Gerrymandering Incumbency: Does Nonpartisan Redistricting Increase Electoral Competition?

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Abstract

Many political advocacy groups, journalists, and scholars view redistricting as a major force insulating legislative incumbents from electoral defeat. Motivated by this concern, reformers have proposed giving control over redistricting to 'politically-neutral' independent commissions. Freed from partisan and electoral pressure, independent redistrictors would be expected to draw districts without giving favor to parties or incumbents. We analyze two novel datasets of simulated and alternative redistricting plans to evaluate whether maps drawn by independent commissions are more electorally competitive than those produced by party-controlled legislatures. We find that redistrictors marginally help sustain the electoral security of incumbents. Yet, counter to expectations, we find that independent redistrictors produce virtually the same degree of insulation as plans devised in legislatures or by politician commissions. Overall, our results suggest caution in overhauling state redistricting institutions as a mechanism to increase electoral competition: independent commissions may not be as politically-neutral as theorized.

Keywords: non-partisan redistricting, elections, competition, representation, simulations, incumbency

Supplementary materials are available in an appendix published online. All data and replication materials are available on the *Journal of Politics* archive on Dataverse at `http://dx.doi.org/10.7910/DVN/DKKP4I`.

What can explain the electoral security of legislative incumbents in the U.S.? Popular accounts often point to the insulating effects of redistricting, with legislators drawing their own district boundaries to ensure reelection. Characteristic of this concern, a recent *New York Times* column argues that

> "political control of the map-drawing process ... aided by sophisticated computer programs that can micro-target political affiliation ... has stuffed Congress and state legislatures with increasingly safe seats, making lawmakers difficult to dislodge no matter what they do" (Hulse 2016).

This view is not uncommon, including among scholars who worry that such insulation may allow polarized legislators to enact policies out of step with public opinion, or to obstruct policymaking (e.g., Carson et al. 2014). Given this concern, reformers are pushing to wrest control of redistricting away from partisan legislatures, and to empower nonpartisan commissions as a way to increase electoral competition and improve representation. And these efforts are gaining momentum, in part due to their broad public support.[1] Voters in Ohio and South Dakota recently considered ballot initiatives to create independent commissions, with Ohio passing its measure. Petitioners in Illinois collected nearly half a million signatures to initiate a ballot question, and citizens in Colorado filed a similar proposal before both were rejected by state courts.

When redistricting is controlled by elected officials, political considerations can loom large. Legislatures with unified party control may gerrymander to increase the majority's seat share, potentially insulating incumbents of their own party. Divided legislatures or bipartisan agencies may log roll maps to protect incumbents of *both* parties. In contrast, independent commissioners need not compete in elections or for political appointments, and thus promise a redistricting process divorced from political pressures and ambition. Consequently, commissions are expected to draw districts that more closely adhere to

[1] Polls in a number of states (e.g., NC) show large majorities of voters support non-partisan redistricting.

constitutional constraints (e.g., compactness, minority non-dilution), and in turn, expose incumbents to greater electoral risk (Edwards et al. 2017; Lindgren and Southwell 2013).

Yet, empirical evidence about the consequences of adopting independent commissions is decidedly mixed. Some studies find that districts drawn by legislatures are less competitive than districts drawn by 'politically-neutral' commissions (Carson et al. 2014; Grainger 2010; Lindgren and Southwell 2013; McDonald 2006). Yet, other work indicates that redistricting institutions have little effect on electoral competition, neither decreasing incumbent win margins nor increasing challenger entry (Abramowitz et al. 2006; Forgette et al. 2009; Masket et al. 2012). Some scholars uncover countervailing trends – incumbents may be *less likely* to face strong challengers when legislatures redistrict, but paradoxically get unseated at similar or higher rates (Cottrill 2012; Gelman and King 1994).

We offer new evidence on the link between redistricting and competition. Expanding on innovations by Chen and Rodden (2013) and Altman and McDonald (2015), we compare measures of district competitiveness between enacted redistricting plans and two sets of counterfactual maps that *could have been, but were not* enacted during the 2010 cycle. Specifically, we observe whether independent commissions adopt maps that are as competitive as the plausible set of alternatives *for the same state*, comparing this to the distortion found in states where legislatures conduct redistricting. We first analyze maps built from randomized redistricting simulations that only incorporate equal population and contiguity requirements (Chen and Cottrell 2016). We augment this with an original collection of all alternative maps officially debated by state redistricting institutions (Altman and McDonald 2015). Together, these data provide the strongest evidence to date about whether independent agencies increase electoral competitiveness.

We find that redistricting *sustains* the electoral security of incumbents in general. Maps that are eventually implemented show consistently larger incumbent vote advantages than would be expected given geographic and legal-political constraints. Most notably, we find that the incumbency bias persists regardless of whether politicians or citizens are in charge of redistricting. Independent redistrictors produce virtually the same degree of insulation as (bi)partisan plans devised in legislatures or by political appointees. Our findings suggest that replacing parties with independent redistrictors is unlikely to increase competition in legislative elections, offering little remedy to contemporary concerns about representation.

Data and Empirical Strategy

We draw on data collected by Levitt (2010), and our own analysis, to categorize each state's actual redistricting procedure for each legislative body. Our analyses focus on five categories: DEMOCRATIC PLANS (legislature with unified Democratic control), REPUBLICAN PLANS (legislature with unified Republican control), BIPARTISAN PLANS (legislature under divided government or a political commission), COURT PLANS, and INDEPENDENT COMMISSION PLANS. Though simplifying, this categorization captures the range of institutional devices of most interest to political actors, reformers, and scholars of redistricting (Carson et al. 2014; Chen and Cottrell 2016; Edwards et al. 2017; Gelman and King 1994; Levitt 2010).[2] In 2010, most U.S. House maps were drawn by Republican-controlled state legislatures. While most state-level jurisdictions were drawn by bipartisan legislatures or politician commissions, many more were created by Republican- rather than Democratic-controlled legislatures. Independent commissions redistricted only a handful of states.

Rather than compare final maps across states with and without independent commissions (Edwards et al. 2017; Forgette et al. 2009), we adopt an approach similar to Chen and Rodden (2013), Chen and Cottrell (2016), and Altman and McDonald (2014, 2015) in their analyses of partisan gerrymandering.[3] These authors make comparisons between the final maps adopted by redistricting agents, and counterfactual sets of alternative maps that *could have been adopted within the same state.* Using 2000 cycle data, Chen and Rodden

[2] See Table A1 in the Appendix for a state-by-state breakdown of redistricting institutions. For more detail on how states conduct redistricting, see http://redistricting.lls.edu.

[3] Cross-state comparisons are likely misleading. Independent redistricting is only found in Western states, with lopsided majorities that naturally dampen district competitiveness.

(2013) simulate randomly-generated redistricting plans that use only equal apportionment and contiguity as criteria. Political motivations, like partisanship or incumbency, are never considered. In being randomly drawn, these maps should capture whatever baseline electoral security we would expect absent any 'politics' in redistricting. For each simulated plan, Chen and Rodden (2013) analyze precinct-level vote data to determine which hypothetical U.S. House, Assembly, and State Senate districts would have received more Bush or Gore votes in the 2000 election. From this, the authors estimate the expected share of districts likely to elect Republicans for each simulation, and compare this baseline to the number of Republican-won districts in the final enacted plans. Chen and Cottrell (2016) extend this analysis for House districts in the 2010 cycle using 2008 presidential vote. Any surplus of party seats is interpreted as evidence of partisan gerrymandering.

We conduct a similar analysis of electoral competition. Initially, we analyze the simulated House maps produced by Chen and Cottrell (2016) for redistricting in 2010. Our main measure of competitiveness, Expected Margin of Victory, is the absolute value of the *party* win margin in a district, or roughly, the surplus of votes a winning incumbent might expect on average for a given plan. This measure incorporates the logic that incumbents with larger win margins would survive bigger electoral downturns year-to-year, and deter strong challengers from contesting their seats.[4] Following prior work, we use the 2008 two-party presidential vote (Chen and Cottrell 2016; Chen and Rodden 2013).[5]

[4]We find identical results examining median win margins, counts of competitive seats at empirically-derived thresholds, and seat flip probabilities. See the Online Appendix for details.

[5]We use 2008 elections because these are not conditional on the redistricting process, while elections in 2012 and after would be 'post-treatment' to enacted plans. Many things could change across precincts in being assigned to a particular district and map, and we cannot observe precinct voting under any alternatives redistrictors did not implement. We use 2008 party registration for Florida and Nevada as precinct presidential vote data were not available for those states.

Despite the usefulness of simulation approaches, we recognize their limitations. By design, simulations produce districts that adhere to minimalist constitutional requirements, approximated algorithmically. These exclude real legal and political constraints mapmakers face, including mandates to preserve majority-minority districts. A possible consequence is that simulations may overly concentrate Democrats in urban districts, thus overstating the baseline effect of geography on redistricting (Altman and McDonald 2014).[6]

Given these concerns, we introduce and examine a novel data source – the set of all alternative maps redistrictors *publicly* considered during the apportionment process. In many states, regardless of the redistricting procedure in place, redistrictors solicit map proposals from the public, including legislators, researchers, citizens, and interest groups.[7] In some states, these maps are made publicly available, alongside all the proposals introduced as legislation or considered during the meetings of the redistricting commissioners. We follow a similar approach innovated by Altman and McDonald (2014, 2015) in their study of party gerrymandering in Florida, Ohio and Virginia. We collected 1,627 maps across 15 states that meet equal population and contiguity constraints from the full set of data made publicly available by state legislatures or redistricting commissions. Importantly, the states in our sample vary in the way redistricting is conducted.[8]

[6] However, such an effect would likely *understate* the degree to which redistrictors adopt plans that insulate incumbents above what would emerge as a product of population or geography.

[7] In North Carolina, community meetings were held across the state to collect public input. Citizens were encouraged to propose maps online, and comment on official proposals.

[8] We collected virtually every map publicly considered in AK, AZ, CA, CO, FL, ID, MT, NC, NM, NV, OH, SC, TX, VA, and WA. Any maps restricted to the public would be excluded. Our convenience sample includes every non-partisan state, but no Democratic legislative plans, and only a handful of Republican, court and bipartisan maps. This may limit the generalizability of our findings if we expect that Democratic legislatures redistrict in fundamentally different ways than Republicans. We have no *ex ante* reason to suspect

To construct measures of 2008 district win-margins for each alternative map, we use both block-to-precinct data made available by McDonald and Altman (2011) and voting district shapefile data collected by Ansolabehere et al. (2015). In contrast to simulations, these maps incorporate the fuller set of legal and political considerations, like minority vote protections, redistrictors must balance. These counterfactuals allow us to assess the real tradeoffs redistrictors made in selecting final maps from among the reasonable alternatives, and especially how these choices differ depending on who is in charge of the process.

Results from 2010 U.S. House Redistricting

Turning to analysis of 2010 congressional maps, Figure 1 plots the distribution of (a) simulated House plans for the 42 states analyzed by Chen and Cottrell (2016), and (b) both simulated and alternative proposals for 12 (of 15) states which publicized alternative House plans. In both Figures 1(a) and 1(b), grey dots represent the average district EXPECTED MARGIN OF VICTORY for all House seats in a state under one simulated plan. In Figure 1(b), dark blue dots indicate the average seat EXPECTED MARGIN OF VICTORY for each alternative plan. Circles indicate the mean margin of victory under the final adopted map, with circumferences proportional to the standardized distance from the average of the distribution of simulated or alternative maps (interpretable as a z score).

Shown in Figure 1(a), we find that redistrictors typically adopt U.S. House maps in 2010 that are consistently less competitive than simulated plans. Across the states, adopted plans yield vote margins that are 2.4 percentage points safer than the average simulated map. Further, 43% of states have final maps that are less competitive than every single simulation, with the median plan being less competitive than 99% of simulations. Turning to Figure 1(b), we see again that redistrictors enact plans that are much less competitive

this as other research suggests that Democrats (e.g., Maryland) and Republicans (e.g., Texas) both gerrymander when possible. Future research should examine this, and other generalizability concerns.

than the alternatives debated during the mapmaking process. Of the non-court plans (i.e., by legislature or commission), expected win margins under the final maps were 1.2 percentage points less competitive than the alternative proposals, with 33% of these House plans being safer than nearly *all the maps proposed* to redistrictors. Perhaps most striking, the median final (non-court) House plan is more insulated than 76% of the remaining, unimplemented proposals. This distortion amounts to roughly 8,529 to 17,058 additional votes for each incumbent, and while modest, substantiates the general concern that the redistricting process may dampen electoral competitiveness in Congress.

Examining both simulated and proposed maps, we find this distortion persists regardless of whether redistricting is done by incumbents or independent commissioners. For instance, highly contested, Republican-drawn plans in Florida, North Carolina and South Carolina produced districts with expected win margins that were 2.1, 7.5, and 6.3 percentage points greater, respectively, than the average district under each state's simulated alternatives. A similar distortion is found (0.4, 7.0, and 0.4) when comparing enacted plans to those considered by the legislatures. Yet, in Arizona, California, and Washington – states with independent redistricting commissions designed to be insulated from political considerations – we see relatively little improvement. Incumbent vote margins under the final plans enacted in Arizona, California and Washington were 4.7, 3.5, and 0.7 points less competitive, respectively, than those recovered in the average simulated districts, and even 1.0, 0.3, and 0.2 points safer than the alternative plans considered by commissioners.

We next compare differences in the level of competitiveness found between each state's adopted and counterfactual plans, across the different redistricting institutions. Here, we compute the proportion of comparison *plans* that are less competitive than the adopted map overall, using the average of each plan's Expected Margin of Victory. We consider whether states with independent commissions adopt maps with relatively competitive districts compared to states where politicians conduct redistricting, using difference-in-means t-tests. Initially, we find that politicians produce maps that are safer than 77.1% of the simulated alternatives for their states, in terms of average win margins. Yet, this insulation is virtually identical to that uncovered for states with independent commissions, with 74.9% of simulations ($p = 0.935$) being more competitive than adopted plans. An analysis of the publicized alternative maps indicates again that independent commissions choose maps that are as uncompetitive as those enacted by politicians. Legislative plans are less competitive than 77.3% of the alternative map submissions overall, compared to 76.2% ($p = 0.949$) for non-politician maps. In sum, independent commissions do not draw House maps that encourage any greater electoral competition than partisan legislatures.

Results from 2010 State Assembly and Senate Redistricting

We restrict analysis of State Assembly and Senate redistricting to the alternative proposals considered, but not enacted in 2010. Figures 1(c) and 1(d) compare expected competitiveness of final plans to the alternatives publicly considered for the Senate and Assembly, respectively. Results mirror the findings for the U.S. House. With few exceptions, competition in adopted plans is lower than that produced under the plausible alternatives redistrictors considered. Moreover, we find no systematic improvement in competition from non-partisan redistricting. Independent commissions enact Senate plans that are considerably *less competitive* relative to alternatives (0.659), than (bi)partisan legislatures (0.328; $p = 0.058$), though this somewhat weakly reverses for Assembly maps (0.559 - 0.785; $p = 0.111$). Counter to expectations, our results again confirm that independent commissions do not consistently increase electoral competition at the statehouse.

Discussion and Conclusion

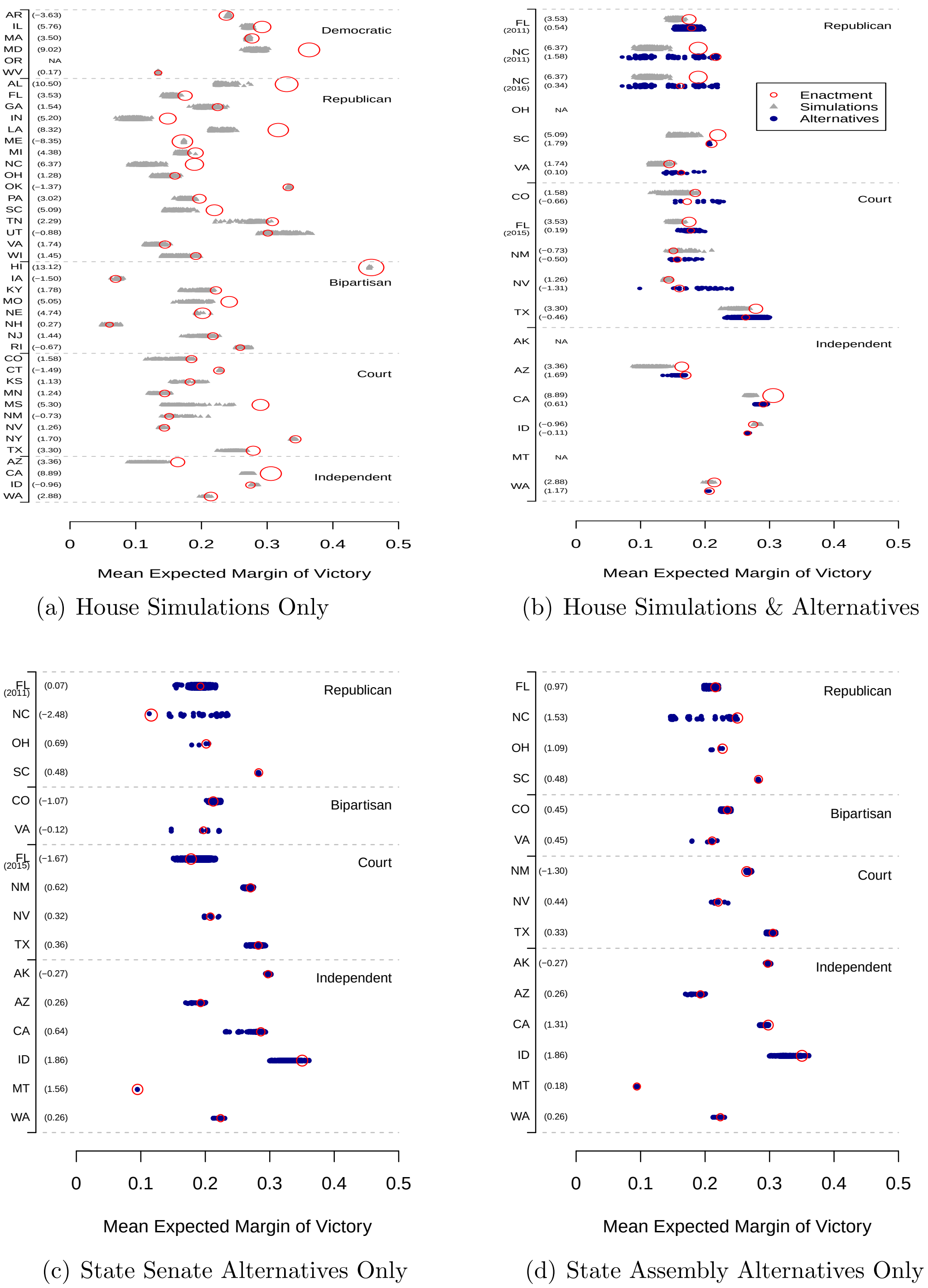
Redistricting is an oft-cited source of American political dysfunction. Reformers posit that most incumbents, in facing minimal electoral competition, have leeway to discount constituent demands, encouraging polarization and poor representation. We show that simply changing how legislative districts are drawn, even in a process ostensibly divorced of political ambition, may not bring about competitive elections, at either the federal or state level. Redistricting marginally dampens electoral competitiveness as a whole, but these effects are similar regardless of whether maps are drawn by (bi)partisan legislatures or independent commissions. Independent redistricting may foster competition in other complex ways (e.g., incumbent retirement). Importantly, our data and design can only assess how redistrictors systematically put their thumb on the scale prior to adoption, and not what downstream consequences different choices may have in equilibrium.

Our work introduces novel data to the study of redistricting – the set of alternative maps considered by redistrictors. We believe these are representative of the set of maps that *could* have been adopted, but were not. Of course, it is possible that mapmakers (un)intentionally censor their deliberations over plans, such that our data overweight especially competitive maps relative to all feasible alternatives. If so, we would also overestimate the dampening 'effect' of redistricting on election competition overall. We doubt redistrictors censor maps strategically, and think it particularly unlikely that they publicize only those that make the final enacted plans look 'worse' than most available alternatives. Still, we suggest caution in concluding that redistrictors systematically insulate incumbents as this requires an untestable assumption that our simulated and collected maps approximate all feasible alternatives. Our study relies on a weaker *parallel trends* assumption to interpret the marginal impact of redistricting modes: roughly that any bias in collecting or simulating counterfactual maps is uncorrelated with how states conduct redistricting. We strongly doubt that non-partisan redistrictors censor the least competitive alternatives they consider, while legislatures obscure the most competitive ones. Simulations guard against this concern by using fixed constraints and geographies that ignore politics by construction.[9] Future research should assess this concern more extensively, however, with particular focus on how mapmakers determine which plans to consider or implement.

In closing, we suggest that the apparent differences in district competition across states with and without independent redistricting may be an artifact of natural or demographic

[9]California bolsters this interpretation, switching from partisan to independent redistricting without any appreciable change in relative competitiveness of enacted maps.

variation, rather than any politics in the process. Or, it may be that partisan and political forces play a larger role in citizen redistricting than anticipated by scholars or reformers (Pierce and Larson 2011). Notably, we also find when courts intervene, they produce very competitive maps, suggesting politics and not geography are to blame. Further research is needed to uncover precisely why independent redistricting falls short of expectations, especially as reform efforts gain momentum across the country. Greater understanding of the politics of non-partisan redistricting can help improve the match between institutional design and the intended effects of political reform. Nevertheless, our findings caution against replacing state legislatures with non-politician commissions wholesale, at least solely on the basis of increasing electoral competition.



(a) House Simulations Only

(b) House Simulations & Alternatives

(c) State Senate Alternatives Only

(d) State Assembly Alternatives Only

Figure 1: **Competitiveness of U.S. House, State Senate and Assembly Districts for Enacted, Simulated and Alternative Plans in 2010**: Expected incumbent win margins are displayed for each enacted plan (red), simulation (grey) and publicized alternative (dark blue) in the 2010 cycle, by redistricting mode. U.S. House (a) simulations for 42 states and (b) simulations *and* publicized plans for 13 states are presented at top, with publicized maps for 15 states displayed below for (c) State Senate and (d) Assembly. Red circles are proportional to standardized distances between enactments and average alternatives.

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Online Appendix to "Gerrymandering Incumbency: Does Nonpartisan Redistricting Increase Electoral Competition?"

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A Appendix

A.1 Changing Authorities Empowered to Conduct Redistricting

Table A1: **Redistricting Institutions Across the States, 2000 and 2010**

Category	Congressional	Statehouse
2000 Redistricting		
Democratic	AL, CA, GA, MD, NC, WV	AL, CA, NC, WV
Republican	FL, KS, MI, OH, PA, UT, VA	KS, MI, UT, VA
Bipartisan	AR, CT, HI, IL, IN, IA, KY, LA, MA, MO, NE, NV, NH, NJ, NY, RI, TN, WI	AR, CO, CT, HI, MS, MO, NJ, OH, PA
Court	CO, ME, MN, MS, NM, OK, OR, SC, TX	FL, GA, ME, MD, MN, NH, NM, SC, SD TX, WI
Independent	AZ, ID, WA	AK, AZ, ID, MT, WA
2010 Redistricting		
Democratic	AR, IL, MD, MA, OR, WV	MA, WV
Republican	AL, FL, GA, LA, ME, MI, NC, OH, OK, PA, SC, TN, UT, VA, WI	AL, FL, GA, IN, LA, ME, MI, NC, SC, TN, UT
Bipartisan	CT, HI, IN, IA, KY, MO, NE, NH, NJ, RI	AR, CO, CT, HI, IL, MD, MS, MO, NJ, OH, OK, OR, PA, TX
Court	CO, CT, KS, MN, MS, NV, NM, NY, TX	KS, MN, NV, NM, NY, TX, WI
Independent	AZ, CA, ID, WA	AK, AZ, CA, ID, MT, WA

A.2 Seat Flip Probabilities and Counts for 2010 Redistricting

We analyze a number of alternative measures of competition. One such measure, Seat Flip Probability, follows an approach similar to Chen and Rodden (2013), to estimate the likelihood a seat will change party control given an expected vote margin under various plans. This measure is built from a simple bivariate model of Congressional party seat switches. Denote a party seat switch indicator Y_d for congressional district d, which equals 1 if the seat changes party control between 2012 and 2014, and 0 otherwise. Given binary seat flips, we use a logit model to regress Y_d on 2012 (absolute) district vote win-margin V_d. Precisely, denote R_d to be the two-party Republican vote share in a district. Then $V_d = \text{abs}\{R_d - (1 - R_d)\}$. The resulting logit linear model is $Pr(Y_d = 1) = \Phi(-1.837 - 9.646 \times V_d)$, where Φ is the logit density function. From this model, we estimate predicted flip probabilities for each district using either observed or hypothetical vote margins across each redistricting plan. Data used for this measure originate from the Congressional Quarterly (2014). The results are virtually identical to those drawn from average win margins, and are presented in Figure A1.

Table A2: **Minimum and Maximum Party Vote Shares for States, 2002 to 2010**

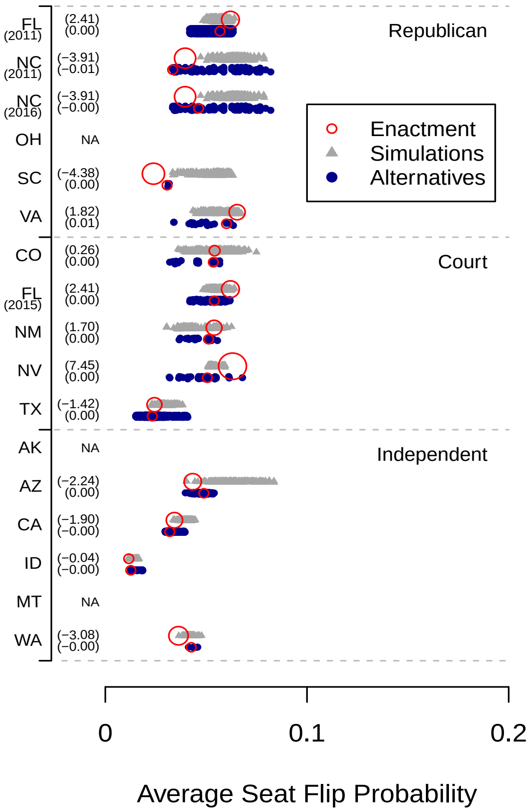
State	Min. R	Max R.	Dif. R	Min. D	Max D.	Dif. D	Avg. τ
AK	50.20	74.51	24.31	17.00	45.00	28.00	26.16
AZ	44.48	55.28	10.81	40.98	51.62	10.64	10.72
CA	36.64	42.62	5.99	54.10	60.30	6.20	6.09
CO	42.44	52.95	10.51	43.04	56.01	12.98	11.74
FL	48.38	60.97	12.59	38.91	59.37	20.46	16.52
ID	55.95	70.10	14.15	28.65	39.80	11.15	12.65
MT	51.50	64.62	13.12	32.40	46.30	13.90	13.51
NV	40.83	57.70	16.87	36.98	52.13	15.16	16.01
NM	39.60	55.78	16.18	44.19	56.13	11.94	14.06
NC	44.85	53.88	9.03	45.38	54.71	9.32	9.18
OH	45.74	55.75	10.01	42.88	53.31	10.42	10.21
SC	49.98	57.27	7.28	40.88	49.22	8.33	7.81
TX	49.13	56.07	6.94	41.15	49.43	8.28	7.61
VA	50.77	55.24	4.47	44.13	47.83	3.71	4.09
WA	39.13	51.84	12.70	48.16	60.32	12.16	12.43

A nonparametric way to measure incumbent vulnerability is to estimate the widest possible partisan shift we observe across legislative districts in a state, and then calculate the number of seats that would switch party control under different redistricting plans if that maximal vote swing occurred. We estimate this maximal interval by calculating the maximum and minimum Democratic (or Republican) vote share across all districts for each state over the five elections from 2002 to 2010. These minimum and maximum party vote shares are displayed in Table A2. To illustrate, in Alaska's 2002 midterm, the best year for Republicans in that decade, Don Young won 74.51% of the vote, while his Democratic opponent got 17.28%. Six years later in 2008, Don Young's margin was reduced to 50.2%, with Ethan Berkowitz getting 45% of the vote. The remaining 2004, 2006 and 2010 elections in Alaska all fall somewhere in between. Thus, we consider a maximal shift of 26.16 percentage points to bound the most extreme volatility in electoral security Don Young, and other Alaska legislative incumbents, might experience in a worst case election.

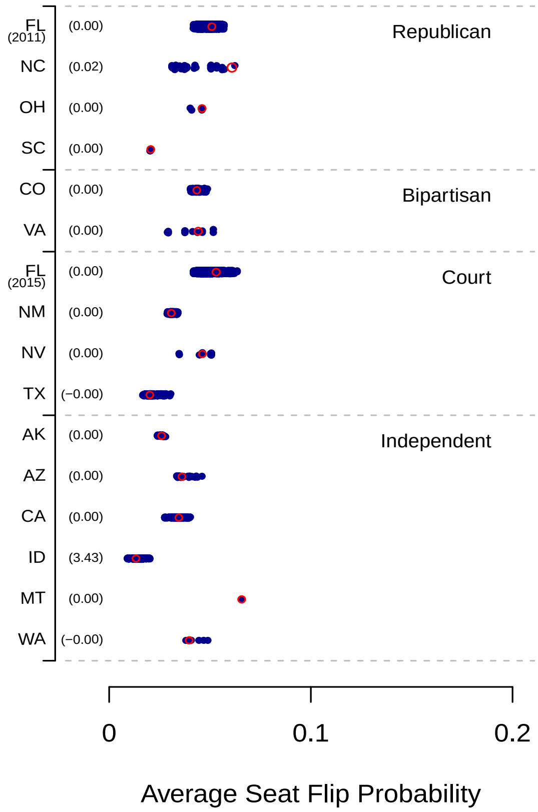
As seen in Table A2, states differ substantially in the size of their maximal party vote swings. Consequently, we doubt that using some fixed competitiveness threshold is appropriate. For instance, researchers will often set some pre-determined value, like 5% or 10%, and then examine which seats are won by a margin smaller than that competitiveness threshold. The analogy here would be to add (or subtract) 2.5 or 5 percentage points to all incumbent's vote share and see how many districts would flip party control. Yet, the values in Table A2 illustrate that using a hypothetical swing (say of 10%) would simultaneously under- and over-estimate vulnerability across different states. Instead, we use this maximal shift to measure the state-specific vulnerability incumbents experience, when calculating how often legislative seats would switch party control across different redistricting maps. To do this, we add or subtract each states' maximal party shift to incumbents' Expected Margin of Victory under enacted, simulated and publicized plans, and observe how many seats would shift across the different plans. These results are presented in Figure A2, and mirror our main findings.

A.3 Results from 2000 U.S. House, State Assembly and Senate Redistricting

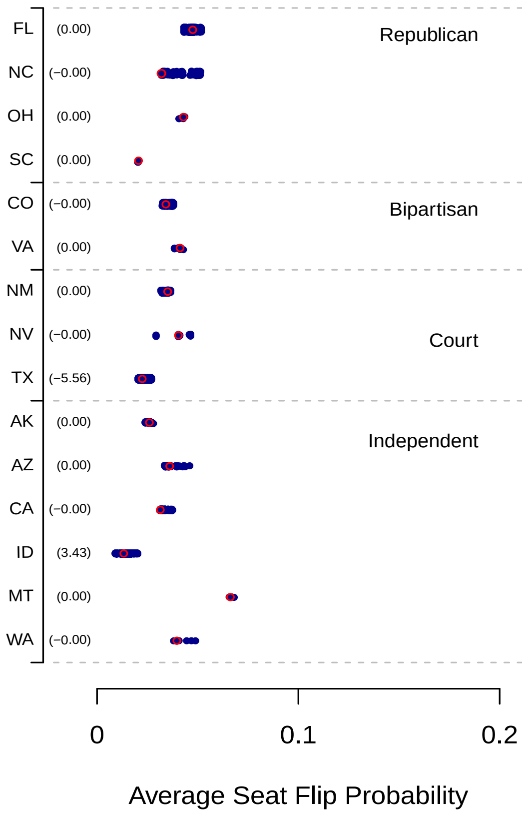
We focus on the 2010 redistricting cycle in the main paper because we have simulated maps and alternative maps available to compare to the actual redistricting plans in a large number of states. Chen and Rodden (2013), however, also have made available simulated data for the 2000 redistricting cycle at both the federal and state level in a sample of states. These results are presented in Figure A3. Our 2000 results are consistent with the findings using simulations in 2010 for the U.S. House, and broadly mirror our other findings using publicized statehouse maps in 2010. A shortcoming here is that we only have data for one state (ID) with an independent commission, and so cannot say much about non-partisan redistricting in the 2000 cycle.



(a) House Simulations & Alternatives

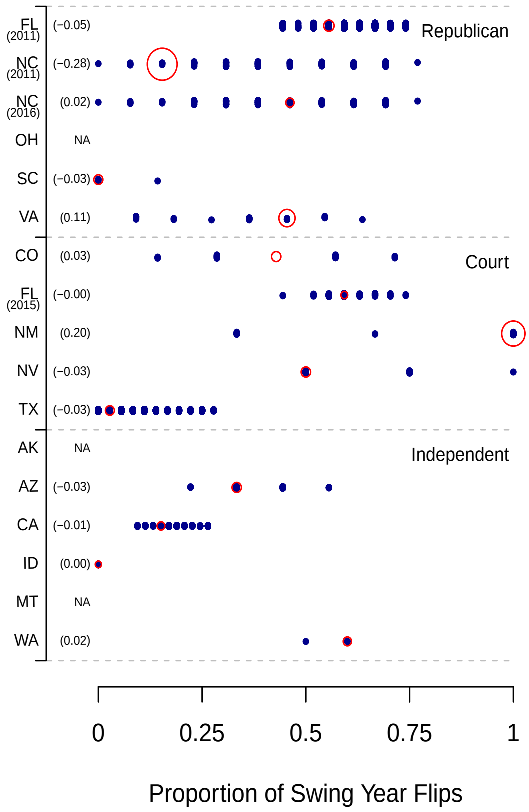


(b) State Senate Alternatives Only

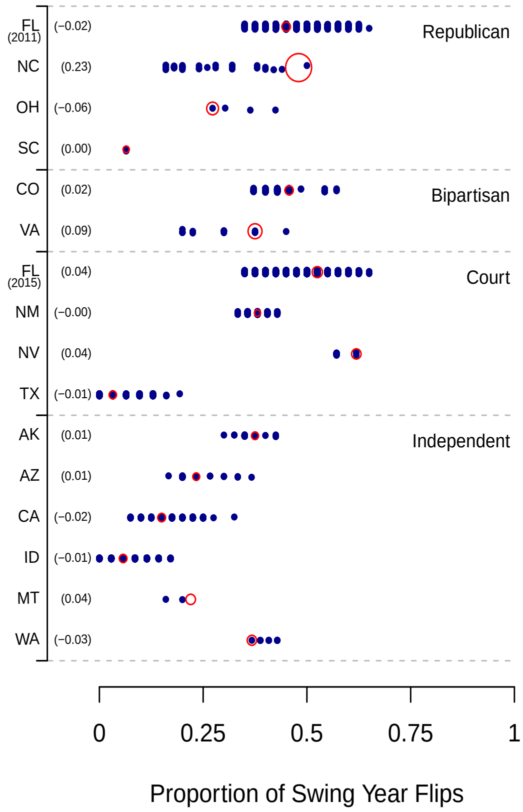


(c) State Assembly Alternatives Only

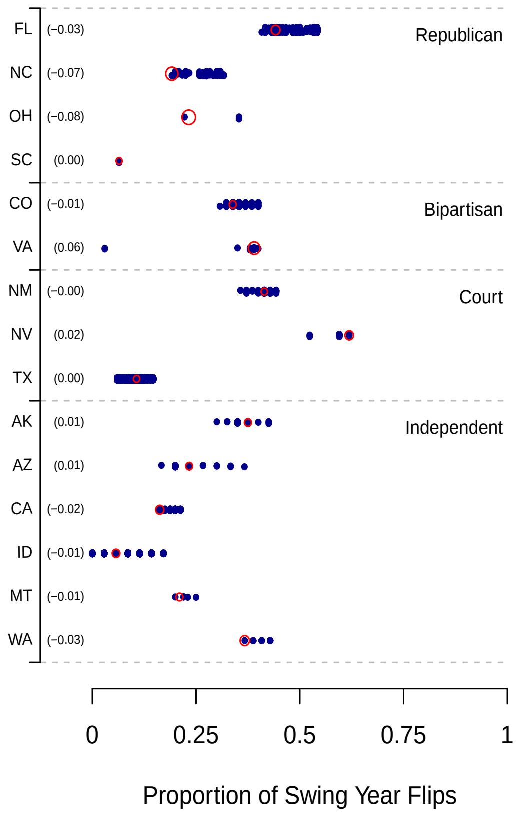
Figure A1: **Incumbent Loss Probabilities of U.S. House, State Senate and Assembly Districts for Enacted, Simulated and Alternatives Plans in 2010**



(a) House Alternatives Only

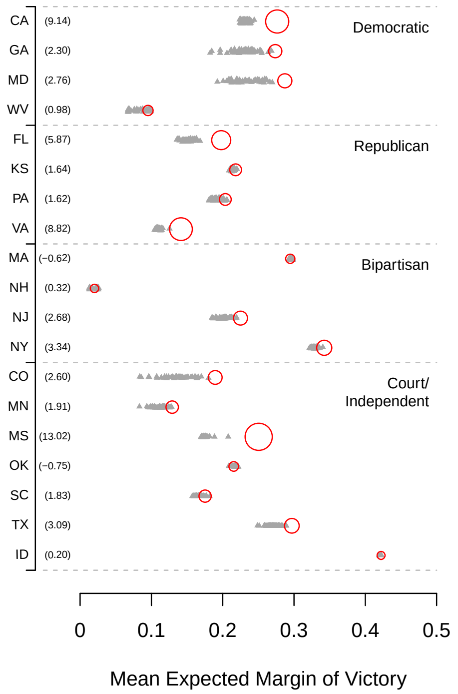


(b) State Senate Alternatives Only

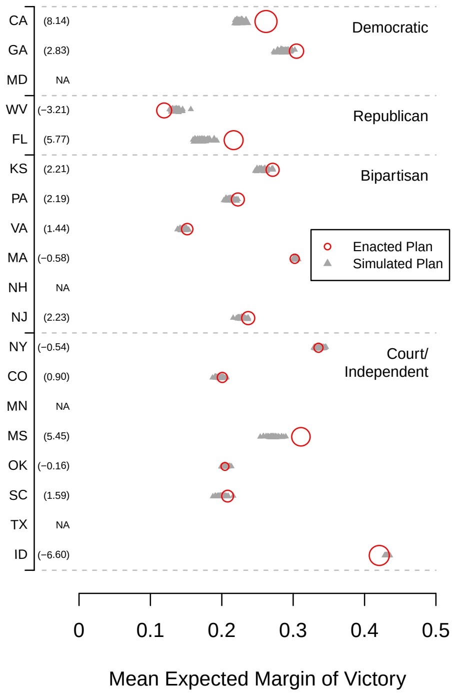


(c) State Assembly Alternatives Only

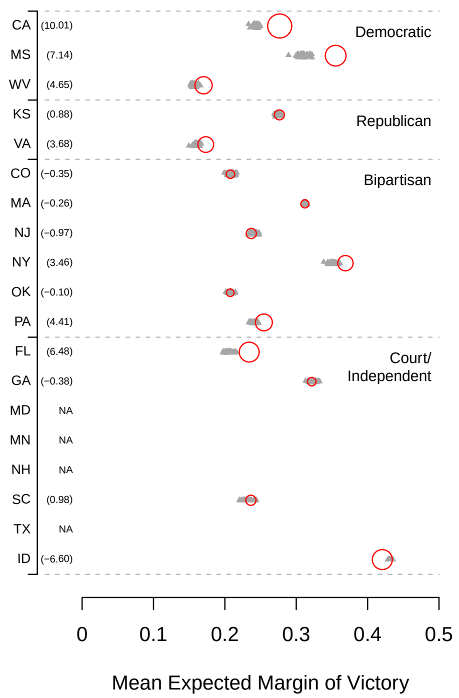
Figure A2: **Counts of Incumbent Seat Losses of U.S. House, State Senate and Assembly Districts for Enacted, Simulated and Alternatives Plans in 2010**



(a) House Simulations



(b) State Senate Simulations



(c) State Assembly Simulations

Figure A3: **Competitiveness of U.S. House, State Senate and Assembly Districts for Enacted and Simulated Plans in 2000**

A.4 California's Redistricting Experiment

Estimating the marginal impact of independent redistricting using our data and approach requires a *parallel trends* assumption. Specifically, this does not require that simulations or the publicized counterfactuals be an unbiased sample from some true distribution of all feasible alternatives, but only that any bias in our comparison maps be independent of how states conduct redistricting. For example, this would be violated if it were the case that independent redistrictors under-reported the non-competitive alternatives they debated, while partisan legislators over-reported these uncompetitive maps when making their deliberations public. A similar sort of bias would need to be present in the simulations as well for our findings to be faulty: that simulating using minimalist constitution conditions also under-samples uncompetitive maps in non-partisan states, but over-samples them in partisan places. We doubt either of these, much less both occur.

An admittedly imperfect test of this looks at simulations data for California, which switched from having a partisan legislative process in 2000 to an independent commission in 2010. Such a test mirrors that found in Grainger (2010), who analyzed multiple redistricting cycles in California. While many things are changing between 2000 and 2010, the basic geography of the state did not, including the distribution of census blocks and voting precincts, which are the fundamental units redistrictors use to draw maps. We would expect that whatever geographical factors bias simulations to over- or under-sample competitive maps are unlikely to change much for the same state within a decade. If this stability is the case, then we can look at the relative competitiveness of implemented plans for California in 2000 and 2010 against simulated alternatives drawn from the same simulation approach for both years.

In doing so, we observe that California's 2010 non-partisan map was 8.89 points less competitive than the average simulated map in terms of standardized win margins, and less competitive than all the simulations. For 2000, the party legislative map was 9.14 standardized points less competitive than the average simulation, and also outside the range

Table A3: **Comparing the Improvement in Estimates of Relative Competitiveness from Adopting Non-Partisan Redistricting in California to Other Non-Changing States**

State	2000 Difference	2010 Difference	Diff.-in-Diff.
MS	13.02	5.30	-7.72
VA	8.82	1.74	-7.08
FL	5.87	3.53	-2.34
NY	3.34	1.70	-1.64
NJ	2.68	1.44	-1.24
ID	0.20	-0.96	-1.16
CO	2.60	1.58	-1.02
WV	0.98	0.17	-0.81
GA	2.30	1.54	-0.76
MN	1.91	1.24	-0.67
OK	-0.75	-1.37	-0.62
KS	1.64	1.13	-0.51
CA	**9.14**	**8.89**	**-0.25**
NH	0.32	0.27	-0.05
TX	3.09	3.30	0.21
PA	1.62	3.02	1.40
SC	1.83	5.09	3.26
MA	-0.62	3.50	4.12
MD	2.76	9.02	6.26

of simulations. We can compare this estimate to the changes in the relative competitiveness we observe for 18 other states where we have simulated House maps in 2010 and 2000, and that did not alter their mode of redistricting. As shown in Table A3, we see that California's improvement in relative competitiveness (-0.25) over the decade is smaller than that observed in 12 of 18 (67%) states that did not change their redistricting, with the average change in relative competitiveness for all 18 states being -0.58. These coefficients estimate relative competitiveness taken from the 2010 simulation results in Figure 1(a) and the 2000 results presented in Figure A3(a) in the Appendix. Thus, in a place where we can be reasonably assured that any bias in simulations cannot be correlated with how redistricting is conducted, we again observe no substantial improvement in the relative competitiveness of maps produced by non-partisan redistrictors.

A.5 Generalizability Using Convenience Sample of Maps From 15 States

For 2010, we managed to collect 1,627 maps from the full set of data made publicly available by 15 state legislatures and redistricting commissions. These data contain maps from AK, AZ, CA, CO, FL, ID, MT, NC, NM, NV, OH, SC, TX, VA, and WA. This includes every state that has independent redistricting, but no Democratic plans, and only a handful of maps from Republican, court and bipartisan redistrictors. This incomplete coverage may limit the generalizability of our findings, especially if we expect that Democratic legislatures redistrict in fundamentally different ways than Republicans. Scant research suggests this possibility, typically finding that both Democrats (e.g., Maryland) and Republicans (e.g., Texas) gerrymander whenever they can. Nevertheless, there are clear differences in the types of states which collect and publicize alternative maps and those which do not. If there is heterogeneity in how redistricting is conducted in those states excluded from our counterfactuals sample, then this could alter our conclusions about redistricting generally, and about non-partisan redistricting in particular.

Our combined analysis of both simulations and counterfactual maps helps us address this generalizability issue. An important benefit in using both simulations and publicized counterfactuals for data is that these are generated in different ways and cover different, but overlapping sets of states and seats. Though imperfect, this overlap can give us greater assurance that our results are not idiosyncratic to the particular places where data are available. On this front, we were able to analyze U.S. House simulations produced by Chen and Cottrell (2016) for 42 (of all 43) multi-district states in 2010. Again, these results, presented in Figure 1(a) in the manuscript, affirm our main findings. Moreover, these results include the full range of states that redistrict House seats, including those with Democratic-controlled or bipartisan processes.

A limitation using these data, however, is the lack of simulations for 2010 State Assembly and Senate districts. In Figure A3, we present simulations for 19 states at the House, State Assembly and State Senate jurisdictions for 2000 (Chen and Rodden 2013). These mirror our findings for House districting in 2010. Note though that we coverage for all non-partisan states, save Idaho. Though limited in coverage of states, our counterfactual maps data allow us to better understand the consequences of non-partisan districting in state legislative districts and not just the U.S. House, which has been the major focus in work using simulations (e.g., Chen and Cottrell 2016).

In summary, though each source of data faces particular limits in coverage, these limits are different across each type of data. Consistent results across different sets of states and political jurisdictions at different redistricting cycles, using these very different data, strongly suggest that our findings are not idiosyncratic to any particular dataset or analysis. At the very least, these data can help bound our conclusions under different beliefs about how redistricting is conducted in left-out states. Namely, either (a) the simulations data significantly understate how uncompetitive redistricting really is in excluded partisan (i.e., Democratic) states, and independent redistricting does produce more competitiveness or (b) the simulations miss how much more competitive redistricting is in excluded partisan (i.e., Democratic) places, and independent redistricting is actually much *worse* at producing competitive districts.

A.6 Some Theoretical Expectations About Redistricting Institutions

Very little theoretical work has been done to explain the differences in how various institutions (i.e., legislatures, courts, political commissions, non-partisan commissions) draw maps. Most prior work relies on an explanation of the strategic behavior of political elites. This work starts with the theoretical prediction that partisan redistricting would lead to uncompetitive districts because incumbent politicians wish to stay in office, and have the legal means of facilitating this, either through party gerrymanders or incumbent protection plans. In contrast, independent commissions are thought to not have these same political incentives. Decisions are not made by (or influenced by) politicians seeking office, and so independent commissioners should produce maps freed from the desire to protect incumbents. The logic behind court plans is relatively similar. The courts usually draw maps only when a partisan legislature or politician commission fails to agree on a map, or when aggrieved plaintiffs sue (typically) on the basis of voting rights (VRA) violations. Given this, the job of the court is simply to draw maps as guided by the Constitution and prior court rulings. We should not expect that court plans introduce a pro-incumbent bias either.

Our data suggest that partisan redistricting marginally protects incumbents, and that independent redistricting does too. As a result, what we suggest is that the conventional wisdom surrounding the (non)-political incentives of independent redistricting does not comport with the evidence. Interestingly, we do find that courts produce the most relatively competitive plans, as these typically fall inside the range of plans (often near median) considered by redistrictors. We hesitate to offer a theoretical account of this finding given the many complexities associated with how courts make decisions about maps, including how and when they intervene, and on what grounds they base their decisions. This result certainly suggests that courts are the most politically-neutral remedy available, something that deserves greater scholarly attention in the future.

Clearly,the next step in this research area is a serious theoretical treatment, in light of our findings, on the incentives surrounding the actors involved in each redistricting method. Without such a theory, it will be difficult to explain the mechanisms driving our findings. Nevertheless, we see our work as a critical first step in addressing whether claims put forth by pundits and the reform community pass empirical muster.

A.7 Additional Details on Data Collection and Construction

We clarify here how we constructed our dataset of publicized maps, briefly including our approach to acquiring the maps from redistrictors. On the latter, we manually searched for public maps from the webpages of every state's official redistricting authority, including legislatures. Most, but not all states have a public webpage describing how redistricting is conducted. Of those that do, we were only able to find any alternative maps for 15 states. Collecting maps for some of these states necessitated us to use the Wayback Machine to get archived links to public deliberations occurring in 2010 and after, but that were subsequently removed from state webpages. We searched each state until we were satisfied that no such maps were ever made public in electronic form online, or that if maps were made public, that no viable means existed to collect them during our online search. We then contacted the redistricting authorities for all 35 states where we could not find any maps to see if any such maps were available in any other form. Few agencies responded, and none that did provided us with additional maps.

The maps we were able to collect typically took one of two forms, either a matrix of block equivalency file linking census blocks to districts for each public map, or a database of shapefiles. We also collected a small number (≈ 0.005) of maps that were just compiled .pdf files visualizing district boundaries. We discarded these .pdf maps since we could not reliably link census blocks or precinct/voting districts (VTD) to the proposed jurisdictions. We collected election outcomes for VTDs for most states using the Ansolabehere et al. (2015) dataset. These data include shapefiles of VTDs along with presidential voting and party registration in 2008. With these data, we used spatial geocoding packages in **R** to identify which VTDs/precincts were incorporated into proposed districts for the various alternative redistricting plans. When a VTD was entirely subsumed by the geographical boundary of a proposed district, that precinct/VTD's election data were added to that legislative jurisdiction's vote summary. Though relatively rare, VTDs were sometimes geographically split over multiple proposed legislative districts – in these cases we divided the vote summary data for that VTD equally across the connected legislative districts.

We used a different approach for maps that were provided as block equivalency files. Here, we required vote summaries, disaggregated from VTDs and linked to each census block, so we could re-aggregate block-level voting back to proposed legislative districts. Of course, voting data is collected at the VTD, and not block level. Thus, an initial step is required to split vote figures from VTDs into the various census blocks that are contained within each VTD. We did not do this, and rather relied on the data provided by McDonald and Altman (2011). These data connect 2008 election data to blocks, which we aggregated to districts across the various proposed maps.

Naturally, both approaches will contain random measurement error, and this error may vary somewhat when using shapefiles rather than block equivalencies. Importantly, any such measurement error will be constant within a state, since competitiveness across the alternative proposals are produced using the same basis in data within each state. Further, this error can only attenuate our relative comparisons, so that our comparisons of enacted and alternative plans may *understate* the level of uncompetitiveness we find following the redistricting process. Finally, we have a handful of maps that were provided as both block equivalencies and as shapefiles, and so we can evaluate how the results might vary due to differential measurement error from disaggregating and aggregating election data across different geographies. In inspecting these maps, we find differences are small and ignorable.

Once linked, our Expected Margin of Victory measure is produced for each district in a given map, by taking the absolute value of the head start provided a party incumbent. For example, assume a state with four districts had the following two-party Democratic vote share: $x = 0.72, 0.55, 0.51, 0.37$. The district expected margin of victory is the absolute winning margin, or $\text{abs}((1 - x_i) - x_i) = 0.44, 0.10, 0.02, 0.26$. This is then averaged over the plan to produce a map-level measure, $\text{E}\{\text{abs}((1 - x_i) - x_i)\}$, which is 0.205 in this example.

Notably, we do not discard any legislative districts from enacted or alternative maps based on whether or not these were contested or open seats in any elections, including 2008, 2010, and 2012. We do not discard any post-2010 districts either as this would likely bias our findings by conditioning on post-treatment effects of states enacting a particular plan. We also do not discard any districts depending on whether or not incumbents retired or were challenged in the pre-redistricting period. Any variation in voting patterns across precincts or VTDs due to pre-redistricting strategic choices would be absorbed into the entire distribution of counterfactual maps or simulations, and so would not affect the relative differences between enacted and alternative plans, or simulations. We also use 2008 presidential elections in our analyses since presidential voting is unaffected by down-ballot turnout and vote (e.g., Broockman 2009).

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