so.

Accordingly, in my judgment, Trende's examination of particular state legislative and congressional plans is unreliable and entitled to no weight by the court. The state legislative analysis ignores the actual elements of plaintiffs' proposed test, and would have led to the opposite conclusion if these elements had been taken into account. Likewise, the congressional analysis ignores the test's prongs, fails to convert the efficiency gap from percentage points to seats, improperly considers states with small House delegations, improperly substitutes presidential election results whenever congressional results are missing—and deals with federal elections that simply are not part of this case.

Dated December 21, 2015

/s/ Simon Jackman

Simon Jackman, PhD

Department of Political Science

Stanford University

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Case

Vieth v. Jubilerer, 541 U.S. 267 (2004).

Sensitivity of the Efficiency Gap to Uniform Swing

How sensitive is the efficiency gap to reasonable swings in vote shares? In his report, Goedert asserts that it is extremely sensitive (pp. 11-15), but his claim is based on a small number of examples (pp. 12-13) as well as his own work at the congressional level involving only two elections (Goedert 2015). Sections 1-4 of my rebuttal report show that the first efficiency gap observed under a plan is a reliable indicator of the efficiency gap's magnitude and direction over the remainder of the plan's lifespan. These sections, however, are based on historical efficiency gap data rather than the "sensitivity testing for future results" deemed "crucial" by Goedert (p. 13). Accordingly, we conduct sensitivity testing here of exactly the kind earlier carried out by Stephanopoulos & McGhee (pp. 889-90, 898-99) and recommended by Goedert. This testing confirms the findings in Sections 1-4 of my rebuttal report, and further corroborates my conclusions therein about the efficiency gap's durability and reliability.

Methodologically, we investigate the behavior of the efficiency gap when we perturb it by mimicking "uniform swing" across a 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 known as uniform swing. In real-world elections swings are never precisely 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 for state legislative elections where we often lack useful district-level predictors of swing (or, more tellingly, predictors of the way the swing in a given state legislative district might depart from the statewide swing).

We restrict the following exercise to elections since the 2010 round of redistricting. For each election we simulate a series of uniform swings, evenly spaced between -5% to +5%, a quite large set of swings by the standards of state legislative elections. For instance, swings in Wisconsin state legislative elections from 1972 to 2014 are estimated to range between -7.6 percentage points from 2008 to 2010 (Democratic share of two-party vote, averaged by district) and +5.0 percentage points from 2004 to 2006. Similarly, Stephanopoulos & McGhee found that a swing of +/- 5.5 percentage points covered the vast majority of state legislative elections from 1972 to 2012 (p. 874).

At each level of uniform swing, we record the new vote shares and seat shares (some seats change hands if the swing pushes Democratic two-party vote share to the other side of 50%) and recompute the efficiency gap. We then examine how much the simulated efficiency gaps—generated under different levels of uniform swing—depart from the efficiency gap observed under the actual election. In particular, if relatively small swings produce large changes in *EG*, we might rightly be concerned about the stability and reliability of the efficiency gap as a characterization of a district plan. Keep in mind that this exercise keeps the district plan as it is and simply shifts vote shares up and down over a range of hypothetical levels of statewide swing, held constant over districts.

Figure 1 shows the relationships between efficiency gaps estimated using actual election results in state legislative elections held since the 2010 round of redistricting, and efficiency gaps estimated using a range of uniform swings. When uniform swing is zero, the simulation exercise leaves the actual election results unperturbed, and we simply recover the original efficiency gap estimates; all the data in the panel labelled “Swing +0.0” lies on the 45-degree line. As we increase the magnitude of hypothetical levels of uniform swing, the relationship between the observed efficiency gaps and the simulated efficiency gaps weakens, but only by a moderate amount. Even at high levels of uniform swing (approaching +/- five percentage points), the relationship between observed efficiency gaps and simulated efficiency gaps remains of significant strength; the blue line in each panel of Figure 1 is a regression line and in every case has a large and unambiguously positive slope, indicating a positive correlation between actual and simulated efficiency gaps.

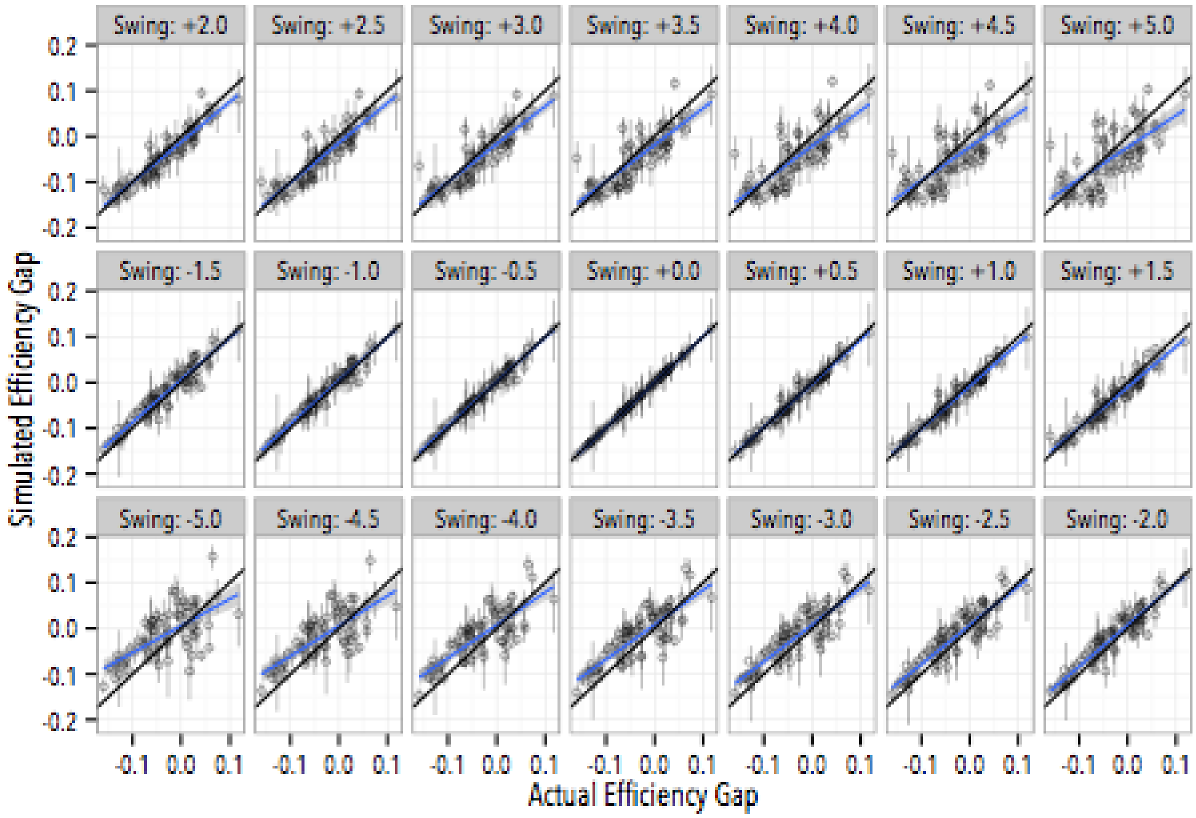

Figure 1: Actual efficiency gaps from state legislative elections 2012 to 2014 (horizontal axis), and corresponding simulated efficiency gaps generated by varying levels of uniform swing. Vertical lines indicate 95% confidence intervals. Dark diagonal lines are at forty-five degrees, the fit to the data that would result if actual and simulated efficiency gaps were equal (as is the case when the simulated level of uniform swing is set to zero, as in the middle panel of the second row). The blue line indicates a regression fit. For small to even moderately large values of uniform swing, there is a high degree of correspondence between the actual and simulated efficiency gaps.

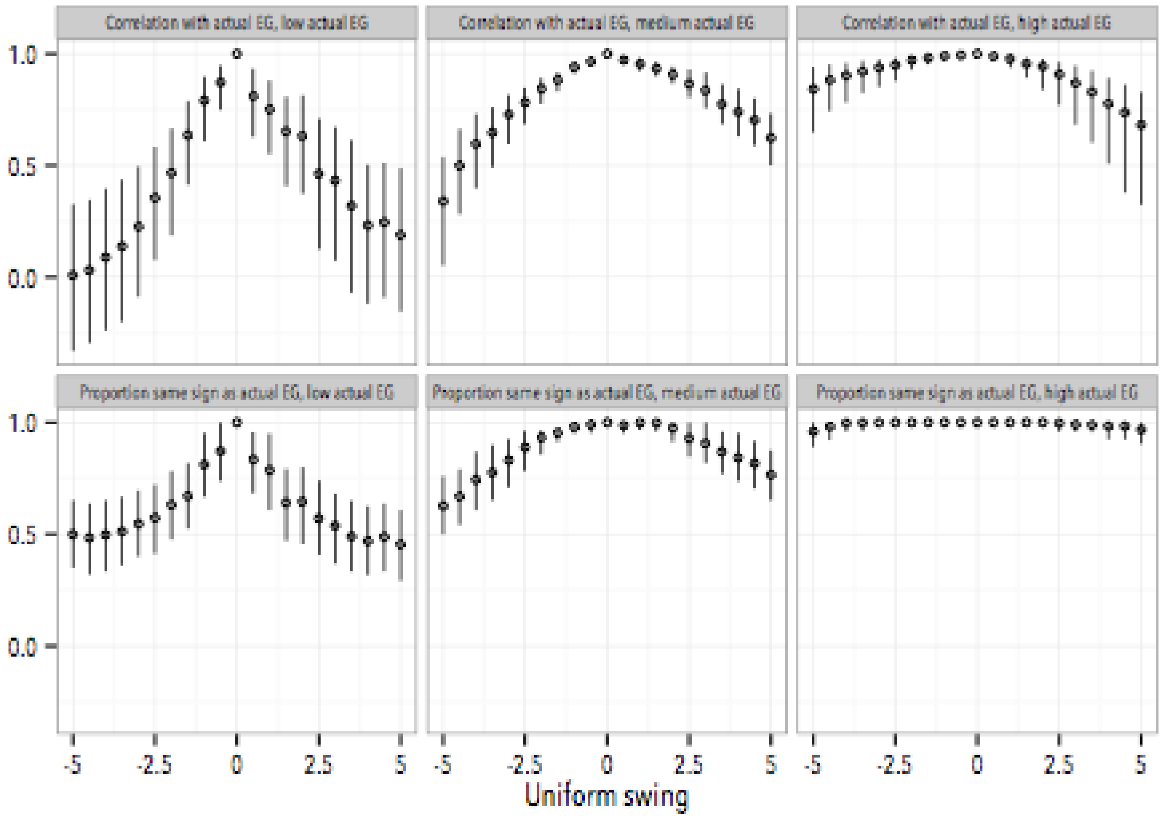

Figure 2: Correlation between actual efficiency gaps and simulated efficiency gaps (top row) and proportion of simulated efficiency gaps with same sign as actual efficiency gaps (bottom row), by hypothetical levels of uniform swing (horizontal axis). Vertical lines are 95% confidence intervals. The three columns correspond to actual efficiency gaps that are low in magnitude (less than .03 in absolute value; left column), medium (.03 to .07 in absolute value, middle column) and high (above .07 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.

The top row of Figure 2 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. As levels of uniform swing increase, the correlation between actual and simulated efficiency gaps diminishes. Small efficiency gaps (less than .03 in absolute value) are less resistant to perturbations from uniform swing; at high levels of uniform swing for small actual efficiency gaps, the correlation between actual efficiency gaps and simulated efficiency gaps approaches zero. However, larger values of the efficiency gap are much more robust to perturbations from uniform swing. In fact, for large actual efficiency gaps (greater than .07 in magnitude), the correlation between actual and simulated efficiency gaps stays impressively large over the entire range of uniform swing levels considered here (top right panel of Figure 2).

The bottom row of Figure 2 displays the proportion of simulated efficiency gaps that have the same sign as 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 we see that small efficiency gaps—less than .03 in magnitude and hence relatively close to zero—are reasonably likely to flip signs under moderate to large values of hypothetical uniform swing: about half of these small efficiency gap estimates flip signs when subjected to reasonably large statewide swings one way or the other. But large efficiency gaps—those greater than .07 in magnitude—show great resistance to flipping signs even in the face of moderate or even large hypothetical statewide swings (lower right panel of Figure 2). None of the large efficiency gaps flip signs when swings are below 2.5 percentage points and *barely any* flip signs even we consider larger statewide swings. Just 11% of actual efficiency gaps greater than .07 in magnitude flip signs when exposed to a very large, hypothetical statewide swing of minus five percentage points and only 9% flip signs when we consider a statewide swing of positive five percentage points.

In short, efficiency gap estimates display a high level of resistance to perturbations from even large levels of uniform swing. This further bolsters our confidence that the efficiency gap is measuring a durable property of a district plan. Moreover, the analysis reported here demonstrates that efficiency gaps are especially reliable when they are large, as is the case for the efficiency gaps generated under the Wisconsin plan. The efficiency gap changes if vote totals change, even if the district plan remains constant; this is "hardwired" into the definition and accompanying arithmetic of the efficiency gap. But to reiterate a conclusion from my original report: the amount of election-to-election variation in the efficiency gap is small relative to the variation in the efficiency gap across plans.

State	Year	Chamber	District	Party	Votes
Michigan	1996	House	1	Democratic	14737
Michigan	1996	House	1	Republican	23493
Michigan	1996	House	2	Democratic	18517
Michigan	1996	House	2	Republican	1758
Michigan	1996	House	3	Democratic	20940
Michigan	1996	House	3	Republican	992
Michigan	1996	House	4	Democratic	20127
Michigan	1996	House	4	Republican	810
Michigan	1996	House	5	Democratic	20431
Michigan	1996	House	5	Republican	1242
Michigan	1996	House	6	Democratic	19269
Michigan	1996	House	6	Republican	1479
Michigan	1996	House	7	Democratic	22404
Michigan	1996	House	7	Republican	889
Michigan	1996	House	8	Democratic	14631
Michigan	1996	House	8	Republican	1516
Michigan	1996	House	9	Democratic	20800
Michigan	1996	House	9	Republican	1017
Michigan	1996	House	10	Democratic	28913
Michigan	1996	House	10	Republican	799
Michigan	1996	House	11	Democratic	23460
Michigan	1996	House	11	Republican	1548
Michigan	1996	House	12	Democratic	28526
Michigan	1996	House	12	Republican	1216
Michigan	1996	House	13	Democratic	19865
Michigan	1996	House	13	Republican	2021
Michigan	1996	House	14	Democratic	19201
Michigan	1996	House	14	Republican	1969
Michigan	1996	House	15	Democratic	21047
Michigan	1996	House	15	Republican	15597
Michigan	1996	House	16	Democratic	18345
Michigan	1996	House	16	Republican	18135
Michigan	1996	House	17	Democratic	18694
Michigan	1996	House	17	Republican	5709
Michigan	1996	House	18	Democratic	15306
Michigan	1996	House	18	Republican	9959
Michigan	1996	House	19	Democratic	13871
Michigan	1996	House	19	Republican	22047
Michigan	1996	House	20	Democratic	13580
Michigan	1996	House	20	Republican	29875
Michigan	1996	House	21	Democratic	15022
Michigan	1996	House	21	Republican	17923
Michigan	1996	House	22	Democratic	18515
Michigan	1996	House	22	Republican	4890
Michigan	1996	House	23	Democratic	18595
Michigan	1996	House	23	Republican	16877
Michigan	1996	House	24	Democratic	21683

State	Year	Chamber	District	Party	Votes
Michigan	1996	House	24	Republican	5298
Michigan	1996	House	25	Democratic	19464
Michigan	1996	House	25	Republican	7292
Michigan	1996	House	26	Democratic	19831
Michigan	1996	House	26	Republican	17343
Michigan	1996	House	27	Democratic	22215
Michigan	1996	House	27	Republican	9667
Michigan	1996	House	28	Democratic	19768
Michigan	1996	House	28	Republican	7610
Michigan	1996	House	29	Democratic	23927
Michigan	1996	House	29	Republican	14249
Michigan	1996	House	30	Democratic	13621
Michigan	1996	House	30	Republican	20670
Michigan	1996	House	31	Democratic	21978
Michigan	1996	House	31	Republican	11237
Michigan	1996	House	32	Democratic	15355
Michigan	1996	House	32	Republican	23603
Michigan	1996	House	33	Democratic	16151
Michigan	1996	House	33	Republican	23830
Michigan	1996	House	34	Democratic	18665
Michigan	1996	House	34	Republican	9919
Michigan	1996	House	35	Democratic	24632
Michigan	1996	House	35	Republican	7809
Michigan	1996	House	36	Democratic	26550
Michigan	1996	House	36	Republican	7528
Michigan	1996	House	37	Democratic	16506
Michigan	1996	House	37	Republican	23202
Michigan	1996	House	38	Democratic	12020
Michigan	1996	House	38	Republican	25152
Michigan	1996	House	39	Democratic	17239
Michigan	1996	House	39	Republican	26333
Michigan	1996	House	40	Democratic	12889
Michigan	1996	House	40	Republican	32453
Michigan	1996	House	41	Democratic	12185
Michigan	1996	House	41	Republican	24596
Michigan	1996	House	42	Democratic	10790
Michigan	1996	House	42	Republican	25547
Michigan	1996	House	43	Democratic	17007
Michigan	1996	House	43	Republican	5790
Michigan	1996	House	44	Democratic	13260
Michigan	1996	House	44	Republican	23919
Michigan	1996	House	45	Democratic	10314
Michigan	1996	House	45	Republican	29640
Michigan	1996	House	46	Democratic	12725
Michigan	1996	House	46	Republican	25600
Michigan	1996	House	47	Democratic	20064
Michigan	1996	House	47	Republican	18887

State	Year	Chamber	District	Party	Votes
Michigan	1996	House	48	Democratic	23920
Michigan	1996	House	48	Republican	2825
Michigan	1996	House	49	Democratic	20998
Michigan	1996	House	49	Republican	5735
Michigan	1996	House	50	Democratic	23009
Michigan	1996	House	50	Republican	12815
Michigan	1996	House	51	Democratic	26349
Michigan	1996	House	51	Republican	12995
Michigan	1996	House	52	Democratic	25200
Michigan	1996	House	52	Republican	13954
Michigan	1996	House	53	Democratic	24733
Michigan	1996	House	53	Republican	10889
Michigan	1996	House	54	Democratic	19371
Michigan	1996	House	54	Republican	7487
Michigan	1996	House	55	Democratic	15185
Michigan	1996	House	55	Republican	21042
Michigan	1996	House	56	Democratic	17650
Michigan	1996	House	56	Republican	12522
Michigan	1996	House	57	Democratic	16596
Michigan	1996	House	57	Republican	16843
Michigan	1996	House	58	Democratic	7557
Michigan	1996	House	58	Republican	21163
Michigan	1996	House	59	Democratic	9181
Michigan	1996	House	59	Republican	20530
Michigan	1996	House	60	Democratic	16848
Michigan	1996	House	60	Republican	10660
Michigan	1996	House	61	Democratic	16178
Michigan	1996	House	61	Republican	27418
Michigan	1996	House	62	Democratic	17520
Michigan	1996	House	62	Republican	14301
Michigan	1996	House	63	Democratic	13816
Michigan	1996	House	63	Republican	22590
Michigan	1996	House	64	Democratic	15853
Michigan	1996	House	64	Republican	9423
Michigan	1996	House	65	Democratic	11990
Michigan	1996	House	65	Republican	19914
Michigan	1996	House	66	Democratic	10741
Michigan	1996	House	66	Republican	27904
Michigan	1996	House	67	Democratic	10010
Michigan	1996	House	67	Republican	21872
Michigan	1996	House	68	Democratic	17424
Michigan	1996	House	68	Republican	10940
Michigan	1996	House	69	Democratic	14559
Michigan	1996	House	69	Republican	6453
Michigan	1996	House	70	Democratic	17828
Michigan	1996	House	70	Republican	9935
Michigan	1996	House	71	Democratic	14944

State	Year	Chamber	District	Party	Votes
Michigan	1996	House	71	Republican	24053
Michigan	1996	House	72	Democratic	11819
Michigan	1996	House	72	Republican	30529
Michigan	1996	House	73	Democratic	12721
Michigan	1996	House	73	Republican	31478
Michigan	1996	House	74	Democratic	10602
Michigan	1996	House	74	Republican	28996
Michigan	1996	House	75	Democratic	13304
Michigan	1996	House	75	Republican	20463
Michigan	1996	House	76	Democratic	17753
Michigan	1996	House	76	Republican	10102
Michigan	1996	House	77	Democratic	12980
Michigan	1996	House	77	Republican	16808
Michigan	1996	House	78	Democratic	9229
Michigan	1996	House	78	Republican	18094
Michigan	1996	House	79	Democratic	10339
Michigan	1996	House	79	Republican	17989
Michigan	1996	House	80	Democratic	10718
Michigan	1996	House	80	Republican	20656
Michigan	1996	House	81	Democratic	16249
Michigan	1996	House	81	Republican	19036
Michigan	1996	House	82	Democratic	19705
Michigan	1996	House	82	Republican	16565
Michigan	1996	House	83	Democratic	12080
Michigan	1996	House	83	Republican	21653
Michigan	1996	House	84	Democratic	12465
Michigan	1996	House	84	Republican	21009
Michigan	1996	House	85	Democratic	17828
Michigan	1996	House	85	Republican	13974
Michigan	1996	House	86	Democratic	12262
Michigan	1996	House	86	Republican	21796
Michigan	1996	House	87	Democratic	10360
Michigan	1996	House	87	Republican	19087
Michigan	1996	House	88	Democratic	11862
Michigan	1996	House	88	Republican	24291
Michigan	1996	House	89	Democratic	9756
Michigan	1996	House	89	Republican	34949
Michigan	1996	House	90	Democratic	9483
Michigan	1996	House	90	Republican	34053
Michigan	1996	House	91	Democratic	23173
Michigan	1996	House	91	Republican	9638
Michigan	1996	House	92	Democratic	19689
Michigan	1996	House	92	Republican	6776
Michigan	1996	House	93	Democratic	11972
Michigan	1996	House	93	Republican	19070
Michigan	1996	House	94	Democratic	11808
Michigan	1996	House	94	Republican	23556

State	Year	Chamber	District	Party	Votes
Michigan	1996	House	95	Democratic	21154
Michigan	1996	House	95	Republican	4038
Michigan	1996	House	96	Democratic	22135
Michigan	1996	House	96	Republican	17546
Michigan	1996	House	97	Democratic	24261
Michigan	1996	House	97	Republican	8355
Michigan	1996	House	98	Democratic	10072
Michigan	1996	House	98	Republican	25427
Michigan	1996	House	99	Democratic	10694
Michigan	1996	House	99	Republican	18739
Michigan	1996	House	100	Democratic	11615
Michigan	1996	House	100	Republican	21207
Michigan	1996	House	101	Democratic	14821
Michigan	1996	House	101	Republican	21787
Michigan	1996	House	102	Democratic	11709
Michigan	1996	House	102	Republican	24273
Michigan	1996	House	103	Democratic	26156
Michigan	1996	House	103	Republican	10713
Michigan	1996	House	104	Democratic	15930
Michigan	1996	House	104	Republican	26443
Michigan	1996	House	105	Democratic	12221
Michigan	1996	House	105	Republican	27290
Michigan	1996	House	106	Democratic	14816
Michigan	1996	House	106	Republican	24335
Michigan	1996	House	107	Democratic	25254
Michigan	1996	House	107	Republican	11437
Michigan	1996	House	108	Democratic	27819
Michigan	1996	House	108	Republican	7921
Michigan	1996	House	109	Democratic	21359
Michigan	1996	House	109	Republican	8347
Michigan	1996	House	110	Democratic	21265
Michigan	1996	House	110	Republican	13886
Michigan	2014	House	1	Democratic	23320
Michigan	2014	House	1	Republican	10130
Michigan	2014	House	2	Democratic	25751
Michigan	2014	House	2	Republican	9447
Michigan	2014	House	3	Democratic	26812
Michigan	2014	House	3	Republican	695
Michigan	2014	House	4	Democratic	21215
Michigan	2014	House	4	Republican	1085
Michigan	2014	House	5	Democratic	16477
Michigan	2014	House	5	Republican	748
Michigan	2014	House	6	Democratic	26419
Michigan	2014	House	6	Republican	1373
Michigan	2014	House	7	Democratic	32283
Michigan	2014	House	7	Republican	581
Michigan	2014	House	8	Democratic	32503

State	Year	Chamber	District	Party	Votes
Michigan	2014	House	8	Republican	927
Michigan	2014	House	9	Democratic	24186
Michigan	2014	House	9	Republican	1060
Michigan	2014	House	10	Democratic	30197
Michigan	2014	House	10	Republican	5771
Michigan	2014	House	11	Democratic	23122
Michigan	2014	House	11	Republican	8999
Michigan	2014	House	12	Democratic	20140
Michigan	2014	House	12	Republican	8449
Michigan	2014	House	13	Democratic	22172
Michigan	2014	House	13	Republican	12551
Michigan	2014	House	14	Democratic	19557
Michigan	2014	House	14	Republican	8106
Michigan	2014	House	15	Democratic	20149
Michigan	2014	House	15	Republican	9318
Michigan	2014	House	16	Democratic	20751
Michigan	2014	House	16	Republican	8968
Michigan	2014	House	17	Democratic	19424
Michigan	2014	House	17	Republican	13237
Michigan	2014	House	18	Democratic	24714
Michigan	2014	House	18	Republican	15956
Michigan	2014	House	19	Democratic	18562
Michigan	2014	House	19	Republican	29809
Michigan	2014	House	20	Democratic	18670
Michigan	2014	House	20	Republican	29109
Michigan	2014	House	21	Democratic	23517
Michigan	2014	House	21	Republican	18107
Michigan	2014	House	22	Democratic	17759
Michigan	2014	House	22	Republican	8884
Michigan	2014	House	23	Democratic	21428
Michigan	2014	House	23	Republican	20585
Michigan	2014	House	24	Democratic	15118
Michigan	2014	House	24	Republican	22608
Michigan	2014	House	25	Democratic	19293
Michigan	2014	House	25	Republican	17072
Michigan	2014	House	26	Democratic	22557
Michigan	2014	House	26	Republican	15007
Michigan	2014	House	27	Democratic	33223
Michigan	2014	House	27	Republican	9731
Michigan	2014	House	28	Democratic	17324
Michigan	2014	House	28	Republican	9727
Michigan	2014	House	29	Democratic	19946
Michigan	2014	House	29	Republican	7154
Michigan	2014	House	30	Democratic	13461
Michigan	2014	House	30	Republican	16726
Michigan	2014	House	31	Democratic	21219
Michigan	2014	House	31	Republican	13910

State	Year	Chamber	District	Party	Votes
Michigan	2014	House	32	Democratic	13610
Michigan	2014	House	32	Republican	21432
Michigan	2014	House	33	Democratic	12646
Michigan	2014	House	33	Republican	23284
Michigan	2014	House	34	Democratic	23506
Michigan	2014	House	34	Republican	2024
Michigan	2014	House	35	Democratic	44456
Michigan	2014	House	35	Republican	8538
Michigan	2014	House	36	Democratic	11407
Michigan	2014	House	36	Republican	30250
Michigan	2014	House	37	Democratic	27409
Michigan	2014	House	37	Republican	19595
Michigan	2014	House	38	Democratic	14755
Michigan	2014	House	38	Republican	26359
Michigan	2014	House	39	Democratic	19684
Michigan	2014	House	39	Republican	24928
Michigan	2014	House	40	Democratic	23174
Michigan	2014	House	40	Republican	32687
Michigan	2014	House	41	Democratic	19052
Michigan	2014	House	41	Republican	25824
Michigan	2014	House	42	Democratic	15727
Michigan	2014	House	42	Republican	34962
Michigan	2014	House	43	Democratic	18424
Michigan	2014	House	43	Republican	28508
Michigan	2014	House	44	Democratic	13956
Michigan	2014	House	44	Republican	32555
Michigan	2014	House	45	Democratic	18145
Michigan	2014	House	45	Republican	27036
Michigan	2014	House	46	Democratic	12606
Michigan	2014	House	46	Republican	31431
Michigan	2014	House	47	Democratic	10110
Michigan	2014	House	47	Republican	30354
Michigan	2014	House	48	Democratic	23435
Michigan	2014	House	48	Republican	13826
Michigan	2014	House	49	Democratic	24016
Michigan	2014	House	49	Republican	8374
Michigan	2014	House	50	Democratic	21869
Michigan	2014	House	50	Republican	15665
Michigan	2014	House	51	Democratic	18225
Michigan	2014	House	51	Republican	25568
Michigan	2014	House	52	Democratic	26149
Michigan	2014	House	52	Republican	21848
Michigan	2014	House	53	Democratic	29815
Michigan	2014	House	53	Republican	5342
Michigan	2014	House	54	Democratic	24529
Michigan	2014	House	54	Republican	7587
Michigan	2014	House	55	Democratic	25522

State	Year	Chamber	District	Party	Votes
Michigan	2014	House	55	Republican	11341
Michigan	2014	House	56	Democratic	16077
Michigan	2014	House	56	Republican	19218
Michigan	2014	House	57	Democratic	13308
Michigan	2014	House	57	Republican	19794
Michigan	2014	House	58	Democratic	8467
Michigan	2014	House	58	Republican	26437
Michigan	2014	House	59	Democratic	10128
Michigan	2014	House	59	Republican	21256
Michigan	2014	House	60	Democratic	21067
Michigan	2014	House	60	Republican	8638
Michigan	2014	House	61	Democratic	17184
Michigan	2014	House	61	Republican	23090
Michigan	2014	House	62	Democratic	15119
Michigan	2014	House	62	Republican	17103
Michigan	2014	House	63	Democratic	15549
Michigan	2014	House	63	Republican	24154
Michigan	2014	House	64	Democratic	11659
Michigan	2014	House	64	Republican	19392
Michigan	2014	House	65	Democratic	13747
Michigan	2014	House	65	Republican	22374
Michigan	2014	House	66	Democratic	14391
Michigan	2014	House	66	Republican	21251
Michigan	2014	House	67	Democratic	21802
Michigan	2014	House	67	Republican	18981
Michigan	2014	House	68	Democratic	25410
Michigan	2014	House	68	Republican	8119
Michigan	2014	House	69	Democratic	24680
Michigan	2014	House	69	Republican	12049
Michigan	2014	House	70	Democratic	10391
Michigan	2014	House	70	Republican	18081
Michigan	2014	House	71	Democratic	21821
Michigan	2014	House	71	Republican	23276
Michigan	2014	House	72	Democratic	10311
Michigan	2014	House	72	Republican	26103
Michigan	2014	House	73	Democratic	14475
Michigan	2014	House	73	Republican	38295
Michigan	2014	House	74	Democratic	11143
Michigan	2014	House	74	Republican	27153
Michigan	2014	House	75	Democratic	15133
Michigan	2014	House	75	Republican	6409
Michigan	2014	House	76	Democratic	19722
Michigan	2014	House	76	Republican	22532
Michigan	2014	House	77	Democratic	9948
Michigan	2014	House	77	Republican	23003
Michigan	2014	House	78	Democratic	8873
Michigan	2014	House	78	Republican	21115

State	Year	Chamber	District	Party	Votes
Michigan	2014	House	79	Democratic	12023
Michigan	2014	House	79	Republican	23519
Michigan	2014	House	80	Democratic	11669
Michigan	2014	House	80	Republican	27243
Michigan	2014	House	81	Democratic	13436
Michigan	2014	House	81	Republican	24708
Michigan	2014	House	82	Democratic	15687
Michigan	2014	House	82	Republican	25134
Michigan	2014	House	83	Democratic	12234
Michigan	2014	House	83	Republican	20765
Michigan	2014	House	84	Democratic	14809
Michigan	2014	House	84	Republican	27506
Michigan	2014	House	85	Democratic	18019
Michigan	2014	House	85	Republican	24773
Michigan	2014	House	86	Democratic	12471
Michigan	2014	House	86	Republican	30834
Michigan	2014	House	87	Democratic	12360
Michigan	2014	House	87	Republican	28696
Michigan	2014	House	88	Democratic	7841
Michigan	2014	House	88	Republican	33372
Michigan	2014	House	89	Democratic	12834
Michigan	2014	House	89	Republican	30290
Michigan	2014	House	90	Democratic	6630
Michigan	2014	House	90	Republican	28063
Michigan	2014	House	91	Democratic	16248
Michigan	2014	House	91	Republican	17936
Michigan	2014	House	92	Democratic	17017
Michigan	2014	House	92	Republican	9122
Michigan	2014	House	93	Democratic	14862
Michigan	2014	House	93	Republican	26086
Michigan	2014	House	94	Democratic	18190
Michigan	2014	House	94	Republican	30838
Michigan	2014	House	95	Democratic	24368
Michigan	2014	House	95	Republican	7520
Michigan	2014	House	96	Democratic	26042
Michigan	2014	House	96	Republican	12958
Michigan	2014	House	97	Democratic	12461
Michigan	2014	House	97	Republican	23204
Michigan	2014	House	98	Democratic	15942
Michigan	2014	House	98	Republican	28135
Michigan	2014	House	99	Democratic	12527
Michigan	2014	House	99	Republican	15570
Michigan	2014	House	100	Democratic	11678
Michigan	2014	House	100	Republican	23188
Michigan	2014	House	101	Democratic	23372
Michigan	2014	House	101	Republican	27511
Michigan	2014	House	102	Democratic	11826

State	Year	Chamber	District	Party	Votes
Michigan	2014	House	102	Republican	22526
Michigan	2014	House	103	Democratic	14947
Michigan	2014	House	103	Republican	29291
Michigan	2014	House	104	Democratic	19123
Michigan	2014	House	104	Republican	28878
Michigan	2014	House	105	Democratic	16239
Michigan	2014	House	105	Republican	35023
Michigan	2014	House	106	Democratic	21351
Michigan	2014	House	106	Republican	26209
Michigan	2014	House	107	Democratic	15370
Michigan	2014	House	107	Republican	31473
Michigan	2014	House	108	Democratic	15031
Michigan	2014	House	108	Republican	23186
Michigan	2014	House	109	Democratic	23229
Michigan	2014	House	109	Republican	13499
Michigan	2014	House	110	Democratic	20018
Michigan	2014	House	110	Republican	14415
Minnesota	2008	House	1-1	Democratic	8726
Minnesota	2008	House	1-1	Republican	7851
Minnesota	2008	House	1-2	Democratic	10193
Minnesota	2008	House	1-2	Republican	7116
Minnesota	2008	House	2-1	Democratic	11411
Minnesota	2008	House	2-1	Republican	6535
Minnesota	2008	House	2-2	Democratic	10773
Minnesota	2008	House	2-2	Republican	9117
Minnesota	2008	House	3-1	Democratic	11219
Minnesota	2008	House	3-1	Republican	6506
Minnesota	2008	House	3-2	Democratic	11951
Minnesota	2008	House	3-2	Republican	8732
Minnesota	2008	House	4-1	Democratic	10259
Minnesota	2008	House	4-1	Republican	8666
Minnesota	2008	House	4-2	Democratic	11032
Minnesota	2008	House	4-2	Republican	12820
Minnesota	2008	House	5-1	Democratic	16230
Minnesota	2008	House	5-1	Republican	4461
Minnesota	2008	House	5-2	Democratic	13956
Minnesota	2008	House	5-2	Republican	5953
Minnesota	2008	House	6-1	Democratic	15932
Minnesota	2008	House	6-1	Republican	6213
Minnesota	2008	House	6-2	Democratic	14807
Minnesota	2008	House	6-2	Republican	7823
Minnesota	2008	House	7-1	Democratic	15029
Minnesota	2008	House	7-1	Republican	7595
Minnesota	2008	House	7-2	Democratic	13364
Minnesota	2008	House	7-2	Republican	3648
Minnesota	2008	House	8-1	Democratic	13042
Minnesota	2008	House	8-1	Republican	6956

State	Year	Chamber	District	Party	Votes
Minnesota	2008	House	8-2	Democratic	10258
Minnesota	2008	House	8-2	Republican	9951
Minnesota	2008	House	9-1	Democratic	8064
Minnesota	2008	House	9-1	Republican	11739
Minnesota	2008	House	9-2	Democratic	15095
Minnesota	2008	House	9-2	Republican	5054
Minnesota	2008	House	10-1	Democratic	8981
Minnesota	2008	House	10-1	Republican	11212
Minnesota	2008	House	10-2	Democratic	8226
Minnesota	2008	House	10-2	Republican	11477
Minnesota	2008	House	11-1	Democratic	6328
Minnesota	2008	House	11-1	Republican	13699
Minnesota	2008	House	11-2	Democratic	10681
Minnesota	2008	House	11-2	Republican	9682
Minnesota	2008	House	12-1	Democratic	13766
Minnesota	2008	House	12-1	Republican	7410
Minnesota	2008	House	12-2	Democratic	10071
Minnesota	2008	House	12-2	Republican	9995
Minnesota	2008	House	13-1	Democratic	8128
Minnesota	2008	House	13-1	Republican	11422
Minnesota	2008	House	13-2	Democratic	10211
Minnesota	2008	House	13-2	Republican	8770
Minnesota	2008	House	14-1	Democratic	10572
Minnesota	2008	House	14-1	Republican	12927
Minnesota	2008	House	14-2	Democratic	14415
Minnesota	2008	House	14-2	Republican	7045
Minnesota	2008	House	15-1	Democratic	9873
Minnesota	2008	House	15-1	Republican	11446
Minnesota	2008	House	15-2	Democratic	11008
Minnesota	2008	House	15-2	Republican	5322
Minnesota	2008	House	16-1	Democratic	11044
Minnesota	2008	House	16-1	Republican	10955
Minnesota	2008	House	16-2	Democratic	8996
Minnesota	2008	House	16-2	Republican	15863
Minnesota	2008	House	17-1	Democratic	10212
Minnesota	2008	House	17-1	Republican	12448
Minnesota	2008	House	17-2	Democratic	13464
Minnesota	2008	House	17-2	Republican	11789
Minnesota	2008	House	18-1	Democratic	7122
Minnesota	2008	House	18-1	Republican	12312
Minnesota	2008	House	18-2	Democratic	8296
Minnesota	2008	House	18-2	Republican	11813
Minnesota	2008	House	19-1	Democratic	11219
Minnesota	2008	House	19-1	Republican	12443
Minnesota	2008	House	19-2	Democratic	11194
Minnesota	2008	House	19-2	Republican	17455
Minnesota	2008	House	20-1	Democratic	9032

State	Year	Chamber	District	Party	Votes
Minnesota	2008	House	20-1	Republican	8506
Minnesota	2008	House	20-2	Democratic	11732
Minnesota	2008	House	20-2	Republican	5790
Minnesota	2008	House	21-1	Democratic	5827
Minnesota	2008	House	21-1	Republican	11915
Minnesota	2008	House	21-2	Democratic	8586
Minnesota	2008	House	21-2	Republican	9111
Minnesota	2008	House	22-1	Democratic	7887
Minnesota	2008	House	22-1	Republican	9831
Minnesota	2008	House	22-2	Democratic	6669
Minnesota	2008	House	22-2	Republican	10180
Minnesota	2008	House	23-1	Democratic	13209
Minnesota	2008	House	23-1	Republican	7545
Minnesota	2008	House	23-2	Democratic	14218
Minnesota	2008	House	23-2	Republican	7629
Minnesota	2008	House	24-1	Democratic	6697
Minnesota	2008	House	24-1	Republican	10752
Minnesota	2008	House	24-2	Democratic	9781
Minnesota	2008	House	24-2	Republican	10275
Minnesota	2008	House	25-1	Democratic	8484
Minnesota	2008	House	25-1	Republican	12589
Minnesota	2008	House	25-2	Democratic	12642
Minnesota	2008	House	25-2	Republican	11028
Minnesota	2008	House	26-1	Democratic	11527
Minnesota	2008	House	26-1	Republican	8710
Minnesota	2008	House	26-2	Democratic	10079
Minnesota	2008	House	26-2	Republican	7353
Minnesota	2008	House	27-1	Democratic	10960
Minnesota	2008	House	27-1	Republican	8031
Minnesota	2008	House	27-2	Democratic	11844
Minnesota	2008	House	27-2	Republican	6085
Minnesota	2008	House	28-1	Democratic	9911
Minnesota	2008	House	28-1	Republican	10702
Minnesota	2008	House	28-2	Democratic	9050
Minnesota	2008	House	28-2	Republican	10980
Minnesota	2008	House	29-1	Democratic	10583
Minnesota	2008	House	29-1	Republican	13638
Minnesota	2008	House	29-2	Democratic	12142
Minnesota	2008	House	29-2	Republican	7553
Minnesota	2008	House	30-1	Democratic	10768
Minnesota	2008	House	30-1	Republican	6624
Minnesota	2008	House	30-2	Democratic	13478
Minnesota	2008	House	30-2	Republican	10763
Minnesota	2008	House	31-1	Democratic	12941
Minnesota	2008	House	31-1	Republican	6138
Minnesota	2008	House	31-2	Democratic	9466
Minnesota	2008	House	31-2	Republican	9873

State	Year	Chamber	District	Party	Votes
Minnesota	2008	House	32-1	Democratic	9827
Minnesota	2008	House	32-1	Republican	19340
Minnesota	2008	House	32-2	Democratic	9809
Minnesota	2008	House	32-2	Republican	11129
Minnesota	2008	House	33-1	Democratic	8564
Minnesota	2008	House	33-1	Republican	14346
Minnesota	2008	House	33-2	Democratic	9019
Minnesota	2008	House	33-2	Republican	13097
Minnesota	2008	House	34-1	Democratic	8725
Minnesota	2008	House	34-1	Republican	18187
Minnesota	2008	House	34-2	Democratic	8034
Minnesota	2008	House	34-2	Republican	13456
Minnesota	2008	House	35-1	Democratic	12553
Minnesota	2008	House	35-1	Republican	15431
Minnesota	2008	House	35-2	Democratic	8949
Minnesota	2008	House	35-2	Republican	15298
Minnesota	2008	House	36-1	Democratic	8277
Minnesota	2008	House	36-1	Republican	14470
Minnesota	2008	House	36-2	Democratic	11144
Minnesota	2008	House	36-2	Republican	14235
Minnesota	2008	House	37-1	Democratic	10633
Minnesota	2008	House	37-1	Republican	11666
Minnesota	2008	House	37-2	Democratic	12668
Minnesota	2008	House	37-2	Republican	11388
Minnesota	2008	House	38-1	Democratic	10667
Minnesota	2008	House	38-1	Republican	9641
Minnesota	2008	House	38-2	Democratic	10712
Minnesota	2008	House	38-2	Republican	10193
Minnesota	2008	House	39-1	Democratic	12798
Minnesota	2008	House	39-1	Republican	8373
Minnesota	2008	House	39-2	Democratic	16291
Minnesota	2008	House	39-2	Republican	5876
Minnesota	2008	House	40-1	Democratic	10297
Minnesota	2008	House	40-1	Republican	8095
Minnesota	2008	House	40-2	Democratic	14066
Minnesota	2008	House	40-2	Republican	7335
Minnesota	2008	House	41-1	Democratic	7626
Minnesota	2008	House	41-1	Republican	8925
Minnesota	2008	House	41-2	Democratic	11434
Minnesota	2008	House	41-2	Republican	10227
Minnesota	2008	House	42-1	Democratic	12806
Minnesota	2008	House	42-1	Republican	9154
Minnesota	2008	House	42-2	Democratic	10002
Minnesota	2008	House	42-2	Republican	13452
Minnesota	2008	House	43-1	Democratic	10756
Minnesota	2008	House	43-1	Republican	12839
Minnesota	2008	House	43-2	Democratic	12372

State	Year	Chamber	District	Party	Votes
Minnesota	2008	House	43-2	Republican	9872
Minnesota	2008	House	44-1	Democratic	14394
Minnesota	2008	House	44-1	Republican	6553
Minnesota	2008	House	44-2	Democratic	14524
Minnesota	2008	House	44-2	Republican	7162
Minnesota	2008	House	45-1	Democratic	11447
Minnesota	2008	House	45-1	Republican	8054
Minnesota	2008	House	45-2	Democratic	13719
Minnesota	2008	House	45-2	Republican	7335
Minnesota	2008	House	46-1	Democratic	9275
Minnesota	2008	House	46-1	Republican	4730
Minnesota	2008	House	46-2	Democratic	10189
Minnesota	2008	House	46-2	Republican	4726
Minnesota	2008	House	47-1	Democratic	11693
Minnesota	2008	House	47-1	Republican	7977
Minnesota	2008	House	47-2	Democratic	12382
Minnesota	2008	House	47-2	Republican	10187
Minnesota	2008	House	48-1	Democratic	9335
Minnesota	2008	House	48-1	Republican	14066
Minnesota	2008	House	48-2	Democratic	6946
Minnesota	2008	House	48-2	Republican	13057
Minnesota	2008	House	49-1	Democratic	9523
Minnesota	2008	House	49-1	Republican	13934
Minnesota	2008	House	49-2	Democratic	11019
Minnesota	2008	House	49-2	Republican	8413
Minnesota	2008	House	50-1	Democratic	11318
Minnesota	2008	House	50-1	Republican	6652
Minnesota	2008	House	50-2	Democratic	11968
Minnesota	2008	House	50-2	Republican	9185
Minnesota	2008	House	51-1	Democratic	10679
Minnesota	2008	House	51-1	Republican	11813
Minnesota	2008	House	51-2	Democratic	11276
Minnesota	2008	House	51-2	Republican	6921
Minnesota	2008	House	52-1	Democratic	8085
Minnesota	2008	House	52-1	Republican	16512
Minnesota	2008	House	52-2	Democratic	11698
Minnesota	2008	House	52-2	Republican	14689
Minnesota	2008	House	53-1	Democratic	11524
Minnesota	2008	House	53-1	Republican	10488
Minnesota	2008	House	53-2	Democratic	10147
Minnesota	2008	House	53-2	Republican	12041
Minnesota	2008	House	54-1	Democratic	12693
Minnesota	2008	House	54-1	Republican	9402
Minnesota	2008	House	54-2	Democratic	11573
Minnesota	2008	House	54-2	Republican	6852
Minnesota	2008	House	55-1	Democratic	11749
Minnesota	2008	House	55-1	Republican	5961

State	Year	Chamber	District	Party	Votes
Minnesota	2008	House	55-2	Democratic	13158
Minnesota	2008	House	55-2	Republican	7246
Minnesota	2008	House	56-1	Democratic	11963
Minnesota	2008	House	56-1	Republican	10694
Minnesota	2008	House	56-2	Democratic	14466
Minnesota	2008	House	56-2	Republican	11820
Minnesota	2008	House	57-1	Democratic	11855
Minnesota	2008	House	57-1	Republican	8391
Minnesota	2008	House	57-2	Democratic	9770
Minnesota	2008	House	57-2	Republican	13102
Minnesota	2008	House	58-1	Democratic	11814
Minnesota	2008	House	58-1	Republican	2530
Minnesota	2008	House	58-2	Democratic	11960
Minnesota	2008	House	58-2	Republican	1912
Minnesota	2008	House	59-1	Democratic	13785
Minnesota	2008	House	59-1	Republican	2520
Minnesota	2008	House	59-2	Democratic	12037
Minnesota	2008	House	59-2	Republican	4463
Minnesota	2008	House	60-1	Democratic	17609
Minnesota	2008	House	60-1	Republican	4263
Minnesota	2008	House	60-2	Democratic	18868
Minnesota	2008	House	60-2	Republican	4418
Minnesota	2008	House	61-1	Democratic	11005
Minnesota	2008	House	61-1	Republican	1325
Minnesota	2008	House	61-2	Democratic	8795
Minnesota	2008	House	61-2	Republican	1356
Minnesota	2008	House	62-1	Democratic	17190
Minnesota	2008	House	62-1	Republican	3368
Minnesota	2008	House	62-2	Democratic	17394
Minnesota	2008	House	62-2	Republican	4517
Minnesota	2008	House	63-1	Democratic	15314
Minnesota	2008	House	63-1	Republican	5280
Minnesota	2008	House	63-2	Democratic	11265
Minnesota	2008	House	63-2	Republican	5755
Minnesota	2008	House	64-1	Democratic	16638
Minnesota	2008	House	64-1	Republican	4776
Minnesota	2008	House	64-2	Democratic	16143
Minnesota	2008	House	64-2	Republican	6284
Minnesota	2008	House	65-1	Democratic	9226
Minnesota	2008	House	65-1	Republican	2297
Minnesota	2008	House	65-2	Democratic	12810
Minnesota	2008	House	65-2	Republican	4169
Minnesota	2008	House	66-1	Democratic	9001
Minnesota	2008	House	66-1	Republican	2665
Minnesota	2008	House	66-2	Democratic	16029
Minnesota	2008	House	66-2	Republican	4390
Minnesota	2008	House	67-1	Democratic	10550

State	Year	Chamber	District	Party	Votes
Minnesota	2008	House	67-1	Republican	2911
Minnesota	2008	House	67-2	Democratic	10142
Minnesota	2008	House	67-2	Republican	4011
Michigan	1994	Senate	1	Democratic	39208
Michigan	1994	Senate	1	Republican	27288
Michigan	1994	Senate	2	Democratic	47174
Michigan	1994	Senate	2	Republican	6081
Michigan	1994	Senate	3	Democratic	45788
Michigan	1994	Senate	3	Republican	3887
Michigan	1994	Senate	4	Democratic	62715
Michigan	1994	Senate	4	Republican	2865
Michigan	1994	Senate	5	Democratic	51291
Michigan	1994	Senate	5	Republican	5155
Michigan	1994	Senate	6	Democratic	43947
Michigan	1994	Senate	6	Republican	39570
Michigan	1994	Senate	7	Democratic	41049
Michigan	1994	Senate	7	Republican	25708
Michigan	1994	Senate	8	Democratic	31011
Michigan	1994	Senate	8	Republican	32744
Michigan	1994	Senate	9	Democratic	30376
Michigan	1994	Senate	9	Republican	59463
Michigan	1994	Senate	10	Democratic	42919
Michigan	1994	Senate	10	Republican	29249
Michigan	1994	Senate	11	Democratic	42562
Michigan	1994	Senate	11	Republican	37313
Michigan	1994	Senate	12	Democratic	23777
Michigan	1994	Senate	12	Republican	56492
Michigan	1994	Senate	13	Democratic	28871
Michigan	1994	Senate	13	Republican	59846
Michigan	1994	Senate	14	Democratic	48351
Michigan	1994	Senate	14	Republican	33767
Michigan	1994	Senate	15	Democratic	32357
Michigan	1994	Senate	15	Republican	65592
Michigan	1994	Senate	16	Democratic	25426
Michigan	1994	Senate	16	Republican	63804
Michigan	1994	Senate	17	Democratic	37667
Michigan	1994	Senate	17	Republican	34032
Michigan	1994	Senate	18	Democratic	46721
Michigan	1994	Senate	18	Republican	35593
Michigan	1994	Senate	19	Democratic	18367
Michigan	1994	Senate	19	Republican	47194
Michigan	1994	Senate	20	Democratic	19815
Michigan	1994	Senate	20	Republican	46574
Michigan	1994	Senate	21	Democratic	30383
Michigan	1994	Senate	21	Republican	42171
Michigan	1994	Senate	22	Democratic	18285
Michigan	1994	Senate	22	Republican	62971

State	Year	Chamber	District	Party	Votes
Michigan	1994	Senate	23	Democratic	22521
Michigan	1994	Senate	23	Republican	49119
Michigan	1994	Senate	24	Democratic	28743
Michigan	1994	Senate	24	Republican	54194
Michigan	1994	Senate	25	Democratic	54647
Michigan	1994	Senate	25	Republican	32012
Michigan	1994	Senate	26	Democratic	32141
Michigan	1994	Senate	26	Republican	58046
Michigan	1994	Senate	27	Democratic	28813
Michigan	1994	Senate	27	Republican	56887
Michigan	1994	Senate	28	Democratic	46741
Michigan	1994	Senate	28	Republican	29071
Michigan	1994	Senate	29	Democratic	57263
Michigan	1994	Senate	29	Republican	23933
Michigan	1994	Senate	30	Democratic	30719
Michigan	1994	Senate	30	Republican	43911
Michigan	1994	Senate	31	Democratic	17604
Michigan	1994	Senate	31	Republican	65919
Michigan	1994	Senate	32	Democratic	32441
Michigan	1994	Senate	32	Republican	48076
Michigan	1994	Senate	33	Democratic	34764
Michigan	1994	Senate	33	Republican	48647
Michigan	1994	Senate	34	Democratic	30566
Michigan	1994	Senate	34	Republican	48824
Michigan	1994	Senate	35	Democratic	23870
Michigan	1994	Senate	35	Republican	61121
Michigan	1994	Senate	36	Democratic	26794
Michigan	1994	Senate	36	Republican	65241
Michigan	1994	Senate	37	Democratic	31711
Michigan	1994	Senate	37	Republican	54213
Michigan	1994	Senate	38	Democratic	55936
Michigan	1994	Senate	38	Republican	22612
Michigan	1998	Senate	1	Democratic	36150
Michigan	1998	Senate	1	Republican	21656
Michigan	1998	Senate	2	Democratic	42832
Michigan	1998	Senate	2	Republican	3641
Michigan	1998	Senate	3	Democratic	42976
Michigan	1998	Senate	3	Republican	2402
Michigan	1998	Senate	4	Democratic	57458
Michigan	1998	Senate	4	Republican	2085
Michigan	1998	Senate	5	Democratic	47093
Michigan	1998	Senate	5	Republican	4087
Michigan	1998	Senate	6	Democratic	40801
Michigan	1998	Senate	6	Republican	33461
Michigan	1998	Senate	7	Democratic	43886
Michigan	1998	Senate	7	Republican	19516
Michigan	1998	Senate	8	Democratic	30289

State	Year	Chamber	District	Party	Votes
Michigan	1998	Senate	8	Republican	33501
Michigan	1998	Senate	9	Democratic	29548
Michigan	1998	Senate	9	Republican	53396
Michigan	1998	Senate	10	Democratic	44916
Michigan	1998	Senate	10	Republican	21971
Michigan	1998	Senate	11	Democratic	41890
Michigan	1998	Senate	11	Republican	38588
Michigan	1998	Senate	12	Democratic	31339
Michigan	1998	Senate	12	Republican	54116
Michigan	1998	Senate	13	Democratic	29411
Michigan	1998	Senate	13	Republican	57752
Michigan	1998	Senate	14	Democratic	53497
Michigan	1998	Senate	14	Republican	28201
Michigan	1998	Senate	15	Democratic	39414
Michigan	1998	Senate	15	Republican	62847
Michigan	1998	Senate	16	Democratic	30563
Michigan	1998	Senate	16	Republican	68366
Michigan	1998	Senate	17	Democratic	35353
Michigan	1998	Senate	17	Republican	41859
Michigan	1998	Senate	18	Democratic	53024
Michigan	1998	Senate	18	Republican	32211
Michigan	1998	Senate	19	Democratic	20675
Michigan	1998	Senate	19	Republican	47714
Michigan	1998	Senate	20	Democratic	20244
Michigan	1998	Senate	20	Republican	44362
Michigan	1998	Senate	21	Democratic	33207
Michigan	1998	Senate	21	Republican	38349
Michigan	1998	Senate	22	Democratic	19092
Michigan	1998	Senate	22	Republican	69986
Michigan	1998	Senate	23	Democratic	21743
Michigan	1998	Senate	23	Republican	50995
Michigan	1998	Senate	24	Democratic	27990
Michigan	1998	Senate	24	Republican	53361
Michigan	1998	Senate	25	Democratic	55200
Michigan	1998	Senate	25	Republican	25816
Michigan	1998	Senate	26	Democratic	28784
Michigan	1998	Senate	26	Republican	66144
Michigan	1998	Senate	27	Democratic	30315
Michigan	1998	Senate	27	Republican	58413
Michigan	1998	Senate	28	Democratic	48249
Michigan	1998	Senate	28	Republican	25513
Michigan	1998	Senate	29	Democratic	53673
Michigan	1998	Senate	29	Republican	20472
Michigan	1998	Senate	30	Democratic	28525
Michigan	1998	Senate	30	Republican	47499
Michigan	1998	Senate	31	Democratic	19577
Michigan	1998	Senate	31	Republican	77949

State	Year	Chamber	District	Party	Votes
Michigan	1998	Senate	32	Democratic	27989
Michigan	1998	Senate	32	Republican	57363
Michigan	1998	Senate	33	Democratic	35390
Michigan	1998	Senate	33	Republican	45764
Michigan	1998	Senate	34	Democratic	34073
Michigan	1998	Senate	34	Republican	47404
Michigan	1998	Senate	35	Democratic	25900
Michigan	1998	Senate	35	Republican	61510
Michigan	1998	Senate	36	Democratic	34576
Michigan	1998	Senate	36	Republican	65415
Michigan	1998	Senate	37	Democratic	33352
Michigan	1998	Senate	37	Republican	55143
Michigan	1998	Senate	38	Democratic	59717
Michigan	1998	Senate	38	Republican	17884
Michigan	2002	Senate	1	Democratic	47679
Michigan	2002	Senate	1	Republican	3226
Michigan	2002	Senate	2	Democratic	40087
Michigan	2002	Senate	2	Republican	18899
Michigan	2002	Senate	3	Democratic	53395
Michigan	2002	Senate	3	Republican	12855
Michigan	2002	Senate	4	Democratic	53614
Michigan	2002	Senate	4	Republican	2404
Michigan	2002	Senate	5	Democratic	49780
Michigan	2002	Senate	5	Republican	10334
Michigan	2002	Senate	6	Democratic	39152
Michigan	2002	Senate	6	Republican	46257
Michigan	2002	Senate	7	Democratic	40675
Michigan	2002	Senate	7	Republican	52435
Michigan	2002	Senate	8	Democratic	43874
Michigan	2002	Senate	8	Republican	22128
Michigan	2002	Senate	9	Democratic	47609
Michigan	2002	Senate	9	Republican	28588
Michigan	2002	Senate	10	Democratic	42822
Michigan	2002	Senate	10	Republican	36424
Michigan	2002	Senate	11	Democratic	26365
Michigan	2002	Senate	11	Republican	55859
Michigan	2002	Senate	12	Democratic	32412
Michigan	2002	Senate	12	Republican	54569
Michigan	2002	Senate	13	Democratic	37222
Michigan	2002	Senate	13	Republican	63793
Michigan	2002	Senate	14	Democratic	65538
Michigan	2002	Senate	14	Republican	28249
Michigan	2002	Senate	15	Democratic	35305
Michigan	2002	Senate	15	Republican	57583
Michigan	2002	Senate	16	Democratic	25604
Michigan	2002	Senate	16	Republican	39894
Michigan	2002	Senate	17	Democratic	30262

State	Year	Chamber	District	Party	Votes
Michigan	2002	Senate	17	Republican	44773
Michigan	2002	Senate	18	Democratic	52912
Michigan	2002	Senate	18	Republican	27726
Michigan	2002	Senate	19	Democratic	39673
Michigan	2002	Senate	19	Republican	32281
Michigan	2002	Senate	20	Democratic	34327
Michigan	2002	Senate	20	Republican	44642
Michigan	2002	Senate	21	Democratic	23473
Michigan	2002	Senate	21	Republican	43239
Michigan	2002	Senate	22	Democratic	27866
Michigan	2002	Senate	22	Republican	59853
Michigan	2002	Senate	23	Democratic	44136
Michigan	2002	Senate	23	Republican	38581
Michigan	2002	Senate	24	Democratic	32170
Michigan	2002	Senate	24	Republican	57906
Michigan	2002	Senate	25	Democratic	34517
Michigan	2002	Senate	25	Republican	43806
Michigan	2002	Senate	26	Democratic	47878
Michigan	2002	Senate	26	Republican	37852
Michigan	2002	Senate	27	Democratic	52019
Michigan	2002	Senate	27	Republican	26699
Michigan	2002	Senate	28	Democratic	25425
Michigan	2002	Senate	28	Republican	72993
Michigan	2002	Senate	29	Democratic	36746
Michigan	2002	Senate	29	Republican	44202
Michigan	2002	Senate	30	Democratic	21701
Michigan	2002	Senate	30	Republican	71160
Michigan	2002	Senate	31	Democratic	54352
Michigan	2002	Senate	31	Republican	35486
Michigan	2002	Senate	32	Democratic	37668
Michigan	2002	Senate	32	Republican	45338
Michigan	2002	Senate	33	Democratic	26800
Michigan	2002	Senate	33	Republican	45487
Michigan	2002	Senate	34	Democratic	41233
Michigan	2002	Senate	34	Republican	42180
Michigan	2002	Senate	35	Democratic	30942
Michigan	2002	Senate	35	Republican	51405
Michigan	2002	Senate	36	Democratic	44487
Michigan	2002	Senate	36	Republican	46511
Michigan	2002	Senate	37	Democratic	35852
Michigan	2002	Senate	37	Republican	53490
Michigan	2002	Senate	38	Democratic	51348
Michigan	2002	Senate	38	Republican	33063
Michigan	2006	Senate	1	Democratic	52367
Michigan	2006	Senate	1	Republican	2458
Michigan	2006	Senate	2	Democratic	47223
Michigan	2006	Senate	2	Republican	17105

State	Year	Chamber	District	Party	Votes
Michigan	2006	Senate	3	Democratic	58063
Michigan	2006	Senate	3	Republican	12353
Michigan	2006	Senate	4	Democratic	55163
Michigan	2006	Senate	4	Republican	2246
Michigan	2006	Senate	5	Democratic	56252
Michigan	2006	Senate	5	Republican	8164
Michigan	2006	Senate	6	Democratic	52500
Michigan	2006	Senate	6	Republican	47596
Michigan	2006	Senate	7	Democratic	56156
Michigan	2006	Senate	7	Republican	59647
Michigan	2006	Senate	8	Democratic	58501
Michigan	2006	Senate	8	Republican	21727
Michigan	2006	Senate	9	Democratic	60769
Michigan	2006	Senate	9	Republican	27294
Michigan	2006	Senate	10	Democratic	62737
Michigan	2006	Senate	10	Republican	30067
Michigan	2006	Senate	11	Democratic	42279
Michigan	2006	Senate	11	Republican	65543
Michigan	2006	Senate	12	Democratic	43069
Michigan	2006	Senate	12	Republican	59498
Michigan	2006	Senate	13	Democratic	56484
Michigan	2006	Senate	13	Republican	57204
Michigan	2006	Senate	14	Democratic	78346
Michigan	2006	Senate	14	Republican	26571
Michigan	2006	Senate	15	Democratic	45782
Michigan	2006	Senate	15	Republican	66542
Michigan	2006	Senate	16	Democratic	28126
Michigan	2006	Senate	16	Republican	52699
Michigan	2006	Senate	17	Democratic	45445
Michigan	2006	Senate	17	Republican	52113
Michigan	2006	Senate	18	Democratic	72774
Michigan	2006	Senate	18	Republican	29075
Michigan	2006	Senate	19	Democratic	50612
Michigan	2006	Senate	19	Republican	32078
Michigan	2006	Senate	20	Democratic	48353
Michigan	2006	Senate	20	Republican	51553
Michigan	2006	Senate	21	Democratic	36078
Michigan	2006	Senate	21	Republican	50182
Michigan	2006	Senate	22	Democratic	43419
Michigan	2006	Senate	22	Republican	65790
Michigan	2006	Senate	23	Democratic	64404
Michigan	2006	Senate	23	Republican	27931
Michigan	2006	Senate	24	Democratic	46480
Michigan	2006	Senate	24	Republican	64737
Michigan	2006	Senate	25	Democratic	43935
Michigan	2006	Senate	25	Republican	49272
Michigan	2006	Senate	26	Democratic	65711

State	Year	Chamber	District	Party	Votes
Michigan	2006	Senate	26	Republican	41445
Michigan	2006	Senate	27	Democratic	71402
Michigan	2006	Senate	27	Republican	22804
Michigan	2006	Senate	28	Democratic	42227
Michigan	2006	Senate	28	Republican	78234
Michigan	2006	Senate	29	Democratic	45600
Michigan	2006	Senate	29	Republican	51960
Michigan	2006	Senate	30	Democratic	31328
Michigan	2006	Senate	30	Republican	83242
Michigan	2006	Senate	31	Democratic	78923
Michigan	2006	Senate	31	Republican	23569
Michigan	2006	Senate	32	Democratic	45295
Michigan	2006	Senate	32	Republican	45727
Michigan	2006	Senate	33	Democratic	40771
Michigan	2006	Senate	33	Republican	49687
Michigan	2006	Senate	34	Democratic	50870
Michigan	2006	Senate	34	Republican	52233
Michigan	2006	Senate	35	Democratic	40836
Michigan	2006	Senate	35	Republican	62199
Michigan	2006	Senate	36	Democratic	39757
Michigan	2006	Senate	36	Republican	65079
Michigan	2006	Senate	37	Democratic	43736
Michigan	2006	Senate	37	Republican	63479
Michigan	2006	Senate	38	Democratic	66307
Michigan	2006	Senate	38	Republican	27974
Michigan	2010	Senate	1	Democratic	40122
Michigan	2010	Senate	1	Republican	2895
Michigan	2010	Senate	2	Democratic	34858
Michigan	2010	Senate	2	Republican	17459
Michigan	2010	Senate	3	Democratic	43849
Michigan	2010	Senate	3	Republican	11994
Michigan	2010	Senate	4	Democratic	42722
Michigan	2010	Senate	4	Republican	1241
Michigan	2010	Senate	5	Democratic	44055
Michigan	2010	Senate	5	Republican	8856
Michigan	2010	Senate	6	Democratic	46471
Michigan	2010	Senate	6	Republican	37001
Michigan	2010	Senate	7	Democratic	43173
Michigan	2010	Senate	7	Republican	55284
Michigan	2010	Senate	8	Democratic	37845
Michigan	2010	Senate	8	Republican	25280
Michigan	2010	Senate	9	Democratic	42039
Michigan	2010	Senate	9	Republican	33258
Michigan	2010	Senate	10	Democratic	37369
Michigan	2010	Senate	10	Republican	44486
Michigan	2010	Senate	11	Democratic	34166
Michigan	2010	Senate	11	Republican	65403

State	Year	Chamber	District	Party	Votes
Michigan	2010	Senate	12	Democratic	36526
Michigan	2010	Senate	12	Republican	56894
Michigan	2010	Senate	13	Democratic	42830
Michigan	2010	Senate	13	Republican	62324
Michigan	2010	Senate	14	Democratic	62863
Michigan	2010	Senate	14	Republican	31944
Michigan	2010	Senate	15	Democratic	39233
Michigan	2010	Senate	15	Republican	65216
Michigan	2010	Senate	16	Democratic	26181
Michigan	2010	Senate	16	Republican	47504
Michigan	2010	Senate	17	Democratic	32980
Michigan	2010	Senate	17	Republican	51657
Michigan	2010	Senate	18	Democratic	60333
Michigan	2010	Senate	18	Republican	31771
Michigan	2010	Senate	19	Democratic	26657
Michigan	2010	Senate	19	Republican	46543
Michigan	2010	Senate	20	Democratic	34507
Michigan	2010	Senate	20	Republican	47680
Michigan	2010	Senate	21	Democratic	25062
Michigan	2010	Senate	21	Republican	49818
Michigan	2010	Senate	22	Democratic	29325
Michigan	2010	Senate	22	Republican	65170
Michigan	2010	Senate	23	Democratic	49990
Michigan	2010	Senate	23	Republican	28133
Michigan	2010	Senate	24	Democratic	30052
Michigan	2010	Senate	24	Republican	64039
Michigan	2010	Senate	25	Democratic	26393
Michigan	2010	Senate	25	Republican	53342
Michigan	2010	Senate	26	Democratic	36231
Michigan	2010	Senate	26	Republican	49700
Michigan	2010	Senate	27	Democratic	51666
Michigan	2010	Senate	27	Republican	23920
Michigan	2010	Senate	28	Democratic	26276
Michigan	2010	Senate	28	Republican	74529
Michigan	2010	Senate	29	Democratic	36830
Michigan	2010	Senate	29	Republican	41042
Michigan	2010	Senate	30	Democratic	20061
Michigan	2010	Senate	30	Republican	74409
Michigan	2010	Senate	31	Democratic	36629
Michigan	2010	Senate	31	Republican	51678
Michigan	2010	Senate	32	Democratic	32692
Michigan	2010	Senate	32	Republican	43577
Michigan	2010	Senate	33	Democratic	25206
Michigan	2010	Senate	33	Republican	50222
Michigan	2010	Senate	34	Democratic	33261
Michigan	2010	Senate	34	Republican	49065
Michigan	2010	Senate	35	Democratic	30819

State	Year	Chamber	District	Party	Votes
Michigan	2010	Senate	35	Republican	56318
Michigan	2010	Senate	36	Democratic	32154
Michigan	2010	Senate	36	Republican	56634
Michigan	2010	Senate	37	Democratic	34122
Michigan	2010	Senate	37	Republican	62697
Michigan	2010	Senate	38	Democratic	39320
Michigan	2010	Senate	38	Republican	49868
Michigan	2014	Senate	1	Democratic	67005
Michigan	2014	Senate	1	Republican	23568
Michigan	2014	Senate	2	Democratic	57976
Michigan	2014	Senate	2	Republican	18573
Michigan	2014	Senate	3	Democratic	61269
Michigan	2014	Senate	3	Republican	13951
Michigan	2014	Senate	4	Democratic	73246
Michigan	2014	Senate	4	Republican	13806
Michigan	2014	Senate	5	Democratic	85990
Michigan	2014	Senate	5	Republican	16961
Michigan	2014	Senate	6	Democratic	57684
Michigan	2014	Senate	6	Republican	32433
Michigan	2014	Senate	7	Democratic	63945
Michigan	2014	Senate	7	Republican	71868
Michigan	2014	Senate	8	Democratic	46551
Michigan	2014	Senate	8	Republican	75172
Michigan	2014	Senate	9	Democratic	62095
Michigan	2014	Senate	9	Republican	30557
Michigan	2014	Senate	10	Democratic	42170
Michigan	2014	Senate	10	Republican	66894
Michigan	2014	Senate	11	Democratic	100520
Michigan	2014	Senate	11	Republican	30699
Michigan	2014	Senate	12	Democratic	48256
Michigan	2014	Senate	12	Republican	70607
Michigan	2014	Senate	13	Democratic	56764
Michigan	2014	Senate	13	Republican	83825
Michigan	2014	Senate	14	Democratic	45200
Michigan	2014	Senate	14	Republican	63525
Michigan	2014	Senate	15	Democratic	48688
Michigan	2014	Senate	15	Republican	75569
Michigan	2014	Senate	16	Democratic	33089
Michigan	2014	Senate	16	Republican	58819
Michigan	2014	Senate	17	Democratic	43994
Michigan	2014	Senate	17	Republican	51131
Michigan	2014	Senate	18	Democratic	83509
Michigan	2014	Senate	18	Republican	29616
Michigan	2014	Senate	19	Democratic	35121
Michigan	2014	Senate	19	Republican	62108
Michigan	2014	Senate	20	Democratic	46968
Michigan	2014	Senate	20	Republican	51295

State	Year	Chamber	District	Party	Votes
Michigan	2014	Senate	21	Democratic	30136
Michigan	2014	Senate	21	Republican	64546
Michigan	2014	Senate	22	Democratic	47411
Michigan	2014	Senate	22	Republican	80977
Michigan	2014	Senate	23	Democratic	66522
Michigan	2014	Senate	23	Republican	35287
Michigan	2014	Senate	24	Democratic	53713
Michigan	2014	Senate	24	Republican	73758
Michigan	2014	Senate	25	Democratic	47649
Michigan	2014	Senate	25	Republican	65759
Michigan	2014	Senate	26	Democratic	33271
Michigan	2014	Senate	26	Republican	67236
Michigan	2014	Senate	27	Democratic	68298
Michigan	2014	Senate	27	Republican	19432
Michigan	2014	Senate	28	Democratic	32066
Michigan	2014	Senate	28	Republican	80351
Michigan	2014	Senate	29	Democratic	42505
Michigan	2014	Senate	29	Republican	69095
Michigan	2014	Senate	30	Democratic	30714
Michigan	2014	Senate	30	Republican	88330
Michigan	2014	Senate	31	Democratic	49399
Michigan	2014	Senate	31	Republican	67181
Michigan	2014	Senate	32	Democratic	59755
Michigan	2014	Senate	32	Republican	67642
Michigan	2014	Senate	33	Democratic	33205
Michigan	2014	Senate	33	Republican	50087
Michigan	2014	Senate	34	Democratic	39665
Michigan	2014	Senate	34	Republican	53870
Michigan	2014	Senate	35	Democratic	46468
Michigan	2014	Senate	35	Republican	75031
Michigan	2014	Senate	36	Democratic	42871
Michigan	2014	Senate	36	Republican	75696
Michigan	2014	Senate	37	Democratic	43997
Michigan	2014	Senate	37	Republican	89400
Michigan	2014	Senate	38	Democratic	42023
Michigan	2014	Senate	38	Republican	65281

State	Code	FIP	Year	Commission	Court	Any Unified Govt	Divided Govt	Unified Dem Govt	Unified Rep Govt	Non-Unified Control
Alabama	AL	1	1970							
Alaska	AK	2	1970							
Arizona	AZ	4	1970							
Arkansas	AR	5	1970							
California	CA	6	1970							
Colorado	CO	8	1970							
Connecticut	CT	9	1970							
Delaware	DE	10	1970							
Florida	FL	12	1970							
Georgia	GA	13	1970							
Hawaii	HI	15	1970							
Idaho	ID	16	1970							
Illinois	IL	17	1970							
Indiana	IN	18	1970							
Iowa	IA	19	1970							
Kansas	KS	20	1970							
Kentucky	KY	21	1970							
Louisiana	LA	22	1970							
Maine	ME	23	1970							
Maryland	MD	24	1970							
Massachusetts	MA	25	1970							
Michigan	MI	26	1970							
Minnesota	MN	27	1970							
Mississippi	MS	28	1970							
Missouri	MO	29	1970							
Montana	MT	30	1970							
Nebraska	NE	31	1970							
Nevada	NV	32	1970							
New Hampshire	NH	33	1970							
New Jersey	NJ	34	1970							
New Mexico	NM	35	1970							
New York	NY	36	1970							
North Carolina	NC	37	1970							

State	Code	FIP	Year	Commission	Court	Any Unified Govt	Divided Govt	Unified Dem Govt	Unified Rep Govt	Non-Unified Control
North Dakota	ND	38	1970							
Ohio	OH	39	1970							
Oklahoma	OK	40	1970							
Oregon	OR	41	1970							
Pennsylvania	PA	42	1970							
Rhode Island	RI	44	1970							
South Carolina	SC	45	1970							
South Dakota	SD	46	1970							
Tennessee	TN	47	1970							
Texas	TX	48	1970							
Utah	UT	49	1970							
Vermont	VT	50	1970							
Virginia	VA	51	1970							
Washington	WA	53	1970							
West Virginia	WV	54	1970							
Wisconsin	WI	55	1970							
Wyoming	WY	56	1970							
Alaska	AK	2	1972							
Arizona	AZ	4	1972							
Arkansas	AR	5	1972							
California	CA	6	1972	0	0	1	0	1	0	0
Colorado	CO	8	1972	0	0	1	0	0	1	0
Connecticut	CT	9	1972	1	0	0	0	0	0	1
Delaware	DE	10	1972	0	0	1	0	0	1	0
Florida	FL	12	1972	0	0	1	0	1	0	0
Georgia	GA	13	1972	0	0	1	0	1	0	0
Hawaii	HI	15	1972							
Idaho	ID	16	1972							
Illinois	IL	17	1972							
Indiana	IN	18	1972	0	0	1	0	0	1	0
Iowa	IA	19	1972	0	1	0	0	0	0	1
Kansas	KS	20	1972	0	0	0	1	0	0	1
Kentucky	KY	21	1972							

State	Code	FIP	Year	Commission	Court	Any Unified Govt	Divided Govt	Unified Dem Govt	Unified Rep Govt	Non-Unified Control
Louisiana	LA	22	1972							
Maine	ME	23	1972							
Massachusetts	MA	25	1972	0	0	1	0	1	0	0
Michigan	MI	26	1972	0	1	0	0	0	0	1
Minnesota	MN	27	1972							
Mississippi	MS	28	1972	0	0	1	0	1	0	0
Missouri	MO	29	1972	0	1	0	0	0	0	1
Montana	MT	30	1972							
Nebraska	NE	31	1972							
Nevada	NV	32	1972	0	0	0	1	0	0	1
New Hampshire	NH	33	1972							
New Jersey	NJ	34	1972							
New Mexico	NM	35	1972	0	0	1	0	1	0	0
New York	NY	36	1972	0	0	1	0	0	1	0
North Carolina	NC	37	1972							
North Dakota	ND	38	1972							
Ohio	OH	39	1972	0	0	0	1	0	0	1
Oklahoma	OK	40	1972	0	0	1	0	1	0	0
Oregon	OR	41	1972	0	0	0	1	0	0	1
Pennsylvania	PA	42	1972	1	0	0	0	0	0	1
Rhode Island	RI	44	1972	0	0	1	0	1	0	0
South Carolina	SC	45	1972	0	0	1	0	1	0	0
South Dakota	SD	46	1972							
Tennessee	TN	47	1972	0	1	0	0	0	0	1
Texas	TX	48	1972	0	0	1	0	1	0	0
Utah	UT	49	1972	0	0	0	1	0	0	1
Vermont	VT	50	1972							
Virginia	VA	52	1972							
Washington	WA	53	1972	0	1	0	0	0	0	1
West Virginia	WV	54	1972							
Wisconsin	WI	55	1972	0	0	0	1	0	0	1
Wyoming	WY	56	1972							
Alabama	AL	1	1974	0	1	0	0	0	0	1

State	Code	FIP	Year	Commission	Court	Any Unified Govt	Divided Govt	Unified Dem Govt	Unified Rep Govt	Non-Unified Control
Alaska	AK	2	1974							
Arizona	AZ	4	1974							
Arkansas	AR	5	1974							
California	CA	6	1974	0	1	0	0	0	0	1
Colorado	CO	8	1974	0	0	1	0	0	1	0
Connecticut	CT	9	1974	1	0	0	0	0	0	1
Delaware	DE	10	1974	0	0	1	0	0	1	0
Florida	FL	12	1974	0	0	1	0	1	0	0
Georgia	GA	13	1974	0	0	1	0	1	0	0
Hawaii	HI	15	1974							
Idaho	ID	16	1974							
Illinois	IL	17	1974							
Indiana	IN	18	1974	0	0	1	0	0	1	0
Iowa	IA	19	1974	0	1	0	0	0	0	1
Kansas	KS	20	1974	0	0	0	1	0	0	1
Kentucky	KY	21	1974							
Maine	ME	23	1974	0	1	0	0	0	0	1
Maryland	MD	24	1974							
Massachusetts	MA	25	1974	0	0	1	0	1	0	0
Michigan	MI	26	1974	0	1	0	0	0	0	1
Minnesota	MN	27	1974	0	1	0	0	0	0	1
Missouri	MO	29	1974	0	1	0	0	0	0	1
Montana	MT	30	1974	1	0	0	0	0	0	1
Nebraska	NE	31	1974							
Nevada	NV	32	1974	0	0	0	1	0	0	1
New Hampshire	NH	33	1974							
New Jersey	NJ	34	1974							
New Mexico	NM	35	1974	0	0	1	0	1	0	0
New York	NY	36	1974	0	0	1	0	0	1	0
North Carolina	NC	37	1974							
North Dakota	ND	38	1974							
Ohio	OH	39	1974	0	0	0	1	0	0	1
Oklahoma	OK	40	1974	0	0	1	0	1	0	0

State	Code	FIP	Year	Commission	Court	Any Unified Govt	Divided Govt	Unified Dem Govt	Unified Rep Govt	Non-Unified Control
Oregon	OR	41	1974	0	0	0	1	0	0	1
Pennsylvania	PA	42	1974	1	0	0	0	0	0	1
Rhode Island	RI	44	1974	0	0	1	0	1	0	0
South Carolina	SC	45	1974	0	0	1	0	1	0	0
South Dakota	SD	46	1974							
Tennessee	TN	47	1974	0	0	1	1	0	0	1
Texas	TX	48	1974	0	0	1	0	1	0	0
Utah	UT	49	1974	0	0	0	1	0	0	1
Vermont	VT	50	1974							
Virginia	VA	51	1974							
Washington	WA	53	1974	0	1	0	0	0	0	1
West Virginia	WV	54	1974	0	0	1	0	1	0	0
Wisconsin	WI	55	1974	0	0	0	1	0	0	1
Wyoming	WY	56	1974							
Alaska	AK	2	1976							
Arizona	AZ	4	1976							
Arkansas	AR	5	1976							
California	CA	6	1976	0	1	0	0	0	0	1
Colorado	CO	8	1976	0	0	1	0	0	1	0
Connecticut	CT	9	1976	1	0	0	0	0	0	1
Delaware	DE	10	1976	0	0	1	0	0	1	0
Florida	FL	12	1976	0	0	1	0	1	0	0
Georgia	GA	13	1976	0	0	1	0	1	0	0
Hawaii	HI	15	1976							
Idaho	ID	16	1976	0	0	0	1	0	0	1
Illinois	IL	17	1976							
Indiana	IN	18	1976	0	0	1	0	0	1	0
Iowa	IA	19	1976	0	1	0	0	0	0	1
Kansas	KS	20	1976	0	0	0	1	0	0	1
Kentucky	KY	21	1976							
Louisiana	LA	22	1976							
Maine	ME	23	1976	0	1	0	0	0	0	1
Massachusetts	MA	25	1976	0	0	1	0	1	0	0

State	Code	FIP	Year	Commission	Court	Any Unified Govt	Divided Govt	Unified Dem Govt	Unified Rep Govt	Non-Unified Control
Michigan	MI	26	1976	0	1	0	0	0	0	1
Minnesota	MN	27	1976	0	1	0	0	0	0	1
Mississippi	MS	28	1976	0	0	1	0	1	0	0
Missouri	MO	29	1976	0	1	0	0	0	0	1
Montana	MT	30	1976	1	0	0	0	0	0	1
Nebraska	NE	31	1976							
Nevada	NV	32	1976	0	0	0	1	0	0	1
New Hampshire	NH	33	1976							
New Jersey	NJ	34	1976							
New Mexico	NM	35	1976	0	0	1	0	1	0	0
New York	NY	36	1976	0	0	1	0	0	1	0
North Carolina	NC	37	1976							
North Dakota	ND	38	1976							
Ohio	OH	39	1976	0	0	0	1	0	0	1
Oklahoma	OK	40	1976	0	0	1	0	1	0	0
Oregon	OR	41	1976	0	0	0	1	0	0	1
Pennsylvania	PA	42	1976	1	0	0	0	0	0	1
Rhode Island	RI	44	1976	0	0	1	0	1	0	0
South Carolina	SC	45	1976	0	0	1	0	1	0	0
South Dakota	SD	46	1976							
Tennessee	TN	47	1976	0	0	1	1	0	0	1
Texas	TX	48	1976	0	0	1	0	1	0	0
Utah	UT	49	1976	0	0	0	1	0	0	1
Vermont	VT	50	1976							
Virginia	VA	51	1976							
Washington	WA	53	1976	0	1	0	0	0	0	1
West Virginia	WV	54	1976	0	0	1	0	1	0	0
Wisconsin	WI	55	1976	0	0	0	1	0	0	1
Wyoming	WY	56	1976							
Alabama	AL	1	1978	0	1	0	0	0	0	1
Alaska	AK	2	1978							
Arizona	AZ	4	1978							
Arkansas	AR	5	1978							

State	Code	FIP	Year	Commission	Court	Any Unified Govt	Divided Govt	Unified Dem Govt	Unified Rep Govt	Non-Unified Control
California	CA	6	1978	0	1	0	0	0	0	1
Colorado	CO	8	1978	0	0	1	0	0	1	0
Connecticut	CT	9	1978	1	0	0	0	0	0	1
Delaware	DE	10	1978	0	0	1	0	0	1	0
Florida	FL	12	1978	0	0	1	0	1	0	0
Georgia	GA	13	1978	0	0	1	0	1	0	0
Hawaii	HI	15	1978							
Idaho	ID	16	1978	0	0	0	1	0	0	1
Illinois	IL	17	1978							
Indiana	IN	18	1978	0	0	1	0	0	1	0
Iowa	IA	19	1978	0	1	0	0	0	0	1
Kansas	KS	20	1978	0	0	0	1	0	0	1
Kentucky	KY	21	1978							
Maine	ME	23	1978	0	1	0	0	0	0	1
Maryland	MD	24	1978							
Massachusetts	MA	25	1978	0	0	1	0	1	0	0
Michigan	MI	26	1978	0	1	0	0	0	0	1
Minnesota	MN	27	1978	0	1	0	0	0	0	1
Missouri	MO	29	1978	0	1	0	0	0	0	1
Montana	MT	30	1978	1	0	0	0	0	0	1
Nebraska	NE	31	1978							
Nevada	NV	32	1978	0	0	0	1	0	0	1
New Hampshire	NH	33	1978							
New Jersey	NJ	34	1978							
New Mexico	NM	35	1978	0	0	1	0	1	0	0
New York	NY	36	1978	0	0	1	0	0	1	0
North Carolina	NC	37	1978							
North Dakota	ND	38	1978							
Ohio	OH	39	1978	0	0	0	1	0	0	1
Oklahoma	OK	40	1978	0	0	1	0	1	0	0
Oregon	OR	41	1978	0	0	0	1	0	0	1
Pennsylvania	PA	42	1978	1	0	0	0	0	0	1
Rhode Island	RI	44	1978	0	0	1	0	1	0	0

State	Code	FIP	Year	Commission	Court	Any Unified Govt	Divided Govt	Unified Dem Govt	Unified Rep Govt	Non-Unified Control
South Carolina	SC	45	1978	0	0	1	0	1	0	0
South Dakota	SD	46	1978							
Tennessee	TN	47	1978	0	0	1	1	0	0	1
Texas	TX	48	1978	0	0	1	0	1	0	0
Utah	UT	49	1978	0	0	0	1	0	0	1
Vermont	VT	50	1978							
Virginia	VA	51	1978							
Washington	WA	53	1978	0	1	0	0	0	0	1
West Virginia	WV	54	1978	0	0	1	0	1	0	0
Wisconsin	WI	55	1978	0	0	0	1	0	0	1
Wyoming	WY	56	1978							
Alaska	AK	2	1980							
Arizona	AZ	4	1980							
Arkansas	AR	5	1980							
California	CA	6	1980	0	1	0	0	0	0	1
Colorado	CO	8	1980	0	0	1	0	0	1	0
Connecticut	CT	9	1980	1	0	0	0	0	0	1
Delaware	DE	10	1980	0	0	1	0	0	1	0
Florida	FL	12	1980	0	0	1	0	1	0	0
Georgia	GA	13	1980	0	0	1	0	1	0	0
Hawaii	HI	15	1980							
Idaho	ID	16	1980	0	0	0	1	0	0	1
Illinois	IL	17	1980							
Indiana	IN	18	1980	0	0	1	0	0	1	0
Iowa	IA	19	1980	0	1	0	0	0	0	1
Kansas	KS	20	1980	0	0	0	1	0	0	1
Kentucky	KY	21	1980							
Louisiana	LA	22	1980							
Maine	ME	23	1980	0	1	0	0	0	0	1
Massachusetts	MA	25	1980	0	0	1	0	1	0	0
Michigan	MI	26	1980	0	1	0	0	0	0	1
Minnesota	MN	27	1980	0	1	0	0	0	0	1
Mississippi	MS	28	1980	0	0	1	0	1	0	0

State	Code	FIP	Year	Commission	Court	Any Unified Govt	Divided Govt	Unified Dem Govt	Unified Rep Govt	Non-Unified Control
Missouri	MO	29	1980	0	1	0	0	0	0	1
Montana	MT	30	1980	1	0	0	0	0	0	1
Nebraska	NE	31	1980							
Nevada	NV	32	1980	0	0	0	1	0	0	1
New Hampshire	NH	33	1980							
New Jersey	NJ	34	1980							
New Mexico	NM	35	1980	0	0	1	0	1	0	0
New York	NY	36	1980	0	0	1	0	0	1	0
North Carolina	NC	37	1980							
North Dakota	ND	38	1980							
Ohio	OH	39	1980	0	0	0	1	0	0	1
Oklahoma	OK	40	1980	0	0	1	0	1	0	0
Oregon	OR	41	1980	0	0	0	1	0	0	1
Pennsylvania	PA	42	1980	1	0	0	0	0	0	1
Rhode Island	RI	44	1980	0	0	1	0	1	0	0
South Carolina	SC	45	1980	0	0	1	0	1	0	0
South Dakota	SD	46	1980							
Tennessee	TN	47	1980	0	0	1	1	0	0	1
Texas	TX	48	1980	0	0	1	0	1	0	0
Utah	UT	49	1980	0	0	0	1	0	0	1
Vermont	VT	50	1980							
Virginia	VA	51	1980							
Washington	WA	53	1980	0	1	0	0	0	0	1
West Virginia	WV	54	1980	0	0	1	0	1	0	0
Wisconsin	WI	55	1980	0	0	0	1	0	0	1
Wyoming	WY	56	1980							
Alabama	AL	1	1982	0	0	1	0	1	0	0
Alaska	AK	2	1982	0	0	0	1	0	0	1
Arizona	AZ	4	1982							
Arkansas	AR	5	1982	0	0	1	0	0	1	0
California	CA	6	1982	0	0	1	0	1	0	0
Colorado	CO	8	1982	1	0	0	0	0	0	1
Connecticut	CT	9	1982	0	0	1	0	1	0	0

State	Code	FIP	Year	Commission	Court	Any Unified Govt	Divided Govt	Unified Dem Govt	Unified Rep Govt	Non-Unified Control
Delaware	DE	10	1982	0	0	0	1	0	0	1
Florida	FL	12	1982	0	0	1	0	1	0	0
Georgia	GA	13	1982	0	0	1	0	1	0	0
Hawaii	HI	15	1982	1	0	0	0	0	0	1
Idaho	ID	16	1982	0	0	0	1	0	0	1
Illinois	IL	17	1982	0	0	1	0	1	0	0
Indiana	IN	18	1982	0	0	1	0	0	1	0
Iowa	IA	19	1982	1	0	0	0	0	0	1
Kansas	KS	20	1982	0	0	0	1	0	0	1
Maine	ME	23	1982	0	0	0	1	0	0	1
Maryland	MD	24	1982							
Massachusetts	MA	25	1982	0	0	1	0	1	0	0
Michigan	MI	26	1982	0	1	0	0	0	0	1
Minnesota	MN	27	1982	0	1	0	0	0	0	1
Missouri	MO	29	1982	1	0	0	0	0	0	1
Montana	MT	30	1982	1	0	0	0	0	0	1
Nebraska	NE	31	1982							
Nevada	NV	32	1982	0	0	0	1	0	0	1
New Hampshire	NH	33	1982							
New Jersey	NJ	34	1982							
New Mexico	NM	35	1982	0	0	1	0	1	0	0
New York	NY	36	1982	0	0	0	1	0	0	1
North Carolina	NC	37	1982							
North Dakota	ND	38	1982							
Ohio	OH	39	1982	0	0	0	1	0	0	1
Oklahoma	OK	40	1982	0	0	1	0	1	0	0
Oregon	OR	41	1982	0	0	0	1	0	0	1
Pennsylvania	PA	42	1982	1	0	0	0	0	0	1
Rhode Island	RI	44	1982	0	0	1	0	1	0	0
South Carolina	SC	45	1982	0	0	1	0	1	0	0
South Dakota	SD	46	1982							
Tennessee	TN	47	1982	0	0	1	0	1	0	0
Texas	TX	48	1982	0	1	0	0	0	0	1

State	Code	FIP	Year	Commission	Court	Any Unified Govt	Divided Govt	Unified Dem Govt	Unified Rep Govt	Non-Unified Control
Utah	UT	49	1982	0	0	1	0	0	1	0
Vermont	VT	50	1982							
Virginia	VA	51	1982							
Washington	WA	53	1982	0	0	1	0	0	1	0
West Virginia	WV	54	1982	0	0	1	0	1	0	0
Wisconsin	WI	55	1982	0	1	0	0	0	0	1
Wyoming	WY	56	1982							
Alabama	AL	1	1984	0	0	1	0	1	0	0
Alaska	AK	2	1984	0	0	0	1	0	0	1
Arizona	AZ	4	1984							
Arkansas	AR	5	1984	0	0	1	0	0	1	0
California	CA	6	1984	0	0	1	0	1	0	0
Colorado	CO	8	1984	1	0	0	0	0	0	1
Connecticut	CT	9	1984	0	0	1	0	1	0	0
Delaware	DE	10	1984	0	0	0	1	0	0	1
Florida	FL	12	1984	0	0	1	0	1	0	0
Georgia	GA	13	1984	0	0	1	0	1	0	0
Hawaii	HI	15	1984	1	0	0	0	0	0	1
Idaho	ID	16	1984	0	1	0	0	0	0	1
Illinois	IL	17	1984	0	0	1	0	1	0	0
Indiana	IN	18	1984	0	0	1	0	0	1	0
Iowa	IA	19	1984	1	0	0	0	0	0	1
Kansas	KS	20	1984	0	0	0	1	0	0	1
Kentucky	KY	21	1984	0	0	1	0	1	0	0
Louisiana	LA	22	1984							
Maine	ME	23	1984	0	0	0	1	0	0	1
Massachusetts	MA	25	1984	0	0	1	0	1	0	0
Michigan	MI	26	1984	0	1	0	0	0	0	1
Minnesota	MN	27	1984	0	1	0	0	0	0	1
Mississippi	MS	28	1984	0	0	1	0	1	0	0
Missouri	MO	29	1984	1	0	0	0	0	0	1
Montana	MT	30	1984	1	0	0	0	0	0	1
Nebraska	NE	31	1984							

State	Code	FIP	Year	Commission	Court	Any Unified Govt	Divided Govt	Unified Dem Govt	Unified Rep Govt	Non-Unified Control
Nevada	NV	32	1984	0	0	0	1	0	0	1
New Hampshire	NH	33	1984							
New Jersey	NJ	34	1984							
New Mexico	NM	35	1984	0	0	1	0	1	0	0
New York	NY	36	1984	0	0	0	1	0	0	1
North Carolina	NC	37	1984							
North Dakota	ND	38	1984							
Ohio	OH	39	1984	0	0	0	1	0	0	1
Oklahoma	OK	40	1984	0	0	1	0	1	0	0
Oregon	OR	41	1984	0	0	0	1	0	0	1
Pennsylvania	PA	42	1984	1	0	0	0	0	0	1
Rhode Island	RI	44	1984	0	0	1	0	1	0	0
South Carolina	SC	45	1984	0	0	1	0	1	0	0
South Dakota	SD	46	1984							
Tennessee	TN	47	1984	0	0	1	0	1	0	0
Texas	TX	48	1984	0	0	1	0	1	0	0
Utah	UT	49	1984	0	0	1	0	0	1	0
Vermont	VT	50	1984							
Virginia	VA	51	1984							
Washington	WA	53	1984	0	0	1	0	0	1	0
West Virginia	WV	54	1984	0	0	1	0	1	0	0
Wisconsin	WI	55	1984	0	0	0	1	0	0	1
Wyoming	WY	56	1984							
Alabama	AL	1	1986	0	0	1	0	1	0	0
Alaska	AK	2	1986	0	0	0	1	0	0	1
Arizona	AZ	4	1986							
Arkansas	AR	5	1986	0	0	1	0	0	1	0
California	CA	6	1986	0	0	1	0	1	0	0
Colorado	CO	8	1986	1	0	0	0	0	0	1
Connecticut	CT	9	1986	0	0	1	0	1	0	0
Delaware	DE	10	1986	0	0	0	1	0	0	1
Florida	FL	12	1986	0	0	1	0	1	0	0
Georgia	GA	13	1986	0	0	1	0	1	0	0

State	Code	FIP	Year	Commission	Court	Any Unified Govt	Divided Govt	Unified Dem Govt	Unified Rep Govt	Non-Unified Control
Hawaii	HI	15	1986	1	0	0	0	0	0	1
Idaho	ID	16	1986	0	1	0	0	0	0	1
Illinois	IL	17	1986	0	0	1	0	1	0	0
Indiana	IN	18	1986	0	0	1	0	0	1	0
Iowa	IA	19	1986	1	0	0	0	0	0	1
Kansas	KS	20	1986	0	0	0	1	0	0	1
Kentucky	KY	21	1986	0	0	1	0	1	0	0
Maine	ME	23	1986	0	0	0	1	0	0	1
Maryland	MD	24	1986							
Massachusetts	MA	25	1986	0	0	1	0	1	0	0
Michigan	MI	26	1986	0	1	0	0	0	0	1
Minnesota	MN	27	1986	0	1	0	0	0	0	1
Missouri	MO	29	1986	1	0	0	0	0	0	1
Montana	MT	30	1986	1	0	0	0	0	0	1
Nebraska	NE	31	1986							
Nevada	NV	32	1986	0	0	0	1	0	0	1
New Hampshire	NH	33	1986							
New Jersey	NJ	34	1986							
New Mexico	NM	35	1986	0	0	1	0	1	0	0
New York	NY	36	1986	0	0	0	1	0	0	1
North Carolina	NC	37	1986							
North Dakota	ND	38	1986							
Ohio	OH	39	1986	0	0	0	1	0	0	1
Oklahoma	OK	40	1986	0	0	1	0	1	0	0
Oregon	OR	41	1986	0	0	0	1	0	0	1
Pennsylvania	PA	42	1986	1	0	0	0	0	0	1
Rhode Island	RI	44	1986	0	0	1	0	1	0	0
South Carolina	SC	45	1986	0	0	1	0	1	0	0
South Dakota	SD	46	1986							
Tennessee	TN	47	1986	0	0	1	0	1	0	0
Texas	TX	48	1986	0	0	1	0	1	0	0
Utah	UT	49	1986	0	0	1	0	0	1	0
Vermont	VT	50	1986	0	0	1	0	0	1	0

State	Code	FIP	Year	Commission	Court	Any Unified Govt	Divided Govt	Unified Dem Govt	Unified Rep Govt	Non-Unified Control
Virginia	VA	51	1986							
Washington	WA	53	1986	0	0	1	0	0	1	0
West Virginia	WV	54	1986	0	0	1	0	1	0	0
Wisconsin	WI	55	1986	0	0	0	1	0	0	1
Wyoming	WY	56	1986							
Alaska	AK	2	1988	0	0	0	1	0	0	1
Arizona	AZ	4	1988							
Arkansas	AR	5	1988	0	0	1	0	0	1	0
California	CA	6	1988	0	0	1	0	1	0	0
Colorado	CO	8	1988	1	0	0	0	0	0	1
Connecticut	CT	9	1988	0	0	1	0	1	0	0
Delaware	DE	10	1988	0	0	0	1	0	0	1
Florida	FL	12	1988	0	0	1	0	1	0	0
Georgia	GA	13	1988	0	0	1	0	1	0	0
Hawaii	HI	15	1988	1	0	0	0	0	0	1
Idaho	ID	16	1988	0	1	0	0	0	0	1
Illinois	IL	17	1988	0	0	1	0	1	0	0
Indiana	IN	18	1988	0	0	1	0	0	1	0
Iowa	IA	19	1988	1	0	0	0	0	0	1
Kansas	KS	20	1988	0	0	0	1	0	0	1
Kentucky	KY	21	1988	0	0	1	0	1	0	0
Louisiana	LA	22	1988							
Maine	ME	23	1988	0	0	0	1	0	0	1
Massachusetts	MA	25	1988	0	0	1	0	1	0	0
Michigan	MI	26	1988	0	1	0	0	0	0	1
Minnesota	MN	27	1988	0	1	0	0	0	0	1
Mississippi	MS	28	1988	0	0	1	0	1	0	0
Missouri	MO	29	1988	1	0	0	0	0	0	1
Montana	MT	30	1988	1	0	0	0	0	0	1
Nebraska	NE	31	1988							
Nevada	NV	32	1988	0	0	0	1	0	0	1
New Hampshire	NH	33	1988							
New Jersey	NJ	34	1988							

State	Code	FIP	Year	Commission	Court	Any Unified Govt	Divided Govt	Unified Dem Govt	Unified Rep Govt	Non-Unified Control
New Mexico	NM	35	1988	0	0	1	0	1	0	0
New York	NY	36	1988	0	0	0	1	0	0	1
North Carolina	NC	37	1988							
North Dakota	ND	38	1988							
Ohio	OH	39	1988	0	0	0	1	0	0	1
Oklahoma	OK	40	1988	0	0	1	0	1	0	0
Oregon	OR	41	1988	0	0	0	1	0	0	1
Pennsylvania	PA	42	1988	1	0	0	0	0	0	1
Rhode Island	RI	44	1988	0	0	1	0	1	0	0
South Carolina	SC	45	1988	0	0	1	0	1	0	0
South Dakota	SD	46	1988							
Tennessee	TN	47	1988	0	0	1	0	1	0	0
Texas	TX	48	1988	0	0	1	0	1	0	0
Utah	UT	49	1988	0	0	1	0	0	1	0
Vermont	VT	50	1988	0	0	1	0	0	1	0
Virginia	VA	51	1988							
Washington	WA	53	1988	0	0	1	0	0	1	0
West Virginia	WV	54	1988	0	0	1	0	1	0	0
Wisconsin	WI	55	1988	0	0	0	1	0	0	1
Wyoming	WY	56	1988							
Alabama	AL	1	1990	0	0	1	0	1	0	0
Alaska	AK	2	1990	0	0	0	1	0	0	1
Arizona	AZ	4	1990							
Arkansas	AR	5	1990	0	0	1	0	0	1	0
California	CA	6	1990	0	0	1	0	1	0	0
Colorado	CO	8	1990	1	0	0	0	0	0	1
Connecticut	CT	9	1990	0	0	1	0	1	0	0
Delaware	DE	10	1990	0	0	0	1	0	0	1
Florida	FL	12	1990	0	0	1	0	1	0	0
Georgia	GA	13	1990	0	0	1	0	1	0	0
Hawaii	HI	15	1990	1	0	0	0	0	0	1
Idaho	ID	16	1990	0	1	0	0	0	0	1
Illinois	IL	17	1990	0	0	1	0	1	0	0

State	Code	FIP	Year	Commission	Court	Any Unified Govt	Divided Govt	Unified Dem Govt	Unified Rep Govt	Non-Unified Control
Indiana	IN	18	1990	0	0	1	0	0	1	0
Iowa	IA	19	1990	1	0	0	0	0	0	1
Kansas	KS	20	1990	0	0	0	1	0	0	1
Kentucky	KY	21	1990	0	0	1	0	1	0	0
Maine	ME	23	1990	0	0	0	1	0	0	1
Maryland	MD	24	1990							
Massachusetts	MA	25	1990	0	0	1	0	1	0	0
Michigan	MI	26	1990	0	1	0	0	0	0	1
Minnesota	MN	27	1990	0	1	0	0	0	0	1
Missouri	MO	29	1990	1	0	0	0	0	0	1
Montana	MT	30	1990	1	0	0	0	0	0	1
Nebraska	NE	31	1990							
Nevada	NV	32	1990	0	0	0	1	0	0	1
New Hampshire	NH	33	1990							
New Jersey	NJ	34	1990							
New Mexico	NM	35	1990	0	0	1	0	1	0	0
New York	NY	36	1990	0	0	0	1	0	0	1
North Carolina	NC	37	1990							
North Dakota	ND	38	1990							
Ohio	OH	39	1990	0	0	0	1	0	0	1
Oklahoma	OK	40	1990	0	0	1	0	1	0	0
Oregon	OR	41	1990	0	0	0	1	0	0	1
Pennsylvania	PA	42	1990	1	0	0	0	0	0	1
Rhode Island	RI	44	1990	0	0	1	0	1	0	0
South Carolina	SC	45	1990	0	0	1	0	1	0	0
South Dakota	SD	46	1990							
Tennessee	TN	47	1990	0	0	1	0	1	0	0
Texas	TX	48	1990	0	0	1	0	1	0	0
Utah	UT	49	1990	0	0	1	0	0	1	0
Vermont	VT	50	1990	0	0	1	0	0	1	0
Virginia	VA	51	1990							
Washington	WA	53	1990	0	0	1	0	0	1	0
West Virginia	WV	54	1990	0	0	1	0	1	0	0

State	Code	FIP	Year	Commission	Court	Any Unified Govt	Divided Govt	Unified Dem Govt	Unified Rep Govt	Non-Unified Control
Wisconsin	WI	55	1990	0	0	0	1	0	0	1
Wyoming	WY	56	1990							
Alaska	AK	2	1992	0	1	0	0	0	0	1
Arizona	AZ	4	1992	0	0	0	1	0	0	1
Arkansas	AR	5	1992	0	0	1	0	1	0	0
California	CA	6	1992	0	1	0	0	0	0	1
Colorado	CO	8	1992	1	0	0	0	0	0	1
Connecticut	CT	9	1992	1	0	0	0	0	0	1
Delaware	DE	10	1992	0	0	0	1	0	0	1
Florida	FL	12	1992	0	0	1	0	1	0	0
Georgia	GA	13	1992	0	0	1	0	1	0	0
Hawaii	HI	15	1992	1	0	0	0	0	0	1
Idaho	ID	16	1992	0	0	0	1	0	0	1
Illinois	IL	17	1992	0	0	1	0	0	1	0
Indiana	IN	18	1992	0	0	0	1	0	0	1
Iowa	IA	19	1992	1	0	0	0	0	0	1
Kansas	KS	20	1992	0	0	0	1	0	0	1
Kentucky	KY	21	1992	0	0	1	0	1	0	0
Louisiana	LA	22	1992	0	0	1	0	1	0	0
Maine	ME	23	1992	0	1	0	0	0	0	1
Massachusetts	MA	25	1992	0	0	0	1	0	0	1
Michigan	MI	26	1992	0	1	0	0	0	0	1
Minnesota	MN	27	1992	0	1	0	0	0	0	1
Mississippi	MS	28	1992	0	0	1	0	1	0	0
Missouri	MO	29	1992	1	0	0	0	0	0	1
Montana	MT	30	1992	1	0	0	0	0	0	1
Nebraska	NE	31	1992							
Nevada	NV	32	1992	0	0	1	0	1	0	0
New Hampshire	NH	33	1992	0	0	1	0	0	1	0
New Jersey	NJ	34	1992	1	0	0	0	0	0	1
New Mexico	NM	35	1992	0	0	1	0	1	0	0
New York	NY	36	1992	0	0	0	1	0	0	1
North Carolina	NC	37	1992	0	0	1	0	1	0	0

State	Code	FIP	Year	Commission	Court	Any Unified Govt	Divided Govt	Unified Dem Govt	Unified Rep Govt	Non-Unified Control
North Dakota	ND	38	1992	0	0	0	1	0	0	1
Ohio	OH	39	1992	0	0	0	1	0	0	1
Oklahoma	OK	40	1992	0	0	1	0	1	0	0
Oregon	OR	41	1992	0	0	0	1	0	0	1
Pennsylvania	PA	42	1992	1	0	0	0	0	0	1
Rhode Island	RI	44	1992	0	0	1	0	1	0	0
South Carolina	SC	45	1992	0	1	0	0	0	0	1
South Dakota	SD	46	1992	0	0	1	0	0	1	0
Tennessee	TN	47	1992	0	0	1	0	1	0	0
Texas	TX	48	1992	0	0	1	0	1	0	0
Utah	UT	49	1992	0	0	1	0	0	1	0
Vermont	VT	50	1992	0	0	0	1	0	0	1
Virginia	VA	51	1992	0	0	1	0	1	0	0
Washington	WA	53	1992	1	0	0	0	0	0	1
West Virginia	WV	54	1992	0	0	1	0	1	0	0
Wisconsin	WI	55	1992	0	1	0	0	0	0	1
Wyoming	WY	56	1992	0	0	0	1	0	0	1
Alabama	AL	1	1994	0	1	0	0	0	0	1
Alaska	AK	2	1994	0	1	0	0	0	0	1
Arizona	AZ	4	1994	0	0	0	1	0	0	1
Arkansas	AR	5	1994	0	0	1	0	1	0	0
California	CA	6	1994	0	1	0	0	0	0	1
Colorado	CO	8	1994	1	0	0	0	0	0	1
Connecticut	CT	9	1994	1	0	0	0	0	0	1
Delaware	DE	10	1994	0	0	0	1	0	0	1
Florida	FL	12	1994	0	0	1	0	1	0	0
Georgia	GA	13	1994	0	0	1	0	1	0	0
Hawaii	HI	15	1994	1	0	0	0	0	0	1
Idaho	ID	16	1994	0	0	0	1	0	0	1
Illinois	IL	17	1994	0	0	1	0	0	1	0
Indiana	IN	18	1994	0	0	0	1	0	0	1
Iowa	IA	19	1994	1	0	0	0	0	0	1
Kansas	KS	20	1994	0	0	0	1	0	0	1

State	Code	FIP	Year	Commission	Court	Any Unified Govt	Divided Govt	Unified Dem Govt	Unified Rep Govt	Non-Unified Control
Kentucky	KY	21	1994	0	0	1	0	1	0	0
Maine	ME	23	1994	0	1	0	0	0	0	1
Maryland	MD	24	1994	0	0	1	0	1	0	0
Massachusetts	MA	25	1994	0	0	0	1	0	0	1
Michigan	MI	26	1994	0	1	0	0	0	0	1
Minnesota	MN	27	1994	0	1	0	0	0	0	1
Missouri	MO	29	1994	1	0	0	0	0	0	1
Montana	MT	30	1994	1	0	0	0	0	0	1
Nebraska	NE	31	1994							
Nevada	NV	32	1994	0	0	1	0	1	0	0
New Hampshire	NH	33	1994	0	0	1	0	0	1	0
New Jersey	NJ	34	1994	1	0	0	0	0	0	1
New Mexico	NM	35	1994	0	0	1	0	1	0	0
New York	NY	36	1994	0	0	0	1	0	0	1
North Carolina	NC	37	1994	0	0	1	0	1	0	0
North Dakota	ND	38	1994	0	0	0	1	0	0	1
Ohio	OH	39	1994	0	0	0	1	0	0	1
Oklahoma	OK	40	1994	0	0	1	0	1	0	0
Oregon	OR	41	1994	0	0	0	1	0	0	1
Pennsylvania	PA	42	1994	1	0	0	0	0	0	1
Rhode Island	RI	44	1994	0	0	1	0	1	0	0
South Carolina	SC	45	1994	0	1	0	0	0	0	1
South Dakota	SD	46	1994	0	0	1	0	0	1	0
Tennessee	TN	47	1994	0	0	1	0	1	0	0
Texas	TX	48	1994	0	0	1	0	1	0	0
Utah	UT	49	1994	0	0	1	0	0	1	0
Vermont	VT	50	1994	0	0	0	1	0	0	1
Virginia	VA	51	1994	0	0	1	0	1	0	0
Washington	WA	53	1994	1	0	0	0	0	0	1
West Virginia	WV	54	1994	0	0	1	0	1	0	0
Wisconsin	WI	55	1994	0	1	0	0	0	0	1
Wyoming	WY	56	1994	0	0	0	1	0	0	1
Alaska	AK	2	1996	0	1	0	0	0	0	1

State	Code	FIP	Year	Commission	Court	Any Unified Govt	Divided Govt	Unified Dem Govt	Unified Rep Govt	Non-Unified Control
Arizona	AZ	4	1996	0	0	0	1	0	0	1
Arkansas	AR	5	1996	0	0	1	0	1	0	0
California	CA	6	1996	0	1	0	0	0	0	1
Colorado	CO	8	1996	1	0	0	0	0	0	1
Connecticut	CT	9	1996	1	0	0	0	0	0	1
Delaware	DE	10	1996	0	0	0	1	0	0	1
Florida	FL	12	1996	0	0	1	0	1	0	0
Georgia	GA	13	1996	0	0	1	0	1	0	0
Hawaii	HI	15	1996	1	0	0	0	0	0	1
Idaho	ID	16	1996	0	0	0	1	0	0	1
Illinois	IL	17	1996	0	0	1	0	0	1	0
Indiana	IN	18	1996	0	0	0	1	0	0	1
Iowa	IA	19	1996	1	0	0	0	0	0	1
Kansas	KS	20	1996	0	0	0	1	0	0	1
Kentucky	KY	21	1996	0	0	1	0	1	0	0
Louisiana	LA	22	1996	0	0	1	0	1	0	0
Maine	ME	23	1996	0	1	0	0	0	0	1
Massachusetts	MA	25	1996	0	0	0	1	0	0	1
Michigan	MI	26	1996	0	1	0	0	0	0	1
Minnesota	MN	27	1996	0	1	0	0	0	0	1
Mississippi	MS	28	1996	0	0	1	0	1	0	0
Missouri	MO	29	1996	1	0	0	0	0	0	1
Montana	MT	30	1996	1	0	0	0	0	0	1
Nebraska	NE	31	1996							
Nevada	NV	32	1996	0	0	1	0	1	0	0
New Hampshire	NH	33	1996	0	0	1	0	0	1	0
New Jersey	NJ	34	1996	1	0	0	0	0	0	1
New Mexico	NM	35	1996	0	0	1	0	1	0	0
New York	NY	36	1996	0	0	0	1	0	0	1
North Carolina	NC	37	1996	0	0	1	0	1	0	0
North Dakota	ND	38	1996	0	0	0	1	0	0	1
Ohio	OH	39	1996	0	0	0	1	0	0	1
Oklahoma	OK	40	1996	0	0	1	0	1	0	0

State	Code	FIP	Year	Commission	Court	Any Unified Govt	Divided Govt	Unified Dem Govt	Unified Rep Govt	Non-Unified Control
Oregon	OR	41	1996	0	0	0	1	0	0	1
Pennsylvania	PA	42	1996	1	0	0	0	0	0	1
Rhode Island	RI	44	1996	0	0	1	0	1	0	0
South Carolina	SC	45	1996	0	1	0	0	0	0	1
South Dakota	SD	46	1996	0	0	1	0	0	1	0
Tennessee	TN	47	1996	0	0	1	0	1	0	0
Texas	TX	48	1996	0	0	1	0	1	0	0
Utah	UT	49	1996	0	0	1	0	0	1	0
Vermont	VT	50	1996	0	0	0	1	0	0	1
Virginia	VA	51	1996	0	0	1	0	1	0	0
Washington	WA	53	1996	1	0	0	0	0	0	1
West Virginia	WV	54	1996	0	0	1	0	1	0	0
Wisconsin	WI	55	1996	0	1	0	0	0	0	1
Wyoming	WY	56	1996	0	0	0	1	0	0	1
Alabama	AL	1	1998	0	1	0	0	0	0	1
Alaska	AK	2	1998	0	1	0	0	0	0	1
Arizona	AZ	4	1998	0	0	0	1	0	0	1
Arkansas	AR	5	1998	0	0	1	0	1	0	0
California	CA	6	1998	0	1	0	0	0	0	1
Colorado	CO	8	1998	1	0	0	0	0	0	1
Connecticut	CT	9	1998	1	0	0	0	0	0	1
Delaware	DE	10	1998	0	0	0	1	0	0	1
Florida	FL	12	1998	0	0	1	0	1	0	0
Georgia	GA	13	1998	0	0	1	0	1	0	0
Hawaii	HI	15	1998	1	0	0	0	0	0	1
Idaho	ID	16	1998	0	0	0	1	0	0	1
Illinois	IL	17	1998	0	0	1	0	0	1	0
Indiana	IN	18	1998	0	0	0	1	0	0	1
Iowa	IA	19	1998	1	0	0	0	0	0	1
Kansas	KS	20	1998	0	0	0	1	0	0	1
Kentucky	KY	21	1998	0	0	1	0	1	0	0
Maine	ME	23	1998	0	1	0	0	0	0	1
Maryland	MD	24	1998	0	0	1	0	1	0	0

State	Code	FIP	Year	Commission	Court	Any Unified Govt	Divided Govt	Unified Dem Govt	Unified Rep Govt	Non-Unified Control
Massachusetts	MA	25	1998	0	0	0	1	0	0	1
Michigan	MI	26	1998	0	1	0	0	0	0	1
Minnesota	MN	27	1998	0	1	0	0	0	0	1
Missouri	MO	29	1998	1	0	0	0	0	0	1
Montana	MT	30	1998	1	0	0	0	0	0	1
Nebraska	NE	31	1998							
Nevada	NV	32	1998	0	0	1	0	1	0	0
New Hampshire	NH	33	1998	0	0	1	0	0	1	0
New Jersey	NJ	34	1998	1	0	0	0	0	0	1
New Mexico	NM	35	1998	0	0	1	0	1	0	0
New York	NY	36	1998	0	0	0	1	0	0	1
North Carolina	NC	37	1998	0	0	1	0	1	0	0
North Dakota	ND	38	1998	0	0	0	1	0	0	1
Ohio	OH	39	1998	0	0	0	1	0	0	1
Oklahoma	OK	40	1998	0	0	1	0	1	0	0
Oregon	OR	41	1998	0	0	0	1	0	0	1
Pennsylvania	PA	42	1998	1	0	0	0	0	0	1
Rhode Island	RI	44	1998	0	0	1	0	1	0	0
South Carolina	SC	45	1998	0	1	0	0	0	0	1
South Dakota	SD	46	1998	0	0	1	0	0	1	0
Tennessee	TN	47	1998	0	0	1	0	1	0	0
Texas	TX	48	1998	0	0	1	0	1	0	0
Utah	UT	49	1998	0	0	1	0	0	1	0
Vermont	VT	50	1998	0	0	0	1	0	0	1
Virginia	VA	51	1998	0	0	1	0	1	0	0
Washington	WA	53	1998	1	0	0	0	0	0	1
West Virginia	WV	54	1998	0	0	1	0	1	0	0
Wisconsin	WI	55	1998	0	1	0	0	0	0	1
Wyoming	WY	56	1998	0	0	0	1	0	0	1
Alaska	AK	2	2000	0	1	0	0	0	0	1
Arizona	AZ	4	2000	0	0	0	1	0	0	1
Arkansas	AR	5	2000	0	0	1	0	1	0	0
California	CA	6	2000	0	1	0	0	0	0	1

State	Code	FIP	Year	Commission	Court	Any Unified Govt	Divided Govt	Unified Dem Govt	Unified Rep Govt	Non-Unified Control
Colorado	CO	8	2000	1	0	0	0	0	0	1
Connecticut	CT	9	2000	1	0	0	0	0	0	1
Delaware	DE	10	2000	0	0	0	1	0	0	1
Florida	FL	12	2000	0	0	1	0	1	0	0
Georgia	GA	13	2000	0	0	1	0	1	0	0
Hawaii	HI	15	2000	1	0	0	0	0	0	1
Idaho	ID	16	2000	0	0	0	1	0	0	1
Illinois	IL	17	2000	0	0	1	0	0	1	0
Indiana	IN	18	2000	0	0	0	1	0	0	1
Iowa	IA	19	2000	1	0	0	0	0	0	1
Kansas	KS	20	2000	0	0	0	1	0	0	1
Kentucky	KY	21	2000	0	0	1	0	1	0	0
Louisiana	LA	22	2000	0	0	1	0	1	0	0
Maine	ME	23	2000	0	1	0	0	0	0	1
Massachusetts	MA	25	2000	0	0	0	1	0	0	1
Michigan	MI	26	2000	0	1	0	0	0	0	1
Minnesota	MN	27	2000	0	1	0	0	0	0	1
Mississippi	MS	28	2000	0	0	1	0	1	0	0
Missouri	MO	29	2000	1	0	0	0	0	0	1
Montana	MT	30	2000	1	0	0	0	0	0	1
Nebraska	NE	31	2000							
Nevada	NV	32	2000	0	0	1	0	1	0	0
New Hampshire	NH	33	2000	0	0	1	0	0	1	0
New Jersey	NJ	34	2000	1	0	0	0	0	0	1
New Mexico	NM	35	2000	0	0	1	0	1	0	0
New York	NY	36	2000	0	0	0	1	0	0	1
North Carolina	NC	37	2000	0	0	1	0	1	0	0
North Dakota	ND	38	2000	0	0	0	1	0	0	1
Ohio	OH	39	2000	0	0	0	1	0	0	1
Oklahoma	OK	40	2000	0	0	1	0	1	0	0
Oregon	OR	41	2000	0	0	0	1	0	0	1
Pennsylvania	PA	42	2000	1	0	0	0	0	0	1
Rhode Island	RI	44	2000	0	0	1	0	1	0	0

State	Code	FIP	Year	Commission	Court	Any Unified Govt	Divided Govt	Unified Dem Govt	Unified Rep Govt	Non-Unified Control
South Carolina	SC	45	2000	0	1	0	0	0	0	1
South Dakota	SD	46	2000	0	0	1	0	0	1	0
Tennessee	TN	47	2000	0	0	1	0	1	0	0
Texas	TX	48	2000	0	0	1	0	1	0	0
Utah	UT	49	2000	0	0	1	0	0	1	0
Vermont	VT	50	2000	0	0	0	1	0	0	1
Virginia	VA	51	2000	0	0	1	0	1	0	0
Washington	WA	53	2000	1	0	0	0	0	0	1
West Virginia	WV	54	2000	0	0	1	0	1	0	0
Wisconsin	WI	55	2000	0	1	0	0	0	0	1
Wyoming	WY	56	2000	0	0	0	1	0	0	1
Illinois	IL	17	2006	0	0	1	0	1	0	0
Alaska	AK	2	2002	1	0	0	0	0	0	1
Arizona	AZ	4	2002	1	0	0	0	0	0	1
Arkansas	AR	5	2002	0	0	1	0	0	1	0
Illinois	IL	17	2008	0	0	1	0	1	0	0
Colorado	CO	8	2002	1	0	0	0	0	0	1
Connecticut	CT	9	2002	1	0	0	0	0	0	1
Delaware	DE	10	2002	0	0	0	1	0	0	1
Florida	FL	12	2002	0	0	1	0	0	1	0
Illinois	IL	17	2004	0	0	1	0	1	0	0
Hawaii	HI	15	2002	1	0	0	0	0	0	1
Idaho	ID	16	2002	1	0	0	0	0	0	1
California	CA	6	2008	0	0	1	0	1	0	0
Indiana	IN	18	2002	0	0	0	1	0	0	1
Iowa	IA	19	2002	1	0	0	0	0	0	1
Kansas	KS	20	2002	0	0	1	0	0	1	0
Kentucky	KY	21	2002	0	0	0	1	0	0	1
Maine	ME	23	2002	0	0	0	1	0	0	1
Illinois	IL	17	2002	0	0	1	0	1	0	0
Alabama	AL	1	2010	0	0	1	0	1	0	0
Michigan	MI	26	2002	0	0	1	0	0	1	0
Minnesota	MN	27	2002	0	1	0	0	0	0	1

State	Code	FIP	Year	Commission	Court	Any Unified Govt	Divided Govt	Unified Dem Govt	Unified Rep Govt	Non-Unified Control
Missouri	MO	29	2002	1	0	0	0	0	0	1
Montana	MT	30	2002	1	0	0	0	0	0	1
Nebraska	NE	31	2002							
Nevada	NV	32	2002	0	0	0	1	0	0	1
New Hampshire	NH	33	2002	0	1	0	0	0	0	1
New Jersey	NJ	34	2002	1	0	0	0	0	0	1
New Mexico	NM	35	2002	0	1	0	0	0	0	1
New York	NY	36	2002	0	0	0	1	0	0	1
Delaware	DE	10	2012	0	0	1	0	1	0	0
North Dakota	ND	38	2002	0	0	1	0	0	1	0
Ohio	OH	39	2002	0	0	1	0	0	1	0
Oklahoma	OK	40	2002	0	0	0	1	0	0	1
Oregon	OR	41	2002	0	0	0	1	0	0	1
Pennsylvania	PA	42	2002	1	0	0	0	0	0	1
Massachusetts	MA	25	2012	0	0	1	0	1	0	0
South Carolina	SC	45	2002	0	1	0	0	0	0	1
South Dakota	SD	46	2002	0	0	1	0	0	1	0
Tennessee	TN	47	2002	0	0	0	1	0	0	1
Texas	TX	48	2002	0	1	0	0	0	0	1
Utah	UT	49	2002	0	0	1	0	0	1	0
Vermont	VT	50	2002	0	0	0	1	0	0	1
Virginia	VA	51	2002	0	0	1	0	0	1	0
Washington	WA	53	2002	1	0	0	0	0	0	1
West Virginia	WV	54	2014	0	0	1	0	1	0	0
Wisconsin	WI	55	2002	0	1	0	0	0	0	1
Wyoming	WY	56	2002	0	0	1	0	0	1	0
Alaska	AK	2	2004	1	0	0	0	0	0	1
Arizona	AZ	4	2004	1	0	0	0	0	0	1
Arkansas	AR	5	2004	0	0	1	0	0	1	0
California	CA	6	2006	0	0	1	0	1	0	0
Colorado	CO	8	2004	1	0	0	0	0	0	1
Connecticut	CT	9	2004	1	0	0	0	0	0	1
Delaware	DE	10	2004	0	0	0	1	0	0	1

State	Code	FIP	Year	Commission	Court	Any Unified Govt	Divided Govt	Unified Dem Govt	Unified Rep Govt	Non-Unified Control
Florida	FL	12	2004	0	0	1	0	0	1	0
Georgia	GA	13	2004	0	1	0	0	0	0	1
Hawaii	HI	15	2004	1	0	0	0	0	0	1
Idaho	ID	16	2004	1	0	0	0	0	0	1
West Virginia	WV	54	2008	0	0	1	0	1	0	0
Indiana	IN	18	2004	0	0	0	1	0	0	1
Iowa	IA	19	2004	1	0	0	0	0	0	1
Kansas	KS	20	2004	0	0	1	0	0	1	0
Kentucky	KY	21	2004	0	0	0	1	0	0	1
Louisiana	LA	22	2004	0	0	0	1	0	0	1
Maine	ME	23	2004	0	0	0	1	0	0	1
Illinois	IL	17	2012	0	0	1	0	1	0	0
Michigan	MI	26	2004	0	0	1	0	0	1	0
Minnesota	MN	27	2004	0	1	0	0	0	0	1
West Virginia	WV	54	2004	0	0	1	0	1	0	0
Missouri	MO	29	2004	1	0	0	0	0	0	1
Montana	MT	30	2004	1	0	0	0	0	0	1
Nebraska	NE	31	2004							
Nevada	NV	32	2004	0	0	0	1	0	0	1
New Hampshire	NH	33	2004	0	1	0	0	0	0	1
New Jersey	NJ	34	2004	1	0	0	0	0	0	1
New Mexico	NM	35	2004	0	1	0	0	0	0	1
New York	NY	36	2004	0	0	0	1	0	0	1
North Carolina	NC	37	2008	0	0	1	0	1	0	0
North Dakota	ND	38	2004	0	0	1	0	0	1	0
Ohio	OH	39	2004	0	0	1	0	0	1	0
Oklahoma	OK	40	2004	0	0	0	1	0	0	1
Oregon	OR	41	2004	0	0	0	1	0	0	1
Pennsylvania	PA	42	2004	1	0	0	0	0	0	1
West Virginia	WV	54	2006	0	0	1	0	1	0	0
South Carolina	SC	45	2004	0	1	0	0	0	0	1
South Dakota	SD	46	2004	0	0	1	0	0	1	0
Tennessee	TN	47	2004	0	0	0	1	0	0	1

State	Code	FIP	Year	Commission	Court	Any Unified Govt	Divided Govt	Unified Dem Govt	Unified Rep Govt	Non-Unified Control
Texas	TX	48	2004	0	1	0	0	0	0	1
Utah	UT	49	2004	0	0	1	0	0	1	0
Vermont	VT	50	2004	0	0	0	1	0	0	1
Virginia	VA	51	2004	0	0	1	0	0	1	0
Washington	WA	53	2004	1	0	0	0	0	0	1
Arkansas	AR	5	2014	0	0	1	0	1	0	0
Wisconsin	WI	55	2004	0	1	0	0	0	0	1
Wyoming	WY	56	2004	0	0	1	0	0	1	0
North Carolina	NC	37	2002	0	0	1	0	1	0	0
Alaska	AK	2	2006	1	0	0	0	0	0	1
Arizona	AZ	4	2006	1	0	0	0	0	0	1
Arkansas	AR	5	2006	0	0	1	0	0	1	0
Alabama	AL	1	2006	0	0	1	0	1	0	0
Colorado	CO	8	2006	1	0	0	0	0	0	1
Connecticut	CT	9	2006	1	0	0	0	0	0	1
Delaware	DE	10	2006	0	0	0	1	0	0	1
Florida	FL	12	2006	0	0	1	0	0	1	0
Georgia	GA	13	2006	0	1	0	0	0	0	1
Hawaii	HI	15	2006	1	0	0	0	0	0	1
Idaho	ID	16	2006	1	0	0	0	0	0	1
California	CA	6	2004	0	0	1	0	1	0	0
Indiana	IN	18	2006	0	0	0	1	0	0	1
Iowa	IA	19	2006	1	0	0	0	0	0	1
Kansas	KS	20	2006	0	0	1	0	0	1	0
Kentucky	KY	21	2006	0	0	0	1	0	0	1
Maine	ME	23	2006	0	0	0	1	0	0	1
California	CA	6	2002	0	0	1	0	1	0	0
North Carolina	NC	37	2006	0	0	1	0	1	0	0
Michigan	MI	26	2006	0	0	1	0	0	1	0
Minnesota	MN	27	2006	0	1	0	0	0	0	1
Missouri	MO	29	2006	1	0	0	0	0	0	1
Montana	MT	30	2006	1	0	0	0	0	0	1
Nebraska	NE	31	2006							

State	Code	FIP	Year	Commission	Court	Any Unified Govt	Divided Govt	Unified Dem Govt	Unified Rep Govt	Non-Unified Control
Nevada	NV	32	2006	0	0	0	1	0	0	1
New Hampshire	NH	33	2006	0	1	0	0	0	0	1
New Jersey	NJ	34	2006	1	0	0	0	0	0	1
New Mexico	NM	35	2006	0	1	0	0	0	0	1
New York	NY	36	2006	0	0	0	1	0	0	1
California	CA	6	2010	0	0	1	0	1	0	0
North Dakota	ND	38	2006	0	0	1	0	0	1	0
Ohio	OH	39	2006	0	0	1	0	0	1	0
Oklahoma	OK	40	2006	0	0	0	1	0	0	1
Oregon	OR	41	2006	0	0	0	1	0	0	1
Pennsylvania	PA	42	2006	1	0	0	0	0	0	1
North Carolina	NC	37	2004	0	0	1	0	1	0	0
South Carolina	SC	45	2006	0	1	0	0	0	0	1
South Dakota	SD	46	2006	0	0	1	0	0	1	0
Tennessee	TN	47	2006	0	0	0	1	0	0	1
Texas	TX	48	2006	0	1	0	0	0	0	1
Utah	UT	49	2006	0	0	1	0	0	1	0
Vermont	VT	50	2006	0	0	0	1	0	0	1
Virginia	VA	51	2006	0	0	1	0	0	1	0
Washington	WA	53	2006	1	0	0	0	0	0	1
Rhode Island	RI	44	2006	0	0	1	0	1	0	0
Wisconsin	WI	55	2006	0	1	0	0	0	0	1
Wyoming	WY	56	2006	0	0	1	0	0	1	0
Alaska	AK	2	2008	1	0	0	0	0	0	1
Arizona	AZ	4	2008	1	0	0	0	0	0	1
Arkansas	AR	5	2008	0	0	1	0	0	1	0
Massachusetts	MA	25	2006	0	0	1	0	1	0	0
Colorado	CO	8	2008	1	0	0	0	0	0	1
Connecticut	CT	9	2008	1	0	0	0	0	0	1
Delaware	DE	10	2008	0	0	0	1	0	0	1
Florida	FL	12	2008	0	0	1	0	0	1	0
Georgia	GA	13	2008	0	1	0	0	0	0	1
Hawaii	HI	15	2008	1	0	0	0	0	0	1

State	Code	FIP	Year	Commission	Court	Any Unified Govt	Divided Govt	Unified Dem Govt	Unified Rep Govt	Non-Unified Control
Idaho	ID	16	2008	1	0	0	0	0	0	1
Delaware	DE	10	2014	0	0	1	0	1	0	0
Indiana	IN	18	2008	0	0	0	1	0	0	1
Iowa	IA	19	2008	1	0	0	0	0	0	1
Kansas	KS	20	2008	0	0	1	0	0	1	0
Kentucky	KY	21	2008	0	0	0	1	0	0	1
Louisiana	LA	22	2008	0	0	0	1	0	0	1
Maine	ME	23	2008	0	0	0	1	0	0	1
Illinois	IL	17	2010	0	0	1	0	1	0	0
Michigan	MI	26	2008	0	0	1	0	0	1	0
Minnesota	MN	27	2008	0	1	0	0	0	0	1
North Carolina	NC	37	2010	0	0	1	0	1	0	0
Missouri	MO	29	2008	1	0	0	0	0	0	1
Montana	MT	30	2008	1	0	0	0	0	0	1
Nebraska	NE	31	2008							
Nevada	NV	32	2008	0	0	0	1	0	0	1
New Hampshire	NH	33	2008	0	1	0	0	0	0	1
New Jersey	NJ	34	2008	1	0	0	0	0	0	1
New Mexico	NM	35	2008	0	1	0	0	0	0	1
New York	NY	36	2008	0	0	0	1	0	0	1
Massachusetts	MA	25	2004	0	0	1	0	1	0	0
North Dakota	ND	38	2008	0	0	1	0	0	1	0
Ohio	OH	39	2008	0	0	1	0	0	1	0
Oklahoma	OK	40	2008	0	0	0	1	0	0	1
Oregon	OR	41	2008	0	0	0	1	0	0	1
Pennsylvania	PA	42	2008	1	0	0	0	0	0	1
West Virginia	WV	54	2012	0	0	1	0	1	0	0
South Carolina	SC	45	2008	0	1	0	0	0	0	1
South Dakota	SD	46	2008	0	0	1	0	0	1	0
Tennessee	TN	47	2008	0	0	0	1	0	0	1
Texas	TX	48	2008	0	1	0	0	0	0	1
Utah	UT	49	2008	0	0	1	0	0	1	0
Vermont	VT	50	2008	0	0	0	1	0	0	1

State	Code	FIP	Year	Commission	Court	Any Unified Govt	Divided Govt	Unified Dem Govt	Unified Rep Govt	Non-Unified Control
Virginia	VA	51	2008	0	0	1	0	0	1	0
Washington	WA	53	2008	1	0	0	0	0	0	1
Oregon	OR	41	2012	0	0	1	0	1	0	0
Wisconsin	WI	55	2008	0	1	0	0	0	0	1
Wyoming	WY	56	2008	0	0	1	0	0	1	0
Arkansas	AR	5	2012	0	0	1	0	1	0	0
Alaska	AK	2	2010	1	0	0	0	0	0	1
Arizona	AZ	4	2010	1	0	0	0	0	0	1
Arkansas	AR	5	2010	0	0	1	0	0	1	0
Alabama	AL	1	2002	0	0	1	0	1	0	0
Colorado	CO	8	2010	1	0	0	0	0	0	1
Connecticut	CT	9	2010	1	0	0	0	0	0	1
Delaware	DE	10	2010	0	0	0	1	0	0	1
Florida	FL	12	2010	0	0	1	0	0	1	0
Georgia	GA	13	2010	0	1	0	0	0	0	1
Hawaii	HI	15	2010	1	0	0	0	0	0	1
Idaho	ID	16	2010	1	0	0	0	0	0	1
Illinois	IL	17	2014	0	0	1	0	1	0	0
Indiana	IN	18	2010	0	0	0	1	0	0	1
Iowa	IA	19	2010	1	0	0	0	0	0	1
Kansas	KS	20	2010	0	0	1	0	0	1	0
Kentucky	KY	21	2010	0	0	0	1	0	0	1
Maine	ME	23	2010	0	0	0	1	0	0	1
Oregon	OR	41	2014	0	0	1	0	1	0	0
West Virginia	WV	54	2010	0	0	1	0	1	0	0
Michigan	MI	26	2010	0	0	1	0	0	1	0
Minnesota	MN	27	2010	0	1	0	0	0	0	1
Missouri	MO	29	2010	1	0	0	0	0	0	1
Montana	MT	30	2010	1	0	0	0	0	0	1
Nebraska	NE	31	2010							
Nevada	NV	32	2010	0	0	0	1	0	0	1
New Hampshire	NH	33	2010	0	1	0	0	0	0	1
New Jersey	NJ	34	2010	1	0	0	0	0	0	1

State	Code	FIP	Year	Commission	Court	Any Unified Govt	Divided Govt	Unified Dem Govt	Unified Rep Govt	Non-Unified Control
New Mexico	NM	35	2010	0	1	0	0	0	0	1
New York	NY	36	2010	0	0	0	1	0	0	1
Massachusetts	MA	25	2008	0	0	1	0	1	0	0
North Dakota	ND	38	2010	0	0	1	0	0	1	0
Ohio	OH	39	2010	0	0	1	0	0	1	0
Oklahoma	OK	40	2010	0	0	0	1	0	0	1
Oregon	OR	41	2010	0	0	0	1	0	0	1
Pennsylvania	PA	42	2010	1	0	0	0	0	0	1
Massachusetts	MA	25	2010	0	0	1	0	1	0	0
South Carolina	SC	45	2010	0	1	0	0	0	0	1
South Dakota	SD	46	2010	0	0	1	0	0	1	0
Tennessee	TN	47	2010	0	0	0	1	0	0	1
Texas	TX	48	2010	0	1	0	0	0	0	1
Utah	UT	49	2010	0	0	1	0	0	1	0
Vermont	VT	50	2010	0	0	0	1	0	0	1
Virginia	VA	51	2010	0	0	1	0	0	1	0
Washington	WA	53	2010	1	0	0	0	0	0	1
Massachusetts	MA	25	2014	0	0	1	0	1	0	0
Wisconsin	WI	55	2010	0	1	0	0	0	0	1
Wyoming	WY	56	2010	0	0	1	0	0	1	0
Alaska	AK	2	2012	1	0	0	0	0	0	1
Arizona	AZ	4	2012	1	0	0	0	0	0	1
Rhode Island	RI	44	2004	0	0	1	0	1	0	0
California	CA	6	2012	1	0	0	0	0	0	1
Colorado	CO	8	2012	1	0	0	0	0	0	1
Connecticut	CT	9	2012	1	0	0	0	0	0	1
Rhode Island	RI	44	2002	0	0	1	0	1	0	0
Florida	FL	12	2012	0	0	1	0	0	1	0
Georgia	GA	13	2012	0	0	1	0	0	1	0
Hawaii	HI	15	2012	1	0	0	0	0	0	1
Idaho	ID	16	2012	1	0	0	0	0	0	1
Georgia	GA	13	2002	0	0	1	0	1	0	0
Indiana	IN	18	2012	0	0	1	0	0	1	0

State	Code	FIP	Year	Commission	Court	Any Unified Govt	Divided Govt	Unified Dem Govt	Unified Rep Govt	Non-Unified Control
Iowa	IA	19	2012	1	0	0	0	0	0	1
Kansas	KS	20	2012	0	0	1	0	0	1	0
Kentucky	KY	21	2012	0	0	0	1	0	0	1
Louisiana	LA	22	2012	0	0	1	0	0	1	0
Maine	ME	23	2012	0	0	0	1	0	0	1
Massachusetts	MA	25	2002	0	0	1	0	1	0	0
Michigan	MI	26	2012	0	0	1	0	0	1	0
Minnesota	MN	27	2012	0	1	0	0	0	0	1
Rhode Island	RI	44	2012	0	0	1	0	1	0	0
Missouri	MO	29	2012	1	0	0	0	0	0	1
Montana	MT	30	2012	1	0	0	0	0	0	1
Nebraska	NE	31	2012							
Nevada	NV	32	2012	0	1	0	0	0	0	1
New Hampshire	NH	33	2012	0	0	1	0	0	1	0
New Jersey	NJ	34	2012	1	0	0	0	0	0	1
New Mexico	NM	35	2012	0	1	0	0	0	0	1
New York	NY	36	2012	0	0	0	1	0	0	1
North Carolina	NC	37	2012	0	0	1	0	0	1	0
North Dakota	ND	38	2012	0	0	1	0	0	1	0
Ohio	OH	39	2012	0	0	1	0	0	1	0
Oklahoma	OK	40	2012	0	0	1	0	0	1	0
Vermont	VT	50	2012	0	0	1	0	1	0	0
Pennsylvania	PA	42	2012	1	0	0	0	0	0	1
West Virginia	WV	54	2002	0	0	1	0	1	0	0
South Carolina	SC	45	2012	0	0	1	0	0	1	0
South Dakota	SD	46	2012	0	0	1	0	0	1	0
Tennessee	TN	47	2012	0	0	1	0	0	1	0
Texas	TX	48	2012	0	0	1	0	0	1	0
Utah	UT	49	2012	0	0	1	0	0	1	0
Vermont	VT	50	2014	0	0	1	0	1	0	0
Virginia	VA	51	2012	0	0	0	1	0	0	1
Washington	WA	53	2012	1	0	0	0	0	0	1
Rhode Island	RI	44	2008	0	0	1	0	1	0	0

State	Code	FIP	Year	Commission	Court	Any Unified Govt	Divided Govt	Unified Dem Govt	Unified Rep Govt	Non-Unified Control
Wisconsin	WI	55	2012	0	0	1	0	0	1	0
Wyoming	WY	56	2012	0	0	1	0	0	1	0
Alabama	AL	1	2014	0	0	1	0	0	1	0
Alaska	AK	2	2014	1	0	0	0	0	0	1
Arizona	AZ	4	2014	1	0	0	0	0	0	1
Rhode Island	RI	44	2010	0	0	1	0	1	0	0
California	CA	6	2014	1	0	0	0	0	0	1
Colorado	CO	8	2014	1	0	0	0	0	0	1
Connecticut	CT	9	2014	1	0	0	0	0	0	1
Rhode Island	RI	44	2014	0	0	1	0	1	0	0
Florida	FL	12	2014	0	0	1	0	0	1	0
Georgia	GA	13	2014	0	0	1	0	0	1	0
Hawaii	HI	15	2014	1	0	0	0	0	0	1
Idaho	ID	16	2014	1	0	0	0	0	0	1
Maryland	MD	24	2002	0	0	1	0	1	0	0
Indiana	IN	18	2014	0	0	1	0	0	1	0
Iowa	IA	19	2014	1	0	0	0	0	0	1
Kansas	KS	20	2014	0	0	1	0	0	1	0
Kentucky	KY	21	2014	0	0	0	1	0	0	1
Maine	ME	23	2014	0	0	0	1	0	0	1
Maryland	MD	24	2006	0	0	1	0	1	0	0
Maryland	MD	24	2010	0	0	1	0	1	0	0
Michigan	MI	26	2014	0	0	1	0	0	1	0
Minnesota	MN	27	2014	0	1	0	0	0	0	1
Missouri	MO	29	2014	1	0	0	0	0	0	1
Montana	MT	30	2014	1	0	0	0	0	0	1
Nebraska	NE	31	2014							
Nevada	NV	32	2014	0	1	0	0	0	0	1
New Hampshire	NH	33	2014	0	0	1	0	0	1	0
New Jersey	NJ	34	2014	1	0	0	0	0	0	1
New Mexico	NM	35	2014	0	1	0	0	0	0	1
New York	NY	36	2014	0	0	0	1	0	0	1
North Carolina	NC	37	2014	0	0	1	0	0	1	0

State	Code	FIP	Year	Commission	Court	Any Unified Govt	Divided Govt	Unified Dem Govt	Unified Rep Govt	Non-Unified Control
North Dakota	ND	38	2014	0	0	1	0	0	1	0
Ohio	OH	39	2014	0	0	1	0	0	1	0
Oklahoma	OK	40	2014	0	0	1	0	0	1	0
Maryland	MD	24	2014	0	0	1	0	1	0	0
Pennsylvania	PA	42	2014	1	0	0	0	0	0	1
Mississippi	MS	28	2004	0	0	1	0	1	0	0
South Carolina	SC	45	2014	0	0	1	0	0	1	0
South Dakota	SD	46	2014	0	0	1	0	0	1	0
Tennessee	TN	47	2014	0	0	1	0	0	1	0
Texas	TX	48	2014	0	0	1	0	0	1	0
Utah	UT	49	2014	0	0	1	0	0	1	0
Mississippi	MS	28	2008	0	0	1	0	1	0	0
Virginia	VA	51	2014	0	0	0	1	0	0	1
Washington	WA	53	2014	1	0	0	0	0	0	1
Mississippi	MS	28	2012	0	0	1	0	1	0	0
Wisconsin	WI	55	2014	0	0	1	0	0	1	0
Wyoming	WY	56	2014	0	0	1	0	0	1	0

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Measuring Partisan Bias in Single-Member District Electoral Systems

In recent decades, the literature has coalesced around either symmetry or responsiveness as measures of partisan bias in single-member district systems. I argue neither accurately captures the traditional idea of an "efficient" gerrymander, where one party claims more seats without more votes. I suggest a better measure of efficiency and then use this new measure to reconsider a classic study of partisan gerrymandering. Contrary to the original study findings, I show that the effects of party control on bias are small and decay rapidly, suggesting that redistricting is at best a blunt tool for promoting partisan interests.

The literature on single-member district (SMD) elections has long been concerned with the problem of partisan efficiency. Only a bare plurality of votes is required to win any seat, so additional votes are "wasted," and a clever party can manipulate this fact to its advantage.[1] The more a party manipulates wasted votes, the more seats it gains at the opposing party's expense. Moreover, a party can gain the benefits of efficiency even if no deliberate manipulation is involved. Partisan disparities in wasted votes can occur for any number of reasons, including minority-representation requirements and the distribution of partisan supporters in geographic space through demographic and socioeconomic change.

In recent decades, the literature has coalesced around the twin concepts of *symmetry* and *responsiveness* as the best means of measuring this sort of partisan advantage. Symmetry hinges on uniform treatment—a system is asymmetrical if one party receives a larger share of seats than the other party for the same share of votes—while responsiveness captures the overall competitiveness of the system, as measured by the number of seats that change hands for a given shift in the aggregate vote. These measures are potentially useful because they are easy to calculate with simple election returns, and they promise consistency of meaning in a way that simplifies comparisons across time and space.

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In this article, I suggest that symmetry and responsiveness, while certainly of value for some purposes and concepts, are not logically related to the seat gains from efficiency that concern many scholars and analysts. I suggest a more direct measure of efficiency and use data from state legislative elections to demonstrate that it is a more appropriate means of representing this particular concept. The new measure is also more flexible. Analysts can use it to evaluate electoral systems where one party typically dominates others—a condition advocates of symmetry studiously avoid—and it actually converges on symmetry as a party's aggregate vote share approaches 0.5. Thus, the new measure can be considered a more general extension of the symmetry measure itself—one that preserves some of symmetry's normative meaning for uncompetitive SMD systems.

The value of this new measure is largely unrelated to the specific strategy a party takes to maximize its seat share. Several important works have suggested caveats to the seat-maximization strategy to account for the uncertainty of partisan support and other factors (Friedman and Holden 2008; Owen and Grofman 1988). But the end goal of all of these strategies is to maximize expected seats given expected votes, so the measure of efficiency I offer here will still be of considerable utility.

I then use this new measure of partisan bias to reconsider our understanding of redistricting in the United States. In a classic study, Gelman and King (1994) argued that party control of redistricting has substantial effects on bias that endure through the redistricting cycle but that the disruptive nonpartisan effects of redistricting are strong enough to make the process normatively beneficial all the same. I show that these findings are artifacts of symmetry; when the new measure is used instead, redistricting has only small and transient effects on bias and is hardly more disruptive than the natural back and forth of electoral competition. This casts doubt on the utility of redistricting as a means of achieving an efficient seat-share advantage and thus on part of the rationale of judicial intervention in partisan gerrymandering cases.

Efficiency, Symmetry, and Responsiveness

Scholarship on redistricting has long understood that a particular plan can be biased toward one party based on the efficient distribution of partisan voters across districts. The candidate who receives one vote more than 50% (in a two-party contest) is the winner, so any vote beyond that threshold is wasted in the sense that it does not contribute to victory. All the votes cast for a losing candidate are also wasted because they do not support a winning effort. In either case, a party could avoid wasting the votes by moving them to a different district where they could be part of a minimally winning coalition. The party with greater efficiency—fewer wasted votes—will generally win more seats by smaller margins than the opposition. Along these lines, experts frequently speak of "cracking"—dividing a partisan community between two or more districts when they could be a majority in a single district—and "packing"—filling a district with more supporters of the opposing party than needed to win the seat. Both aim to make supporters of the disadvantaged party either a slim minority in each district or the overwhelming majority. This is the essence of partisan efficiency.

Some version of efficiency is typically the core concept of interest in the literature on redistricting. Most of this literature focuses on deliberate efforts to advantage one party or the other through gerrymandering (e.g., Cain 1985; Engstrom 2006; Owen and Grofman 1988). It is easy to see why the effort to maximize seat share this way might be troubling to normative democratic theory, since it offers a party a means of increasing its margin of control over policy without winning more votes from the public. However, the importance of efficiency need not be limited to partisan gerrymandering. A substantial literature ponders the unintended partisan consequences of a variety of phenomena with no overt partisan origin or objective. These include redistricting to boost minority representation (Brace, Grofman, and Handley 1987; Schotts 2001), differential rates of turnout across districts (Campbell 1996; Grofman, Koetzle, and Brunell 1997), and simple differences in residential patterns for supporters of each party (Chen and Rodden 2011). In all these examples, efficiency is believed to reflect a more fundamental partisan disparity. It is a question of lost potential: a biased system wastes more votes on one party and so restricts the number of seats that party might otherwise win.

While efficiency is the concept of greatest interest, the most common measure of partisan advantage in the literature is *symmetry*. Symmetry is achieved when each party receives the same share of seats for identical shares of votes. The idea is easiest to understand at an aggregate vote share of 0.5—a party that receives half the vote ought to receive half the seats—but a similar logic can apply across the "seats-votes curve" that traces out how seat shares change as vote shares rise and fall. For example, if a party receives a vote share of 0.55 and a seat share of 0.60, the opposing party should also expect to receive a seat share of 0.60 if it were to receive a vote share of 0.55. An unbiased system means that for V share of the votes a party should receive S share of the seats, and this should be true for all parties and vote percentages (Niemi and Deegan 1978).

Symmetry has never been strictly compared to efficiency. In place of such an assessment, the literature has taken one of three different approaches. The first simply defines partisan bias as asymmetry in the seats-votes curve. Some of the earliest expositions of partisan symmetry were most likely to take this approach. For example, Soper and Rydon (1958) equate the median vote (i.e., 50%) with what they call the "effective" vote—a term that is now typically reserved specifically for partisan efficiency. Brookes (1959) also starts the derivations of the measures he proposes under the presumption that symmetry is the concept of interest. And while this approach started early, there are more recent examples of authors who adopt symmetry without much further explanation (Altman 2002; Gelman and King 1994b; Jackman 1994; Niemi and Deegan 1978). The implication is that the measure is facially valid.

The second approach recognizes the existence of partisan efficiency, but either argues that symmetry is a superior measure or ceases to mention efficiency after some cursory introduction. The principal example here is Grofman and King, who recognize that "Journalistic accounts of partisan gerrymandering often describe it as a process of packing one's opponents into as few districts as possible and seeking to win the remaining districts by the barest of margins" (2007, 13), but who then dismiss this notion as flawed and define a partisan gerrymander in terms other than seat gain.

The most common perspective simply equates the two measures, either implicitly or explicitly. This approach dates back to Tufte's (1973) seminal analysis of the relationship between votes and seats, which at first seems to ignore efficiency and advocate symmetry, but then emphasizes differential constituency size as a possible explanation for the shifts in symmetry he finds. (Constituency size is a matter of efficiency because the party that wins districts with smaller numbers of voters claims more seats with fewer votes.) Many important studies since then have also used symmetry as a measure but described some form of efficiency as the concept of interest (e.g., Campagna and Grofman 1990; Cox and Katz 2002; Engstrom 2006; Erikson 1972; Gilligan and Matsusaka 1999; Grofman, Koetzle, and Brunell 1997; Kastellec, Gelman, and Chandler 2008).

While symmetry is the most common measure of bias, a few scholars have suggested that *responsiveness* can also serve as an effective measure of an efficient seat-maximizing gerrymander (Cox and Katz 1999). Responsiveness records how many seats change hands as vote shares rise and fall, so it can be thought of as the slope of the seats-votes curve across a range of vote shares. Usually this range is between 10 and 30 points wide and is centered upon either the actual election result or a vote share of 0.5. Responsiveness is greater than 1 when seat share rises faster than vote share and less than 1 when the opposite is true. Because responsiveness will be higher when more seats are clustered close to the 0.5 threshold, it will be higher when there are more competitive seats. A party drawing an aggressive seat-maximizing gerrymander will strive to win its seats by as narrow a margin as possible, so such a gerrymander might plausibly increase the overall responsiveness of the system. Some scholars have also suggested measuring party advantage as the deviation from an ideal level of responsiveness, usually defined as proportionality (i.e., responsiveness = 1.0), but such approaches have never caught on in the literature on SMD systems.[2]

Two aspects of symmetry and responsiveness deserve special attention. First, they are almost always counterfactual calculations. For any given election, there is only one actual seats-votes pair. To identify the seats-votes curve—and with it, partisan symmetry and responsiveness—the vote share in every seat must be shifted a uniform amount. For each new hypothetical vote share, the new seat share is recorded based on the number of seats that changed hands as a result of the exercise. Since each district's vote share is shifted a uniform amount, the hypothetical assumes that the order of the district outcomes would not change if overall party performance were suddenly equal (Grofman and King 2007).[3] There are some partial exceptions to this counterfactual—in particular, the work of Gelman and King (1994b) allows for a certain amount of randomness around this vote shift, in the understanding that no counterfactual's consequences can ever be known with certainty—but the core logic of the measure is the same.

Second, both measures reflect the shape of the seats-votes curve and not any particular point on it, so they are relatively immune to shifts in party performance. For instance, a partisan tide that moves all seats a uniform amount alters only where the actual election result falls on the curve; it cannot change the shape of the curve itself, and so it cannot have much if any impact on symmetry or responsiveness. Even many shifts in relative district position—which do change the curve—will not affect symmetry or responsiveness unless they fall within range of the counterfactual. By necessity, then, both symmetry and responsiveness presume bias is a strongly enduring feature of a redistricting plan, something that should not and usually will not respond to the dynamics of ordinary partisan competition.

There is a portion of the literature that largely or entirely disregards symmetry and responsiveness and adopts a more direct measure of efficiency instead. Though the exact method varies somewhat, these studies all compare outcomes before the redistricting to what they would have been for the same election under the new plan, and the intent is always to isolate the gain in seat share induced by the redistricting alone (Born 1985; Cain 1985; Engstrom 2006; Kousser 1996). This approach is probably closest to the concept of efficiency, but it also imposes significant costs on the analyst. It only applies to before-and-after comparisons, and it is most effective and valid where it is possible to aggregate political data from before the redistricting into the new district lines. In many circumstances, either the relevant data are unavailable or there is no redistricting intervention to evaluate in the first place. Thus, it remains attractive to have a measure that is meaningful across a broad range of circumstances and which can be calculated with easily available data. Both symmetry and responsiveness strive to fill that need.

In short, much of the literature seeks to measure efficiency while assuming that either symmetry or responsiveness is an adequate measure of the concept. There is, of course, no reason why an analyst *must* measure efficiency. One can certainly have a broader normative debate about the value of efficiency as a measure of partisan fairness compared to either symmetry or responsiveness. (I will return to this question again in the conclusion.) But many studies actually assume that either symmetry or responsiveness, or both, accurately measure efficiency. This is a far stronger, and fundamentally empirical, claim. Given the dependence of so many studies on this core idea, it is surprising that no systematic evaluation of it has ever been made. Should we expect these measures to capture efficiency, or are the concepts distinct?

Understanding Efficiency

To evaluate symmetry and responsiveness as measures of efficiency, we need some standard for comparing them. What does it mean for a plan to be efficient? First, a party that strives to maximize efficiency wants to win more seats, not protect the ones it already holds. A redistricting party might certainly draw safe seats to preserve gains it has recently made, especially if it feels those gains are unlikely to be surpassed before the next redistricting. Moreover, scholars have rightly noted that an effective gerrymander will avoid making majority party seats so competitive that the gerrymander comes undone in the face of contrary partisan tides (Friedman and Holden 2008; Owen and Grofman 1988). However, neither of these ideas represents the core concept of efficiency as described above and as employed in the literature, so I set them aside for now. I will take up these points again in the discussion.

Second, efficient bias is not just about winning more seats—it is about winning them with the same number of supporters: that is, “the effect of redistricting on the allocation of seats between the parties *given their average district votes*" (Gelman and King 1994a, 550, emphasis added). Thus, it assumes a fixed number of party voters who are then redistributed among the districts. It is an advantage that stems from where a party's supporters live and how the district lines have been drawn around them. The less reliable a party's supporters, the more difficult it is to make an efficient gerrymander "stick" (Friedman and Holden 2008).

These two components—higher seat share and a constant vote share—suggest the following proposition:

Proposition 1: If a party's seat share grows without any change in its vote share, then the efficiency of the system also shifts in that party's favor.

It is important to note that Proposition 1 is definitional: it identifies a condition under which a party's advantage in efficiency *must* be changing, since it is the very nature of efficiency to extract more seats from the same level of partisan support. Proposition 1 also implies that an appropriate measure of efficiency should capture the shift in seat share described. This suggests a second proposition:

Proposition 2: A valid measure of efficiency will suggest a monotonic increase (decrease) in the advantage for one party if that party gains (loses) seat share without any corresponding increase (decrease) in its vote share.

Proposition 2 is a necessary condition for a valid measure of efficiency: any measure that does not satisfy the proposition may be appropriate for some other concept of interest, but it is not, ipso facto, a measure of efficient bias. Thus, to cast doubt on symmetry and responsiveness as measures of efficiency, we need only establish that the measures can, under certain circumstances, violate Proposition 2. I turn now to some theoretical examples that produce just such an unintended result.

First consider symmetry under the case when there are only two parties, a government (G) and an opposition (O), who fight to control 10 seats. Party G's share of the vote in any given district is V_i^G, its actual (i.e., prior to counterfactual) aggregate share of the total vote across all districts is V_G, and its actual share of all seats is S_G. G receives a majority of the vote, so $V_G > 0.5$. As with any two-party system, 0.5 is the win-loss threshold: any seat with an actual government vote share above this threshold (i.e., $V_i^G > 0.5$) will be held by G, while any seat below it will be held by the opposition O. Panel 1 of Figure 1 presents one possible configuration of these values across the continuum of vote shares, with G holding six of 10 seats ($S_G = 0.6$).

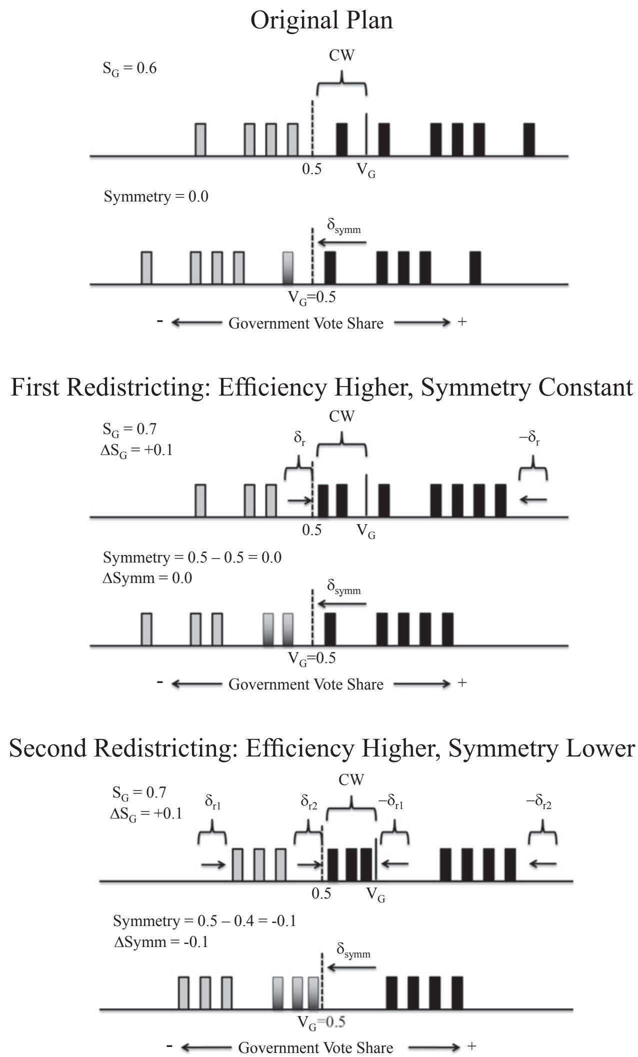
Since calculating symmetry requires shifting the vote shares of all districts and recording the number of seats that change hands, there exists a range of vote shares within which any seat will change hands as a consequence of this counterfactual, and outside of which no seat will change hands. Let us refer to this range of votes as the *counterfactual window*, or CW. Symmetry typically involves shifting V_G to 0.5, so the CW in this case is the range of vote shares defined by $0.5 < V_i^G < V_G$. Any seat falling in that range belongs to party G under the actual election results but will belong to party O under the counterfactual scenario. Panel 1 of Figure 1 demonstrates this effect: G only retains the seats that fall above V_G, which in this case means "losing" one seat. Moreover, in this example the system is symmetrical, since after the counterfactual each party holds half the seats for half the votes.

Now consider what happens under the following redistricting plans where G improves its efficiency by increasing its seat share without any change in vote share:[4]

1) *Figure 1, Panel 2: A seat held by Party O moves into the CW (Party G efficiency gain)*. Under this scenario, presented as Panel 2 of Figure 1, the opposition party loses a seat to the government as G transfers some of its supporters from its safest seat to the most marginal seat held by O. (This is, in fact, a typical ploy for a partisan gerrymander.) Efficiency increases for G, as it gains a seat without gaining any additional votes. Yet, using symmetry, the seat it gains is lost again under the counterfactual, leaving no trace of the change.
2) *Figure 1, Panel 3: A government and an opposition seat are both moved into the CW (Party G efficiency gain)*. The government increases its efficiency by gaining a seat without any gain in vote share, but under the counterfactual, it loses this seat and an additional one besides. This leads to the conclusion that the opposition has gained from the redistricting, when the government is the real beneficiary.

If symmetry is sometimes a poor measure of efficiency, responsiveness may have even greater limitations. Symmetry is a count of the number of seats above and below a particular threshold (the aggregate vote share, in the case of the most common measure of symmetry). Responsiveness, by contrast, is effectively a count of the number of

FIGURE 1
Redistricting and Symmetry in Three Hypothetical Plans



Note: Each pair of figures corresponds to a scenario described in the text. The top figure is the actual election outcome and the bottom figure the outcome under the symmetry counterfactual. δ_{symm} is the shift in votes required for the symmetry counterfactual. δ_r is the shift in votes for particular districts from redistricting. ΔS_G is the change in the government's share of seats relative to the baseline redistricting plan in the first panel, and ΔSymm is the change in symmetry relative to the same baseline plan. All other values are defined in the text.

districts in the CW. If that number increases, responsiveness will also increase. Specifically, it is defined as:

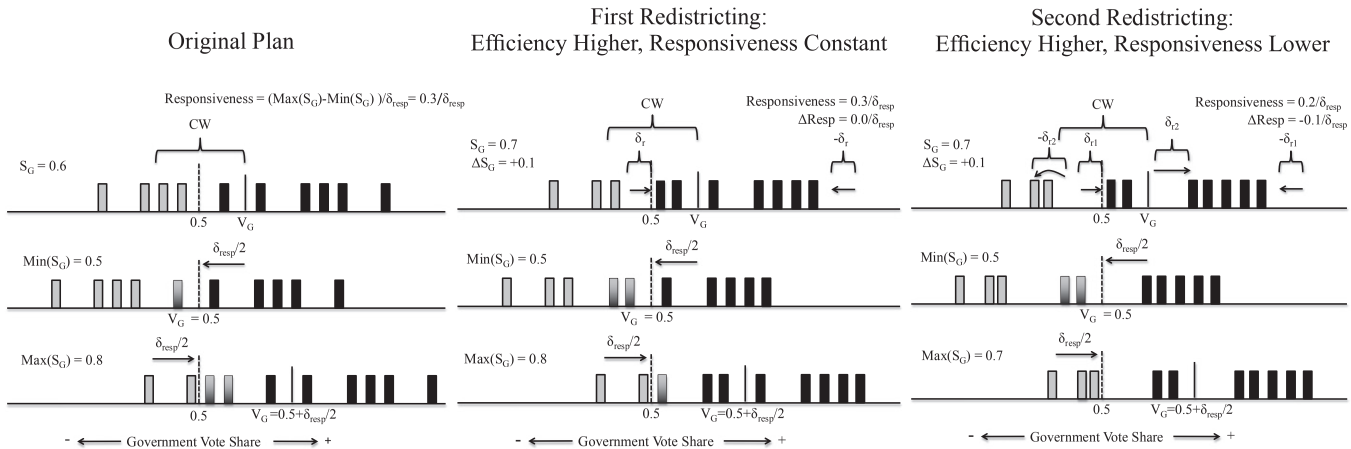
$$Responsiveness = (Max(S_G) - Min(S_G))/\delta_{resp}. \tag{1}$$

δ_{resp} is the range of vote shares considered for the calculation. This range is somewhat arbitrary, and analysts have used values ranging from as high as +/- 15 percentage points off the actual aggregate vote share to as small as virtually nothing (specifically, the derivative of the seats-votes curve at the point of the actual election outcome). $Max(S_G)$ and $Min(S_G)$ are the maximum and minimum seat shares for the government in that range of vote shares. The first panel in Figure 2 presents a base plan altered slightly from the one found in Figure 1, with a responsiveness of $0.3/\delta_{resp}$. From this baseline, the following two scenarios produce the results similar to the ones in Figure 1:

1) *Figure 2, Panel 2: A seat held by Party O crosses the win-loss threshold but remains in the CW, while a seat held by Party G becomes more competitive.* Party G's relative efficiency increases, but there is no change in responsiveness because the number of seats in the CW does not change.
2) *Figure 2, Panel 3: The redistricting plan in the first scenario also moves an opposition seat out of the CW while making a government seat less competitive*. The first scenario already improved efficiency for the government without changing responsiveness; these additional changes also decrease the responsiveness of the plan as well. The result is an improvement in efficiency for the government but a decline in responsiveness.

While these examples demonstrate specific violations of Proposition 2, there are more conceptual reasons to think responsiveness does not make sense as a measure of efficiency. It was designed to measure competition, not partisan advantage, and so it has no partisan valence. A redistricting plan that improves efficiency for either party may also improve responsiveness by increasing the number of competitive seats for that party. That means responsiveness cannot distinguish between a plan that favors one party from a plan that favors the other. Moreover, even a classic partisan gerrymander might not improve responsiveness, since the redistricting party could be expected to make minority seats uncompetitive at the same time that it makes its own seats competitive (Cain 1985). If these changes balance each other out and leave the same

FIGURE 2
Redistricting and Responsiveness in Three Hypothetical Plans



Note: Each pair of figures corresponds to a scenario described in the text. The top figure is the actual election outcome and the bottom figure the outcome under the responsiveness counterfactual. δ_{resp} is the shift in votes required for the responsiveness counterfactual. δ_r is the shift in votes for particular districts, as induced by various alternative redistricting plans. ΔS_G is the change in the government's share of seats relative to the baseline redistricting plan in the first panel and $\Delta Resp$ is the change in responsiveness relative to the same baseline plan. All other values are defined in the text.

number of seats in the CW, there will be no change to responsiveness at all.

It is important to emphasize that these examples have been specifically selected to demonstrate a violation of Proposition 2. As such, they show only that symmetry and responsiveness are distinct from efficiency. They do not, and cannot, tell us whether symmetry and responsiveness are *correlated* with efficiency, such that they might serve as reasonable proxies for the concept. I address that related notion in the next section.

Testing Symmetry and Responsiveness

The arguments above present evidence that symmetry and responsiveness may be poor measures of seat gains from redistricting. Yet the sort of scenarios I have identified as problematic may be rare in the real world. How well do each of these measures correlate with the seat share they must reflect in order to be valid measures of efficiency? Do symmetry and responsiveness correspond better to real redistricting plans than they appear to in the theoretical discussion above?

To answer this question, I turn to a data set of elections to lower state legislative chambers from 1970 to 2003 (Carsey et al. 2008). The data are the standard source for studies of legislative elections, and past efforts at validation have found them to be in excellent condition (Gelman and King 1994a). Because the claims I have presented here only apply to single-member district elections, I drop all multimember districts from the analysis. I then use JudgeIt (Gelman, King, and Thomas 2008) to calculate estimated vote shares and seat shares, as well as symmetry and responsiveness, for every state in every election year.[5]

Consistent with Proposition 2, I assume that an appropriate measure of efficiency should reflect changes in seat share when vote share is controlled: in other words, it should identify additional seat share above and beyond what is predicted by vote share. To test this idea, I regress seat share separately on both symmetry and responsiveness, controlling for aggregate vote share. Since symmetry is calculated at an aggregate vote share of 0.5, it becomes a more accurate proxy for efficiency as the actual vote share approaches that value. Thus, I also present a second model for each measure where I interact the predictors with an indicator for elections in which one party received more than 55% of the two-party vote. (This distinction splits the data roughly in half, with 219 (44%) state-election pairs falling in the uncompetitive category.) If the arguments I have made are correct, symmetry ought to predict seat share

TABLE 1
Explaining Seat Share in State Legislative Races with Three Measures of Partisan Advantage, 1970–2003

	Symmetry		Responsiveness	
	(1)	(2)	(1)	(2)
Partisan Advantage Measure	0.246***	0.731***	0.010**	0.000
	(0.036)	(0.050)	(0.004)	(0.004)
Vote Share	2.038***	1.797***	2.359***	2.700***
	(0.054)	(0.116)	(0.040)	(0.117)
Uncompetitive = 1	—	0.015**	—	−0.012*
		(0.006)		(0.006)
Advantage × Uncompetitive	—	−0.799***	—	0.031***
		(0.064)		(0.008)
Vote Share × Uncompetitive	—	0.581***	—	−0.254*
		(0.129)		(0.128)
Intercept	−0.010***	0.046	0.000	0.013**
Adjusted R^2	0.898	0.924	0.891	0.895
Root MSE	0.050	0.043	0.051	0.050
N	501	501	501	501

Note: Cell entries are ordinary least-squares coefficients, and estimation was run in R 2.14.0. The dependent variable in each case was two-party seat share. The measure of partisan advantage used in each model is identified in the column heading. Vote share, seat share, and both measures of partisan advantage are mean deviated.
*$p < 0.05$; **$p < 0.01$; ***$p < 0.001$.

poorly, but better in the competitive range of outcomes. That means the interaction model should produce a modest main effect and a strongly negative interaction. Responsiveness, on the other hand, ought to perform poorly in both competitive and uncompetitive elections.

The results can be found in Table 1 and largely conform to expectations. In the first model with no interactions, responsiveness has almost no predictive power (0.010), while symmetry's relationship to seat share is weak (0.246). When the model is split into competitive and uncompetitive elections, the results for responsiveness are unchanged—a zero coefficient becomes 0.031 in uncompetitive elections—but the models for symmetry now suggest an important distinction. Symmetry's predictive power is reasonably good in the competitive range (0.731) but becomes effectively zero and even slightly negative (−0.068) where one party is stronger than the other.

Thus, it appears the concerns raised about symmetry and responsiveness extend beyond hypotheticals and apply to a broad range of actual election results. Responsiveness has particular trouble, but symmetry also struggles outside the relatively narrow range of elections contemplated by the symmetry counterfactual.

An Alternative Measure of Partisan Efficiency

It is possible to imagine a simple measure of efficiency that avoids many of the problems of symmetry and responsiveness and does not require any counterfactual at all. If a party gains seats without any increase in vote share, it ought to be making better use of the votes it wins by claiming its seats with smaller majorities. This suggests the following measure, which we might call *relative wasted votes (ω)* and which is defined as follows for a system with two parties:

$$\omega = (W_O - W_G) \Big/ \sum v_i^t. \tag{2}$$

W_O and W_G are the total surplus votes (in excess of the number required to win) plus the total lost votes (cast for the party in races that party lost) for the opposition and government parties, respectively. W_O and W_G are more formally described as:

$$W_O = \sum Sur_O + \sum Lost_O, \tag{3}$$

$$W_G = \sum Sur_G + \sum Lost_G. \tag{4}$$

Where Sur_G and Sur_O are the surplus votes, and $Lost_G$ and $Lost_O$ the lost votes, for the government and opposition parties. The difference between these numbers should give a sense of relative advantage in wasted votes. This number is then divided by the total number of votes cast in all districts to standardize the value for comparison across electoral systems. In the special case where there are only two parties and all districts are equal in population—a set of constraints virtually universal in the research on symmetry and responsiveness[6]—Equation (2) reduces to an even simpler form:

$$\omega = S_{m\arg} - 2 * V_{m\arg}. \tag{5}$$

Where

$$S_{marg} = S_G - 0.5, \tag{6}$$

$$V_{marg} = V_G - 0.5. \tag{7}$$

The derivation of Equation (5) can be found in Appendix B. In words, the equation states that ω is equal to G's excess seat share over 0.5, minus twice its excess vote share over 0.5. Thus, the balance of wasted votes in Equation (5) will be equal whenever the majority's margin in seats is twice its margin in votes.

Equation (5) is consistent with Proposition 2. When vote share is constant as Proposition 2 requires, V_{marg} is also constant, and ω can only be changed by altering the seat share. Furthermore, when the seat share changes without any change in vote share, ω will change by the same amount, and this rate of change is constant across all values of V_{marg}.[7] Thus, for an SMD system with the characteristics identified, ω is an exact measure for the changes in seat share contemplated by Proposition 2. Indeed, if ω is inserted into the basic model in Table 1, it perfectly predicts seat share with a coefficient of 1.0 for relative wasted votes and 2.0 for vote share.[8]

It is not essential to measure relative wasted votes with the precise functional form in Equation (2). But since any alternative must still reliably identify when Party G has more wasted votes than Party O, the number of options is limited. The most obvious alternative is to use the ratio of wasted votes (i.e., W_O/W_G) rather than the difference, since the ratio still identifies the key condition of equivalence between the parties (in this case, when the ratio is equal to 1.0). But the ratio is a nonlinear function, and so it adds needless complexity without improving on ω in other respects. In fact, *even using the ratio, wasted votes are equal when the majority party's margin in seats is twice its margin in votes*. The proof for this claim can be found in Appendix B. It suggests that, far from a quirk of ω, a "twice vote margin" rule of thumb for the equivalence of wasted votes may be a fundamental feature of this sort of SMD system.

In addition to its other advantages, ω is also flexible. It can be applied to any redistricting system, regardless whether the parties are competitive with one another or not. By contrast, analysts are cautioned to avoid using symmetry unless the actual aggregate seat or vote share is close to 0.5 (Gelman and King 1994a; Grofman and King 2007).

Moreover, ω actually collapses on symmetry when the aggregate vote share is 0.5: if that value is substituted into Equation (5), the V_{marg} term disappears and only the difference between seat share and 0.5 remains. If an aggregate vote share of 0.5 is a substantively interesting value, then it can be used with the confidence that the results will be identical to symmetry; otherwise, the actual outcome or some more realistic counterfactual may be used instead.

ω is not without limitations in extreme cases. When the majority holds all the seats (i.e., $S_{marg} = 1$), any additional votes will actually reduce the party's efficiency advantage and even put it at a disadvantage for any vote share above 0.75. This property of ω is strange and arguably violates a sense that a measure of party advantage should always increase as a party grows stronger. Strictly speaking, however, it does not violate Proposition 2, which concerns changes in seat share for a constant vote share, not vice versa. Indeed, once a party wins all available seats, additional votes are wasted by definition: they do not contribute to additional victories. More important, the levels of partisan dominance required to produce this result are highly unusual. In the legislative elections data, no party has ever won all the seats or even won more than 75% of the vote. By contrast, majorities between 55 and 75% of the vote—which cause serious problems for symmetry but raise no issues for relative wasted votes—are quite common and comprise close to half the data set (44%).

Equation 5 also demonstrates that vote share, seat share, and efficiency are linked: the only way to change seat share for a constant vote share is to alter the distribution of wasted votes between the parties. Thus, ω is both a tally of all the cracking and packing decisions in a given SMD system and a composite measure of aggregate vote and seat share. Far from standing above the political fray, efficiency is and ought to be sensitive to changes in vote share. Symmetry and responsiveness assume that such changes in vote share—especially uniform shifts—are irrelevant for partisan bias. This is a design decision that reflects nothing inherent in the concept of efficiency and can in fact misrepresent efficiency in at least some circumstances, as the earlier discussion makes clear. Even the simple notion that a partisan gerrymander might come "undone" in the face of contrary partisan tides becomes highly unlikely if symmetry or responsiveness is the measure of bias. Unless the tide is stronger in some seats than others—and thereby alters the shape of the seats-votes curve—these measures will consider the change to be irrelevant for evaluating the plan's bias. The measures will thereby suggest stability in efficiency where none may in fact exist.

Partisan Control and Partisan Bias

Thus far I have presented theoretical and empirical evidence that both symmetry and responsiveness are poor measures of efficiency and offered an alternative to replace them. In this section, I apply this new measure to a key dispute in the literature and demonstrate how it alters our understanding of the relevant dynamics.

A long-standing argument in the redistricting literature concerns whether control of the redistricting process matters for the party that possesses it. Most politicians and journalists, and many political scientists, assume that a party with unfettered control will successfully exploit its position to maximize its seat share, and several empirical studies have presented evidence for precisely this result (Engstrom 2006; McDonald 2004). At the same time, many scholars have questioned the link between party control and partisan outcomes. A party in control wants to maximize its seat share, but its incumbents also want safe seats that will protect their political careers. These goals are in tension, since safe seats necessarily waste votes that could be distributed more efficiently elsewhere (Cain 1984). Even if the party seeks to maximize seats, it is not always clear that the plan will produce the results the party expects (Born 1985; Cain 1985; Campagna and Grofman 1990; Niemi and Jackman 1991). The more fluid the party support in the electorate, the less reliable the gerrymander.

Gelman and King summarize the debate this way: "one side holds that gerrymanderers. . .maximize only their party's seat advantage and have a large and lasting effect, while the other argues that whatever gerrymanderers maximize, they have only a small and transitory effect" (1994a, 543). Gelman and King (GK) offer a truce between these two perspectives, arguing that each is partly right. The "large and lasting effect" group is correct that the party in control of redistricting shifts bias in its favor, and that "the effect is substantial and fades only very gradually over the following 10 years" (543); the "small and transitory effect" group is right in the sense that a partisan redistricting plan still upsets established political relationships enough to leave the total level of bias lower than before.

To reach these conclusions, GK develop a parsimonious model where partisan bias is regressed on its lag, an indicator for redistricting years, the interaction between these two variables, an indicator for the party in control of the redistricting process (−1 Republican, 0 bipartisan/nonpartisan, 1 Democratic), and a set of state fixed effects. Formally, the model can be written as follows for state i in year t:

$$Y_{it} = \alpha Y_{i,t-1} + \beta R_{it} + \gamma(Y_{i,t-1} * R_{it}) + \delta P_{it} + s_i + \varepsilon_{it}, \quad (8)$$

where $Y_{i,t-1}$ is the lagged dependent variable, R_{it} is the redistricting dummy, P_{it} is party control, s_i is the set of state fixed effects, ε_{it} is the error term, and α, β, γ, and δ are coefficients to be estimated. α measures the endurance of a plan's bias through time, γ shows how well redistricting disrupts this stability, and δ captures the tendency for a party to skew redistricting in its favor, independent of these other factors. When they estimate this equation using symmetry, GK find a positive δ and a strong and positive α, which says that partisan gerrymandering is real and persists through the life of a redistricting plan. But they also find a strongly negative γ, meaning redistricting so thoroughly "resets" the system as to decrease bias overall.

The main limitation of this analysis as it pertains to efficiency is that it uses symmetry as the measure of bias. As we have seen, symmetry discounts shifts in aggregate vote share by design and focuses instead on the relative distribution of votes across seats as the phenomenon of interest. This makes symmetry more stable within redistricting cycles, where uniform shifts in vote share are more common, but also less stable between cycles, where changes in the distribution prevail. In short, structural features of symmetry incline the measure toward finding the very pattern reported by Gelman and King, whether this pattern reflects actual seat gains or not.

In Table 2, I have updated GK's symmetry model with legislative data through 2003. I use their original coding of party control for the 1972 and 1982 redistricting cycles and then two additional sources for the 1992 and 2002 redistricting years that have been added to the analysis.[9] GK run their model on only 16 states, after omitting those where one party did not win either a vote or a seat majority at any point in the study period.[10] If we follow this approach and limit the analysis to the same 16 states, we confirm GK's basic result: a strong lagged effect ($\alpha = 0.756$), a modest party control effect ($\delta = 0.017$), and a strong interaction ($\gamma = -0.316$). The interaction here suggests that redistricting disrupts almost half the legacy effect from one plan to the next. Yet when the same model is run with relative wasted votes, a different story emerges. A similar party-control effect is still visible ($\delta = 0.020$), but the lagged effect is about a third the size ($\alpha = 0.256$).[11] Furthermore, the interaction term is small and statistically insignificant (though still negative), suggesting that there is little about redistricting that is especially disruptive.

The remaining columns in Table 2 expand the model to include all the states in the sample, thus introducing a comparatively uncompetitive

TABLE 2
Effects of Redistricting on Symmetry and Relative Wasted Votes in State Legislative Elections, 1970–2003

	Competitive States		All States	
	Symmetry	ω	Symmetry	ω
Lagged symmetry/ω (α)	0.752***	0.256***	0.684***	0.227***
	(0.054)	(0.076)	(0.055)	(0.060)
Redistricting year (β)	–0.018*	–0.006	–0.016**	–0.003
	(0.007)	(0.007)	(0.006)	(0.005)
Redistricting year × lagged symmetry/ω (γ)	–0.318**	–0.091	–0.184*	–0.026
	(0.010)	(0.116)	(0.081)	(0.081)
Partisan control (δ)	0.017*	0.020*	0.020**	0.014**
	(0.008)	(0.008)	(0.006)	(0.006)
(State fixed effects)				
Intercept	0.011	0.019	0.009	0.019
Adjusted R^2	0.701	0.375	0.749	0.433
Root MSE	0.044	0.044	0.045	0.041
N	244	244	463	463

Note: Cell entries are ordinary least squares coefficients. Models include state fixed effects and heteroskedastic-consistent standard errors, as estimated in R 2.14.0. Dependent variable is either symmetry or relative wasted votes (ω), depending on the column. "Competitive states" include the 16 identified as such in Gelman and King (1994a).
$^{\#}p < 0.10$; $*p < 0.05$; $**p < 0.01$; $***p < 0.001$.

set of election outcomes into the analysis. For relative wasted votes, the results are broadly similar, although the coefficient on partisan control is somewhat smaller than in the original specification. For symmetry, the additional states actually increase the impact of party control very slightly, while cutting the interaction coefficient almost in half. However, the basic distinction between the two measures remains intact: symmetry suggests a larger effect from redistricting and more persistence through time.

It is also worth noting the significant difference between the two equations in terms of variance explained. The adjusted R^2 for the symmetry model (0.749) is significantly larger than that of the relative wasted-votes model (0.433). Some of this can be explained by the fact that the symmetry variable has a higher variance to begin with, but almost half this difference can be explained by the greater importance of the lag term in the symmetry model. Removing it makes very little difference to the variance explained of the relative wasted-votes model (new $R^2 = 0.403$), but it makes a substantial difference to the variance explained of the symmetry model (new $R^2 = 0.581$). Thus, the greater instability of relative wasted votes, despite its stronger logical connection to efficiency and seat share, remains one of its most important distinctions from symmetry.

Florida's lower house offers a good case study of this fading effect. The redistricting process was controlled by the Democrats in 1991, and they drew a plan that largely appeared to preserve the status quo, albeit marginally improving the party's relative wasted-votes score from 0.03 to 0.04. But the party's seat share dropped through the remainder of the decade, and by the time of the next redistricting cycle, the Republicans were firmly in control with 64% of the seats. Relative wasted votes largely tracked this mid-decade shift, showing a Democratic advantage as high as 0.09 in 1994 that flipped to a Republican advantage of 0.12 in 2000. By contrast, symmetry suggested very little change over the same period: it remained within a narrow range that never gave either party better than a 0.02 advantage.

In sum, relative wasted votes paints a different picture of redistricting than symmetry. Both measures agree that parties seek to use redistricting to shift bias in their favor and that they are successful in these efforts, at least initially. But where symmetry suggests these efforts largely endure until the next redistricting shock, relative wasted votes implies the long-term consequences of a plan are unknowable. Indeed, the estimates suggest the effect of relative wasted votes decays about as rapidly in a single election as the effect of symmetry does in four elections. Drawing new lines adds little to this inherent uncertainty, perhaps because the uncertainty is so substantial in the first place.

Because it translates more directly into seat share, relative wasted votes also gives us better insight into the magnitude of the party-control effect. The coefficients in Table 2 suggest about a 3-point difference in seat share between states where Republicans control redistricting and states where Democrats hold the levers of power. This translates to roughly two to four seats for the lower chambers of most states. While this is a real effect that might have practical political implications, it is not hard to see how it might fade over the course of a redistricting cycle.

On balance, then, these findings support the "small and transitory" perspective on redistricting in contrast to the conclusions one would reach using symmetry. They also cast doubt on any normative benefits that might stem from the supposed power of redistricting to shatter old biases even as it creates new ones. Instead, the normal give and take of elections is probably disruptive enough to overwhelm such benefits.

Discussion

The findings presented here offer reasons to abandon symmetry and responsiveness as measures of partisan efficiency and adopt relative wasted votes (ω). The new measure offers two practical advantages. First, it is more accurate at measuring a core concept of interest in partisan gerrymandering, which is whether one party gains seats through redistricting alone. The seat-gain concept is best evaluated at the actual level of support for each party, not at a counterfactual level the system may or may not reach at any point in the near future. Second, the new measure is available for the entire range of aggregate vote shares a party might expect to receive. Symmetry, by contrast, must be restricted to electoral systems where party control is seriously at issue because the counterfactual is otherwise too unrealistic (Grofman and King 2007).

The new measure also sharpens the link between efficiency and seat share. In the case of two parties and equipopulous districts, there is no distinction between the concepts: a party that gains seat share through redistricting necessarily improves its relative efficiency. Indeed, information about relative efficiency is contained in the seats-votes curve itself: as Equation (5) makes clear, when responsiveness is greater than 2.0, a party's relative efficiency improves as its vote share rises; when it is less than 2.0, a higher vote share has the opposite effect. The literature has always assumed that any level of responsiveness is just as "fair" as any other, but the analysis here casts some doubt on that assumption, at least as concerns wasted votes.

These theoretical insights lead to the key empirical finding of the article. Because efficiency is a function of both vote share and seat share, it is sensitive to changes in party performance. Thus, the effects of partisan gerrymanders, though real, are easily undone. The partisan legacy of the last plan is usually gone by the following redistricting, and redistricting itself is not as disruptive a force as symmetry would suggest. This raises important questions about the utility of court intervention in partisan gerrymandering cases. If partisan gerrymandering has a large, enduring effect, then it might be important for the courts to insert themselves into the process to prevent the advantages a party might otherwise obtain. But if the effects of gerrymandering, though real, are small and ephemeral, court intervention in such an inherently political process might do more harm than good.

One might object that bias ought to capture something immutable about a redistricting plan, or at least something independent of the back and forth of partisan tides, and that symmetry should be preferred for that reason. However, the concept of interest here—partisan efficiency—

requires identifiable and reliable partisan voters. If a large number of voters can change their minds—whether idiosyncratically or as part of a broad partisan tide—their support cannot be counted on and the expected gains from redistricting will never materialize. A measure should not force stability in such circumstances, or it risks ignoring some of the very properties it is meant to test.

Gerrymandering can certainly embrace a wider range of goals than strict efficiency, and it is worth considering some here. Owen and Grofman (1988) incorporate uncertainty into their model of gerrymandering, showing that a smart redistricting party will avoid cutting its margins too close in the districts it wins for fear of losing those seats in future partisan tides and limiting its expected efficiency gains. Friedman and Holden (2008) have extended this logic to situations where gradations of support in the electorate can be identified, demonstrating that under those conditions, it is not sensible to create a handful of districts that the opposing party is guaranteed to win.[12] Though these models do refine the goals of a classic gerrymander, their implications for the discussion here should not be overstated. Models that account for uncertainty do not discount efficient seat maximization as a goal; rather, they offer different tactics for achieving that end, and they emphasize that what matters most is the expected value of efficiency across a number of elections. Since maximizing seat share for a given share of the vote is still the goal, relative wasted votes ought to be a good measure of the concept. Indeed, I have argued at length that relative wasted votes is better than symmetry at capturing the consequences of this sort of uncertainty.

Another possible goal comes from Yoshinaka and Murphy (2009), who note that a gerrymandering party will often destabilize the support base for opposing-party incumbents, even if it gains no additional seats as a result. This is an important point and a key secondary goal of a gerrymander. Nonetheless, in making this point, Yoshinaka and Murphy do not dismiss efficiency as an objective of partisan gerrymandering. On the contrary, they explicitly test for it, albeit with different measures than the one I have offered here. It is conceivable that they might have conducted that portion of their study using relative wasted votes instead.

Finally, one could certainly argue that symmetry offers a distinct and valuable notion of fairness—that equal parties should be treated equally—which should not be discarded lightly. Because an asymmetric system can allow a party to maintain its chamber majority without winning a majority of the vote, it raises special normative concerns that will likely always be relevant to discussions of redistricting. That said, even if symmetry remains a valuable normative concept, it may be more fruitful to treat it as a special case of relative wasted votes, since the two measures collapse on each other at $V = 0.5$. Rather than calculate relative wasted votes and symmetry separately, an analyst could calculate relative wasted votes across a range of substantively meaningful values. If it seems possible for the minority party to claim half the votes in a future election, then that outcome could be part of the range of values that is explored, and symmetry would therefore be part of the discussion. If not, then it need not be included. Again, the point is not to dismiss symmetry as a normative concept but to suggest that an important aspect of it is already captured in the broader notion of wasted votes.

Regardless, if the goal is to measure efficiency, then the twin concepts of symmetry and responsiveness look to be inappropriate for many SMD electoral systems. The measure I have offered here—relative wasted votes—is arguably a more valid and flexible measure of the concept of efficiency in most cases, and it subsumes symmetry at a point in the seats-votes curve where symmetry's counterfactual is often evaluated. Most important, using relative wasted votes in place of symmetry offers us a different understanding of substantive questions about redistricting. If the traditional measures are used to evaluate efficiency, it should only be with great caution and significant caveats, because doing so could in many cases lead analysts to the wrong conclusions.

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APPENDIX A

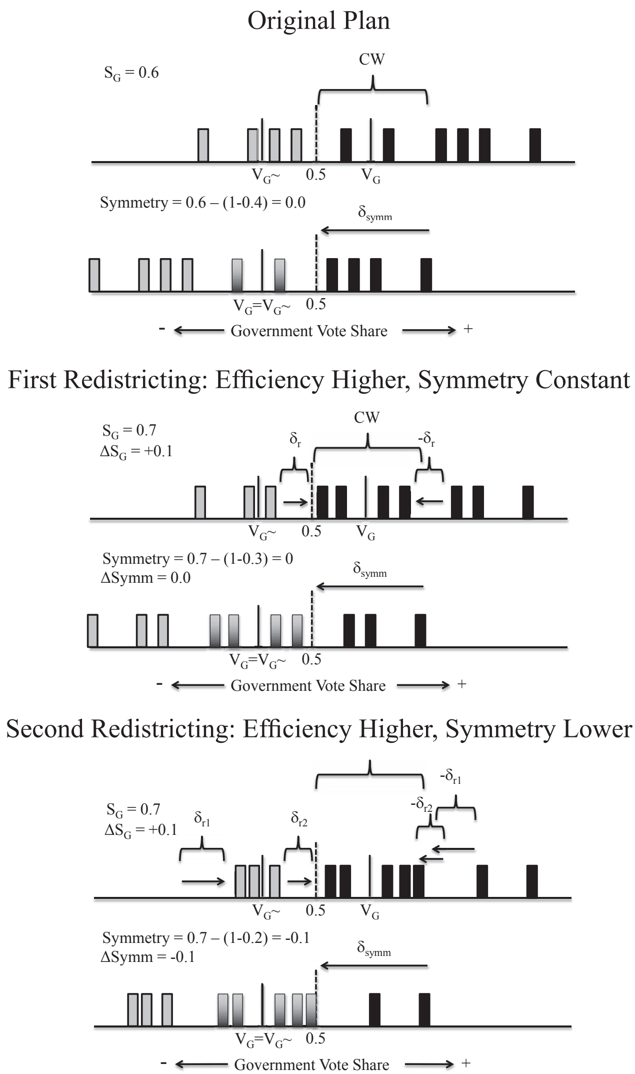
Alternative Measure of Symmetry and Its Consistency with Proposition 2

Symmetry is sometimes calculated as the difference between the seat share that the government receives and the share the opposition would receive for the same share of the vote. If S_G is the share of seats received by the government for an aggregate vote share V_G, let $V_{\tilde{G}} = 1 - V_G$ and $S_{\tilde{G}}$ be the share of seats the opposition would receive for $V_{\tilde{G}}$. This alternative measure of symmetry is simply the difference between the two seat shares: $S_G - S_{\tilde{G}}$. Negative values favor the opposition, while positive values favor the government. A possible configuration for these values is in the first panel of Figure A1.

Consider now two scenarios whose results for this version of symmetry mimic the results for the version of symmetry presented in the main text:

1) *Panel 2, Figure A1: Both a seat held by Party G and a seat held by Party O move into the CW, but only the seat held by Party O crosses the win-loss threshold.* Party

FIGURE A1
An Alternative Measure of Symmetry in Three Hypothetical Plans



Note: Each pair of figures corresponds to a scenario described in the text. The top figure is the actual election outcome and the bottom figure the outcome under the symmetry counterfactual. δ_{symm} is the shift in votes required for the symmetry counterfactual. δ_r is the shift in votes for particular districts from redistricting. ΔS_G is the change in the government's share of seats relative to the baseline redistricting plan in the first panel, and ΔSymm is the change in symmetry relative to the same baseline plan. All other values are defined in the text.

G gains a seat and so improves its relative efficiency, but there is no change in symmetry.

2) *Panel 3, Figure A1: One uncompetitive seat held by Party O becomes more competitive, one seat held by O changes hands, and two Party G seats move into the CW.* Party G increases its relative efficiency, but symmetry suggests Party O improves its situation instead.

APPENDIX B

Deriving and Exploring Equation 5

Here I offer the derivation of Equation 5 in the text. When there are only two parties and each district has exactly the same number of voters, proportions can be substituted for raw votes in all of the formulas. In this case, the total vote in each district becomes equal to 1.0, so,

$$\sum_1^{d_t} V_i^t = d_t \tag{B1}$$

where d_t is simply the total number of districts in the electoral system. Moreover, surplus and lost votes become:

$$Sur_O = \sum_1^{d_O} (0.5 - V_i^G) = 0.5 * d_O - \sum_1^{d_O} V_i^G \tag{B2}$$

$$Sur_G = \sum_1^{d_G} (V_i^G - 0.5) = \sum_1^{d_G} V_i^G - 0.5 * d_G \tag{B3}$$

$$Lost_O = \sum_1^{d_G} (1 - V_i^G) = d_G - \sum_1^{d_G} V_i^G \tag{B4}$$

$$Lost_G = \sum_1^{d_O} V_i^G \tag{B5}$$

where d_O is the total number of districts won by the opposition party, and d_G is the same for the government. Wasted votes then become:

$$W_O = 0.5 * d_O - \sum_1^{d_O} V_i^G + d_G - \sum_1^{d_G} V_i^G \tag{B6}$$

$$W_G = \sum_1^{d_G} V_i^G - 0.5 * d_G + \sum_1^{d_O} V_i^G \tag{B7}$$

Substituting into Equation (1), the formula for ω becomes:

$$\begin{aligned}\omega &= (W_O - W_G)\Big/\sum_1^{d_t} V_i^t \\ &= \left\{\left(0.5 * d_O - \sum_1^{d_O} V_i^G + d_G - \sum_1^{d_G} V_i^G\right) - \left(\sum_1^{d_G} V_i^G - 0.5 * d_G + \sum_1^{d_O} V_i^G\right)\right\}\Big/d_t \\ &= \left\{0.5 * (d_O + d_G) - 2 * \left(\sum_1^{d_O} V_i^G - \sum_1^{d_G} V_i^G\right) + d_G\right\}\Big/d_t\end{aligned} \quad \text{(B8)}$$

Because there are only two parties, and because total votes equals total seats, we can make three additional substitutions:

$$d_t = d_O + d_G \quad \text{(B9)}$$

$$S_G = \frac{d_G}{d_t} \quad \text{(B10)}$$

$$V_G = \left(\sum_1^{d_O} V_i^G + \sum_1^{d_G} V_i^G\right)\Big/d_t \quad \text{(B11)}$$

Equation B9 states that the government and opposition districts comprise the total universe of seats. Equation B10 is a simple identity: the government's share of seats is equal to the number of seats it holds divided by the total number of seats. Equation B11 states that the government's average vote share across all districts is equal to the sum of its shares in seats it holds plus its shares in seats it does not hold, divided by the total number of seats. Rearranging and substituting into Equation (B8), we obtain:

$$\omega = 0.5 - 2 * V_G + S_G$$

If we define $V_{marg} = V_G - 0.5$ and $S_{marg} = S_G - 0.5$, then this rearranges into:

$$\begin{aligned}\omega &= 0.5 - 2 * (V_{marg} + 0.5) + S_G \\ &= 0.5 - 2 * V_{marg} - 1 + S_G \\ &= S_G - 0.5 - 2 * V_{marg} \\ &= S_{marg} - 2 * V_{marg}\end{aligned} \quad \text{(B12)}$$

Equation (B12) is exactly equal to Equation (5) in the text, and it suggests that wasted votes are always equal when one party holds a margin in seats twice as large as its margin in votes (i.e., when the expression is equal to zero, indicating no difference in wasted votes).

As also noted in the text, this core result holds if ω is expressed as the ratio of wasted opposition votes to wasted government votes, rather than the difference. The precise values of ω would change with such a measure, but wasted votes will still be equal when the margin in seats is twice the margin in votes. For the proof, first imagine a measure ω* such that

$$\omega^* = \left(W_O \Big/ \sum_1^{d_t} V_i^t\right) \Big/ \left(W_G \Big/ \sum_1^{d_t} V_i^t\right)$$

From Equations (B6), (B7), (B9), and (B10), and from the fact that $\frac{d_O}{d_t} = S_O = 1 - S_G$, it follows that

$$\begin{aligned}\omega^* &= \frac{\left(0.5 * d_O - \sum_1^{d_O} V_i^G + d_G - \sum_1^{d_G} V_i^G\right) \Big/ d_t}{\left(\sum_1^{d_G} V_i^G - 0.5 * d_G + \sum_1^{d_O} V_i^G\right) \Big/ d_t} \\ &= \frac{0.5 - 0.5 * S_G - V_G + S_G}{V_G - 0.5 * S_G} = \frac{0.5 - V_G + 0.5 * S_G}{V_G - 0.5 * S_G}\end{aligned} \tag{B13}$$

Substituting $V_{marg} + 0.5$ for V_G and $S_{marg} + 0.5$ for S_G and reducing yields

$$\omega^* = \frac{0.5 * S_{m\arg} + 0.25 - V_{m\arg}}{V_{m\arg} + 0.5 - 0.5 * S_{m\arg} - 0.25} \tag{B14}$$

Unlike Equation (5), we must set Equation (B14) equal to 1.0 to identify the point when wasted votes are equal for each party. This leads to the following

$$\frac{0.5 * S_{m\arg} + 0.25 - V_{m\arg}}{V_{m\arg} + 0.5 - 0.5 * S_{m\arg} - 0.25} = 1$$

$$0.5 * S_{m\arg} + 0.25 - V_{m\arg} = V_{m\arg} + 0.5 - 0.5 * S_{m\arg} - 0.25$$

$$S_{m\arg} = 2 * V_{m\arg}$$

While the ratio approach does confirm the basic finding that seat margin is twice the vote margin when wasted votes are equal, outside this point of equivalence the equation is far more awkward to use. The partial derivative of Equation (B13) with respect to S_G (which identifies how ω* changes under the key condition that seat-share changes without a change in vote share) is $\frac{1}{4 * (V_G - 0.5 * S_G)^2}$, making it both nonlinear and dependent on V_G. The same derivative for Equation (5) is a constant of 1.0, meaning a one-unit change in seat share always leads to a one-unit change in ω. Equation (5) is therefore a far cleaner approach that communicates the same information about which party is advantaged while also offering substantively useful information about the consequences of that advantage for seat share.

NOTES

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1. An SMD system need not always require a plurality. A runoff system, for instance, would ultimately require a clear majority to win. But the logic of an efficient seat-maximizing gerrymander is the same regardless.

2. See, e.g., Taagepera and Shugart (1989; chaps. 10 and 14) for a relevant discussion of such measures.

3. Strictly speaking, neither symmetry nor responsiveness need be completely hypothetical. In place of a uniform vote shift, some scholars have regressed actual seat share on actual vote share across several elections and then used this model's prediction at 0.5 as a measure of symmetry and its coefficient as a measure of responsiveness. This approach has its limitations (see, e.g., Grofman and King 2007), but it does have the advantage of rooting the estimates in data as much as possible. Nonetheless, the two approaches are more similar than different. If we assume that the only source of change in aggregate vote share is a uniform partisan tide—that is, if the distribution of votes across seats remains constant from one election to the next—then the two approaches are identical. If the distribution is not constant in this way, then it is not clear whether this approach to symmetry is measuring anything related to redistricting at all. Moreover, the approach still requires a hypothetical extrapolation if the vote share in a given electoral system never crosses 0.5 during the study period.

4. The logic of these two scenarios applies when symmetry is calculated as the difference between seat share and vote share at an aggregate vote share of 0.5. Slightly different logic applies if symmetry is instead a comparison between the seat share one party holds and the seat share the opposing party *would* hold *if* it held the same share of votes. With that version of symmetry, the examples above do not always violate Proposition 2. However, it is easy to imagine slightly different examples where this second version of symmetry does violate Proposition 2, so it is no less problematic as a functional measure of efficiency. These examples are described in greater detail in Appendix A.

5. JudgeIt requires a model of elections to improve estimates and reduce error: I use *incumbency* (–1 Republican, 0 open, 1 Democratic), *party control* (–1 Republican, 1 Democratic), and *uncontestedness* (–1 uncontested Republican, 0 contested, 1 uncontested Democrat) as predictors, plus lagged vote share in years where no redistricting has occurred. I apply JudgeIt's default setting for uncontested races, which assigns uncontested Republicans a vote share of 0.25 and uncontested Democrats a vote share of 0.75. In redistricting years, I presume that a seat is held by the party coded as an incumbent in the data and otherwise by the party that held the same seat number before the redistricting. JudgeIt pools error across several elections to improve estimates, so I have also dropped states in years where there is not enough data for JudgeIt to function properly. After these purges, the data include 38 states, with 501 state-election year pairs.

The states are Alabama, Alaska, California, Colorado, Connecticut, Delaware, Florida, Georgia, Hawaii, Illinois, Indiana, Iowa, Kansas, Kentucky, Massachusetts, Maine, Michigan, Minnesota, Missouri, Montana, Mississippi, North Carolina, Nevada, New York, New Mexico, Ohio, Oklahoma, Oregon, Pennsylvania, Rhode Island, South Carolina, Tennessee, Texas, Utah, Virginia, Wisconsin, West Virginia, and Wyoming.

6. This necessarily assumes away differences in efficiency due to turnout. Ignoring turnout differences in this way is legally mandated for redistricting in the United States, but turnout variation is still a worthy topic of study. In fact, future research could use Equation (2) instead of Equation (5) to explore the subject. For more on turnout effects in SMD elections, see Campbell (1996) and Grofman, Koetzle, and Brunell (1997).

7. More formally, the partial derivative of ω with respect to S_{marg} is a constant of 1.0.

8. This can be seen by solving Equation (5) for seat share, which produces the equivalence $\omega + 2 * V_G - 0.5 = S_G$. I also reran the models from Table 1 with relative wasted votes, and the results confirmed these expectations.

9. For the 1992 redistricting cycle, I rely on a report from the National Conference of State Legislatures (1989) from just prior to the redistricting that spells out the process for drawing legislative plans in each state. I then use information about legislative and gubernatorial partisanship from the redistricting year to assess partisan control. For the 2002 redistricting cycle, I use the coding provided by McDonald (2004).

10. The 16 states are California, Colorado, Connecticut, Delaware, Florida, Iowa, Kansas, Michigan, Montana, Nevada, New York, Ohio, Pennsylvania, Tennessee, Utah, and Wisconsin. Though Gelman and King (1994a) refer to this subset as competitive, many of these states had mean vote shares well beyond the competitive range at some point in the study period. Thus, for many of these states, the symmetry hypothetical is probably unrealistic. Likewise, since the data here extend Gelman and King's original time series by about 14 years, many more states fall into the competitive category as they define it, further blurring the distinction.

11. The standardized versions of these lagged coefficients are virtually identical.

12. However, in the case where a gerrymanderer can only measure support as a binary concept, the Friedman and Holden (2008) model collapses on the Owen and Grofman (1988) model, and some degree of packing becomes optimal.

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A New Automated Redistricting Simulator Using Markov Chain Monte Carlo*

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Abstract

Legislative redistricting is a critical element of representative democracy. A number of substantive scholars have used simulation methods to sample redistricting plans under various constraints in order to assess their impacts on partisanship and other aspects of representation. However, surprisingly few simulation methods exist in the literature, and the standard algorithm has no theoretical justification. To fill this gap, we propose a new automated redistricting simulator using Markov chain Monte Carlo. We formulate redistricting as a graph-cut problem and adopt the Swendsen-Wang algorithm for sampling contiguous districts. We then extend this basic algorithm to incorporate various constraints including equal population and geographical compactness. Finally, we apply simulated and parallel tempering to improve the mixing of the resulting Markov chain. The proposed algorithms, therefore, are designed to approximate the population of redistricting plans under various constraints. Through a small-scale validation study, we show that the proposed algorithm outperforms the existing standard algorithm. We also apply the proposed methodology to the data from New Hampshire and Mississippi. The open-source software is available for implementing the proposed methodology.

Keywords: gerrymandering, graph cuts, Metropolis-Hastings algorithm, simulated tempering, parallel tempering, Swendsen-Wang algorithm

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1 Introduction

Legislative redistricting is a critical element of representative democracy. Previous studies have found that redistricting influences turnout and representation (e.g., Abramowitz, 1983; Gelman and King, 1994; Ansolabehere *et al.*, 2000; McCarty *et al.*, 2009; Barreto *et al.*, 2004). From a public policy perspective, redistricting is potentially subject to partisan gerrymandering. After the controversial 2003 redistricting in Texas, for example, Republicans won 21 congressional seats in the 2004 election (Democrats won 11) whereas they had only 15 seats in 2002 (Democrats won 17). To address this concern, numerous remedies, including geographical compactness and partisan symmetry requirements, have been proposed (see Grofman and King, 2007; Fryer and Holden, 2011, and references therein).

The development of automated redistricting algorithms, which is the goal of this paper, began in the 1960s. Vickrey (1961) argued that such an "automatic and impersonal procedure" can eliminate gerrymandering (p. 110). After *Baker v. Carr* (1962) where the Supreme Court ruled that federal courts may review the constitutionality of state legislative apportionment, citizens, policy makers, and scholars became interested in redistricting. Weaver and Hess (1963) and Nagel (1965) were among the earliest attempts to develop automated redistricting algorithms (see also Hess *et al.*, 1965). Since then, a large number of methods have been developed to find an *optimal* redistricting plan for a given set of criteria (e.g., Garfinkel and Nemhauser, 1970; Browdy, 1990; Bozkaya *et al.*, 2003; Chou and Li, 2006; Fryer and Holden, 2011). These optimization methods serve as useful tools when drawing district boundaries (see Altman *et al.*, 2005, for an overview).

However, the main interest of substantive scholars has been to characterize the *distribution* of possible redistricting plans under various criteria for detecting instances of gerrymandering and understanding the causes and consequences of redistricting (e.g., Engstrom and Wildgen, 1977; O'Loughlin, 1982; Cirincione *et al.*, 2000; McCarty *et al.*, 2009; Chen and Rodden, 2013). In 42 of the 50 U.S. states, for example, state politicians control the redistricting process and approve redistricting plans through standard statutory means. Therefore, an important institutional and policy policy question is how to effectively constrain these politicians through means such as compactness requirements (e.g., Niemi *et al.*, 1990), in order to prevent the manipulation of redistricting for partisan ends. Simulation methods allow substantive scholars to answer these questions by approximating distributions of possible electoral outcomes under various institutional constraints.

Yet, surprisingly few simulation algorithms exist in the methodological literature. In fact, most, if not all, of these existing studies use essentially the same Monte Carlo simulation algorithm where a geographical unit is randomly selected as a "seed" for each district and then neighboring units are added to contiguously grow this district until it reaches the pre-specified population threshold (e.g., Cirincione *et al.*, 2000; Chen and Rodden, 2013). Unfortunately, no theoretical justification is given for these existing simulation algorithms, and some of them are best described as ad-hoc. A commonly used algorithm of this type is proposed by Cirincione *et al.* (2000) and implemented by Altman and McDonald (2011) in their open-source software. We hope to improve this state of the methodological literature.

To fulfill this methodological gap, in Section 2, we propose a new automated redistricting simulator using Markov chain Monte Carlo (MCMC). We formulate the task of drawing districting boundaries as the problem of graph-cuts, i.e., partitioning an adjacency graph into several connected subgraphs. We then adopt a version of the Swendsen-Wang algorithm to sample contiguous districts (Swendsen and Wang, 1987; Barbu and Zhu, 2005). We further extend this basic algorithm to incorporate various constraints commonly imposed on redistricting plans, including equal population requirements and geographical compactness. Finally, we apply simulated and parallel tempering to improve the mixing of the resulting Markov chain (Marinari and Parisi, 1992; Geyer and Thompson, 1995). Therefore, unlike the existing algorithms, the proposed algorithms are designed to yield a representative sample of redistricting plans under various constraints. The open-source software, an R package `redist`, is available for implementing the proposed methodology (Fifield *et al.*, 2015).

In Section 3, we conduct a small-scale validation study where all possible redistricting plans under various constraints can be enumerated in a reasonable amount of time. We show that the proposed algorithms successfully approximate this true population distribution while the standard algorithm fails even in this small-scale redistricting problem. We also conduct an empirical study in realistic settings using redistricting and U.S. Census data from New Hampshire and Mississippi. In this case, the computation of the true population distribution is not feasible and so we evaluate the empirical performance of the proposed algorithms by examining several standard diagnostics of MCMC algorithms. Lastly, Section 4 gives concluding remarks.

2 The Proposed Methodology

In this section, we describe the proposed methodology. We begin by formulating redistricting as a graph-cut problem. We then propose a Markov chain Monte Carlo (MCMC) algorithm to uniformly sample redistricting plans with n contiguous districts. Next, we show how to incorporate various constraints such as equal population and geographical compactness. Finally, we improve the mixing of the MCMC algorithm by applying simulated and parallel tempering. A brief comparison with the existing algorithms is also given.

2.1 Redistricting as a Graph-cut Problem

Consider a typical redistricting problem where a state consisting of m geographical units (e.g., census blocks or voting precincts) must be divided into n contiguous districts. We formulate this redistricting problem as that of graph-cut where an adjacency graph is partitioned into a set of connected subgraphs (Altman, 1997; Mehrotra *et al.*, 1998). Formally, let $G = \{V, E\}$ represent an adjacency graph where $V = \{\{1\}, \{2\}, \ldots, \{m\}\}$ is the set of nodes (i.e., geographical units of redistricting) to be partitioned and E is the set of edges connecting neighboring nodes. This means that if two units, $\{i\}$ and $\{j\}$, are contiguous, there is an edge between their corresponding nodes on the graph, $(i, j) \in E$.

Given this setup, redistricting can be seen equivalent to the problem of partitioning an adjacency graph G. Formally, we partition the set of nodes V into n blocks, $\mathbf{v} = \{V_1, V_2, \ldots, V_n\}$ where each block is a non-empty subset of V, and every node in V belongs to one and only one block, i.e., $V_k \cap V_\ell = \emptyset$ and $\bigcup_{k=1}^{n} V_k = V$. Such a partition $\mathbf{v}$ generates an adjacency subgraph $G_\mathbf{v} = (V, E_\mathbf{v})$ where $E_\mathbf{v}$ is a subset of E. Specifically, an edge (i, j) belongs to $E_\mathbf{v}$ if and only if $(i, j) \in E$ and nodes $\{i\}$ and $\{j\}$ are contained in the same block of the partition, i.e., $\{i\}, \{j\} \in V_k$. Because $E_\mathbf{v}$ is obtained by removing some edges from E or "cutting" them, redistricting represents a graph cut problem. Finally, since each resulting district must be contiguous, a valid partition consists of only connected blocks where for any two nodes $\{i\}$ and $\{j\}$ in a connected block $V_k \in \mathbf{v}$, there exists a path of edges within V_k that joins these two nodes. Formally, there exists a set of nodes $\{\{i\} = \{i_0\}, \{i_1\}, \{i_2\}, \ldots, \{i_{m'-1}\}, \{i_{m'}\} = \{j\}\} \subset V_k$ such that, for all $\ell \in \{1, \ldots, m'\}$, $(i_{\ell-1}, i_\ell) \in E_\mathbf{v}$.

Figure 1 presents two illustrative examples, one of which is used in our validation study in Section 3.1. These examples are taken from actual Florida precinct data in

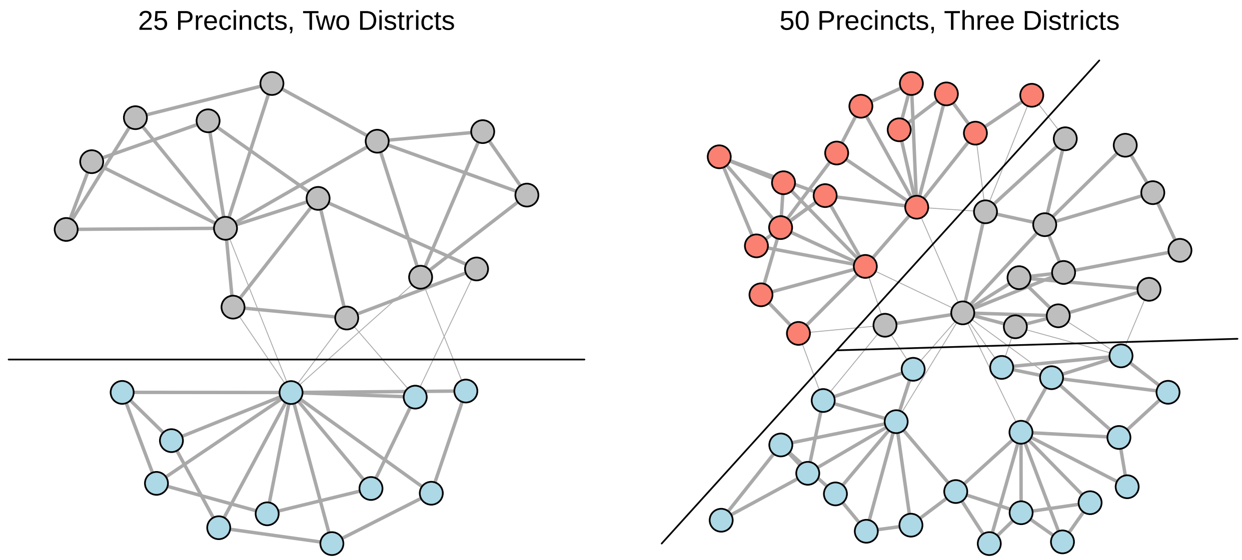

Figure 1: Redistricting as a Graph-cut Problem. A state is represented by an adjacency graph where nodes are geographical units and edges between two nodes imply their contiguity. Under this setting, redistricting is equivalent to removing or cutting some edges (light grey) to form connected subgraphs, which correspond to districts. Different districts are represented by different colors. Two illustrative examples, one of which is used in our validation study in Section 3.1, are given here.

an attempt to create realistic, albeit small, examples. A state is represented by an adjacency graph where nodes are geographical units and edges between two nodes imply their contiguity. The figure demonstrates that redistricting a state into n districts is equivalent to removing some edges of an adjacency graph (light grey) and forming n connected subgraphs.

2.2 The Basic Algorithm for Sampling Contiguous Districts

We propose a new automated simulator to uniformly sample valid redistricting plans with n contiguous districts. The contiguity of valid partitions dramatically increases the difficulty of developing such an algorithm. Intuitive methods for constructing partitions at random – e.g., randomly assigning precincts to districts – have a minuscule chance of yielding contiguous districts, and enumerating all partitions with contiguous districts is too large of a problem to be tractable in realistic redistricting settings. For more discussion, see Section 3.1.

Our MCMC algorithm is designed to obtain a dependent but representative sample from the uniform distribution of valid redistricting plans. In particular, we modify and extend Algorithm 1 of Barbu and Zhu (2005), which combines the Swendsen-Wang algorithm (Swendsen and Wang, 1987) with a Metropolis-Hastings step (Metropolis *et al.*, 1953; Hastings, 1970). This algorithm begins with a valid partition $\mathbf{v}_0$ (e.g., an actual redistricting plan adopted by the state) and transitions from a valid partition $\mathbf{v}_{t-1}$ to another partition $\mathbf{v}_t$ at each iteration t. Here, we describe the basic algorithm for sampling contiguous districts. Later in the paper, we extend this basic algorithm in a couple of important ways so that common constraints imposed on redistricting can be incorporated and the algorithm can be applied to states with a larger number of districts.

Figure 2 illustrates our algorithm using the 50 precinct example with 3 districts given in the right panel of Figure 1. Our algorithm begins by randomly "turning on" edges in $E_{\mathbf{v}_{t-1}}$; each edge is turned on with probability q. In the left upper plot of Figure 2, the edges that are turned on are indicated with darker grey. Next, we identify components that are connected through these "turned-on" edges and are on the boundaries of districts in $\mathbf{v}_{t-1}$. Each such connected component is indicated by a dotted polygon in the right upper plot. Third, among these, a subset of non-adjacent connected components are randomly selected as shown in the left lower plot (two in this case). These connected components are reassigned to adjacent districts to create a candidate partition. Finally, the acceptance probability is computed based on two kinds of edges from each of selected connected components, which are highlighted in the left lower plot: (1) "turned-on" edges, and (2) "turned-off" edges that are connected to adjacent districts. We accept or reject the candidate partition based on this probability.

Our algorithm guarantees that its stationary distribution is equal to the uniform

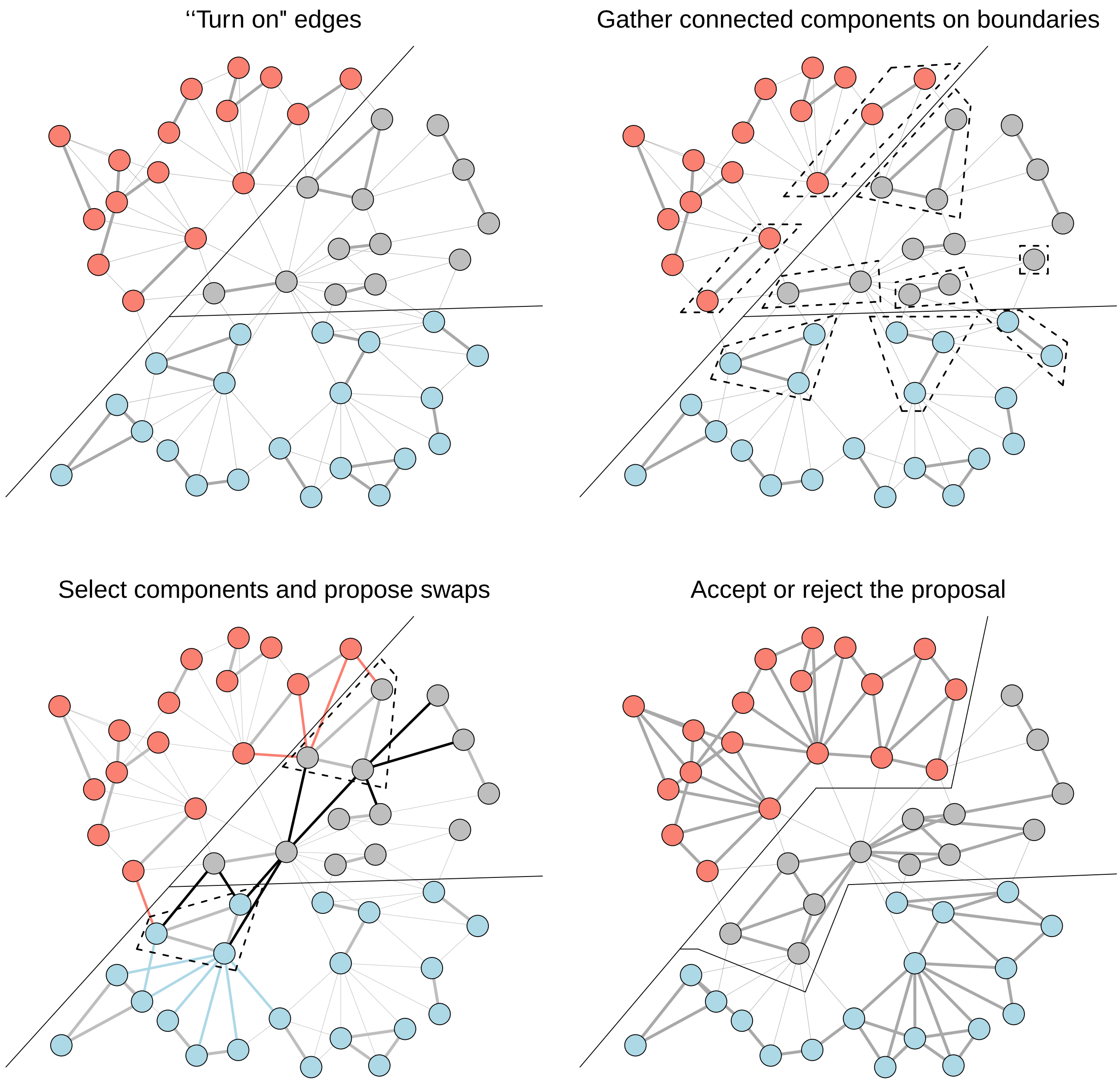

Figure 2: The Basic Algorithm for Sampling Contiguous Districts. The plots illustrate the proposed algorithm (Algorithm 1) using the 50 precinct data given in the right panel of Figure 1. First, in the left upper plot, each edge other than those which are cut in Figure 1 is "turned on" (dark grey) independently with certain probability. Second, in the right upper plot, connected components on the boundaries are identified (dashed polygons). Third, in the left lower plot, a certain number of non-adjacent connected components on boundaries are randomly selected (dashed polygons) and the acceptance ratio is calculated by counting certain edges (colored edges). Finally, in the right lower plot, the proposed swap is accepted using the Metropolis-Hastings ratio.

distribution of all valid partitions, thereby yielding a uniformly sampled sequence of redistricting plans with contiguous districts. We now formally describe this algorithm.

ALGORITHM 1 (SAMPLING CONTIGUOUS REDISTRICTING PLANS) *We initialize the algorithm by obtaining a valid partition* $\mathbf{v}_0 = \{V_{10}, V_{20}, \ldots, V_{n0}\}$ *and then repeat the following steps at each iteration* t*,*

Step 1 ("Turn on" edges): *From the partition* $\mathbf{v}_{t-1} = \{V_{1,t-1}, V_{2,t-1}, \ldots, V_{n,t-1}\}$*, obtain the adjacency graph* $G_{\mathbf{v}_{t-1}} = (V, E_{\mathbf{v}_{t-1}})$*. Obtain the edge set* $E^*_{\mathbf{v}_{t-1}} \subset E_{\mathbf{v}_{t-1}}$ *where each edge* $e \in E_{\mathbf{v}_{t-1}}$ *is independently added to* $E^*_{\mathbf{v}_{t-1}}$ *with probability* q*.*

Step 2 (Gather connected components on boundaries): *Find all components that are connected within* E^*_{t-1} *and adjacent to another block in the partition* $\mathbf{v}_{t-1}$*. Let* C *denote this set of connected components where for all* $C_\ell \in C$*, there exists* $k \in \{1, 2, \ldots, n\}$ *such that* $C_\ell \cap V_{k,t-1} = \emptyset$ *and* $(i, j) \in E$ *for some* $\{i\} \in C_\ell$ *and* $\{j\} \in V_{k,t-1}$*.*

Step 3 (Select non-adjacent connected components): *Randomly select a set of* r *non-adjacent connected components* C^* *from* C *such that* $\mathbf{v}_{t-1} \setminus C^*$ *is a valid partition where each block of nodes* $V_{\ell,t-1} \setminus C^*$ *is connected in* $G_{\mathbf{v}_{t-1}}$*. The sampling is done such that each eligible subset of* C *is selected with equal probability.*

Step 4 (Propose swaps): *Initialize a candidate partition* $\mathbf{v}^*_t = (V^*_{1t}, V^*_{2t}, \ldots, V^*_{nt})$ *by setting* $V^*_{kt} = V_{k,t-1}$*. For each component* $C^*_\ell \in C^*$ *with* $\ell \in \{1, \ldots, r\}$*, find the block* $V_{k,t-1} \in \mathbf{v}_{t-1}$ *that contains* C^*_ℓ*, and let* $A(C^*_\ell, \mathbf{v}_{t-1})$ *denote the set of blocks in* $\mathbf{v}_{t-1}$ *that are adjacent to* C^*_ℓ*, not including the block that contains* C^*_ℓ*. Propose to assign* C^*_ℓ *from block* $V_{k,t-1}$ *to an adjacent block* $V_{j',t-1}$ *with probability* $1/|A(C^*_\ell, \mathbf{v}_{t-1})|$*. If* C^*_ℓ *is assigned to block* $V_{k',t-1}$*, set* $V^*_{k't} = V_{k',t-1} \cup C^*_\ell$ *and* $V^*_{kt} = V_{k,t-1} \setminus C^*_\ell$*. If* $V^*_{kt} = \emptyset$*, go back to Step 3. Observe that, after each proposed swap,* $\mathbf{v}^*_t$ *remains a connected set partition.*

Step 5 (Accept or reject the proposal): *Set*

$$\mathbf{v}_t = \begin{cases} \mathbf{v}^*_t, & \text{with probability} \quad \alpha(\mathbf{v}_{t-1} \to \mathbf{v}^*_t), \\ \mathbf{v}_{t-1}, & \text{with probability} \quad 1 - \alpha(\mathbf{v}_{t-1} \to \mathbf{v}^*_t). \end{cases} \tag{1}$$

The acceptance probability is given by the Metropolis criterion

$$\alpha(\mathbf{v}_{t-1} \to \mathbf{v}^*_t) = \min\left(1, \ (1-q)^{|B(C^*, E_{\mathbf{v}^*_t})| - |B(C^*, E_{\mathbf{v}_{t-1}})|}\right) \tag{2}$$

where $B(C^*, E_{\mathbf{v}}) = \{(i,j) \in E_{\mathbf{v}} : \exists C^*_\ell \in C^*, C^*_\ell \subset V_k \in \mathbf{v}\ s.t.\{i\} \in C^*_\ell, \{j\} \in V_k \setminus C^*_\ell\}$ *denotes the set of edges in* $E_{\mathbf{v}}$ *that need to be cut to form connected components* C^**.*

In the Appendix, we prove the following theorem, which states that if the Markov chain produced by the proposed algorithm is ergodic, the stationary distribution of the chain is uniform on the population of all valid partitions $\Omega(G, n)$ (Tierney, 1994).

THEOREM 1 *If every valid partition can be obtained through a sequence of moves given by Algorithm 1, then the stationary distribution of the resulting Markov chain is uniform on all valid partitions.*

The acceptance ratio given in equation (2) is based on the Metropolis-Hastings detailed balance condition (Metropolis *et al.*, 1953; Hastings, 1970),

$$\alpha(\mathbf{v}_{t-1} \to \mathbf{v}^*_t) \quad = \quad \min\left(1,\ \frac{\pi(\mathbf{v}^*_t \to \mathbf{v}_{t-1})}{\pi(\mathbf{v}_{t-1} \to \mathbf{v}^*_t)}\right), \tag{3}$$

where $\pi(\mathbf{v} \to \mathbf{v}^*)$ denote the probability that, starting from partition $\mathbf{v}$, an iteration of Algorithm 1 described above obtains a candidate partition $\mathbf{v}^*$ through Steps 1–4. Computing numerators and denominators of this ratio separately is combinatorially expensive. However, following Barbu and Zhu (2005), we show in the Appendix that substantial cancellation occurs, yielding a simple expression given in equation (2). Indeed, we only need to find all edges within $E_{\mathbf{v}_{t-1}}$ and $E_{\mathbf{v}^*_t}$ that join a node in a connected component of $C^*_\ell \in C^*$ to a node not contained in the block. Since components in C^* are not adjacent, this will ensure that the node not contained in C^*_ℓ will not be contained in a block in C^*.

Several additional remarks are in order. First, when implementing this algorithm, Step 2 requires the three operations: (1) identify all nodes that form a boundary of multiple partitions by comparing $G_{\mathbf{v}_{t-1}}$ with the original adjacency graph G, (2) identify all connected components that include at least one such node via the breadth-first or depth-first search algorithm, and (3) identify the partition to which each connected component belongs.

Second, in Step 3, we typically choose a positive integer r by randomly sampling it from a distribution with $\Pr(r = 1) > 0$ at each iteration. If $r = 1$, then the ergodicity of the Markov chain is guaranteed but the algorithm moves slowly in the sample space. When $r > 1$, the algorithm can mix faster by proposing multiple swaps. However, depending on the adjacency graph G, the algorithm may fail to reach some valid partitions. Thus, we allow r to take a value greater than 1 while keeping the probability of $r = 1$ positive (e.g., a truncated poisson distribution).

Third, in the original algorithm of Barbu and Zhu (2005), r is set to 1 and instead the authors use a small value of q to create larger connected components. This alternative strategy to improving mixing of the algorithm, though sensible in other settings, is not applicable to the current case. The reason is that larger connected components typically include more units from the interior of each block. This in turn dramatically lowers the acceptance probability.

Finally, while this basic algorithm yields a sample of redistricting plans with contiguous districts, it does not incorporate common constraints imposed on redistricting process, including equal population and geographical compactness. In addition, our experience shows that the algorithm does not scale for states with a medium or larger number of districts. Therefore, we now describe two important modifications to the basic algorithm.

2.3 Constraints and Reweighting

In a typical redistricting process, several additional constraints are imposed. Two most commonly applied constraints are equal population and geographical compactness. We first consider the equal population constraint. Suppose that we use p_i to denote the population size for node $\{i\}$ where the population parity for the state is given by $\bar{p} \equiv \sum_{i=1}^{m} p_i/n$. Then, the population equality constraint can be written as,

$$P_{\mathbf{v}} = \max_{1 \le k \le n} \left| \frac{\sum_{i \in V_k} p_i}{\bar{p}} - 1 \right| \le \delta \tag{4}$$

where δ determines the degree to which one wishes to impose the constraint. For example, $\delta = 0.03$ implies that the population of all districts must be within 3% of the population parity.

Next, we consider the geographical compactness. No consensus exists about the exact meaning of compactness and several alternative definitions have been proposed in the literature (see Niemi *et al.*, 1990). Here, we adopt the measure recently proposed by Fryer and Holden (2011). Let w_i be the population density of node $\{i\}$ and d_{ij} represent the distance between the centroids of nodes $\{i\}$ and $\{j\}$. The measure, which is called the relative proximity index, is based on the sum of squared distances among voters in each district relative to its minimum value. Then, the compactness constraint can be written as,

$$R_{\mathbf{v}} = \frac{\sum_{k=1}^{n} \sum_{i,j \in V_k, i<j} w_i w_j d_{ij}^2}{\min_{\mathbf{v}' \in \Omega(G,n)} \sum_{k=1}^{n} \sum_{i,j \in V'_k, i<j} w_i w_j d_{ij}^2} \le \epsilon \tag{5}$$

where $V'_k \in \mathbf{v}'$, ϵ determines the strength of this constraint, and $\Omega(G, n)$ is the set of all redistricting plans with n contiguous districts. Fryer and Holden (2011) develops an approximate algorithm to efficiently compute the minimum of the sum of squared distances, i.e., the denominator of equation (5). The authors also show that this measure is invariant to geographical size, population density, and the number of districts of a state, thereby allowing researchers to compare the index across different states and time periods.

How can we uniformly sample redistricting plans under these additional constraints? One possibility is to discard any candidate partition that does not satisfy the desired constraints. In Algorithm 1, after Step 4, one could check whether the candidate partition $\mathbf{v}_t^*$ satisfies the constraints and if not go back to Step 3. However, such a strategy often dramatically slows down the algorithm and worsens mixing. Alternatively, researchers could run Algorithm 1 without any modification and then simply discard any sampled redistricting plans that do not meet the constraints. The problem of this approach is that many sampled plans may be discarded when strong constraints are imposed.

To overcome this difficulty, we propose to modify Algorithm 1 in the following manner. We first oversample the redistricting plans that are likely to meet the constraints. This means that fewer sampled plans are discarded due to the failure to satisfy the constraints. We then reweight the remaining valid redistricting plans such that they together approximate the uniform sampling from the population of all valid redistricting plans under the constraints. To do this, we consider the Gibbs distribution from statistical physics,

$$P(\mathbf{v}) \quad = \quad \frac{1}{z(\beta)} \exp\left(-\beta \sum_{V_k \in \mathbf{v}} \psi(V_k)\right) \tag{6}$$

where $\beta \geq 0$ is the inverse temperature and $z(\beta)$ is the normalizing constant. The function $\psi(\cdot)$ is chosen so that it reflects the constraint of interest. For example, we use $\psi(V_k) = |\sum_{i \in V_k} p_i/\bar{p} - 1|$ and $\psi(V_k) = \sum_{i,j \in V_k} w_i w_j d_{ij}^2$ for the equal population and geographical compactness constraints, respectively.

Algorithm 1 can be modified easily to sample from the non-uniform stationary distribution given in equation (6). In particular, we only need to change the acceptance probability in equation (2) of Step 5 to,

$$\alpha(\mathbf{v}_{t-1} \to \mathbf{v}_t^*) \quad = \quad \min\left(1, \ \frac{g_\beta(\mathbf{v}_t^*)}{g_\beta(\mathbf{v}_{t-1})} \cdot (1-q)^{|B(C^*, \mathbf{v}_t^*)| - |B(C^*, \mathbf{v}_{t-1})|}\right) \tag{7}$$

where $g_\beta(\mathbf{v}) \equiv \exp\left(-\beta \sum_{V_k \in \mathbf{v}} \psi(V_k)\right)$. Lastly, we reweight the sampled plans by $1/g_\beta(\mathbf{v})$ to approximate the uniform sampling from the population of all possible valid redistricting plans. If we resample the sampled plans with replacement using this importance weight, then the procedure is equivalent to the sampling/importance resampling (SIR) algorithm (Rubin, 1987).

2.4 Simulated and Parallel Tempering

One major drawback of the reweighting approach is that when each plan is weighted according to equation (6) the algorithm may have a harder time moving through the sample space. We use simulated and parallel tempering to improve the mixing of Algorithm 1 in such situations (Marinari and Parisi, 1992; Geyer and Thompson, 1995). We begin by describing how to apply simulated tempering in this context.

Recall that we want to draw from the distribution given in equation (6). We initialize a sequence of *inverse temperatures* $\{\beta^{(\ell)}\}_{\ell=0}^{r-1}$ where $\beta^{(0)}$ corresponds to the *cold temperature*, which is the target parameter value for inference, and $\beta^{(r-1)} = 0$ represents the *hot temperature* with $\beta^{(0)} > \beta^{(1)} > \cdots > \beta^{(r-1)} = 0$. After many iterations, we keep the MCMC draws obtained when $\beta = \beta^{(0)}$ and discard the rest. By sampling under warm temperatures, simulated tempering allows for greater exploration of the target distribution. We then reweight the draws by the importance weight $1/g_{\beta^{(0)}}(\mathbf{v})$.

Specifically, we perform simulated tempering in two steps. First, we run an iteration of Algorithm 1 using the modified acceptance probability with $\beta = \beta^{(l)}$. We then make another Metropolis-Hastings decision on whether to change to a different value of β. The details of the algorithm are given below.

ALGORITHM 2 (SIMULATED TEMPERING) *Given the initial valid partition* $\mathbf{v}_0$ *and the initial temperature value* $\beta_0 = \beta^{(\kappa_0)}$ *with* $\kappa_0 = r - 1$, *the simulated tempering algorithm repeats the following steps at each iteration* t,

> **Step 1 (Run the basic algorithm with the modified acceptance probability):** *Using the current partition* $\mathbf{v}_{t-1}$ *and the current temperature* $\beta_{t-1} = \beta^{(\kappa_{t-1})}$, *obtain a valid partition* $\mathbf{v}_t$ *by running one iteration of Algorithm 1 with the acceptance probability given in equation* (7)*.*
>
> **Step 2 (Choose a candidate temperature):** *We set* $\kappa_t^* = \kappa_{t-1} - 1$ *with probability* $u(\kappa_{t-1}, \kappa_{t-1} - 1)$ *and set* $\kappa_t^* = \kappa_{t-1} + 1$ *with probability* $u(\kappa_{t-1}, \kappa_{t-1} +$

1) $= 1 - u(\kappa_{t-1}, \kappa_{t-1} - 1)$ *where* $u(\kappa_{t-1}, \kappa_{t-1} - 1) = u(\kappa_{t-1}, \kappa_{t-1} + 1) = 1/2$ *when* $1 \leq \kappa_{t-1} \leq r - 2$, *and* $u(r-1, r-2) = u(0,1) = 1, u(r-1, r) = u(0,-1) = 0$.

Step 3 (Accept or reject the candidate temperature): *Set*

$$\kappa_t = \begin{cases} \kappa_t^*, & \text{with probability } \gamma(\kappa_{t-1} \to \kappa_t^*), \\ \kappa_{t-1}, & \text{with probability } 1 - \gamma(\kappa_{t-1} \to \kappa_t^*) \end{cases} \tag{8}$$

where

$$\gamma(\kappa_{t-1} \to \kappa_t^*) = \min\left(1, \frac{g_{\beta^{(\kappa_t^*)}}(\mathbf{v}_t)\ u(\kappa_t^*, \kappa_{t-1})\ w_{\kappa_t^*}}{g_{\beta^{(\kappa_{t-1})}}(\mathbf{v}_t)\ u(\kappa_{t-1}, \kappa_t^*)\ w_{\kappa_{t-1}}}\right) \tag{9}$$

where w_ℓ *is an optional weight given to each* $l \in \{0, 1, \ldots, r-1\}$.

Much like simulated tempering, parallel tempering is also useful for improving mixing in MCMC algorithms and for sampling from multimodal distributions (Geyer, 1991). Parallel tempering differs from simulated tempering in that instead of varying the temperature within a single Markov chain, we run r copies of Algorithm 1 at r different temperatures, and after a fixed number of iterations we exchange the corresponding temperatures between two randomly selected adjacent chains using the Metropolis criterion. This algorithm has an advantage over Algorithm 2 in that we do not need to choose the prior probability of β, which typically has a significant effect on the mixing performance. However this advantage comes at the expense of increased computation as we are now running r chains instead of just one.

The nature of parallel tempering suggests that it should be implemented in a parallel architecture, which can be used to minimize computation time. Altekar *et al.* (2004) describe such an implementation using parallel computing and MPI, which we use as the basis for implementing our algorithm described below.

ALGORITHM 3 (PARALLEL TEMPERING) *Given* r *initial valid partitions* $\mathbf{v}_0^{(0)}, \mathbf{v}_0^{(1)}, \ldots, \mathbf{v}_0^{(r-1)}$ *and a sequence of* r *decreasing temperatures* $\beta^{(0)} > \beta^{(1)} > \cdots > \beta^{(r-1)} = 0$ *with* $\beta^{(0)}$ *the target temperature for inference, and swapping interval* T, *the parallel tempering algorithm repeats the following steps every iteration* $t \in \{0, T, 2T, 3T, \ldots\}$,

Step 1 (Run the basic algorithm with the modified acceptance probability): *For each chain* $i \in \{0, 1, \ldots, r-1\}$, *using the current partition* $\mathbf{v}_t^{(i)}$ *and* *the corresponding temperature* $\beta^{(i)}$, *obtain a valid partition* $\mathbf{v}_{t+T}^{(i)}$ *by running* T *iterations of Algorithm 1 with the acceptance probability given in equation* (7)*. This step is executed concurrently for each chain*

Step 2 (Propose a temperature exchange between two chains): *Randomly select two adjacent chains* j *and* k *and exchange information about the temperatures* $\beta^{(j)}, \beta^{(k)}$ *and the unnormalized likelihoods of the current partitions* $g_{\beta^{(j)}}\left(\mathbf{v}_{t+T}^{(j)}\right), g_{\beta^{(k)}}\left(\mathbf{v}_{t+T}^{(k)}\right)$ *using MPI*

Step 3 (Accept or reject the temperature exchange): *Exchange temperatures (i.e* $\beta^{(j)} \leftrightarrows \beta^{(k)}$*) with probability* $\gamma\left(\beta^{(j)} \leftrightarrows \beta^{(k)}\right)$ *where*

$$\gamma\left(\beta^{(j)} \leftrightarrows \beta^{(k)}\right) = \min\left(1, \frac{g_{\beta^{(j)}}\left(\mathbf{v}_{t+T}^{(k)}\right) g_{\beta^{(k)}}\left(\mathbf{v}_{t+T}^{(j)}\right)}{g_{\beta^{(j)}}\left(\mathbf{v}_{t+T}^{(j)}\right) g_{\beta^{(k)}}\left(\mathbf{v}_{t+T}^{(k)}\right)}\right) \tag{10}$$

All previously generated samples are assumed to have been generated at the current temperature of the chain

We note that the mixing performance of Algorithm 3 is affected by the choice of the temperature sequence $\beta^{(i)}$. While no sequence has been shown to be optimal in the literature, sequences with power-law spacing have been shown heuristically to produce reasonable results. For this reason, we used the sequence $\beta^{(i)} = \left(\beta^{(0)}\right)^{\frac{i}{r-1}}, i \in \{0, 1, \ldots, r-1\}$ for our implementation.

2.5 Comparison with the Existing Algorithms

A number of substantive researchers used Monte Carlo simulation algorithms to sample possible redistricting plans under various criteria in order to detect the instances of gerrymandering and understand the causes and consequences of redistricting (e.g., Engstrom and Wildgen, 1977; O'Loughlin, 1982; Cirincione *et al.*, 2000; McCarty *et al.*, 2009; Chen and Rodden, 2013). Most of these studies use a similar Monte Carlo simulation algorithm where a geographical unit is randomly selected as a "seed" for each district and then neighboring units are added to contiguously grow this district until it reaches the pre-specified population threshold. A representative of such algorithms, proposed by Cirincione *et al.* (2000) and implemented by Altman and McDonald (2011) in their open-source `BARD` package, is given here.

Algorithm 4 (The Standard Redistricting Simulator (Cirincione *et al.*, 2000)) *For each district, we repeat the following steps.*

> **Step 1:** *From the set of unassigned units, randomly select the seed unit of the district.*
>
> **Step 2:** *Identify all unassigned units adjacent to the district.*
>
> **Step 3:** *Randomly select one of the adjacent units and add it to the district.*
>
> **Step 4:** *Repeat Steps 2 and 3 until the district reaches the predetermined population threshold.*

Additional criteria can be incorporated into this algorithm by modifying Step 3 to select certain units. For example, to improve the compactness of the resulting districts, one may choose an adjacent unassigned unit that falls entirely within the minimum bounding rectangle of the emerging district. Alternatively, an adjacent unassigned unit that is the closest to emerging district can be selected (see Chen and Rodden, 2013).

Nevertheless, the major problem of these simulation algorithms is their adhoc nature. For example, as the documentation of `BARD` package warns, the creation of earlier districts may make it impossible to yield contiguous districts. More importantly, the algorithms come with no theoretical result and are not even designed to uniformly sample redistricting plans even though researchers have a tendency to assume that they are. In contrast, the proposed algorithms described in Sections 2.2–2.4 are built upon the well-known theories and strategies developed in the literature on the Markov chain Monte Carlo methods. The disadvantage of our algorithms, however, is that they yield a dependent sample and hence their performance will hinge upon the degree of mixing. Thus, we now turn to the assessment of the empirical performance of the proposed algorithms.

3 Empirical Performance of the Proposed Algorithms

In this section, we assess the performance of the proposed algorithms in two ways. First, we conduct a small-scale validation study where, due to its size, all possible redistricting maps can be enumerated in a reasonable amount of time. We show that our algorithms can approximate the target distribution well when the standard algorithm commonly used in the literature fails. Second, we use the actual redistricting data to examine the convergence behavior of the proposed algorithms in more realistic settings using the redistricting data from New Hampshire (two districts) and Mississippi (four districts). For these data, the computation of the true population distribution is not feasible. Instead, we evaluate the empirical performance of the proposed algorithms by examining the standard diagnostics of MCMC algorithms.

To conduct these analyses, we integrate precinct-level data from two sources. We utilize precinct-level shape files and electoral returns data from the Harvard Election Data Archive to determine precinct adjacency and voting behavior. We supplement this data with basic demographic information from the U.S. Census Bureau P.L. 94–171 summary files, which are compiled by the Census Bureau and disseminated to the 50 states in order to obtain population parity in decennial redistricting.

3.1 A Small-scale Validation Study

We conduct a validation study where we analyze the convergence of our algorithm to the target distribution on the 25 precinct set, which is shown as an adjacency graph in Figure 1. Due to the small size of these sets, all possible redistricting plans can be enumerated in a reasonable amount of time. We begin by considering the problem of partitioning each of these graphs into two districts. We apply the proposed algorithm

(Algorithm 1) with the starting map obtained randomly by running the standard algorithm (Algorithm 4) once. In addition, we apply the standard algorithm, as implemented in the BARD package (Altman and McDonald, 2011), to compare its performance with that of our proposed algorithm. We then consider partitions of the 25 precinct set into three districts. The results of the proposed algorithm are based on a single chain of 10,000 draws while those of the standard algorithm are based on the same number of independent draws.

Before we give results, it should be noted that, even for this small-scale study, the enumeration of all valid partitions is a non-trivial problem. For partitions of 25 precincts into three districts, of the roughly $3^{25}/6 \approx 1.41 \times 10^{11}$ possible partitions, $82,623$ have three contiguous districts, and $3,617$ have district populations within 20% of parity.

A brief description of our enumeration algorithm is as follows. In the case of two districts, we choose an initial starting node and form a partition where one district is that initial node and the other district is the complement, provided the complement is connected. We then form connected components of two nodes comprised of that starting node and and nodes that are adjacent to that node. We identify all valid partitions where one district is a two-node component and the other district is the complement of the component. We continue forming connected components of incrementally increasing sizes and finding valid partitions until all possible partitions are found. In the case of three precincts, if the complement of a connected component is comprised of two additional connected components, we store that partition as valid. If the complement is a single connected component, we apply the two-district algorithm on the complement. After this enumeration, we identify which partitions have districts with populations within a certain percentage of parity.

Figure 3 presents the results of the validation study with three districts and 25

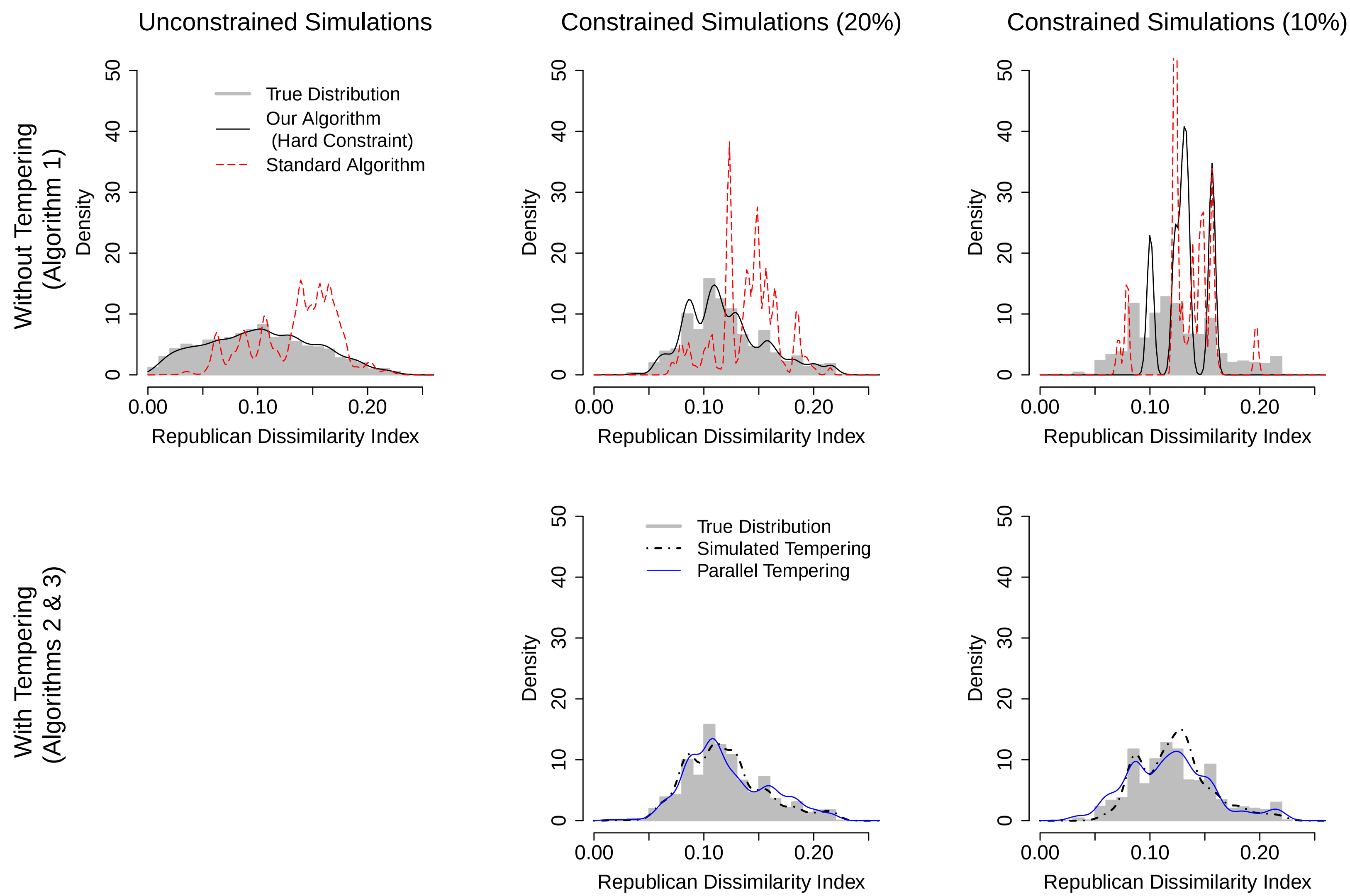

Figure 3: A Small-scale Validation Study with Three Districts. The underlying data is the 25 precinct set shown in the left plot of Figure 1. The plots in the first row show that the proposed algorithm (Algorithm 1; solid black lines) approximates well the true population distribution (grey histograms) when no (left plot) or weak (middle plot) equal population constraint is imposed. However, the algorithm exhibits poor performance when a stronger equal population constraint (right plot) is imposed. Finally, the standard algorithm (Algorithm 4; red dashed lines) fails to approximate the target distribution in all cases. In contrast, in the plots of the second row, the proposed algorithm with simulated tempering (Algorithm 2; black dot-dashed line) approximates the true population distribution well even when a stronger constraint is placed. The same exact pattern is observed for the parallel tempering algorithm (Algorithm 3; blue solid line). The results for each algorithm is based on a single chain of 10,000 draws.

precincts. We apply the proposed algorithm (Algorithm 1) with the starting map obtained randomly from the standard algorithm (Algorithm 4) (upper panel). These algorithms are also implemented with the simulated tempering (Algorithm 2; black dot-dashed lines) and parallel tempering (Algorithm 3; blue solid lines) strategies (the lower panel).

To implement these algorithms, we specify a sequence of temperatures $\{\beta^{(\ell)}\}_{\ell=0}^{r}$. For the population deviation of 20%, we chose a target temperature of $\beta^{(r)} = 5.4$, and for the population deviation of 10%, we chose a target temperature of $\beta^{(r)} = 9$. In both cases, we use $\beta^{(0)} = 0$. We choose these setups so that the rejection ratio is in the recommended 20–40% range (Geyer and Thompson, 1995) and the target temperature value is chosen based on the number of plans that meet the population constraint. In both cases, we use a subset of draws taken under the target temperature. We then resample the remaining draws using the importance weights $1/g_{\beta^{(\ell)}}(\mathbf{v})$, and finally subset down to the set of remaining draws that fall within the population target.

The left-upper plot of Figure 3 shows that when no constraint is imposed the proposed algorithm approximates the target distribution well while the sample from the standard algorithm is far from being representative of the population. In the plots of the middle and right columns, we impose the equal population constraint where only up to 20% and 10% deviation from the population parity is allowed, respectively. It is no surprise that the standard algorithm completely fails to approximate the true distribution as well in these caes (the middle and right plots in the upper panel). In contrast, the proposed algorithms with simulated and parallel tempering approximate the true population distribution well. Even when a stronger constraint, i.e., 10%, is placed, the proposed algorithms with simulated tempering (Algorithm 2) and parallel tempering (Algorithm 3) maintain a good approximation.

Finally, Figure 4 compares the runtime between the proposed basic algorithm

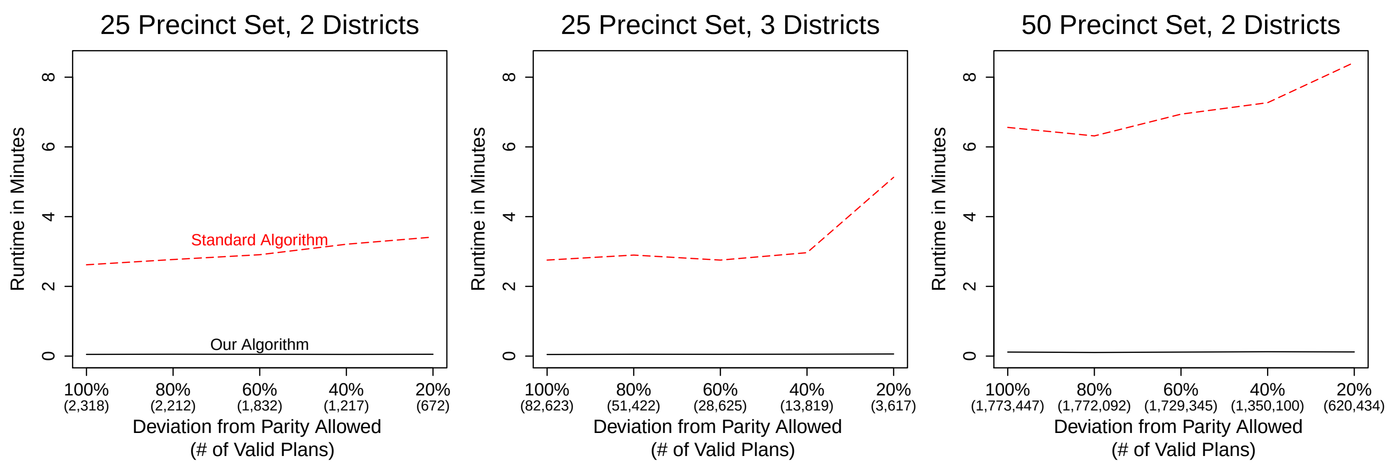

Figure 4: Runtime Comparison between the Proposed and Standard Algorithms in the Small-scale Validation Study. The runtime is compared between the proposed basic algorithm (Algorithm 1; solid black lines) and the standard algorithm (Algorithm 4; red dashed lines) under various settings. Each algorithm is run until it yields 10,000 draws. The runtime is much greater for the standard algorithm than the proposed algorithm. It also increases much more quickly for the former as the number of precincts and the strength of equal population constraint increase.

(Algorithm 1; solid black lines) and the standard algorithm (Algorithm 4; red dashed lines) under various validation study settings. In addition to the 25 precinct set, we also include the 50 precinct set, which is shown in the right plot of Figure 1. Each algorithm is run until it yields 10,000 draws using a node on a Linux server with 2.66 GHz Nehalem processors and 3GB RAM (no parallel computing is used). We find that under all settings we consider here the runtime for the proposed algorithm is at least 50 times shorter than that for the standard algorithm. This difference increases as the number of precincts and the strength of equal population constraint (x-axis) increase. In sum, in terms of computational speed, the proposed algorithm scales much better than the standard algorithm.

3.2 An Empirical Study

The scale of the validation study presented above is small so that we can enumerate all possible redistricting plans in a reasonable amount of time. This allowed us to examine how well each algorithm is able to approximate the true population distri-

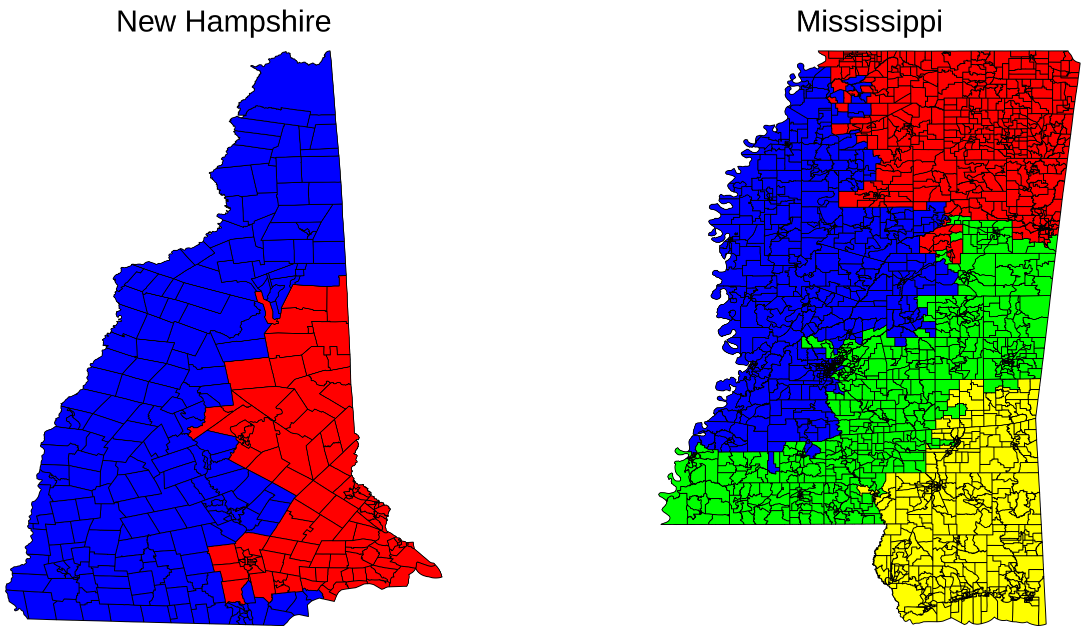

Figure 5: Precinct-level Maps of New Hampshire (327 precincts, two congressional districts) and Mississippi (1,969 precincts, four congressional districts). Colors correspond to precinct congressional district assignments in 2010. In New Hampshire, Democrats and Republicans each hold a single congressional seat. In Mississippi, Republicans hold three congressional seats while Democrats hold a single seat.

bution. However, the scale of the study is too small to be realistic. Below, we apply the proposed algorithms to the 2008 election data and conduct standard convergence diagnostics of MCMC algorithms. While we cannot compare the distribution of sampled maps with the true population distribution, this empirical study enables us to investigate the performance of the proposed methods in realistic settings.

New Hampshire. We first consider New Hampshire. The state has two congressional districts and consists of 327 precincts, and so this is one of the simplest realistic redistricting problems. The left panel of Figure 5 shows the implemented statewide redistricting plan as of 2010. Under this plan, Democrats and Republicans won a single congressional seat each. In 2008, Obama won 54% of votes in this state while his 2012 voteshare was 52%. Redistricting in New Hampshire is determined by its state legislature and plans are passed as standard statutes, which makes them subject to gubernatorial veto. We apply the proposed basic algorithm (Algorithm 1), simulated tempering algorithm (Algorithm 2), and parallel tempering algorithm (Algorithm 3). The target population consists of all redistricting plans with contiguous districts and a maximum of 1% deviation from the population parity.

A total of 10 chains are run until 500,000 draws are obtained for each of the three algorithms. Inference is based on a total of 22,970 draws, which is the lowest number of draws across the three algorithms that both satisfy the population constraint and were drawn under the target temperature value, $\beta^{(r)} = 27$. For starting values, we use independent draws from the standard algorithm (Algorithm 4 as implemented in the `BARD` package). For both the simulated and parallel tempering algorithms, after some preliminary analysis, we have decided to allow $\beta^{(\ell)}$ to take values between 0 and 27, using power-law spacing, with the target temperature value of 27. As in the small-scale verification study, we only use draws taken under the target temperature, and then reweight according to the importance weights $1/g_{\beta^{(\ell)}(\mathbf{v})}$ before selecting all remaining draws that fall within the target parity deviation of 1%.

Figure 6 presents the results. The figure shows the autocorrelation plots (left column), the trace plots (middle column), and the Gelman-Rubin potential scale reduction factors (Gelman and Rubin, 1992; right column) for the basic algorithm (top panel), the simulated tempering algorithm (middle panel) and the parallel tempering algorithm (bottom panel). We use the logit transformed Republican dissimilarity index for all diagnostics. Both the simulated and parallel tempering algorithms significantly outperform the basic algorithm. The former has a lower autocorrelation and mixes better. In addition, the potential scale reduction factor goes down quickly, suggesting that all the chains with different starting maps become indistinguishable from each other after approximately 1,500 draws.

Mississippi. Next, we analyze the 2008 election data from Mississippi. This state has a total of four congressional districts and 1,969 precincts, thereby providing a

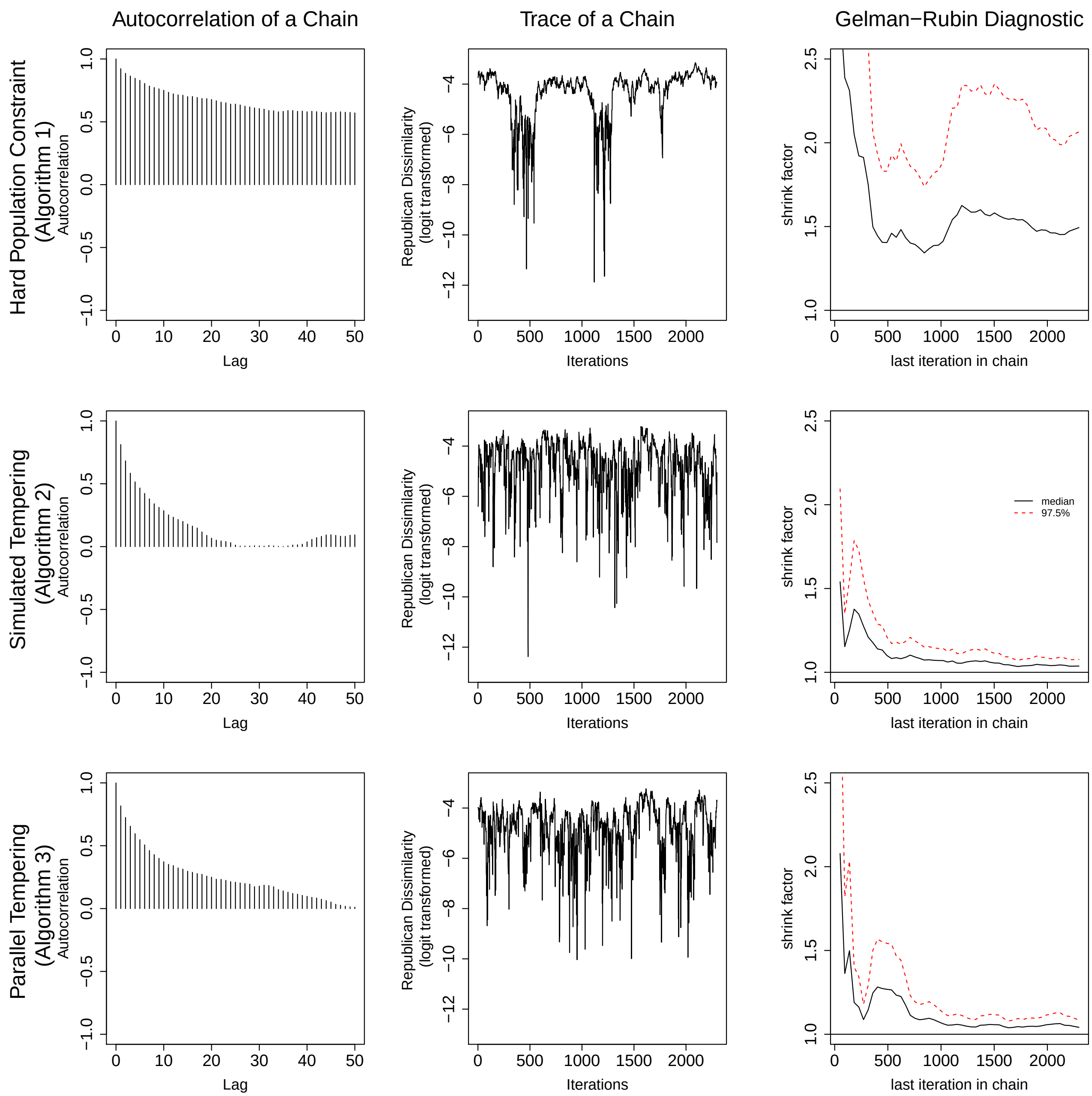

Figure 6: Convergence Diagnostics of the Proposed Algorithm for the 2008 New Hampshire Redistricting Data. The proposed basic algorithm (Algorithm 1; top panel), the simulated tempering algorithm (Algorithm 2; middle panel), and the parallel tempering algorithm (Algorithm 3; bottom panel) are applied to the New Hampshire data with 327 precincts and 2 congressional districts. The target population consists of all redistricting plans with contiguous districts and a maximum of 1% deviation from the population parity. A total of 10 chains are run with different starting maps for each algorithm until 500,000 draws are obtained, and inference is based on a total of 22,970 draws (the number of draws in the simulated tempering algorithm that are both drawn under the target temperature and satisfy the target population constraint). For the logit transformed Republican dissimilarity index, the autocorrelation plots (left column), the trace plots (middle column), and the Gelman-Rubin potential scale reduction factors (right column) are presented. The simulated and parallel tempering algorithms outperform the basic algorithm across all three diagnostics.

more challenging example when compared to New Hampshire. The right-hand panel of Figure 5 shows the implemented redistricting plan in Mississippi as of 2010. In 2008, 43% of the electorate voted for Obama while his voteshare in the 2012 election for this state was 44%. Redistricting in Mississippi is determined by its state legislature subject to gubernatorial veto.

One important feature of Mississippi is its sizable African-American population (37% of the population). This group is concentrated in the capital city, Jackson, and in surrounding areas in the west of the state, which poses a special challenge to the algorithms. Democrats typically win this seat, shaded in blue in Figure 5, while Republicans typically win the other three seats in Mississippi. Mississippi is also one of the nine states fully covered by Section V of the Voting Rights Act, which obligates political officials to submit its proposed redistricting plan to the U.S. Department of Justice. However, following the Supreme Court's decision in *Shelby County v. Holder* (2013) to strike down the pre-clearance formula determining Section V coverage, Mississippi is no longer subject to Section V requirements by default.

Here, we utilize parallel tempering (Algorithm 3) to examine its algorithmic performance for Mississippi. After some preliminary analysis, we chose to anneal $\beta^{(\ell)}$ between 0 and -225 in unequally spaced increments, with the target temperature of $\beta^{(\ell)} = -225$. We run a total of 10 chains for 200,000 simulations each, keeping every 5th draw. Inference is then based off of a total of 138,840 draws, which is the number of remaining simulations drawn under the target $\beta^{(\ell)}$ that fall within 5% of population parity.

Figure 7 presents the results of this analysis. The same set of diagnostics are conducted for the Republican dissimilarity index (top row) and the African-American dissimilarity index (bottom row). The figure shows that although the Mississippi data pose a much more challenging application than the New Hampshire data, the

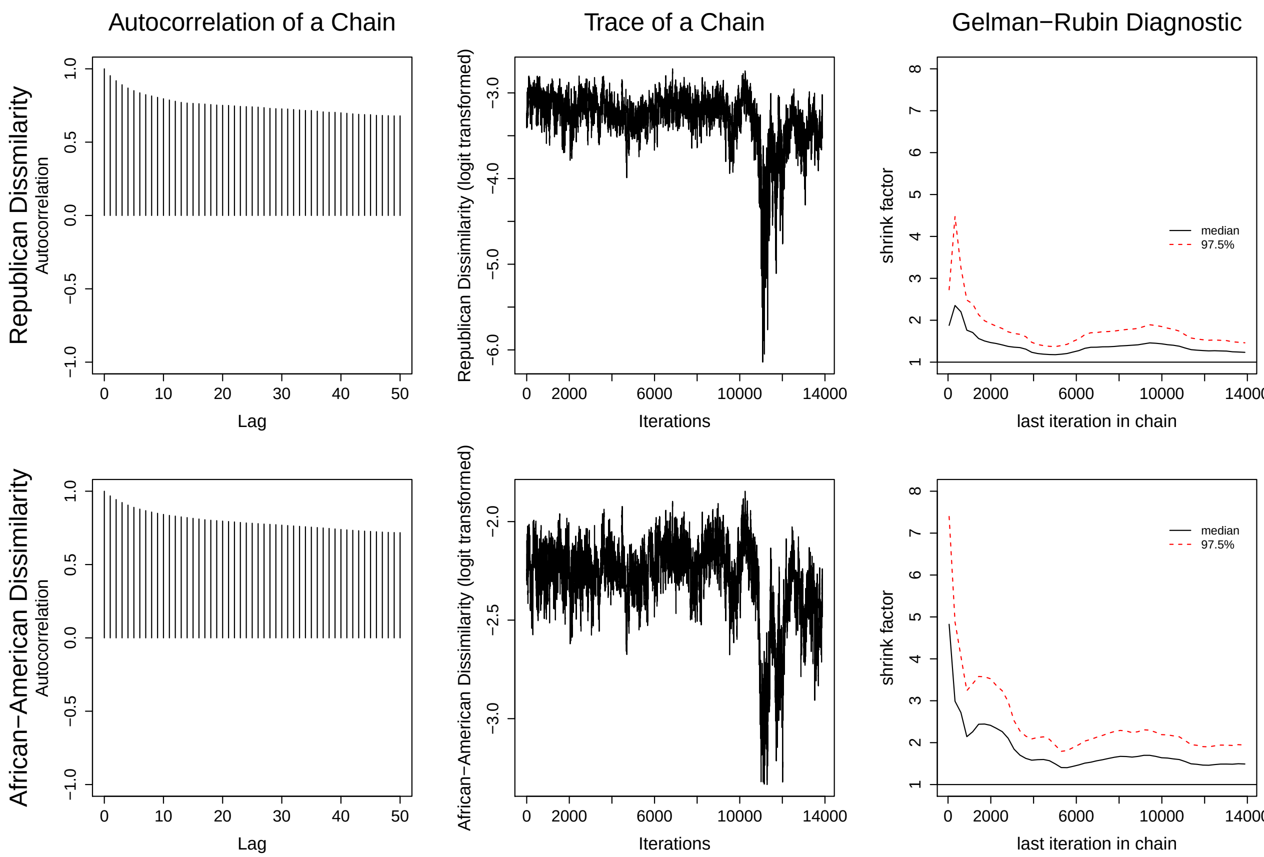

Figure 7: Convergence Diagnostics of the Proposed Algorithm for the 2008 Mississippi Redistricting Data. The information identical to that of Figure 6 is displayed here for two statistics, Republican dissimilarity index and African-American dissimilarity index (both logit transformed). See the caption of Figure 6 for details. The data is obtained from 138,840 draws of the parallel tempering algorithm (Algorithm 3).

parallel tempering algorithm still performs reasonably well. In particular, the potential scale reduction factor (in the plots given in the right column) is relatively low and remains stable for the Republican dissimilarity index, suggesting that the impact of the starting values has mostly disappeared. Because African American voters are geographically concentrated, the algorithm has a harder time mixing for the African-American dissimilarity index. Nevertheless, the scale reduction factor still stabilizes at a reasonably low value, suggesting that the impact of the starting values is limited in this application.

4 Concluding Remarks

Over the last half century, a number of automated redistricting algorithms have been proposed in the methodological literature. Most of these algorithms have been designed to find an optimal redistricting plan given a certain set of criteria. However, many substantive researchers have been interested in characterizing the distribution of redistricting plans under various constraints. Unfortunately, few such simulation algorithms exist and even the ones that are commonly used by applied researchers have no theoretical justification.

In this paper, we propose a new automated redistricting simulator using Markov chain Monte Carlo. Unlike the existing standard algorithm, the proposed algorithms have a theoretical justification and approximate the target distribution well in a small-scale validation study. Even in more realistic settings where the computational challenge is greater, our initial analyses shows a promising performance of the proposed algorithms. Nevertheless, it is still unclear whether these algorithms scale to those states with an even greater number of districts than those considered here. In the future, we plan to investigate whether simulated and parallel tempering strategies can overcome the computational challenge posed by those large states.

Another promising line of research is to examine the factors that predict the redistricting outcome. For example, substantive researchers are interested in how the institutional features of redistricting process (e.g., bipartisan commission vs. state legislature) determines the redistricting process. Such an analysis requires inferences about the parameters that are underlying our generative model. In contrast, in this paper we restricted our attention to the question of how to simulate redistricting plans given these model parameters. Therefore, a different approach is required to address this and other methodological challenges.

Appendix: Proof of Theorem 1

Let $\Gamma(C^*, G_{\mathbf{v}})$ denote all sets of connected components C obtainable through "turning on" edges in $E_{\mathbf{v}}$ such that $C^* \subset C$. Let $p(C \mid G_{\mathbf{v}})$ denote the probability that C is obtained through Steps 1 and 2 of Algorithm 1. Let $p(C^* \mid C)$ denote the probability that, given C, its particular subset C^* is selected at Step 3. Note that this probability does not depend on the partition $\mathbf{v}$. Then, it follows that

$$\pi(\mathbf{v}_{t-1} \to \mathbf{v}_t^*) = \sum_{C' \in \Gamma(C^*, G_{\mathbf{v}_{t-1}})} p(C^* \mid C') p(C' \mid G_{\mathbf{v}_{t-1}}) \prod_{\ell=1}^{r} \frac{1}{|A(C_\ell^*, \mathbf{v}_{t-1})|} \tag{11}$$

$$\pi(\mathbf{v}_t^* \to \mathbf{v}_{t-1}) = \sum_{C' \in \Gamma(C^*, G_{\mathbf{v}_t^*})} p(C^* \mid C') p(C' \mid G_{\mathbf{v}_t^*}) \prod_{\ell=1}^{r} \frac{1}{|A(C_\ell^*, \mathbf{v}_t^*)|} \tag{12}$$

We now simplify equations (11) and (12) to identify common terms, which then cancel each other in equation (3). First, we show

$$|A(C_\ell^*, \mathbf{v}_{t-1})| = |A(C_\ell^*, \mathbf{v}_t^*)| \tag{13}$$

for any connected component $C_\ell^* \in C^*$ where $l \in \{1, \ldots, r\}$.

Suppose that, without loss of generality, C_ℓ^* is adjacent to blocks $V_{1,t-1}, V_{2,t-1}, \ldots, V_{|A(C_\ell^*, \mathbf{v}_{t-1})|, t-1} \in \mathbf{v}_{t-1}$, and C_ℓ^* is contained in block $V_{|A(C_\ell^*, \mathbf{v}_{t-1})|+1, t-1} \in \mathbf{v}_{t-1}$. The check that $V_{kt}^* \neq \emptyset$ in Step 4 of the algorithm ensures that $C_\ell^* \neq V_{|A(C_\ell^*, \mathbf{v}_{t-1})|+1, t-1}$. Since $\mathbf{v}_{t-1}$ is a connected set partition, there must exist $\{i_{|A(C_\ell^*, \mathbf{v}_t^*)|+1}\} \in C_\ell^*$ and $\{j_{|A(C_\ell^*, \mathbf{v}_t^*)|+1}\} \in V_{|A(C_\ell^*, \mathbf{v}_{t-1})|+1, t-1} \setminus C_\ell^*$ that are adjacent in $G_{\mathbf{v}_{t-1}}$. Moreover, there exist pairs of adjacent nodes $(\{i_1\}, \{j_1\}), \ldots, (\{i_{|A(C_\ell^*, \mathbf{v}_{t-1})|}\}, \{j_{|A(C_\ell^*, \mathbf{v}_{t-1})|}\})$ with $\{i_k\} \in C_\ell^*, \{j_k\} \in V_{k,t-1}$ where $1 \le k \le |A(C_\ell^*, \mathbf{v}_{t-1})|$. Since C^* is comprised of non-adjacent connected components, it follows that nodes $\{j_1\}, \ldots, \{j_{|A(C_\ell^*, \mathbf{v}_{t-1})|}\}, \{j_{|A(C_\ell^*, \mathbf{v}_{t-1})|+1}\}$ do not change block assignment when transitioning from $\mathbf{v}_{t-1}$ to $\mathbf{v}_t^*$, and thus, are contained in distinct blocks in $\mathbf{v}_t^*$. Thus, the connected component C_ℓ^* is adjacent to all blocks corresponding to a node in $\{\{j_1\}, \ldots, \{j_{|A(C_\ell^*, \mathbf{v}_t^*)|}\}, \{j_{|A(C_\ell^*, \mathbf{v}_t^*)|+1}\}\}$ except for the block containing C_ℓ^*: $|A(C_\ell^*, \mathbf{v}_{t-1})|$ blocks in total. Hence, $|A(C_\ell^*, \mathbf{v}_t^*)| \ge |A(C_\ell^*, \mathbf{v}_{t-1})|$. Moreover, for any block $V_{k,t-1} \notin A(C_\ell^*, \mathbf{v}_{t-1})$ such that $C_\ell^* \not\subset V_{k,t-1}$, the corresponding block $V_{k,t}^*$ obtained by swapping connected components in C^* will not be contained in $A(C_\ell^*, \mathbf{v}_t^*)$; by definition, for any $\{i\} \in C_\ell^*$, $\{j\} \in V_{k,t-1}$, $(i,j) \notin E$, and since connected components in C^* are not adjacent, it follows that no edge connects a vertex in $V_{k,t}^*$ to a vertex in C_ℓ^*. This proves equation (13).

Next, through a proof by contradiction, we show that

$$\Gamma(C^*, G_{\mathbf{v}_{t-1}}) = \Gamma(C^*, G_{\mathbf{v}_t^*}). \tag{14}$$

By showing this, we also conclude that $\mathbf{v}_{t-1}$ can be a candidate partition when starting from $\mathbf{v}_t^*$, i.e., $\pi(\mathbf{v}_{t-1} \to \mathbf{v}_t^*) > 0$ implies $\pi(\mathbf{v}_t^* \to \mathbf{v}_{t-1}) > 0$. Suppose that there exists a set of connected components $C' \in \Gamma(C^*, G_{\mathbf{v}_{t-1}})$ such that $C' \notin \Gamma(C^*, G_{\mathbf{v}_t^*})$. This means that there exists $C'_\ell \in C'$ that can be formed by turning on edges in $E_{\mathbf{v}_{t-1}^*}$ but not in $E_{\mathbf{v}_t^*}$. Thus, there exists $\{i\}, \{j\} \in C'_\ell$ such that $(i,j) \in E_{\mathbf{v}_{t-1}}$ and

$(i,j) \notin E_{\mathbf{v}_t^*}$. However, according to Step 4 of the algorithm, the only edges deleted in the transition between $\mathbf{v}_{t-1}$ and $\mathbf{v}_t^*$, are those connecting a vertex in $\{i\}$ in C^* to a vertex $\{j\} \notin C^*$. Since $C^* \subset C' \in \Gamma(C^*, G_{\mathbf{v}_{t-1}})$, $\{i\}$ and $\{j\}$ cannot be contained in the same component of C', a contradiction. An analogous argument shows that there is no connected component $C' \in \Gamma(C, \mathbf{v}_t^*)$ such that $C' \notin \Gamma(C, \mathbf{v}_{t-1})$. This proves equation (14).

Third, we decompose $p(C \mid G_{\mathbf{v}})$. For a partition $\mathbf{v}$, let $\Lambda(C, E_{\mathbf{v}})$ denote all subsets of edges of $E_{\mathbf{v}}$ such that, when only those edges in a subset are turned on, the set of connected components C is formed (Step 2). Note that C can be formed if and only if the partition $\mathbf{v}$ satisfies $E_C \subset E_{\mathbf{v}}$, and $\Lambda(C, E_{\mathbf{v}})$ is identical for all such partitions. Specifically, $\Lambda(C, E_{\mathbf{v}_{t-1}}) = \Lambda(C, E_{\mathbf{v}_t^*})$. To see this, observe that every set of edges $E_{\mathbf{v}}^* \in \Lambda(C, E_{\mathbf{v}})$ must connect nodes within each connected component in C, and must not include any edges joining a connected component to a node not included in the connected component. For any connected component $C_\ell \in C$, there must be a block $V_k \in \mathbf{v}$ such that $C_\ell \subset V_k$. Since $E_{\mathbf{v}}$ contains all edges joining two nodes in V_k, it follows that any set of edges connecting nodes in C is contained in $E_{\mathbf{v}}$.

Given a set of "turned-on" edges $E_{\mathbf{v}}^* \in \Lambda(C, E_{\mathbf{v}})$, define $\overline{E}_{\mathbf{v}}^* \equiv E_{\mathbf{v}} \setminus E_{\mathbf{v}}^*$ as the set of "turned-off" edges. Observe that, for $E_{\mathbf{v}_{t-1}}^* \in \Lambda(C, E_{\mathbf{v}_{t-1}})$, $E_{\mathbf{v}_t^*}^* \in \Lambda(C, E_{\mathbf{v}_t})$ with $E_{\mathbf{v}_{t-1}}^* = E_{\mathbf{v}_t}^*$ $\overline{E}_{\mathbf{v}_{t-1}}^*$ may be different from $\overline{E}_{\mathbf{v}_t^*}^*$. That is, if the candidate partition $\mathbf{v}^*$ is obtained from $\mathbf{v}_{t-1}$ by assigning connected component $C' \in C$ from block V_ℓ to block $V_{\ell'}$, $\overline{E}_{\mathbf{v}_t^*}^*$ may contain an edge that connects a node in C' to an adjacent node in $V_{\ell'}$, whereas this edge cannot occur in $\overline{E}_{\mathbf{v}_{t-1}}^*$. Define

$$\begin{aligned} B(C^*, \overline{E}_{\mathbf{v}}^*) &\equiv \{(i,j) \in \overline{E}_{\mathbf{v}}^* : \{i\} \in C^*, \{j\} \notin C^*\} \\ &= \{(i,j) \in \overline{E}_{\mathbf{v}}^* : \exists C_\ell^* \in C^*, C_\ell^* \subset V_k \in \mathbf{v} \; s.t. \{i\} \in C_\ell^*, \{j\} \in V_k \setminus C_\ell^*\} \end{aligned} \tag{15}$$

as the set of edges in $\overline{E}_{\mathbf{v}}^*$ that connect a block of nodes in C^* to a vertex not in C^*, i.e., those edges that need to be "cut" to form blocks of vertices C^*. Since $C^* \subset C$, for partition $\mathbf{v}$, $B(C^*, E_{\mathbf{v}})$ is the same for every set of turned-on edges in $\Lambda(C, E_{\mathbf{v}})$, and is the same across all sets of connected components in $\Gamma(C^*, G_{\mathbf{v}})$. Then, we can write $p(C \mid G_{\mathbf{v}})$ as:

$$p(C \mid G_{\mathbf{v}_{t-1}}) = \prod_{e \in B(C^*, E_{\mathbf{v}_{t-1}})} (1 - q_e) \sum_{E_{\mathbf{v}_{t-1}}^* \in \Lambda(C, E_{\mathbf{v}_{t-1}})} \prod_{e \in E_{\mathbf{v}_{t-1}}^*} q_e \prod_{e \in \overline{E}_{\mathbf{v}_{t-1}}^* \setminus B(C^*, E_{\mathbf{v}_{t-1}})} (1 - q_e) \tag{16}$$

where we allow the edge cut probability to differ across edges.

Finally, we show that, for any $E_{\mathbf{v}_{t-1}}^* \in \Lambda(C, E_{\mathbf{v}_{t-1}})$, $E_{\mathbf{v}_t^*}^* \in \Lambda(C, E_{\mathbf{v}_t})$ with $E_{\mathbf{v}_{t-1}}^* = E_{\mathbf{v}_t}^*$,

$$E_{\mathbf{v}_{t-1}}^* \setminus B(C^*, E_{\mathbf{v}_{t-1}}) = E_{\mathbf{v}_t^*}^* \setminus B(C^*, E_{\mathbf{v}_t^*}) \tag{17}$$

Consider any edge $e \in E_{\mathbf{v}_{t-1}} \setminus B(C^*, E_{\mathbf{v}_{t-1}})$. This edge can either join two nodes within a single connected component or joins two nodes in two distinct connected components. In the former case, both nodes are contained in a single block of $\mathbf{v}_{t-1}$, and since connected components are reassigned to form the candidate partition $\mathbf{v}_t^*$, it follows that both nodes are contained in a single block $V^* \in \mathbf{v}_t^*$. Hence, $e \in E_{\mathbf{v}_t^*}$, and since does not join a node in connected component in C^* to a node in a connected component that is not in C^*, it follows that $e \in E_{\mathbf{v}_t^*} \setminus B(C^*, E_{\mathbf{v}_t^*})$. In the latter case, observe that, since $e \in E_{\mathbf{v}_{t-1}}$, both connected components must be contained within the same block of $\mathbf{v}_{t-1}$. Since they do not belong to C^*, neither component is reassigned to a block, and hence, are contained within the same block $V_{kt}^* \in \mathbf{v}_t^*$. Thus, $e \in E_{\mathbf{v}_t^*}$, and since does not join a node in connected component in C^* to a node in a connected component that is not in C^*, it follows that $e \in E_{\mathbf{v}_t^*} \setminus B(C^*, E_{\mathbf{v}_t^*})$. In both cases, $e \in E_{\mathbf{v}_t^*} \setminus B(C^*, E_{\mathbf{v}_t^*})$. Thus, $E_{\mathbf{v}_{t-1}} \setminus B(C^*, E_{\mathbf{v}_{t-1}}) \subset E_{\mathbf{v}_t^*} \setminus B(C^*, E_{\mathbf{v}_t^*})$. By the same argument, $E_{\mathbf{v}_t^*} \setminus B(C^*, E_{\mathbf{v}_t^*}) \subset E_{\mathbf{v}_{t-1}} \setminus B(C^*, E_{\mathbf{v}_{t-1}})$, and we have shown equation (17). By this observation, we can now write,

$$p(C \mid G_{\mathbf{v}_t^*}) = \prod_{e \in B(C^*, E_{\mathbf{v}_t^*})} (1 - q_e) \sum_{E^*_{\mathbf{v}_{t-1}} \in \Lambda(C, E_{\mathbf{v}_{t-1}})} \prod_{e \in E^*_{\mathbf{v}_{t-1}}} q_e \prod_{e \in E^*_{\mathbf{v}_{t-1}} \setminus B(C^*, E_{\mathbf{v}_{t-1}})} (1 - q_e). \tag{18}$$

Using equation (16) and the fact that the set of edges $B(C^*, \mathbf{v}_{t-1})$ is identical across all sets of connected components $C_\ell \in C^*$, we can write as:

$$\begin{aligned}\pi(\mathbf{v}_{t-1} \to \mathbf{v}_t^*) &= \prod_{e \in B(C^*, E_{\mathbf{v}_{t-1}})} (1 - q_e) \sum_{C \in \Gamma(C^*, \mathbf{v}_{t-1})} \left(\sum_{E^*_{\mathbf{v}_{t-1}} \in \Lambda(C, E_{\mathbf{v}_{t-1}})} \prod_{e \in E^*_{\mathbf{v}_{t-1}}} q_e \prod_{e \in \overline{E}^*_{\mathbf{v}_{t-1}} \setminus B(C^*, E_{\mathbf{v}_{t-1}})} (1 - q_e) \right) \\ &\quad \times \; p(C^* \mid C) \prod_{\ell=1}^{r} \frac{1}{|A(C_\ell^*, \mathbf{v}_{t-1})|} \end{aligned} \tag{19}$$

Similarly, we find that:

$$\begin{aligned}\pi(\mathbf{v}_t^* \to \mathbf{v}_{t-1}) &= \prod_{e \in B(C^*, E_{\mathbf{v}_t^*})} (1 - q_e) \sum_{C \in \Gamma(C^*, \mathbf{v}_{t-1})} \left(\sum_{E^*_{\mathbf{v}_{t-1}} \in \Lambda(C, E_{\mathbf{v}_{t-1}})} \prod_{e \in E^*_{\mathbf{v}_{t-1}}} q_e \prod_{e \in \overline{E}^*_{\mathbf{v}_{t-1}} \setminus B(C^*, E_{\mathbf{v}_{t-1}})} (1 - q_e) \right) \\ &\quad \times \; p(C^* \mid C) \prod_{\ell=1}^{r} \frac{1}{|A(C_\ell^*, \mathbf{v}_{t-1})|}. \end{aligned} \tag{20}$$

Thus, many terms cancel out and we obtain the following expression for the acceptance probability:

$$\alpha(\mathbf{v} \to \mathbf{v}^*) = \min \left(1, \; \frac{\prod_{e \in B(C^*, \mathbf{v}_t^*)} (1 - q_e)}{\prod_{e \in B(C^*, \mathbf{v}_{t-1})} (1 - q_e)} \right). \tag{21}$$

In the special case that edges are turned on with equal probability, i.e., $q = q_e$ for all e, this ratio can be computed by counting the number of edges connecting nodes in blocks of C^* to nodes outside of those blocks:

$$\alpha(\mathbf{v} \to \mathbf{v}^*) = \min \left(1, \; (1 - q)^{|B(C^*, \mathbf{v}_t^*)| - |B(C^*, \mathbf{v}_{t-1})|} \right). \tag{22}$$

$\square$

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Estimating the Electoral Consequences of Legislative Redistricting

ANDREW GELMAN and GARY KING*

We analyze the effects of redistricting as revealed in the votes received by the Democratic and Republican candidates for state legislature. We develop measures of partisan bias and the responsiveness of the composition of the legislature to changes in statewide votes. Our statistical model incorporates a mixed hierarchical Bayesian and non-Bayesian estimation, requiring simulation along the lines of Tanner and Wong (1987). This model provides reliable estimates of partisan bias and responsiveness along with measures of their variabilities from only a single year of electoral data. This allows one to distinguish systematic changes in the underlying electoral system from typical election-to-election variability.

KEY WORDS: Bayesian estimation; Elections; Political science; Random effects; Simulation.

1. INTRODUCTION

State and national legislators in the United States are largely elected by plurality vote in individual geographic districts, whose boundaries are redrawn after every decennial census. In addition to ensuring equal populations in each district, redistricting affects which candidates are elected, the relative strengths of the two parties in a legislative house, and other features of the electoral system in a state.

Partisans on both sides generally expend considerable political and financial resources trying to control the redistricting process. Because redistricting affects not only immediate political outcomes, but also the fundamental rules of the game, it has always been a highly controversial partisan issue (Cain 1984). When partisans do not receive satisfaction in the legislative arena, they often take their case to the courts. After decades of these cases, the Supreme Court finally declared political gerrymandering justifiable (*Davis v. Bandemer* 1986). The court has not yet settled, however, on an acceptable standard for or measure of an unfair redistricting plan.

In this article, we analyze the effects of redistricting as revealed in the votes received by the Democrats and Republicans in elections for state legislative seats. We also develop measures of partisan bias and the responsiveness of the partisan composition of the legislature to changes in statewide votes. Our conclusions depend on the observed distribution of votes across the legislative districts, as affected by redistricting, and on assumptions about how these district-level votes change as the statewide vote changes. We also explicitly model uncontested district elections.

Related quantitative issues that we do not directly discuss here, but that could be studied with our model, include trends in "marginal seats," the importance of incumbency, the effectiveness of racial gerrymandering, the effect of redistricting on individual districts, and the recent declining responsiveness of the U.S. House of Representatives to vote swings (Gelman and King, in press; King and Gelman, in press).

Our statistical methodology involves a hierarchical random-effects model with a mixture of Bayesian and non-Bayesian estimation, summarized probabilistically. Our Bayesian computation requires simulation along the lines of Tanner and Wong (1987).

2. THE DATA

We analyze the votes received by Democratic and Republican candidates for the lower house of the legislatures of Ohio, Connecticut, and Wisconsin, in the seven elections held in even-numbered years from 1968 through 1980. All elections in these states were by plurality vote in single-member districts, and, except for two districts in Wisconsin in 1980, were won by one of the two major-party candidates. As a result of redistricting in the 1960s, all districts had roughly equal populations. As a sample of our data, Table 1 shows votes in each district election in Ohio in 1972 and 1974. (Our data are available from the Inter-University Consortium for Political and Social Research.)

The Democrats controlled the 1971 Ohio redistricting process and redrew the 99 districts. Connecticut had 177 districts in 1968–1970; during the 1971 redistricting, the number of districts was reduced to 151 and the Republicans controlled where the lines were drawn. Wisconsin's 100 districts were redrawn in 1971 by bipartisan agreement.

For convenience, we will henceforth refer to the Democratic proportion of the two-party vote for a given district election as the *district vote*. We label the average of these proportions, over all districts in a given state and election, as the *average district vote*.

Some district elections feature a single candidate with insignificant opposition or none at all. We refer to such an election as *uncontested* if one candidate gets more than 95% of the two-party vote. The proportion of uncontested elections among all of the district elections varies greatly over the three states and seven election years, with an

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Table 1. Votes Received by Democrats and Republicans in Ohio Legislative House Districts, 1972 and 1974

1972						1974					
District	Democrat	Republican	District	Democrat	Republican	District	Democrat	Republican	District	Democrat	Republican
1	18,250	22,798	51	22,488	16,951	1	20,490	15,107	51	20,952	7,473
2	25,679	17,130	52	24,336	14,083	2	18,669	11,969	52	21,499	7,697
3	0	33,954	53	25,932	8,997	3	12,778	20,272	53	19,522	6,225
4	23,684	10,212	54	22,780	15,229	4	15,765	9,813	54	13,885	15,582
5	21,723	16,130	55	20,198	9,583	5	11,711	9,708	55	19,400	6,538
6	28,309	0	56	21,603	10,678	6	20,584	5,763	56	21,361	9,262
7	20,334	12,675	57	16,533	17,114	7	20,193	9,778	57	11,677	13,944
8	16,622	3,656	58	13,587	22,105	8	11,153	2,261	58	12,286	16,158
9	11,946	10,396	59	14,877	20,234	9	9,566	0	59	13,834	14,211
10	12,383	5,316	60	14,556	13,940	10	8,277	1,890	60	12,550	9.659
11	20,091	18,539	61	16,507	17,825	11	22,398	5,221	61	15,589	13,451
12	18,337	20,561	62	23,668	13,428	12	9,865	19,599	62	18,802	8,178
13	16,688	1,970	63	13,868	18,402	13	10,687	966	63	9,713	9,948
14	22,865	11,218	64	13,984	22,593	14	11,478	8,087	64	10,227	17,747
15	21,401	0	65	11,710	29,134	15	15,905	1,936	65	12,282	21,978
16	27,783	12,701	66	15,500	30,156	16	21,909	10,403	66	11,587	24,978
17	24,511	15,716	67	20,409	17,931	17	22,327	11,274	67	17,556	13,500
18	28,805	14,454	68	21,489	15,574	18	22,416	8,138	68	17,070	12,882
19	17,687	23,463	69	16,592	21,816	19	12,431	19,832	69	12,501	17,328
20	15,225	28,639	70	14,172	21,642	20	17,129	19,927	70	12,708	16,905
21	12,392	23,427	71	22,439	20,831	21	10,732	16,700	71	27,279	0
22	16,635	27,940	72	15,616	19,879	22	13,945	21,762	72	12,734	15,738
23	16,986	7,681	73	0	26,079	23	11,332	0	73	13,178	14,974
24	22,856	12,779	74	22,359	12,626	24	16,270	9,187	74	19,691	9,488
25	20,298	12,292	75	14,653	27,063	25	15,566	7,078	75	15,290	19,913
26	15,181	30,866	76	16,438	24,947	26	13,809	24,345	76	13,940	20,516
27	12,045	35,880	77	14,054	23,185	27	11,655	28,036	77	14,526	18,326
28	20,637	27,011	78	18,867	24,829	28	0	27,907	78	12,307	18,867
29	17,418	13,589	79	15,459	26,221	29	14,001	9,433	79	11,312	19,455
30	15,080	9,381	80	24,237	17,392	30	10,117	3,935	80	23,053	10,137
31	19,754	12,971	81	14,606	24,845	31	16,409	7,302	81	14,778	18,131
32	20,068	13,059	82	18,349	24,436	32	16,402	8,042	82	9,825	23,615
33	13,182	22,046	83	12,650	28,287	33	11,627	16,281	83	11,787	21,775
34	15,101	14,159	84	23,448	17,882	34	12,035	8,516	84	22,858	9,891
35	19,344	10,166	85	15,896	24,792	35	12,146	6,785	85	12,670	19,082
36	19,375	7,792	86	18,969	22,815	36	15,336	2,672	86	12,437	18,466
37	17,149	11,274	87	21,828	15,253	37	13,795	8,310	87	18,484	11,590
38	10,759	30,945	88	20,732	12,816	38	0	23,672	88	20,849	0
39	24,246	18,772	89	27,325	16,336	39	20,149	11,974	89	26,780	9,673
40	21,006	20,625	90	25,239	18,272	40	14,268	14,378	90	23,829	14,405
41	29,507	11,524	91	19,783	20,492	41	22,472	6,734	91	14,733	17,729
42	21,635	17,233	92	20,567	20,749	42	15,888	11,543	92	16,859	15,651
43	26,149	9,428	93	11,803	27,093	43	19,881	5,012	93	11,470	21,709
44	24,020	17,601	94	16,508	19,409	44	15,428	18,232	94	12,036	16,015
45	22,872	0	95	10,642	26,685	45	14,622	4,673	95	8,897	21,921
46	23,080	11,743	96	27,270	14,044	46	19,006	7,538	96	23,133	9,397
47	20,465	8,920	97	16,859	13,746	47	17,031	0	97	21,528	9,742
48	18,756	27,079	98	28,857	11,878	48	18,001	19,673	98	22,598	7,454
49	19,809	18,632	99	26,945	14,848	49	17,406	13,021	99	21,235	10,584
50	18,036	19,734				50	14,994	14,481			

average of 10% of the seats uncontested in any election. No statewide election in our study had more than 23% uncontested seats, except for Wisconsin in 1980, with 32%. Election returns in uncontested districts do not adequately reflect support for the two political parties. Since we are interested in this party support, we define the *effective vote* in the case of uncontested districts to be the (unobserved) proportion of the two-party vote that this candidate would have won in his or her district had the election been contested. We approximate the probability density of the effective vote with a stem-and-leaf plot of the vote proportions received by a party in a contested district, one election *before* an uncontested win by that party in that district. Figure 1 presents this plot, based on data from

0.5	11234
	5667789999
0.6	00001111222233333344444
	5566777888888899999999
0.7	000000111112222223333333344444
	5555556667778889
0.8	00011111122333
	66799
0.9	11344

Figure 1. Stem-and-Leaf Plot of the Proportion of the Vote Received by a Party in a Contested District Election, Immediately Preceding an Election in Which That Party Was Unopposed in That District.

1968–1980 in the three state legislatures considered in this article.

3. DATA SUMMARIES AND EXPLORATION

Previous work in this field has involved various theoretical constructs and related data summaries, but extremely few statistical models. One early concept is the "swing ratio"—the change in the proportion of legislative seats won by a party (S), divided by the change in the average district vote (V) received (Ansolabehere, Brady, and Fiorina 1988; Kendall and Stuart 1950). This concept was expanded to the "seats–votes curve," which is the fraction of the legislative seats won by a party, as a function of the average district vote (Niemi and Fett 1986; Quandt 1974). This curve can be expressed as the function $S(V)$, where the variables for fraction of seats won and average district vote each vary from 0 to 1. Figure 2 presents two examples of seats–votes curves. One reflects de facto statewide proportional representation, where $S = V$. The other represents a highly responsive electoral system near the middle of the votes scale, where most elections are usually decided. Following King and Browning (1987) and King (1989), we consider these two symmetric seats–votes curves to represent electoral systems that are fair to the political parties. Deviation from bipartisan symmetry is considered partisan bias.

Of course, a party's legislative representation is not a function only of the number of votes it receives; a deterministic seats–votes curve, as defined, cannot be more than a theoretical construct (Tufte 1973). For this reason, we define the seats–votes curve in real electoral systems to be the *expected* value of S, as a function of V, and we will be interested in both this conditional expectation function and variability around it. Responsiveness and bias can be defined more formally as follows:

$$\text{Responsiveness}(V) = dE(S \mid V)/dV$$
$$\text{Bias}(V) = E(S \mid V) - [1 - E(S \mid 1 - V)]. \quad (1)$$

Past researchers have empirically estimated bias and responsiveness in two ways. The most widely used method uses the statewide Democratic fraction of seats won and the average statewide district vote for a legislature for each of several consecutive elections. One can estimate the seats–votes curve by fitting a nonlinear regression to a scatterplot of these values, and one can calculate summaries of interest from this estimated curve. This method has the disadvantage of ignoring short-term systematic changes in the underlying electoral system, as might result from redistricting. Since only five elections are generally held between redistricting processes, this method is quite limited for present purposes.

The second method, dating back to Butler (1951) [see also Gudgin and Taylor (1979)], creates a "hypothetical" seats–votes curve from the district votes of a single statewide election. This curve plots $S(V)$, under the assumption of "uniform partisan swing"; that is, as the statewide vote V changes, the vote proportion in each district changes

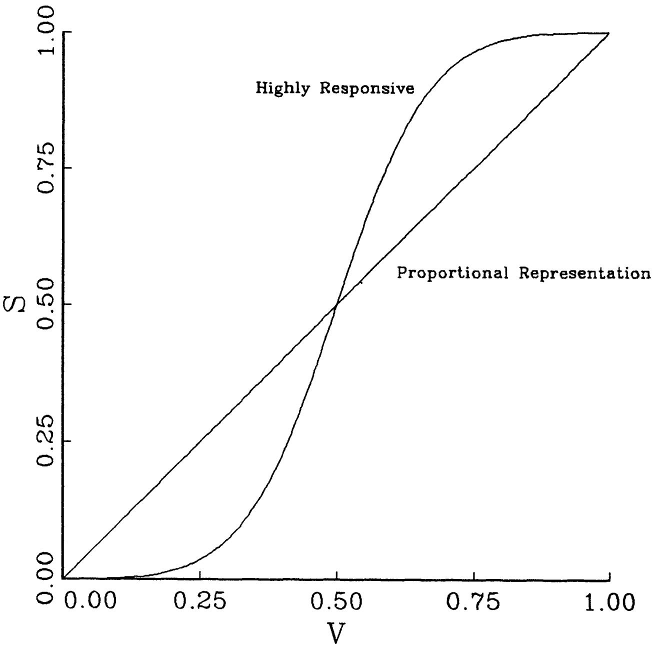

Figure 2. Example of Seats–Votes Curves.

by the same amount. This method breaks down with district votes near 0 or 1 and, in general, is based on an overly strict assumption about voting patterns.

Before describing our stochastic model, we give some exploratory data summaries. We are interested in the distribution of district vote across a state. Figure 3 shows a stem-and-leaf plot of the district votes for the contested elections in Ohio in 1972. This pattern of two main humps with irregular outliers is typical of recent U.S. legislative elections. We identify the two humps with Democratic and Republican "safe seats," and we identify the irregular pattern with the irregular influences of geography on election districts and individual candidates on election results. Sometimes such a plot for an election shows only one main hump in the middle; this corresponds to a competitive system with few safe seats.

0.2	
	5588
0.3	002344
	57777788999
0.4	0222233334
	55778999
0.5	01111233
	55666777788899
0.6	00001112233344
	566667788999
0.7	01134
0.8	1
	9

Figure 3. Stem-and-Leaf Plot of the Democratic Proportion of the Two-Party Vote in Contested District Elections in Ohio, 1972.

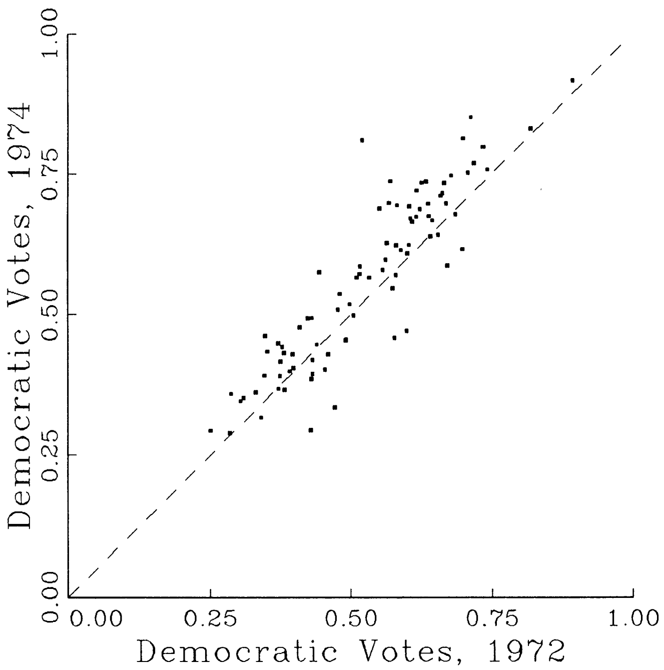

Figure 4. Electoral Swing in Contested Districts, Ohio State House, 1972–1974.

Finally, we would like to know how much partisan voting patterns persist from election to election. As an example of this, Figure 4 shows a scatterplot of district vote proportions for contested elections in Ohio in 1972 and 1974. (Each point on the plot represents one district.) Note that district votes clearly do not move exactly according to "uniform partisan swing"; if they did, all the points would fall precisely on a single line with slope 1. Instead, the points in Figure 4 are scattered around a straight line with slope 1 and intercept equal to the statewide vote swing. We interpret the residual standard deviation in this figure to be within-district random variation about the statewide average vote swing. (A nonuniform shift would be apparent if the points in Fig. 4 fit a clearly nonlinear pattern or no pattern at all.)

4. A PROBABILISTIC MODEL

To avoid problems with vote proportions near 0 or 1, we work with the logit of district votes in contested elections. We label v_{it} as the Democratic vote in district i and election t, and $u_{it} = \text{logit}(v_{it}) = \ln[v_{it}/(1 - v_{it})]$ for contested elections. (For uncontested elections, u_{it} is the logit of the unobserved effective Democratic vote. This will be dealt with in Sec. 5.1.)

Our linear model, fit to a single state, is

$$u_{it} \sim N(\alpha_{it}, \sigma^2), \qquad \alpha_{it} = \gamma_i + \delta_t, \tag{2}$$

where γ_i is a district effect, δ_t is a statewide election effect, and the Normal distributions are independent.

We assume, therefore, that vote swings about the statewide mean are spatially independent across districts. More information about individual districts might enable one to better characterize district-level vote swings. Unfortunately, these data have not been collected, and it would be quite difficult to do so. Modeling districts with additional information such as spatial correlation or covariates, if they were available, would probably yield more accurate estimates of the seats–votes curve. Omitting this unavailable information is unlikely to systematically bias our results.

From the logit effective vote proportions $u_t = (u_{1t}, \ldots, u_{nt})$ for an election t, we define the aggregate Democratic proportions of votes and seats:

$$V_t = \frac{1}{n}\sum_{i=1}^{n} v_{it} = \frac{1}{n}\sum_i \text{logit}^{-1}(u_{it})$$

$$S_t = \frac{1}{n}\sum_{i=1}^{n} s_{it} = \frac{1}{n}\sum_i 1_{(u_{it}>0)}. \tag{3}$$

We consider the vector $\gamma = (\gamma_1, \ldots, \gamma_n)$, along with the variance σ^2, to identify an "electoral system." We will summarize this system by the seats–votes curve $E(S_t \mid V_t, \gamma)$, its variance $\text{var}(S_t \mid V_t, \gamma)$, and functions of these such as the bias and responsiveness functions. Since the elements of γ remain unknown, we model them as random effects by letting the γ_i's be distributed as a three-point Normal-mixture distribution with a prior distribution, all described in Section 5.2. We then average over our uncertainty in γ as represented by this distribution.

The foregoing model is applied to a single observed statewide election, labeled $t = 0$, with observations u_{i0} ($i = 1, \ldots, n$) and the assignment $\delta_0 = 0$. This assignment is arbitrary and does not affect our estimates of the seats–votes curve. If an arbitrary constant were added to each effective district vote u_{i0}, our results would not change. A family of "hypothetical election" results u_t is defined by the linear model, applied to a range of statewide vote shifts δ_t. This assumption that most electoral districts respond approximately as the statewide total does is widely accepted in the political science literature (Butler 1951; Niemi and Fett 1986), although it has not been formalized statistically. Our data, such as those in Figure 4, are consistent with this pattern. This is also consistent with our assumption in Equation (2) of no interaction between γ_i and δ_t.

We apply this model to our data in four steps.

1. *Preliminary Estimation.* With data from several consecutive elections, we estimate the global parameters of the model. These include σ^2 and uncontested effective vote parameters μ_{un} and σ_{un}, described in Section 5.

2. *Bayesian Estimation for a Single Election.* We condition on the data $u_0 = (u_{i0}; i = 1, \ldots, n)$ from a single election to sample from the posterior distribution $P(\gamma \mid u_0)$ of the vector γ. This Bayesian estimation uses the parameters determined in the previous step.

3. *The Seats–Votes Curve.* We average over $P(\gamma \mid u_0)$ to estimate the posterior seats–votes curve:

$$E(S_t \mid V_t, u_0). \tag{4}$$

(We allow V_t to range from 0 to 1 by allowing δ_t to range from $-\infty$ to ∞ on the logit scale.) We estimate the expected variance of results across hypothetical elections:

$$E(\text{var}(S_t \mid V_t, u_0, \gamma)). \tag{5}$$

We also estimate uncertainty in the seats–votes curve due to our uncertainty in γ:

$$\operatorname{var}(E(S_t \mid V_t, u_0, \gamma)). \tag{6}$$

4. Summaries. From the estimated seats–votes curve (4) and related conditional expectations, we estimate bias and responsiveness summaries of the definitons in (1):

(average bias between $V = .45$ and $V = .55$)

$$= \frac{1}{.55 - .45} \int_{.45}^{.55} (E(S \mid V) - [1 - E(S \mid 1 - V)])\, dV$$

(average responsiveness between $V = .45$ and $V = .55$)

$$= \frac{1}{.55 - .45} [E(S \mid V = .55) - E(S \mid V = .45)]. \tag{7}$$

We define these summaries from $V = .45$ to $V = .55$. This is a convenient range, symmetric about .5, within which most statewide votes fall. We calculate the posterior mean and variance of these summaries.

5. ESTIMATION OF HYPERPARAMETERS

5.1 Election-to-Election Variability

Our linear model creates hypothetical district election results u_{it} from the district effects γ_i by adding a constant shift δ_t to the mean in every district. From here, we add the variability in (2); this "unexplained" variance σ^2 determines the scope of the electoral system identified with the family of hypothetical elections. Setting $\sigma^2 = 0$, for example, causes the district effects to be exactly identified: $\gamma_i = u_{i0}$. This assumption of "uniform partisan swing" on the logit scale cannot hope to fit more than a single statewide election.

We estimate σ^2 from a model of the variances in real district-level election results, across time. We use the following conceptual model:

(variance between two elections, Y years apart)

= (variance due to randomness in individual elections) + (variance due to changes in the underlying electoral system).

In this framework, the first term on the right side of this equality is $2\sigma^2$; we imagine the second quantity to be roughly proportional to Y. Note that, from (2), the difference $u_{it_1} - u_{it_2}$ has variance $2\sigma^2$ if their two Normal distributions are independent.

For each state, we calculate the sample variance of the change in district vote between election years t_1 and t_2, for districts contested in both elections:

$$s_{t_1t_2}^2 = \frac{1}{n_{t_1t_2}} \sum_i [u_{it_1} - u_{it_2} - (\bar{u}_{t_1} - \bar{u}_{t_2})]^2,$$

where $n_{t_1t_2}$ is the number of districts in the state contested in both elections t_1 and t_2. We calculate this quantity for all election years (t_1, t_2), $t_1 < t_2$, between 1972 and 1980; that is, we do not track district votes across redistricting.

We then fit a linear regression of the values $s_{t_1t_2}^2$, as a function of the time differences $(t_2 - t_1)$. For each state, our estimate of $2\sigma^2$ is just the estimate of the constant term in this regression, and with an estimate of the regression slope pooled across the three states. This yields estimates of σ (on the logit scale) as .22, .19, and .22 for Ohio, Connecticut, and Wisconsin, respectively, each with a standard error of estimation of .02.

5.2 The Distribution of District Effects γ_i

We need to estimate the vector γ of district effects and our uncertainty in it. Embedding γ in a lower-dimensional probabilistic model allows us to estimate these n district effects from the n data points u_{i0}; we can also then conveniently summarize our results in a posterior distribution.

We consider the district effects to be drawn from a mixture of three Normal distributions, identified by an eight-dimensional parameter $\theta = (\mu_j, \rho_j^2 - \sigma^2, \lambda_j; j = 1, 2, 3)$ of means, variances, and mixture proportions, with the constraint $\lambda_1 + \lambda_2 + \lambda_3 = 1$. These three humps are meant to fit plots like Figure 3, with areas of Democratic strength, areas of Republican strength, and some districts that fit no clear pattern. The parameter ρ_j^2 is the variance of the jth Normal distribution in the density of observed district vote proportions u_{i0}; $(\rho_j^2 - \sigma^2)$ is the variance of the jth Normal distribution in the density of expectations γ_i.

The method of maximum likelihood is inadequate to estimate these eight parameters, since the likelihood function is unbounded. Therefore, we give the eight parameters a prior distribution and move to Bayesian estimation. It is mathematically convenient, and substantively sufficient, to choose a family conjugate to an $N(\gamma_i, \sigma^2)$ distribution:

$$\mu_j \sim N(\mu_{\mu_j}, \sigma_{\mu_j}^2), \qquad j = 1, 2, 3$$

$$\rho_j^{-2} \sim \Gamma(\tfrac{1}{2}\alpha_{\rho_j}, \tfrac{1}{2}\beta_{\rho_j}), \qquad j = 1, 2, 3$$

$$(\lambda_1, \lambda_2, \lambda_3) \sim \text{Dirichlet}(a_{\lambda_1}, a_{\lambda_2}, a_{\lambda_3}). \tag{8}$$

Table 2 specifies these distributions; we have chosen these hyperparameters based on our substantive knowledge, and from inspection of stem-and-leaf plots like Figure 3 and for many statewide elections (King and Gelman in press). When possible, we approximate to make prior assumptions about θ vague rather than overly restrictive. Note that the prior distribution for γ_i is symmetric about 0, hence treating the political parties equally. We allow the parameters γ and θ to change each election year.

Finally, we truncate this distribution so that $(\rho_j^2 - \sigma^2) \geq 0$ for $j = 1, 2, 3$.

Table 2. Specified Hyperparameter Values for the Prior Distribution on θ

Parameter	$j = 1$	$j = 2$	$j = 3$
$\mu_{\mu j}$	−.4	.4	0
$\sigma_{\mu j}$	.4	.4	3
$\alpha_{\rho j}$	4	4	4
$\beta_{\rho j}$	.16	.16	.64
$a_{\lambda j}$	19	19	4

5.3 Uncontested Elections

For an uncontested Democratic district election, we approximate the uncertainty in the effective vote by the information in the stem-and-leaf plot of Figure 1. We then fit this to a Normal density on the logit scale: that is, for each uncontested seat i,

$$u_{i0} \sim N(\mu_{un}, \sigma^2_{un}).$$

Our data yield the estimates $(\hat{\mu}_{un}, \hat{\sigma}_{un}) = (.74, .57)$. Assuming this distribution to be independent of u_{it} in Equation (2), we get another Normal distribution for the uncontested district effects:

$$\gamma_i \sim N(\mu_{un}, \sigma^2_{un} - \sigma^2), \tag{9}$$

where $\sigma^2_{un} > \sigma^2$. We then truncate this distribution to be all-positive, so that an uncontested seat will always favor the winning party. We also symmetrically define γ_i for a Republican uncontested district to be distributed as $N(-\mu_{un}, \sigma^2_{un} - \sigma^2)$, truncated to be negative. (Recall that 0 on the logit scale is .5 on the votes scale.)

6. BAYESIAN ESTIMATION FOR A SINGLE ELECTION

We summarize posterior distributions by sampling from, in the following order:

1. $P(\theta \mid u_0)$
2. $P(\gamma \mid \theta, u_0)$
3. $P(u_t \mid \delta_t, \gamma, \theta, u_0) = P(u_t \mid \delta_t, \gamma)$.

Together, these steps amount to sampling from the desired posterior distribution of election results. (All of these distributions are of course conditional on the parameters specified in Sec. 5.)

6.1 Averaging Over Uncertainty in θ

The likelihood function $P(u_0 \mid \theta)$ is the product of n independent densities: $u_{i0} \sim$ Normal-mixture$(\mu_j, \rho^2_j, \lambda_j; j = 1, 2, 3)$. The posterior density $P(\theta \mid u_0)$ is cumbersome, because of the Normal-mixture terms in the likelihood. Direct sampling or numerical integration over this eight-dimensional distribution seems impossible. With a Normal likelihood, however, simulation of θ would be easy. We exploit this possibility through the data augmentation algorithm of Tanner and Wong (1987).

First, we decompose the Normal mixture through a matrix of unobserved indicator variables $\tau = (\tau_{ij}; i = 1, \ldots, n; j = 1, 2, 3)$. The likelihood $P(\mu_0 \mid \theta)$ can then be factored into independent multinomial distributions for the indicators $(\tau_{i1}, \tau_{i2}, \tau_{i3} \mid \theta) \sim$ multinomial$(\lambda_1, \lambda_2, \lambda_3; 1)$, for $i = 1, \ldots, n$, and a Normal distribution for the data, conditional on the indicators $(u_{i0} \mid \tau_{ij} = 1, \theta) \sim N(\mu_j, \sigma^2_j)$.

Next, we sample from $P(\theta \mid u_0)$, in two steps, using the intermediate variable τ.

1. Sample from $P(\tau \mid u_0)$
2. Sample from $P(\theta \mid \tau, u_0)$.

Step 2, using Bayes's theorem with our conjugate prior distributions (8), is straightforward:

$$(\rho_j^{-2} \mid \tau, u_0) \sim \Gamma(\tfrac{1}{2}(\alpha_{\rho_j} + n_j), \tfrac{1}{2}(\beta_{\rho_j} + SS_j)), \quad j = 1, 2, 3,$$

$$(\mu_j \mid \rho^2_j, \tau, u_0) \sim N(\mu^*_j, \rho^*_j), \qquad j = 1, 2, 3,$$

and

$$(\lambda_1, \lambda_2, \lambda_3 \mid \tau, u_0) \sim \text{Dirichlet}(a_{\lambda_j} + n_j; j = 1, 2, 3),$$

where

$$n_j = \sum_i \tau_{ij}, \qquad \mu^*_j = \frac{\sigma^2_{\mu_j} n_j \bar{u}_j + \rho^2_j \mu_{\mu_j}}{\sigma^2_{u_j} n_j + \rho^2_j},$$

$$SS_j = \sum_i \tau_{ij}(u_{i0} - \bar{u}_j)^2,$$

$$\bar{u}_j = \frac{1}{n_j} \sum_i \tau_{ij} u_{i0}, \qquad \rho^{*2}_j = \frac{\sigma^2_{\mu_j} \rho^2_j}{n_j \sigma^2_{\mu_j} + \rho^2_j}.$$

In addition, the values ρ^2_j are constrained to be no less than σ^2. If we simulate too low a value for a ρ_j, we just keep repeating the simulation of θ until we satisfy the constraint.

Step 1 is intractable as stated but would be easy if θ were known, because

$$(\tau_{i1}, \tau_{i2}, \tau_{i3} \mid \theta, u_0) \sim \text{multinomial}(\lambda^*_{i1}, \lambda^*_{i2}, \lambda^*_{i3}; 1) \quad \text{for } i = 1, \ldots, n,$$

where

$$\lambda^*_{ij} \propto \lambda_j \frac{1}{\rho_j} \phi\left(\frac{u_j - \mu_j}{\rho_j}\right) \quad \text{for each } i, j,$$

and ϕ is the standard Normal density function. In our application of the data augmentation algorithm, we simulate a single random sample θ^* from $P(\theta \mid u_0)$, as follows.

1. Choose a reasonable starting point for θ^*. We use the posterior maximum of $P(\theta \mid u_0)$, which we estimate by the EM algorithm (Dempster, Laird, and Rubin 1977), again treating τ as unobserved data.
2. Repeat the following steps a number of times: (a) sample τ^* from $P(\tau \mid \theta = \theta^*, u_0)$ and (b) sample θ^* from $P(\theta \mid \tau = \tau^*, u_0)$. For our data, the distribution of simulated values θ^* appears to converge after 10 iterations. Increasing the number of iterations did not noticeably change the distribution of simulated values of θ^* or our final results.

Iterations of this procedure yield approximately independent random samples from the posterior distribution of θ. We found that 50 iterations provided sufficient precision.

6.2 Averaging Over Uncertainty in γ

We can factor the conditional posterior density as follows:

$$P(\gamma \mid \theta, u_0) = \prod_i P(\gamma_i \mid \theta, u_{i0}) \propto \prod_i P(u_{i0} \mid \gamma_i, \theta) P(\gamma_i \mid \theta).$$

The first factor here is just the Normal error density from the model (2), and the second factor is the Normal-mixture density parameterized by θ. Their product yields a new Normal-mixture density with easily calculated parameters $\tilde{\theta}_i$ for each district; we sample from these independent distributions.

For each uncontested district, we simulate γ_i from the truncated Normal distribution (9). We combine these with the simulated values γ_i for contested districts to get a sample vector γ from its posterior distribution.

6.3 Averaging Over u_t

To estimate the seats–votes curve and its variability, we first approximate the first two moments of the joint conditional distribution $P(V_t, S_t \mid \gamma, \delta_t)$, for several values of δ_t. Figure 5 provides an intuitive sense of our model and sampling procedure by plotting several simulated values u_{it} for $\delta_t = 0$, as a function of observed district votes u_{i0}, for Ohio in 1972. Note the assumed distribution of effective votes for the uncontested districts.

The aggregate votes and seats are averages [Eqs. (3)] of their district-level counterparts v_{it} and s_{it}, which in turn depend on γ_i and δ_t only through their mean $\alpha_{it} = \gamma_i + \delta_t$. Thus the desired conditional moments can be expressed in terms of the following expectations:

$$E(v_{it} \mid \alpha_{it}) = \int_{-\infty}^{\infty} \frac{e^u}{1 + e^u} \frac{1}{\sigma} \phi\left(\frac{u - \alpha_{it}}{\sigma}\right) du,$$

$$E(s_{it} \mid \alpha_{it}) = \int_0^{\infty} \frac{1}{\sigma} \phi\left(\frac{u - \alpha_{it}}{\sigma}\right) du = \Phi(\alpha_{it}/\sigma),$$

$$\operatorname{var}(v_{it} \mid \alpha_{it}) = \int_{-\infty}^{\infty} \left(\frac{e^u}{1 + e^u}\right)^2 \frac{1}{\sigma} \phi\left(\frac{u - \alpha_{it}}{\sigma}\right) du - [E(v_{it} \mid \alpha_{it})]^2,$$

$$\operatorname{var}(s_{it} \mid \alpha_{it}) = E(s_{it} \mid \alpha_{it})[1 - E(s_{it} \mid \alpha_{it})],$$

and

$$\operatorname{cov}(v_{it}, s_{it} \mid \alpha_{it}) = \int_0^{\infty} \frac{e^u}{1 + e^u} \frac{1}{\sigma} \phi\left(\frac{u - \alpha_{it}}{\sigma}\right) du - E(s_{it} \mid \alpha_{it}) E(V_{it} \mid \alpha_{it}).$$

Some of the foregoing integrals are immediately evaluated through the standard Normal distribution function Φ; we calculate the rest by approximating the inverse logit function $e^u/(1 + e^u)$ by a third-degree polynomial in u.

We now approximate the seats–votes curve $E(S \mid V)$ versus V by the function defined by $E(S_t \mid \alpha_t)$ versus $E(V_t \mid \alpha_t)$, implicitly parameterized by α_t (or, equivalently, by the scalar δ_t). Similarly, we approximate the variance as follows:

$$\operatorname{var}(S_t \mid V_t) \approx \operatorname{var}(S_t \mid \alpha_t) - \frac{\operatorname{cov}(V_t, S_t \mid \alpha_t)}{\operatorname{var}(V_t \mid \alpha_t)}.$$

This variance depends on V_t and is parameterized by δ_t in the foregoing expression. The formula would be exactly

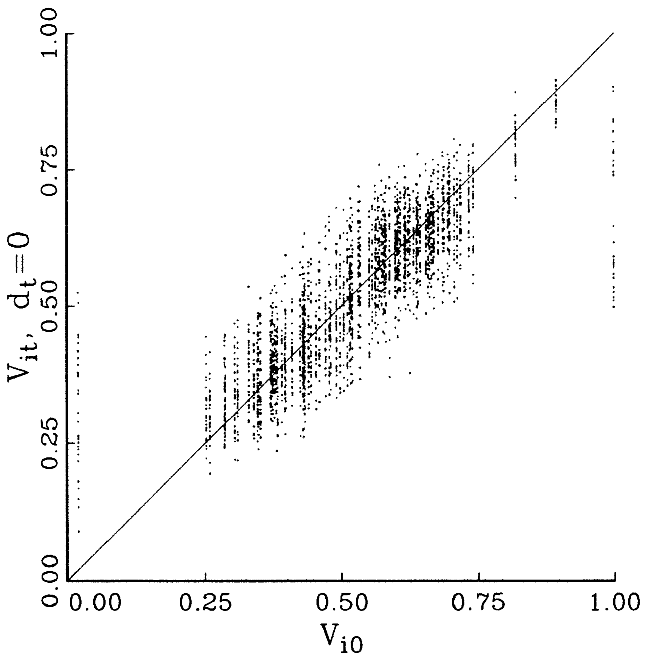

Figure 5. Simulations, Ohio, 1972.

correct if S_t and V_t were jointly Normally distributed, and it is a reasonable approximation for our problem.

6.4 Calculating Summaries

Finally, we simulate several vectors γ from the posterior density $P(\gamma \mid u_0)$. Each of these samples determines an electoral system, for which we approximate the seats–votes curve and its variance, as described previously. From the seats–votes curve, we calculate the bias and responsiveness of the system between 45% and 55% [Eqs. (7)]. Finally, we estimate the bias and responsiveness of the true electoral system, and our uncertainty in these quantities, with the sample mean and variance of these values, over the many independent samples of γ.

All computations were done in the Gauss computer language on an IBM PS/2.

7. RESULTS

The procedure described in Section 6 produces estimates of an electoral system from the results of a single statewide election. This includes estimates of the seats–votes curve, its variability, and summaries such as the bias and responsiveness functions. Our model assumes that district votes move in an approximate uniform manner as the statewide vote totals change. Because of the lack of information, we assume the absence of spatial correlation. Finally, we assume that the district votes roughly follow a three-hump distribution specified by our family of prior distributions. Within these constraints, our model is quite general and fits recent legislative electoral data quite well.

An example of the complete results appear in Figure 6. The solid line in this figure is the estimated seats–votes curve $E(S \mid V)$ for Ohio in 1972. The dotted lines show plus and minus two standard errors of estimation: $E(S \mid V) \pm 2\operatorname{var}(E(S \mid V, \gamma))^{1/2}$. Instead of presenting seven of these figures for each of three states, we summarize the

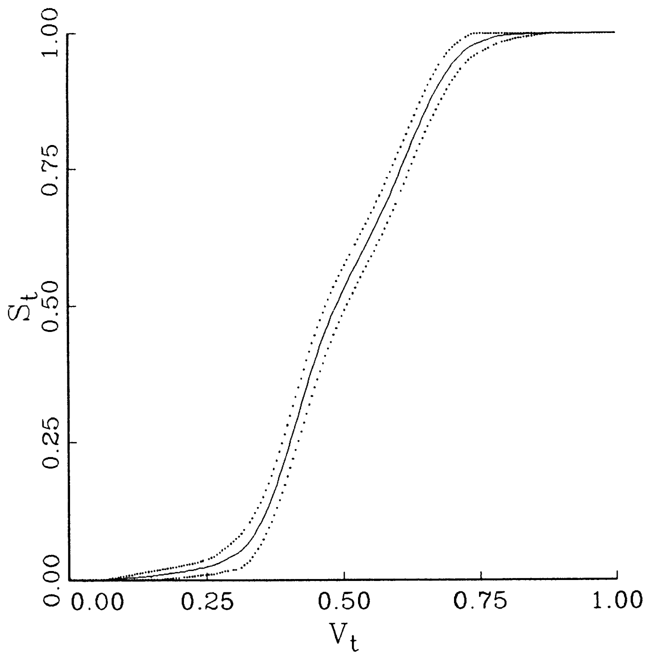

Figure 6. Estimated Seats–Votes Curve, Ohio, 1972.

results for each election from 1968 to 1980, using Formula (7).

The results for all seven years in Ohio appear in Figure 7, where responsiveness is plotted by partisan bias. Pooled standard error estimates appear in the lower left of the figure. The black square marks 1968, a year of moderate responsiveness but with an extreme bias favoring the Republicans. The next square is 1970, which is close to and within two standard errors of 1968. In 1971, the Democrats controlled the redistricting process, dramatically affecting Ohio's electoral system: the dotted line drawn between 1970 and 1972, to indicate redistricting, represents a systematic change from extreme Republican bias to slight Democratic bias—far beyond what one would expect due to mere random variability. The change also appears permanent, as the elections over the course of the rest of the decade remain at or above the initial level of Democratic

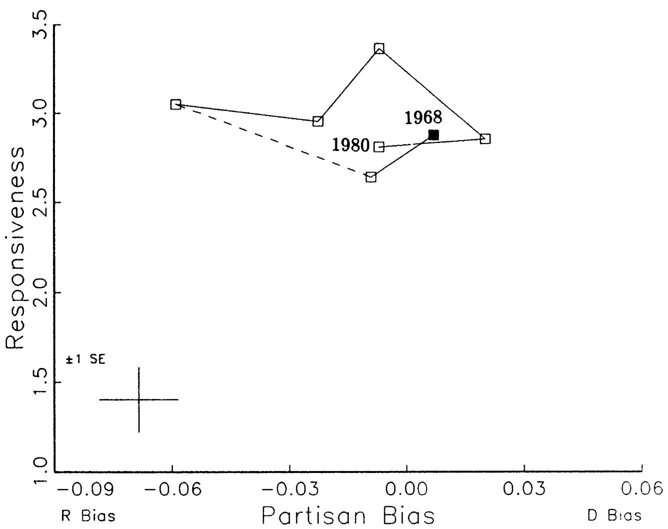

Figure 8. Connecticut House, 1968–1980.

bias. The other change in the figure is a noticeable trend after redistricting toward lower responsiveness.

The changes in Connecticut's electoral system are portrayed in Figure 8. All of the years in Connecticut have electoral systems that are quite responsive, particularly compared with Ohio. In 1968 and 1970, Connecticut had essentially no partisan bias. The 1971 redistricting was controlled by the Republicans, and their effect in biasing the system in their favor seems quite dramatic—again much beyond what one would expect due to random variability. This dramatic effect seems ephemeral, however, since over the course of the rest of the decade the electoral system worked its way back to just about where it began. The Republican gerrymanderers in Connecticut were obviously not as successful as their Democratic counterparts in Ohio. We speculate that the pattern of incumbency retirements accounts for this difference—particularly since the Watergate landslide in 1974 helped to defeat many Republican state legislators.

Figure 9 portrays Wisconsin's electoral system. Because a single party did not elect a governor and a majority of

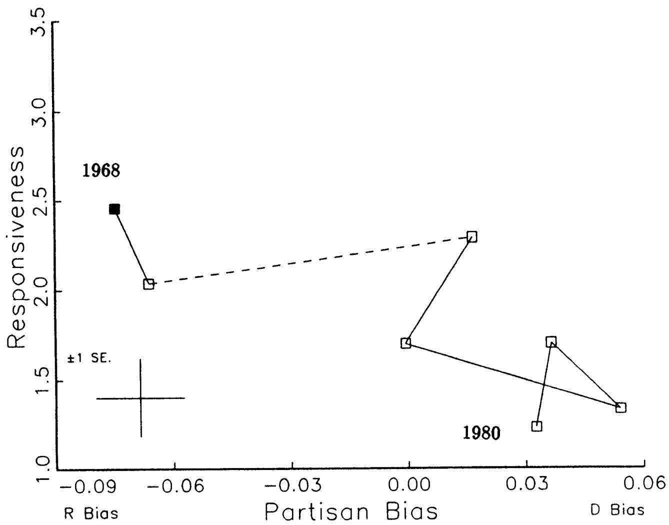

Figure 7. Ohio House, 1968–1980.

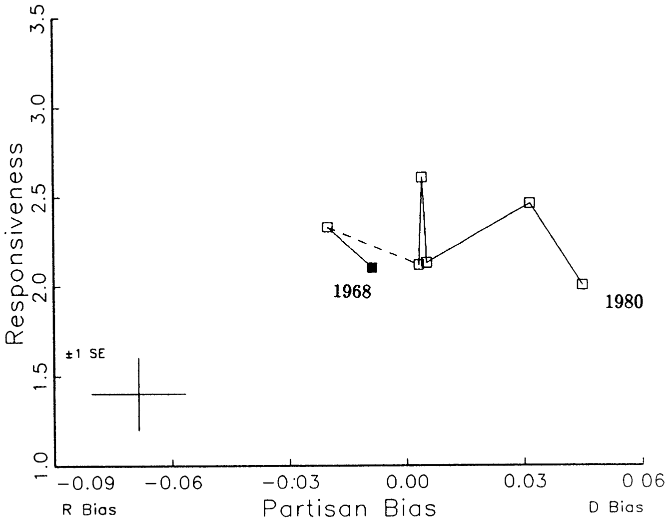

Figure 9. Wisconsin House, 1968–1980.

both houses of the state legislature, Wisconsin was redistricted by a bipartisan agreement between the parties. Redistricting thus has a quite predictable non-effect on the system: the change from 1970 to 1972 is no greater than most other changes between consecutive elections in this graph. Political scientists have speculated that bipartisan redistricters primarily try to protect incumbents; with fewer seats of both parties vulnerable to electoral swings, this would decrease responsiveness (Mayhew 1971). Surprisingly, Wisconsin's responsiveness changes no more across redistricting than between any other two consecutive elections. Of course, responsiveness in Wisconsin started from a low base; perhaps redistricters could not reduce responsiveness any further due to the geographic pattern of voters in the state.

When controlling the redistricting process, partisans have successfully biased the electoral system in their favor, at least in the short term. A glance at Figures 7–9 shows that redistricting had no systematic effect on responsiveness in any of the three states. All previous seats–votes models have been either deterministic, entirely theoretical, or average over many elections. Some have ignored partisan bias and either fit responsiveness or fixed it to the value of 3.0; other models have assumed the electoral system to be constant over several elections. We explicitly model variability and generate estimates and standard errors of bias and responsiveness for each statewide election. A comparison of the changes between elections with the standard errors in Figures 7–9 leads us to reject deterministic models and those with constant bias and responsiveness.

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I

Introduction

ELBRIDGE GERRY'S SALAMANDER

The word *gerrymander* describes a distinctively (albeit not uniquely) American practice, that of redrawing district lines to achieve partisan (or other) advantage. The word also has a distinctively American etymology, dating back to Elbridge Gerry's term as governor of Massachusetts (1810–1812), when political observers made sport of a district drawn by his party that looked something like a salamander.

At the broadest level, indicated by its title, this book is about gerrymandering. The principles of our analysis could be applied to the original Gerry-mander or to any of its various and long line of descendants (for one such effort, see Engstrom 2001).

At a narrower and more specific level, indicated by its subtitle, this book concerns what was arguably the most important change in the practice of American gerrymandering since its invention.[1] Whereas previously the game of drawing salamanders with district lines was limited to legislators and governors, the courts standing scrupulously aside, after 1964 the rules changed. A new process emerged, with new strategic consequences and nuances. We examine how these procedural changes help explain two of the biggest stories in congressional elections since the 1960s: the seemingly invulnerable Democratic majority in the House of Representatives before 1994 and the seemingly unfair and bloated advantage of incumbents over challengers.

[1] The practice of gerrymandering certainly predated the Gerry-mander, but its origin has not been precisely dated, so far as we know.

THE REAPPORTIONMENT REVOLUTION

The Supreme Court's reapportionment decisions, beginning with *Baker v. Carr* in 1962, were soon hailed by legal scholars as revolutionary (see, e.g., Baker 1966, p. 3; Dixon 1968, p. 99).[2] They reversed decades of court decisions that had consistently held that the drawing of legislative district lines, fraught though it was with malapportionment and gerrymandering, was not justiciable. They opened the door to a long chain of subsequent litigation, which continued into the 1990s, with important decisions regarding racial gerrymandering. For these and other reasons, the reapportionment decisions now occupy a standard niche in textbooks on the Court.

The Court's decisions did not simply rewrite case law, however. They also sparked a massive wave of extraordinary redistricting in the mid-1960s.[3] Both state legislative and congressional districts were redrawn more comprehensively – by far – than at any previous time in our nation's history.

In the immediate aftermath of the Court's decisions and the consequent redistricting, scholars looked carefully for political consequences, yet concluded that they were very small. Neither party seemed to benefit nationwide, as their gains in some states were offset by losses elsewhere. Incumbents did not seem to benefit, as their margins of victory increased even where redistricting did not take place. Policy did not seem to shift toward urban interests in the dramatic way widely anticipated.

These conclusions were surprising, not just because of the magnitude of the judicial shift in doctrine or the depth and breadth of 1960s redistricting action, but also because of two statistical regularities later described in the scholarly literature. First, work on how congressional votes translated into congressional seats outside the South found a consistent pro-Republican bias prior to the 1960s. By one estimate (Erikson 1972, p. 1234), the Democrats could expect to win only 44.6% of the nonsouthern seats when the aggregate vote division was a 50–50 partisan split – indicating a 5.4% pro-Republican bias. This bias abruptly disappeared in the mid-1960s. Second, scholars found that the so-called incumbency advantage – a vote premium putatively derived from the

[2] *(Re)apportionment* refers to the (re)allocation of seats in the U.S. House of Representatives to the states after each decennial census.

[3] *Redistricting* refers to the drawing up of new district boundaries within each state – typically but not always pursuant to reapportionment.

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resources of office – jumped up dramatically in 1966, the year of the first election in which a substantial number of districts had been redrawn under court order (Erikson 1972; Gelman and King 1990). The size and abruptness of these two statistical changes, and their coincidence with widespread court-ordered redistricting, seem more than coincidence. Yet the literature has intensely scrutinized these factors and found no causal link between them and redistricting.

REEXAMINING THE CONSEQUENCES OF THE REAPPORTIONMENT REVOLUTION

This book reexamines the electoral consequences of the reapportionment revolution. The bulk of the previous literature has focused on the primary *substantive* consequence of the Court's decisions – the eradication of malapportionment in U.S. legislative districts at both the state and federal levels. Our work focuses on the primary *procedural* consequences of the Court's decisions – the redefinition of the legally mandated default outcome to the redistricting processes in the 50 states, and the increased regularity and frequency with which redistrictings were undertaken. We use these procedural shifts to explain sea changes in two struggles central to congressional elections: that between Democrats and Republicans, on the one hand, and that between incumbents and challengers, on the other.

Democrats and Republicans

As regards the partisan struggle between Democrats and Republicans, our argument starts by noting that the Supreme Court's decisions fundamentally altered the *reversionary* (or default) *outcome* of the redistricting processes in the states. That is, they altered what would happen at law should the state government fail to enact a new congressional districting statute. Once one controls properly for the nature of the legal reversion when analyzing the impact of redistricting, several consequences of the Court's reapportionment decisions come into focus.

First, these decisions made the courts strategic players in all subsequent redistricting actions. The courts were players in those cases where an explicit suit had been brought, because the courts then determined the reversion. But even where no suit had yet been brought, each party might worry that the other would bring a suit, were the redistricting plan not to its liking, at which point each party had to worry about where the suit would be brought and what reversion the relevant court would impose.

Second, the courts' ability to set the reversion, combined with the latitude they had in the early years after *Wesberry v. Sanders* in deciding when a plan was sufficiently well apportioned, gave them substantial influence over the final districting plans adopted. Thus, the partisan complexion of the federal judiciary in the 1960s played a central role in redistricting outcomes, as will be seen.

Third, the Supreme Court's decisions unleashed a wave of redistricting just after Lyndon Baines Johnson's landslide victory over Barry Goldwater in 1964 had substantially weakened Republican control of nonsouthern state governments and during a period when the federal judiciary was heavily Democratic. Thus, redistricting in the 1960s was conducted *by* state governments that were less often under unified Republican control than had historically been the case and *under the supervision of* courts that were largely Democratic. This combination produced a substantial net partisan advantage for the Democrats, evidenced not only in the abrupt disappearance of pro-Republican bias outside the South but also in the detailed patterns of how vote shares changed when district lines were redrawn.

Our explanation of the partisan consequences of the reapportionment revolution is quite different from those offered in the literature, both in the line of argument pursued (no one has stressed reversionary outcomes and the strategic role of the courts in the previous literature) and in the conclusion reached (that there was a substantial net partisan consequence directly attributable to the reapportionment revolution). We detail our argument and findings in Part II of the book.

Incumbents and Challengers

As regards the electoral struggle between incumbents and challengers, we argue that the key to understanding the dramatic growth in the apparent advantage of incumbents is to recognize that they are strategic agents, deciding whether to seek reelection or not based on their forecast vote shares. We show that much of the incumbency advantage, as previously measured, reflects incumbents' prudence – getting out when the getting is good – rather than their superior campaigning ability or resources.

We then explore how redistricting affected incumbents' prudential exits and challengers' strategic entries. One line of argument concerns anticipations of redistricting. After the Supreme Court's reapportionment on the relevant court would

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decisions, politicians soon realized that redistrictings, rare and not always forseeable beforehand, would now be unavoidable and regular. This recognition, in turn, facilitated better coordination between incumbents and strong challengers, inducing a stronger redistricting rhythm to congressional entry and exit. Better coordination meant that incumbents more often got out in the face of a particularly formidable challenge, increasing the statistical association between running an incumbent and the incumbent party's vote share.

Another line of argument begins by noting that the eradication of pro-Republican bias in the translation of congressional votes into seats resulted in an abrupt decline in the Republicans' probability of attaining a majority of seats in the House of Representatives. This intensification of the Republicans' minority status exacerbated a syndrome of recruitment-related woes for the party, resulting in significantly larger estimated incumbency advantages for the Republicans than for the Democrats. This last finding, nonobvious and unnoticed in the previous literature, is much at odds with previous theories of why the incumbency advantage arose but follows naturally from our emphasis on strategic entry and exit (as will be shown).

Normative Concerns

To the extent that our explanation of the reapportionment revolution's electoral consequences is correct, these consequences do not pose the threats to our system that many scholars, journalists, and politicians have associated with them. Those who have seen the increasing resources attached to congressional office as increasing the incumbency advantage, and hence bolstering the Democrats' perennial majority status, have correctly been worried. Whenever the resources of public office are used to insulate individual politicians from electoral risk, their accountability to their constituents is weakened. Whenever government resources are used to entrench a single party in government, its accountability to the public at large is weakened. Thus, insulation from electoral risk of the kind suspected would, at a single stroke, debilitate the two fundamental accountability relationships of a democratic system of government.

However, by our story, the insulation of House incumbents is more apparent than real. It is not just that incumbents always "run scared," per Mann (1977) and others. It is that they retire when scared off, and this propensity inflates standard estimates of the incumbency advantage. Indeed, by our estimates, the incumbency advantage enjoyed by

Democratic incumbents was never – even after 1966 – statistically discernible from zero. Nor, at the aggregate level, has the Democratic party had an unfair advantage in how votes translate into seats.[4] The major premises of the preceding arguments thus largely disappear.

EXCLUDING THE "SOUTH"

In the first part of this book, we focus on redistricting actions in the period before (1946–1962) and immediately after (1964–1970) the Supreme Court's reapportionment decisions. In this part, we exclude southern states from the analysis. There are two main justifications for doing so.

First, the legal default to the redistricting process plays a central role in our theory, yet this default was entirely different in the South from elsewhere. Following enactment of the Voting Rights Act in 1965, the Section 5 preclearance rules imposed on seven southern states made the Justice Department the primary arbiter of redistricting plans rather than the courts. Moreover, these Section 5 states were subject to unique legal restrictions on their redistricting plans, such as prohibitions on vote dilution and retrogression.

Second, our theory of redistricting assumes that there was a significant level of interparty competition and that both parties were unitary actors seeking to maximize their respective expected seat shares.[5] In the South, however, especially before passage of the Voting Rights Act but also in the early years thereafter, the Democratic party utterly dominated the scene and the Republicans were a hopeless minority. We believe that southern redistricting before and even in the 1960s was much more a matter of factional infighting within the dominant party than partisan gerrymandering fought out between the parties.

All told, the politics of redistricting in the South has been theoretically quite distinctive for most of the postwar era on which we focus. Although the basic principles of our approach could be adapted to study southern redistricting, the specific model we employ cannot. Thus, we leave the South for another time.[6]

[4] For a contrary view regarding whether partisan bias has been near zero, see Campbell (1996).

[5] More precisely, we assume that parties seek to maximize the utility they derive from their seats. This allows the model to recognize that parties' attitudes toward risk mattered, as explained in Chapter 3.

[6] For a recent examination of the politics of redistricting in the South, see Canon (1999).

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To be consistent, of course, we should exclude *all* states that were subject to Section 5 preclearance or were "uncompetitive." The first criterion (Section 5 preclearance) is not an issue because no nonsouthern states were affected until after the period of focus in the first part of the book (1946–1970). To systematize the second criterion, we consulted the well-known Ranney index of party competition for the period that most closely matched our period of primary focus (viz., 1964–1970, the elections immediately after the relevant reapportionment decision).[7] All states that were more competitive than all of the already-excluded southern states were included in our analyses. Two border states, Maryland and Tennessee, were even less competitive than some already-excluded states, however, and were accordingly also excluded. Thus, our definition of what is "southern" is slightly more expansive than the definition often used in the literature.

OUTLINE OF THE BOOK

This book is divided into four parts: an introduction, two main substantive parts, and a conclusion. The next chapter sets the stage by describing the Court's decisions, the condition of congressional districts before and after redistricting, and the reasons offered in the literature as to why redistricting should have been relatively inconsequential. In the same chapter, we also elaborate on the theoretical importance of the reversionary outcome to redistricting, describe how it changed with the Court's decisions, and argue that this provides the key to a long-standing puzzle about congressional elections in the 1960s.

Chapter 3 begins Part II by presenting the first half of a general model of the redistricting process(es) in the American states. Chapter 4 uses this model to estimate bias and responsiveness in postwar congressional elections outside the South. Our results clearly demonstrate the importance of the reversionary outcome even before the Court's reapportionment decisions. Chapters 5 and 6 extend the model to include the courts as strategic actors, as is appropriate for redistricting actions after *Baker v. Carr*. Our empirical results show that the partisanship of the judges supervising redistricting cases in the 1960s was at least as important as which party controlled state government in affecting the character of the plan adopted.

[7] The closest match to the 1964–1970 time period is the index for 1962–1973 published in Ranney (1971).

The findings of Chapters 3–6 can be interpreted broadly as follows: outside the South, the nation's congressional districts were mostly products of Republican gerrymanders before *Baker* but increasingly products of bipartisan plans or Democratic gerrymanders afterward. If this general thesis is correct, it implies some very specific district-level consequences of redistricting in the 1960s. Chapter 7 investigates these implications and finds them to hold.

In Chapter 8, we set the stage for the analyses of Part III in two ways. First, we review classic evidence that congressional incumbents abruptly began winning by larger vote margins in the 1960s, review previous attempts to explain this and the related increase in the *incumbency advantage*, and sketch our own explanation. Second, we show that Republican incumbents' margins increased more than did Democratic incumbents' margins, and that the Republican incumbency advantage increased more than did the Democratic incumbency advantage (when measured, as is typical, in vote shares). We thus add to the list of explananda that a complete model of postwar congressional elections must address: although there was no systematic difference between the two parties' incumbency advantages before the reapportionment revolution, afterward Republicans tended to benefit more from incumbency.

In Chapters 9 and 10, we seek to explain the patterns of growth in the incumbency advantage as consequences of the reapportionment revolution's impact on political recruitment and career planning. Chapter 9 explores how incumbents' ability to enter or exit in light of their vote forecasts affects previous estimates of the incumbency advantage. Chapter 10 demonstrates that entry by strong challengers and voluntary exit by incumbents have followed the redistricting cycle more regularly since the mid-1960s, arguing that this partly explains the increasing success of incumbents and strong challengers at avoiding contests against one another. Chapter 11 argues that the considerable differences between the two parties in recruitment and career paths that emerged after the mid-1960s stem in good part from (1) the reapportionment revolution's eradication of pro-Republican bias (shown in Part II), which (2) intensified the Republicans' minority status and hence (3) drove a wedge between how candidates of the two parties valued House seats. Chapter 12 concludes our discussion of the incumbency advantage and compares our thesis to previous explanations.

In Chapter 13 (Part IV), we review the various consequences of the reapportionment revolution. In understanding both the battle between Republicans and Democrats and that between incumbents and chal-

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Assessing the Partisan Effects of Redistricting

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The purpose of this article is to assess the reality behind the politician's perception that redistricting matters. There are, of course, many dimensions to that perception, because redistricting has many effects. This articles focuses on the impact of boundary changes on the partisan composition of seats. In order to do this, it will be necessary to specify what the expected partisan effects of redistricting are and how they can be measured. Thus, I first explain how the impact of redistricting will vary with the strategy of particular plans and then explore some techniques for measuring the partisan impact of boundary changes. I conclude with a detailed analysis of the most important congressional redistricting in 1982—the Burton plan in California.

Most politicians and political insiders believe that redistricting is politically crucial. Although boundary disputes are somewhat esoteric by the standards of normal political discourse, the potential for causing widespread political change by redesigning district lines is great. It is curious, therefore, that the political science evidence about redistricting effects is so undramatic. Early studies indicated that the first reapportionments after *Baker* v. *Carr* advantaged Democrats, especially in urban areas (Erikson, 1972). However, attempts to link boundary with policy changes uncovered nothing striking (Bicker, 1972; O'Rourke, 1980; Saffel, 1983). Other studies seemed to imply that the major effect of redistricting was to aid incumbents (Mayhew, 1971; Tufte, 1973), but to date there has been very little evidence in support of that thesis either (Bullock, 1975; Ferejohn, 1977). Could it be then that redistricting really does not have any important impact upon the political system?

The purpose of this article is to assess the reality behind the politician's perception that redistricting matters. There are, of course, many dimensions to that perception, because redistricting has many effects. This article focuses on the impact of boundary changes on the political control of districts. Since the impact of redistricting varies with the strategy of particular plans, I begin by specifying what the expected political effects of redistricting will be under different strategies, and how these effects can be measured. Then I explore some techniques for measuring the partisan impact of boundary changes and offer a detailed analysis of a major congressional redistricting plan—the 1981 remapping of the California seats.

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A major theme of this article is that redistricting effects are tied closely to incumbency effects. Political scientists have for some time recognized the importance of incumbency in congressional races and the declining relevance of the voter's partisan identification. It should come as no surprise, therefore, that incumbency removal can be more important in determining who wins a redistricted seat than changes in district partisanship per se.

Predicting the Effects of Redistricting

One of the reasons that it has been so difficult to find any systematic or striking redistricting effects is that the types of redistrictings undertaken have varied significantly across states and periods of time. In particular, the way that a plan affects electoral outcomes depends upon the line drawers' strategy and the nature of the demographic constraints they face. As to the first, a redistricting plan can be either partisan or bipartisan in its impact. A partisan effect is one that favors a particular party (usually the majority) over the other, and a bipartisan one favors neither. To be sure, a redistricting plan will have other goals as well, such as the preservation of cities and the protection of minorities, but the political impact is the sole concern of this study.

It is also important to recognize that a plan's effect may be different from its intent. A nonpartisan commission might try to ignore partisan considerations, but any plan that it implements will have them nonetheless (Cain, 1984; Dixon, 1968).

Assume that the strategy of a plan is partisan and that the party controlling reapportionment is the one with a majority in both houses of the state legislature, how can the number of majority party seats be maximized, and what will the predicted pattern of changes be? A redistricting strategy has two components: partisan reconstruction and incumbency removal. Partisan reconstruction is defined as changes in the balance of party identifiers in a given seat (often measured in terms of party registration in states where such information is available). The aim of partisan reconstruction in this instance is to maximize majority party seats by minimizing its electoral inefficiency to the extent demographically possible. The electoral inefficiency of a particular seat is defined as the amount of excess party support enjoyed by the winning candidate. If there is a registration level *r* that guarantees that a party will win almost any contest (within some reasonable range of candidate strength), then any level of strength above *r* is wasted. For example, if the Democrats can win any seat above 60% Democrat in registration, then a 70% seat is inefficient by 10 percentage points. From a partisan gerrymandering point of view, if that excess partisan strength could be traded to a 50% Democratic seat, then the party would have two sure seats instead of one. Classic examples of inefficiently distributed Democratic areas are inner-city minority seats and of inefficiently distributed Republican areas are white, upper-income suburban seats.

Leaving aside for the moment the demographic and bargaining constraints that might obstruct the construction of a partisan gerrymander, what pattern of territorial trades should be observed? To begin with, some number of previously inefficient majority party seats will acquire less favorable territory and experience a drop in partisan strength. To compensate, a certain number of marginal majority party seats will receive favorable areas and so increase their partisan strength. In short, there should be an inverse correlation between the previous level of partisan strength and the reapportionment gain for majority party incumbents.

Just the reverse should apply to minority party seats. The most marginal minority party seats should experience a loss in party strength, and the strongest should experience the gains. Minority party strength is in effect distributed as inefficiently as possible. Hence, the correlation between previous party strength and reapportionment gain should be positive for minority party incumbents.

The second part of a partisan plan is incumbent removal. One common form of this is to parcel the territory of a targeted incumbent into several districts in order to deny the natural advantages of incumbency such as higher name recognition and a good district reputation. By itself, this tactic will not usually be sufficient, since incumbents can use their franking privileges and resource advantages to mail into the new territory to make up some of the difference in the period before the election. Consequently, severe displacement is often used in tandem with partisan reconstruction to undercut party strength and incumbency advantage simultaneously. A more subtle but equally effective strategy is to use displacement to induce a minority party incumbent to give up a seat that could be won by the majority party in order to run for a neighboring seat that could not be won by the majority party. Some specific examples of this will be discussed later. A partisan plan will attempt to remove, or to induce the removal of, minority party incumbents from as many potentially winnable seats as possible.

The key then to the partisan gerrymander is that incumbents in the party controlling redistricting will be treated differently from those in the party that does not. The average level of electoral safety might actually increase more among incumbents in the noncontrolling party than among those in the controlling party, since greater safety is a by-product of higher electoral inefficiency. If one were to consider the average gain or loss of incumbents by party, one might mistakenly conclude that the noncontrolling party was better off. The point is that many of the individual incumbents in the noncontrolling party will be better off, but if the gerrymander is effective, the party as a whole will be worse off. Indeed, one of the great difficulties for leaders in the noncontrolling party during redistricting is to get individual incumbents to forsake their short-term self-interests (i.e., whether their particular districts are to their liking) for the interest of the party (i.e., whether the plan is good or bad for the party as a whole).

The bipartisan gerrymander is much simpler. In this case, neither party gains an advantage from reapportionment without the consent of the other. Whereas the goal of the partisan gerrymander is to make one party's support more electorally efficient than another's, the object of the bipartisan gerrymander is to protect incumbents in both parties—in short, to make the partisan strength of both parties inefficient wherever there is an incumbent (for this reason, it can also be called an incumbents' gerrymander). From the self-interested perspective of the incumbents, the bipartisan gerrymander has much appeal. Incumbents who want to get stronger will seek to dispose of their least desirable areas. Because one party's undesirables are usually the other's most loyal supporters, Democrats will trade Republicans to Republican incumbents, and Republicans will trade Democrats to Democratic incumbents. Because incumbents tend to be risk averse—no margin of safety is ever too much—the result is greater electoral inefficiency and more noncompetitive seats.

In the bipartisan gerrymander, no incumbent who wants to return will be forced, unless demographically necessary, to run against any other incumbent. Moreover, the pattern of incumbency removal should be unbiased. If incumbents must be removed for demographic reasons, the burden will be more or less evenly shared under a bipartisan plan.

The strategies of partisan and bipartisan plans as outlined will not necessarily be implemented as they are intended. Various considerations will compromise the best laid plans of reapportioning men. To start with, population needs will constrain the set of feasible trades. It will, for instance, be easier to make a trade when one of the two adjoining seats is overpopulated and the other underpopulated than it will be when both are overpopulated or underpopulated. Trades between seats with noncomplementary population needs only compound initial population deficits and surpluses and cause more difficult adjustment problems in the rest of the state. Secondly, although trades between members of different parties can often be complementary because both want the other's weakest areas, trades between members of the same party will often be conflictual because both will want each other's strongest areas. This means that some strong incumbents will resist sharing their "wealth" with weaker members of their own party, further distorting the logic of the plan. Finally, there are the idiosyncratic concerns of incumbents. Incumbents will in many instances forego the partisan advantages of trades in order to keep amusement parks, fund-raising locations, favorite donors, their residences, and the like in their districts. So even if partisan malice is in the hearts and minds of the line drawers, the pure patterns of the partisan and bipartisan gerrymanders will be blurred by the noise of bargaining and demographic constraints.

Measuring the Partisan Effects of Reapportionment

Having considered the expected patterns of change associated with various types of redistricting strategies, the question is whether or not it is possible to measure the specific effects of various plans in order to determine whether a given plan is partisan or bipartisan in its impact.[1] The tech-

[1]There are several alternative ways to measure the political effects of reapportionment. The simplest class of methods compare district registrations or vote totals before and after the territorial changes caused by redistricting. For example, in states where the registration figures are published, it is possible to determine whether and by what amount the Democratic or Republican registration increased:

$$r_{d,o} - r_{d,n} \quad (1)$$

where $r_{d,o}$ and $r_{d,n}$ are the Democratic registrations in the old and new districts

Another popular method is to take the vote totals for candidate j in the last election, subtract the votes j won in the areas j loses in reapportionment, and add the votes for candidate k who ran for the same legislative office in the same election in the areas that have been transferred from k to j:

$$v_{j,n} = v_{j,o} - v_{j,l} + v_{k,a} \quad (2)$$

where

$v_{j,n}$ is the predicted vote for candidate j in the new district
$v_{j,o}$ is the vote for candidate j in the old district
$v_{j,l}$ is the vote for candidate j in the lost areas
$v_{k,a}$ is the vote for candidate k in the newly added areas where candidates j and k ran for the same legislative office in different districts in some year before redistricting.

Finally, where the data are available, it is instructive to compare the totals received by some statewide candidate under the various proposed boundary changes.

$$v_{s,n} = v_{s,o} - v_{s,l} + v_{s,a} \quad (3)$$

where

$v_{s,n}$ is the vote received by a state wide candidate in the new district
o, l and a have the meanings previously defined.

All of these methods have their particular flaws, but more generally, the difficulty with this class of methods is that it does not fully and efficiently use all the available information. For instance, two districts with the same Democratic registrations might have different Republican or minority party registrations. Moreover, since redistricting affects incumbency status as well as the underlying partisan strength of a district, merely looking at the registration figures does not give an accurate estimate of the political impact of a proposed plan.

The second class of methods, therefore, tries to eliminate this flaw by utilizing a multivariate estimation procedure to combine several pieces of information. One such technique, for instance, is to develop an expected vote model in which a candidate's vote at time t is regressed on various demographic data and on a statewide candidate's vote. This technique yields a set of estimated parameters that can be multiplied by the post redistricting political and demographic data to yield new district totals:

$$v_p = a + BZ_p + c_1 s_p + u \quad (4)$$

where

v_p is the vote for relevant district race in precinct p
$\mathbf{B}$ is a vector of coefficients
$\mathbf{Z}$ is a vector of demographic variables for precinct p
c_1 is a coefficient
s_p is the vote for a statewide candidate running in the same election in precinct p
u is the error term.

This is a particularly useful technique for redistricting nique developed for the present analysis is to try to estimate the probabilities of the Democrats and Republicans winning various seats, given information about changes in registration and incumbency status as a result of the plan. The model is thus:

$$Pr(v_j = 1) = F(a + BR_j + c_1 d + c_2 r) \qquad (5)$$

where

$Pr(v_j = 1)$ is the probability of a Democrat winning congressional seat j
R is a vector of registration data for various parties in seat j
d is a dummy for a Democratic incumbent in seat j
r is a dummy for a Republican incumbent in seat j
B is a vector of coefficients
c_1, c_2 are coefficients relating the incumbency dummies to the vote

The model is estimated with a probit procedure using the registration, incumbency, and outcome data from the 1980 election that preceded the 1981 reapportionment in California. The new registration and incumbency data resulting from the new boundaries are then inserted into the estimated equation, yielding probit scores that can be converted into probability estimates.[2]

negotiations because it tells an incumbent how he or she specifically would have run in the proposed new district in an election at time t. However, its advantage as a bargaining tool is also its liability as a method for analyzing the general partisan impact of a plan: it is highly candidate specific in its predictions and does not provide a convenient basis for comparing results in open seats with results in seats with incumbents.

[2]The virtue of this model is that it provides a nice, out-of-sample predictive procedure for assessing the political effects of a redistricting plan. Its chief limitation is that it cannot measure the effect of displacement upon incumbency. In other words, incumbents who acquire a lot of new territory might have less incumbency advantage than the fortunate few who retain their old seats intact. To estimate gradations in the incumbency advantage would require abandoning the out-of-sample framework. As it was, only one incumbent who ran for reelection in 1982 lost his seat (Clausen), and his displacement was not great. The primary displacement effect is on the cost of reelection, since its electoral significance is mitigated by the incumbent's ability to mail into and get acquainted with the new areas a full year before the election. In the model as specified, displacement and the removal of the incumbent's home are part of an implicit equation that influences the incumbent's decision to run for a given seat.

The actual estimated parameters were as follows:

$$\begin{aligned} Pr(v_j = 1) = {} & -9.43 + \underset{(.004)}{.016 Demreg} \\ & - \underset{(.083)}{.017 Aipreg} + \underset{(.036)}{.007 Libreg} - \underset{(.136)}{.045 Pfreg} \\ & + \underset{(.012)}{.015 Dec} + \underset{(.460)}{.822 Dinc} - \underset{(.55)}{1.60 Rinc} \qquad (6) \end{aligned}$$

$R^2 = .83$ Chi square = 32

where

Demreg is the percentage Democratic registration
Aipreg is the percentage American Independent party registration
Libreg is the percentage Libertarian party registration
Pfreg is the Peace and Freedom party registration
Dec is the Decline to State (i.e., Independent)
Dinc is the dummy for Democratic incumbent
Rinc is the dummy for Republican incumbent.

The signs of the estimated coefficients on the incumbency and Democratic registration variables are significant and in the proper direction. The minor party coefficients are not, but are left in since they improve the fit marginally. The purpose of this model is predictive and not structural. Clearly, the large estimated incumbency effect is picking up a variety of phenomena related to holding office—for example, spending advantages and resource advantages. The point is to show what the effects of partisan reconstruction and incumbency removal are, not to show the causal routes that lead from incumbency or registration to electoral advantage. The equation is in this sense the most parsimonious reduced form.

The pre-redistricting probabilities referred to in the ensuing discussions are obtained from these estimated parameters by inserting the pre-redistricting registration and incumbency data into the model, taking the predicted score and converting it into a probability number. The post-redistricting probabilities are obtained in the same way using the same estimated parameters and the post-redistricting registration and incumbency data.

Assessing the Burton Plan

The 1981 California congressional redistricting was one of the most important and controversial redistricting plans in the country. Its significance lies partly in the size of the California congressional delegation, which grew in 1982 from 43 to 45, but also in the intense partisan battle it touched off. The plan was authorized by Phil Burton with the technical assistance of Michael Berman—a brother of an assemblyman who won one of the newly created Los Angeles congressional seats in 1982—and Leroy Hardy, a political scientist at Long Beach State who had worked on redistrictings since the sixties. The California delegation had been split 22-21 after the 1980 election and before the redistricting. In 1982, the Democrats held 28 seats and the Republicans held 17, a dramatic shift in power that many Republicans attributed to redistricting. This plan—Burton I—was subsequently rejected by the voters in a Republican sponsored referendum and was replaced in 1982 with a new plan—Burton II. My remarks are directed solely to the now-defunct Burton I plan.

I examine this plan utilizing the framework of expectations discussed earlier to test whether it had the pattern of a partisan strategy. Applying those propositions to California, we get the following:

1) Some number of marginal Democratic seats should have been strengthened.
2) Some number of marginal Republicans should have been weakened.
3) Some number of strong Democrats should have been weakened to assist marginal Democrats.
4) Some number of strong Republicans should have been made even stronger.

The question is, do these expected patterns appear in the data? The evidence for these patterns will consist of 1) simple registration data, 2) the estimated probabilities of a Democrat winning the seat under the assumption that all the seats are open, and 3) the estimated probabilities given information about which incumbents actually ran in 1982 and which seats were open.

The first sign of a partisan plan is that some number of marginal seats in the controlling party should have been strengthened. Table 1 shows the four Democratic incumbents who gained the most from the Burton plan. The first is Phil Burton's brother, John, who represented a district in Marin and areas to the north of San Francisco. Burton had received a strong challenge in the 1980 election and the 52.5% registration in his district was by California standards marginal for a Democrat. Typically, the seats with the highest probability of changing hands fall into the 50 to 55% Democratic registration category, and so it was clear that without assistance, Burton's district would remain marginal throughout the eighties. The solution to Burton's electoral insecurity was a highly controversial district that meandered from Vallejo in Solano County, across the water to Marin, through a narrow corridor on the east side of the San Francisco County, and down into Daly City in San Mateo County. This, more than any of Burton's other districts, brought a great deal of criticism from the press and the public.

The effect of this contorted district was to increase Burton's Democratic registration by about five points to 57.5%. The estimated probability of a Democrat winning John Burton's 5th district in an open race was 83% in 1980. After reapportionment, it was 91%. Adding in the effect of incumbency, the model projects that John Burton, had he run for reelection, would have been elected with a 99% probability, up three points from 1980.

As said before, it is important to note the importance of an incumbent running, a fact that has been much heralded in recent political science research. This analysis clearly shows that the displacement of incumbents is even more important to the outcome of the first post-redistricting election than are any changes in the underlying partisan composition caused by redistricting. (This is quite evident from the size of the estimated coefficients in Table 6.) Democratic incumbency is "worth" an additional 51% Democratic registration (.822/.016), whereas Republican incumbency has the advantage equivalent to a 100% shift in registration (-1.60/.016). The value of incumbency, particularly to Republicans, is clearly enormous.

Many of the changes made in John Burton's seat in 1981 were taken back in 1982. The 1981 plan was rejected by the voters in a June, 1982 referendum, and new lines were redrawn in December. When John Burton chose not to contest the seat in 1982, it was won by Barbara Boxer. In the subsequent redistricting, Boxer's district was neatly shaped into a more marginal seat.

The only other Democrat to receive a boost in 1981 comparable to John Burton's 5th district was George Brown's 36th district. The Mineta and Panetta seats, by comparison, got almost trivial increases that really did not improve their marginal status much. So one can say that in two instances primarily, marginal Democrats were strengthened by the redistricting plan, whereas in the other instances, including some that are not included in this table, the changes were insignificant and did not change the status of the seat.

The second expectation of a partisan plan is that some number of marginal noncontrolling party incumbents—in this case, Republicans—should have been partisanly weakened by the redistricting plan, which appears to be where the Burton plan had its major effect. In several instances, the strategy followed was more subtle than a straight collapse of the Republican incumbent's seat. Rather, the best Democratic portions

Table 1. California Democratic Incumbents Who Benefitted the Most from 1981 Reapportionment (%)

Congressional District	Incumbent	Registration 1980	Registration 1982	Probability: Open 1980	Probability: Open 1982	Probability: Democratic Incumbent 1980	Probability: Democratic Incumbent 1982
5th	Burton	52.5	57.5	83	91	96	99
36th	Brown	51.4	57.7	73	81	92	95
13th	Mineta	49.7	51.5	48	63	78	87
16th	Panetta	49.2	50.0	39	48	71	78

were retained in the old district while the most Republican areas were used to create a new seat for the Republican incumbent. By inducing the Republican incumbent to run for the new seat, Burton was able to create an open seat with favorable registration for the Democrats. This was essentially the procedure used in the Hunter and Feidler seats. Both of these incumbents were sitting in seats with dangerously high Democratic registrations, and so it did not take a great deal of inducement—for example, putting their house in the new district—to get them to move into the safer seat. A glance at Table 2 shows that the partisan composition changed slightly in the case of the old Hunter seat and negatively in the case of the old Feidler seat: the key to winning both seats was the removal of the incumbent, which, as the data show, dramatically altered the chances of a Democrat winning in both instances.

The old Dornan seat is a good example of a district created by both partisan reconstruction and incumbent removal. Dornan, the Republican incumbent, did not have to be given an alternative seat to run in, because he had declared himself a candidate for statewide office. Since the seat was strengthened by 9 points in registration and no longer had an incumbent, it changed from one in which the Democrat had a 1% chance of winning to one in which he or she had a 95% chance. The old Rousselot seat was also dismantled, and he was given no alternative open seat to run in. Portions of his old district were parceled off to various surrounding Republicans, but none of the portions was sufficiently large to give Rousselot a base from which to run. The largest overlap between his old district and the Burton-created districts was the highly Hispanic 33rd, previously represented by George Danielson and then by Marty Martinez after a special election in July, 1982. Rousselot chose to contest the Democrat, Martinez, as a nonincumbent rather than face his Republican colleagues in an expensive primary, and was defeated in the November, 1982 election (Cain & Kiewiet, 1984).

The other two gains by the Democrats in 1982 did not involve the weakening of Republican seats. The 18th congressional district was a newly created central valley district made possible by the allocation of two new districts to California and the rapid population growth in that area. As Table 2 shows, the Clausen seat did not change much in the redistricting plan, and the gain by the Democrats seems to have been the result of the challenger's strength and popularity in the area. So five of the six gains appear to have been reapportionment related, and four of those five involved the forced or induced removal of Republican incumbents.

Although certain Democratic incumbents benefited from the redistricting in 1981, not all of them

Table 2. Gains by Democrats in Congress (%)

Congressional District	Incumbent 1980	Registration 1980	Registration 1982	Probability: Open 1980	Probability: Open 1982	Probability: Incumbent Republican 1980	Probability: Incumbent Democratic 1982
2nd	Clausen	51.6	51.2	54	53	7	81
34th	Rousselot	44.8	65.1	8	96	0	99
18th	New Seat	–	61.3	–	87	–	98
44th	Hunter	54.9	58.0	80	91	23	98
26th	Fiedler/Goldwater	60.3	59.3	85	82	28	96
27th	Dornan	46.8	55.9	25	79	1	95

did. In particular, a few had to give up prime areas or had to take unfavorable areas because they were underpopulated. As a result, some Democrats were made worse off by the Burton plan, including Phil Burton himself. Burton's seat, the 6th, gave up some of the "best" areas in San Francisco County to help boost his brother's seat. Indeed, when the Republicans ran a popular moderate Republican state senator against him in the November, 1982 election, there was an enormous amount of speculation in the California press that Burton might have been too cute and left himself vulnerable to a challenge. My model indicates otherwise. The probability of Burton losing was unaffected by redistricting. Given that the seat had a 62.8% Democratic registration, a large, liberal Independent vote and a well-known incumbent, the sacrifice that he made was by no means extravagant.

In fact, one of the most striking things about Table 3 is the high degree of electoral security enjoyed by all the Democratic "martyrs." All had Democratic registrations above 55%, and with the added advantage of incumbency, they all had a greater than a 95% chance of being reelected even after their districts were altered. Nonetheless, redistricting did affect the result in these seats in the subsequent election. Even though 1982 was a more favorable year for Democrats than 1980, all of them suffered a drop in their margin of victory.

The partisan reconstruction of these seats (holding constant the incumbency status) can be viewed in the aggregate as they are in Figures 1 and 2. The vertical axis of these charts shows the computed probability of a Democrat winning the seat in an open race in 1982 and the horizontal axis shows the corresponding probability in 1980. The line at the 45-degree angle indicates points of no change: that is, where the probabilities in 1980 and 1982 were the same. Points above the line indicate seats that were made more Democratic by redistricting and those below it were made less Democratic. The data are stratified by the party of the incumbent in 1980 so that Figure 1 displays the data for the Democratic seats and Figure 2 the data for Republican seats.

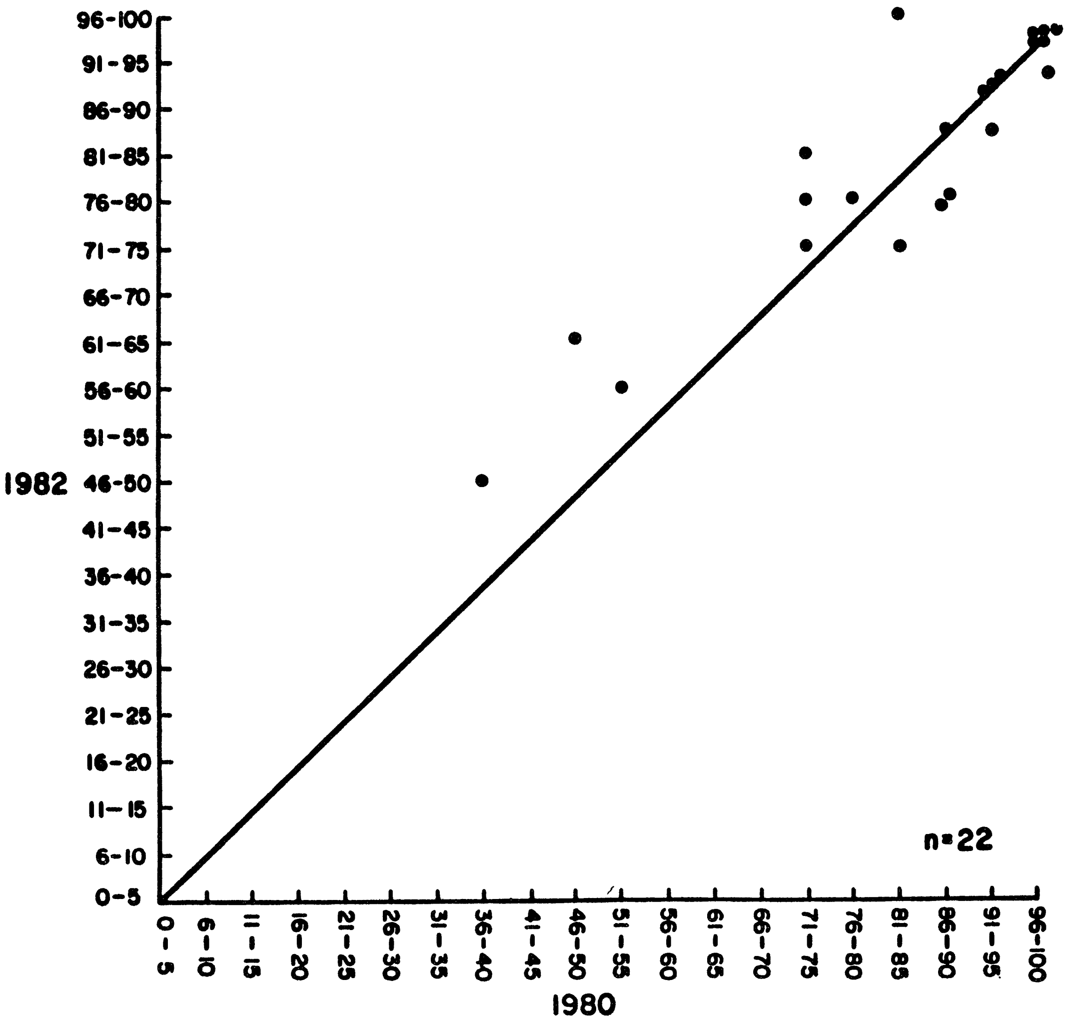
Translating the expectations of a partisan plan as discussed earlier into predicted points on the graph, the pattern in the Democratic seats should be that: 1) some points in the upper right-hand corner, representing the safest Democratic seats in 1980, should fall below the line since they are sharing their partisan wealth in the interests of greater Democratic efficiency; 2) some points in the lower left-hand corner, representing the most marginal Democratic seats, should fall above the line because they would be the natural beneficiaries of Burton's largesse; and 3) most incumbents should stay pretty close to the line because demographic, bargaining, and geographical constraints put severe limits on partisan efficiency. A perusal of Figure 1 would seem to confirm our expectations. The four points furthest above the lines are those discussed in Table 1.

Figure 2 is no less revealing. Once again, our expectations are that: 1) some number of points in the upper right-hand corner, representing the most marginal Republicans, should fall above the line because they are the natural candidates for partisan conversion; 2) some number of those in the lower left-hand corner should fall below the line because the Democrats would like them to be as inefficiently strong as possible; and 3) most points, once again, should cluster fairly close to the line because of demographic, bargaining, and geographical constraints. The data do not conform quite as closely in Figure 2 as they do in Figure 1. To begin with, the three points above the line are scattered across the horizontal axis, implying that the Democrats did not simply target the weakest seats. However, the reader should note that all the points above the 51% category on the vertical axis were won by the Democrats, including all but one of the points to the right of the 51% category on the horizontal axis. In short, the

Table 3. The Democratic "Martyrs" (%)

		Registration		Probability: Open		Probability: Democratic Incumbent		Margin	
	Incumbent	1980	1982	1980	1982	1980	1982	1980	1982
30th	Danielson/ Martinez	69.8	62.8	99	93	100	99	48	8
4th	Fazio	58.5	56.1	86	79	97	95	44	28
6th	Burton	62.8	60.7	99	97	100	100	44	18
23rd	Beilenson	59.6	57.1	88	79	98	95	32	20
32nd	Anderson	62.2	60.3	92	87	99	97	35	18

Figure 1. Changes in Democratic Seats (Congress)



Democrats won all the marginal seats even without changing the composition of some. The reason, which will be seen even more graphically in a moment, is that the Democrats made effective use of incumbent displacement: that is, they kept the registration the same, but moved the incumbent out in order to open up the seat. The three seats that fall above the line in Figure 2 are the Hunter (extreme right), Dornan (middle), and Rousselot (extreme left) seats.

The increased inefficiency of the Republican seats as a result of Burton I is evident in the cluster of points below the line in the lower left-hand corner. These are seats that are already strongly Republican and are made even more so by the plan. Notice also that the deviations from the line are somewhat larger, reflecting the likelihood that Burton felt more constrained by the wishes of his fellow Democrats than by those of the Republicans. This can be taken as support for the position I have argued elsewhere that the risk averse, idiosyncratic preferences of legislators form a moderating influence on partisan designs (Cain, 1984). One suspects that because Burton felt a greater need to accommodate the Democratic incumbents, this inertial force minimized changes in their districts to some degree.

Reapportionment and Electoral Competition

There has been a great deal of academic and popular discussion in recent years about the decline of competition in congressional races (Ferejohn, 1977; Fiorina, 1977a, b; Mayhew, 1974). One particular aspect of this debate is whether redistricting has contributed to the decline of competition in congressional races. Ferejohn and others have expressed doubts about this, and as the hypothesis is stated, these doubts are correct. If the question is whether all incumbents are indiscriminately aided by reapportionment, the answer is, not in all states, and maybe not all incumbents in any state. Not in all states, because some states will have more partisan plans than others; not all incumbents, because geographical, personal, and idiosyncratic considerations will sometimes be more important. However, the hypothesis that reapportionment affects electoral competition may still be accurate in the sense that how it affects electoral competition will vary with the intent of the plan as well as the degree to which geographical, personal, and idiosyncratic considerations introduce random noise into the final outcome.

Reapportionment affects electoral competition in two ways. First, it helps to determine the odds of a Democrat or Republican winning by restructuring the underlying partisan composition of a seat (i.e., partisan reconstruction). Second, it affects the incumbency factor by removing or keeping incumbents in their territory. The model developed earlier can be used to illustrate both of these effects separately and conjointly. Much of the dialogue about the decline of competition begins with the so-called Mayhew diagrams, which are histograms that display the electoral

Figure 2. Changes in Republican Seats (Congress)

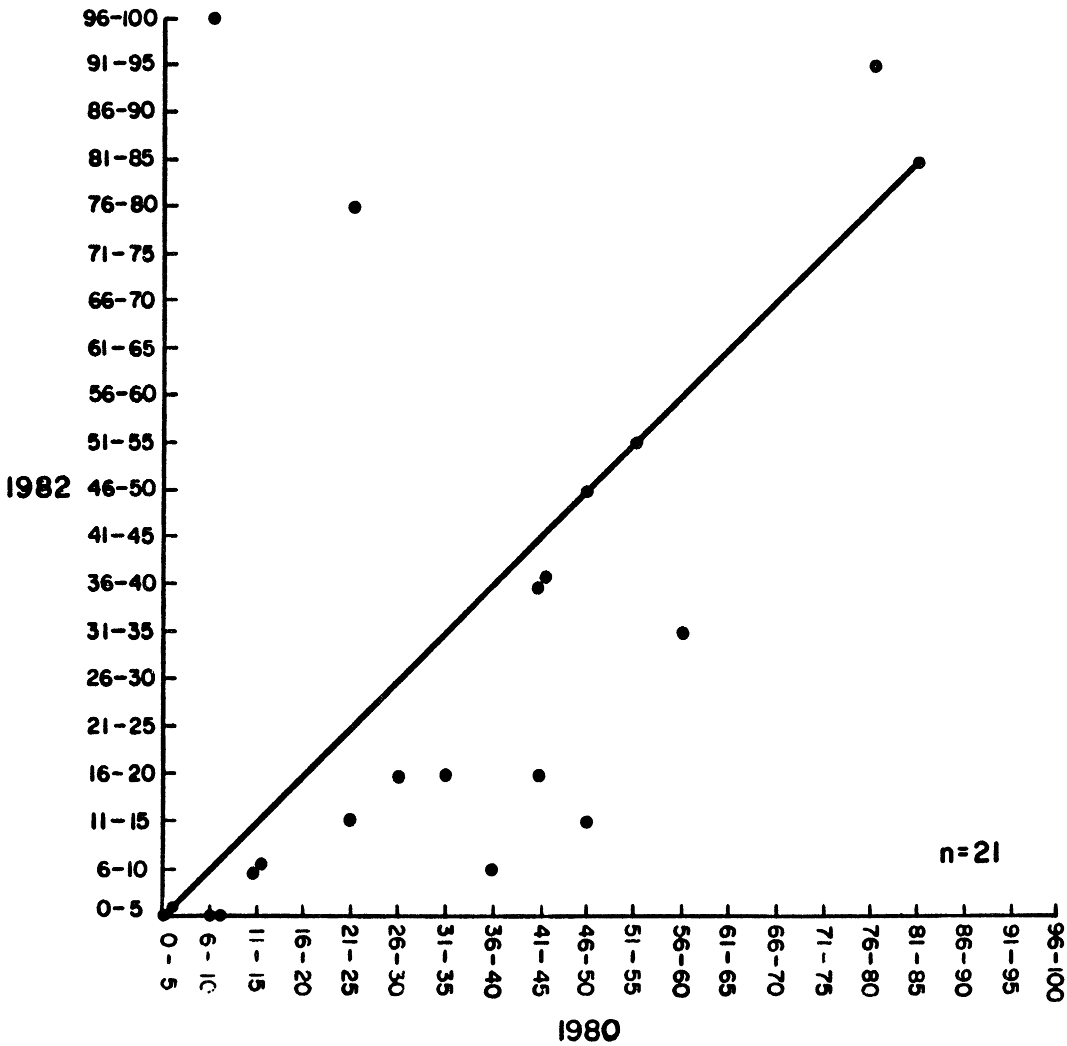
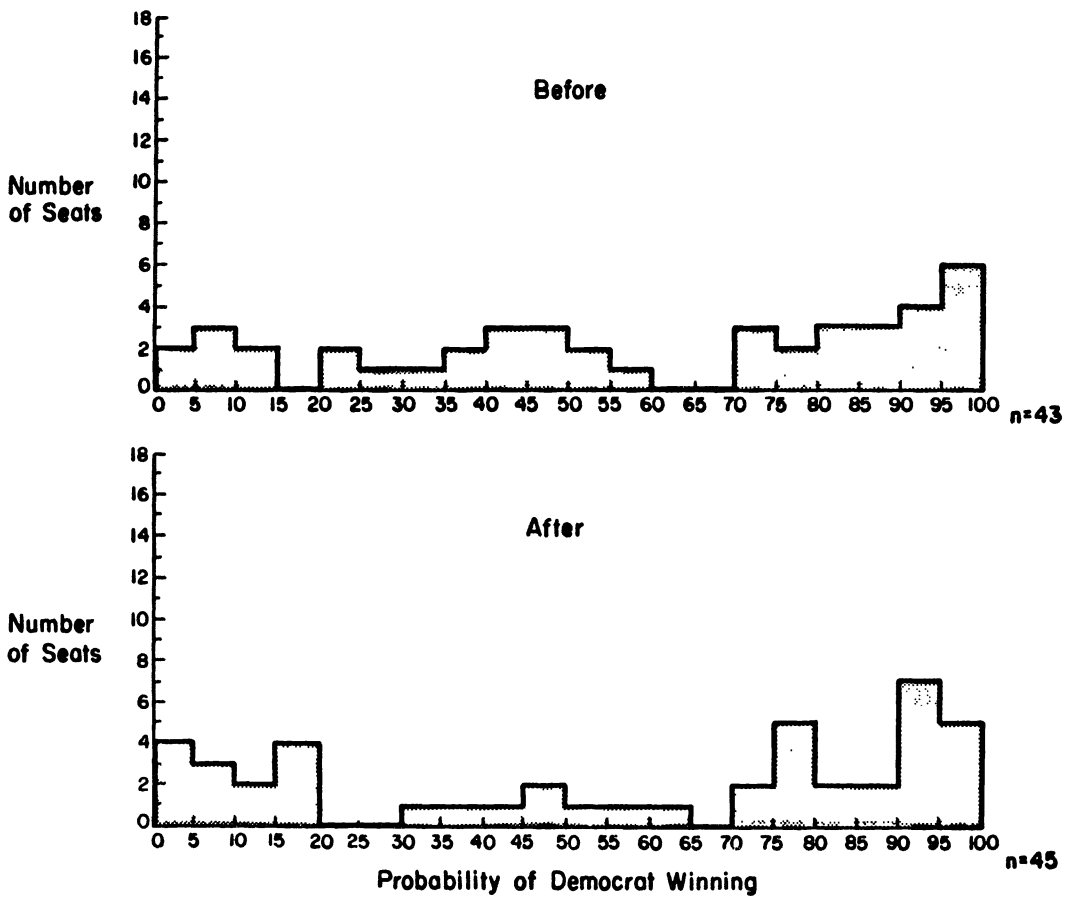


Figure 3. Comparison of Congressional Seats Before and After Reapportionment (Open)



margins of incumbents at various intervals during the postwar period. A variant of this idea is to create a histogram of the estimated probabilities from the probit model and show what happens to electoral competition at various stages in the reapportionment process. This of course leaves unanswered questions about the duration of reapportionment effects and the role that it may have played in the overall trend toward declining competition, but it does at least give us a glimpse of the immediate impact in one state at one period of time.

First, consider the impact of partisan reconstruction. Redistricting changes the competitiveness of seats by increasing the Democratic registration in seats that lean Democratic and the Republican registration in seats that lean Republican. Figure 3 shows the effect of the Burton I on all 45 seats under the assumption that no incumbents would be allowed to run. As the figure demonstrates, the consequence is some visible shrinking of the distribution in the middle. However, the results are not dramatic. There are still some seats left in the most competitive range and the rest are not simply bunched on the ends. Geographic constraints—for example, not being able to use inefficient inner-city Democratic strength to help out weaker Democratic seats in the rural and suburban areas—and the desire of incumbents to minimize displacement—that is, the acquisition of new constituents and the loss of former ones—explains why we do not observe more radical partisan reconstruction.

What about the separate effect of removing the incumbent? This is shown in Figure 4, which compares the distribution of seat safety in 1980 under the assumption that the seats were all open and versus the assumption that all incumbents ran. Here the effect of incumbency on the distribution in the most competitive, middle range is striking. Large numbers of seats cluster on the ends of the distribution, and no seats fall in the 50% range. Of course the reader should bear in mind that the model assumes the average incumbent, whereas in reality there will be enormous variation in the strength of both the incumbent and challenger. To some extent, this may be better modeled with campaign expenditure data, but the quality of the candidates will in any case remain difficult to capture.

The next three figures show the progression of changes in the distribution brought about by redistricting, including both incumbency removal and partisan reconstruction. Figure 5 compares the distribution of seat safety right after the 1980 election and then after the 1981 reapportionment. The post-reapportionment distribution assumes that the incumbents who held the seats in 1980 would run in what most closely approximated their old seat in 1982. Thus, for example, it was assumed that Dornan would run again in the 27th. Even with this strong assumption, the distribution has been changed some by movement to the extremes on both sides of the distribution. However, as was discussed before, the redistricting plan induced some incumbents to abandon their old seats to run for new ones and caused others to lose in the November election. With the new incumbents in place, the situation displayed in Figure 6 shows the almost perfect inefficiency of the Republicans. Even though the Democrats were less clumped on the end of the distribution, only a few of their seats were left in the 75% to 90% area. In short, most incumbents in both parties are in safe positions, but the Democrats are somewhat less inefficiently distributed than the Republicans.

What then has been the total change from 1980 to 1982? The last figure compares the two distributions. The answer would appear to be that

Figure 4. Distribution of Congressional Seats With and Without Incumbents

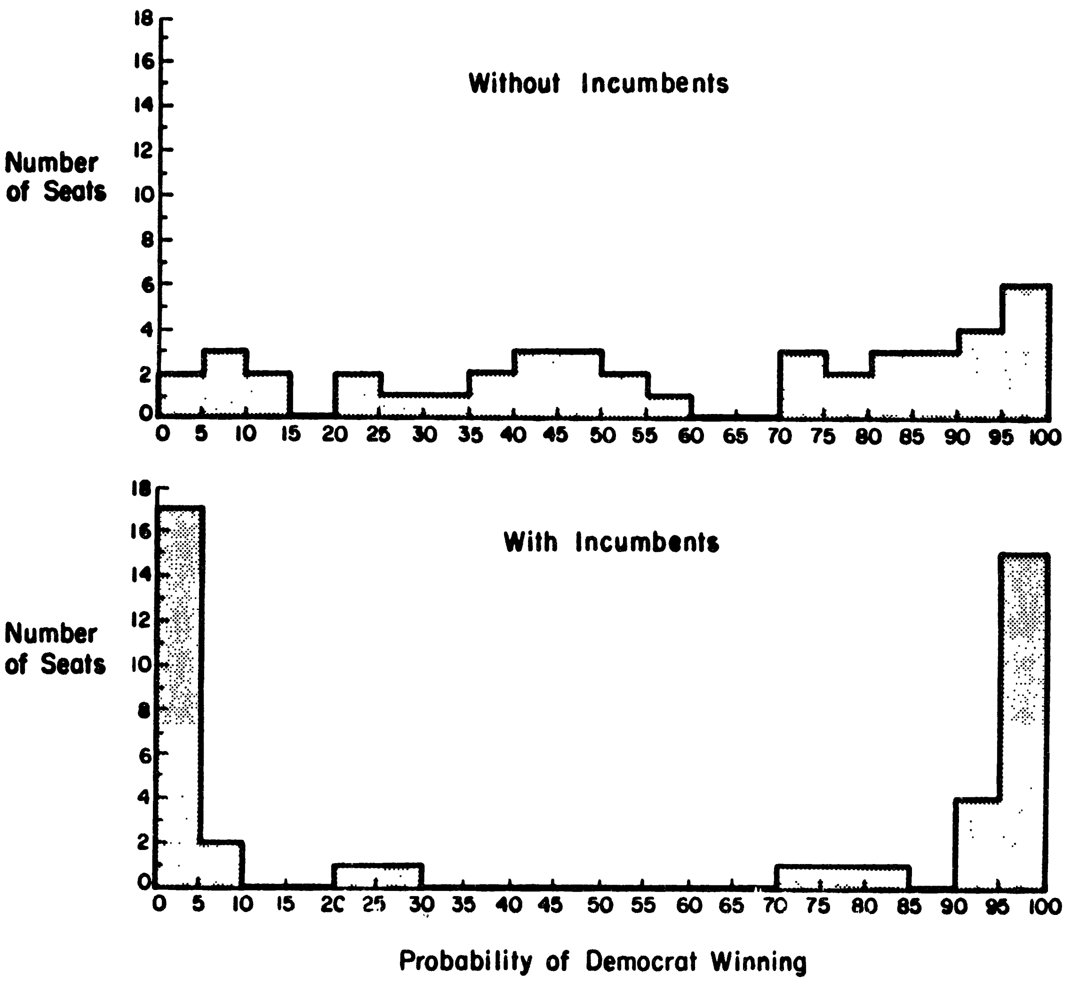
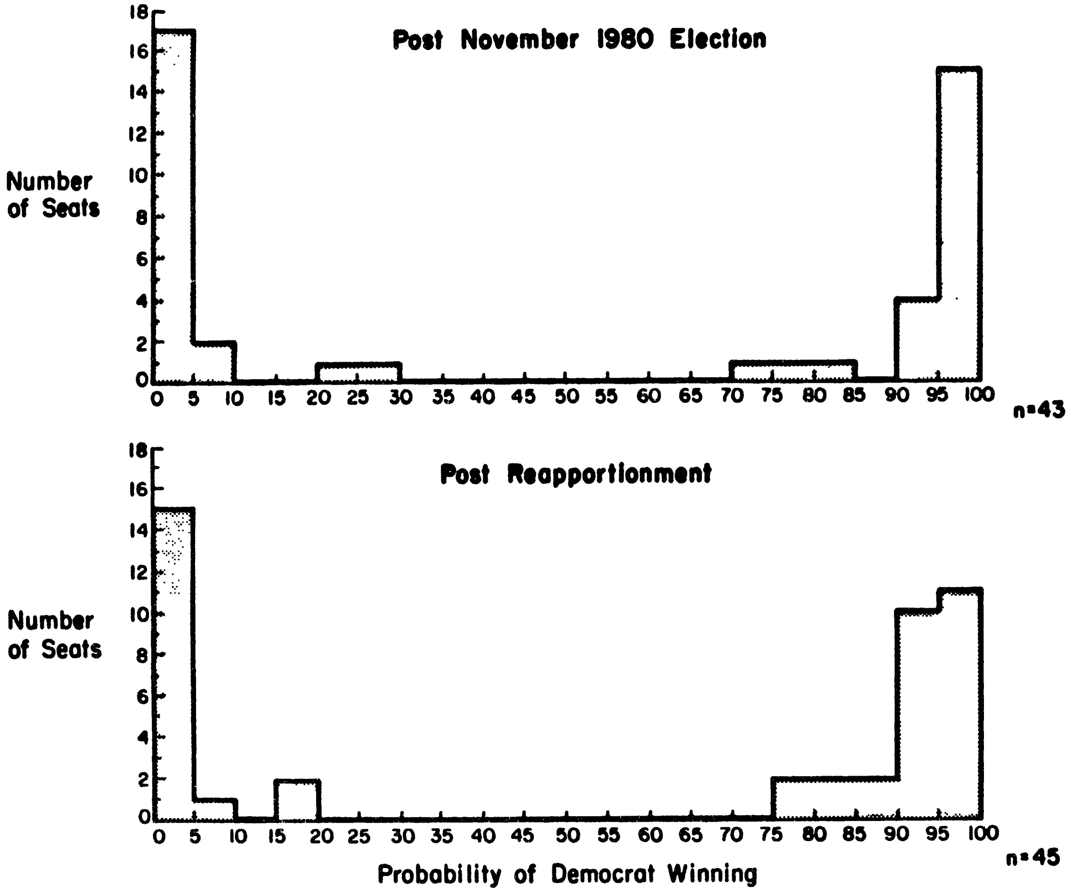


Figure 5.



the combination of partisan reconstruction and the artful removal of inconveniently placed incumbents can alter the seat distribution and make the majority party more efficiently distributed than the minority party. In the case of California, it was enough to help swing five seats to the Democrats.

Conclusion

Are the partisan effects of redistricting important? The answer would seem to be that they are. By changing the partisan composition in a district and removing or retaining the incumbent's base, a reapportionment plan can alter the odds of a party winning a particular seat. The key to a partisan plan is not simply increasing the average margin of victory or even the underlying partisan strength of all majority party legislators. Rather, the key is increasing the efficiency of majority party strength, which will mean a redistribution of electoral strength for the purpose of maximizing the number of winnable seats. Some majority incumbents will get stronger and others weaker in inverse relation to their initial vulnerability. Simply looking at the average registration or vote margin may be misleading.

A second conclusion from this research is that a proper assessment of the partisan effects of redistricting cannot overlook its impact on incumbency. To be sure, the post-redistricting election will introduce a new set of incumbents who will presumably also enjoy the electoral advantages of holding office. However, the temporary scrambling of incumbents can have momentous importance for the election that follows the redistricting. This should not be too surprising to political scientists since it seems logical that in an era when party loyalty counts for less and incumbency counts for more, redistricting tactics should include incumbent considerations. Indeed, if recent trends toward independence from the parties continue, redistrictings in the future could come to focus more on displacement issues and less on the partisan makeup of districts.

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Figure 6.

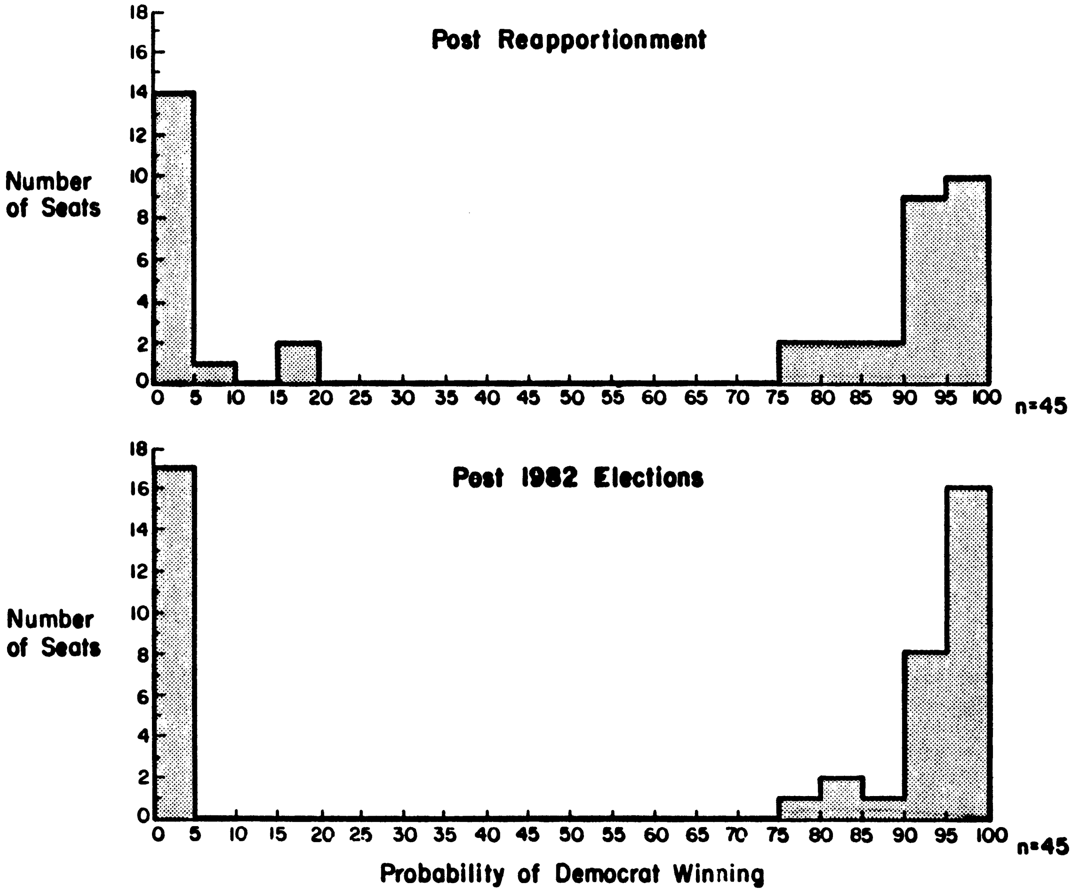

Figure 7. Comparison of Congressional Seats