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**

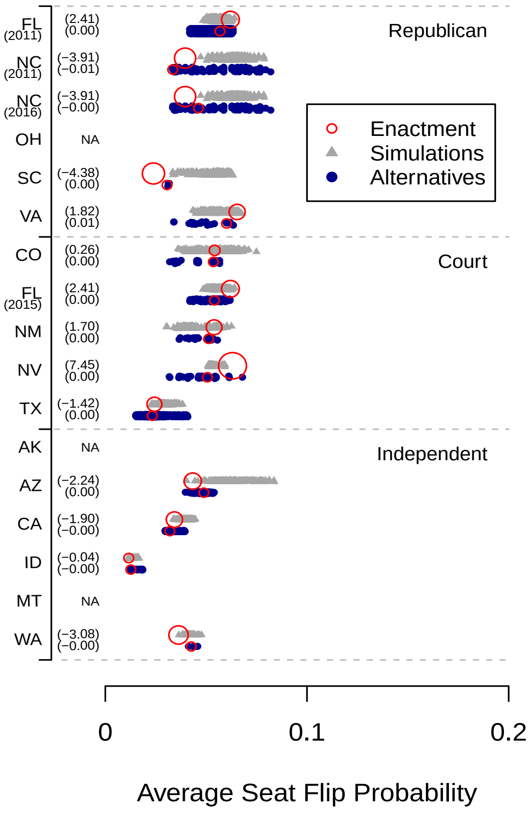
State	Min. R	Max R.	Dif. R	Min. D	Max D.	Dif. D	Avg. τ
AK	50.20	74.51	24.31	17.00	45.00	28.00	26.16
AZ	44.48	55.28	10.81	40.98	51.62	10.64	10.72
CA	36.64	42.62	5.99	54.10	60.30	6.20	6.09
CO	42.44	52.95	10.51	43.04	56.01	12.98	11.74
FL	48.38	60.97	12.59	38.91	59.37	20.46	16.52
ID	55.95	70.10	14.15	28.65	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.

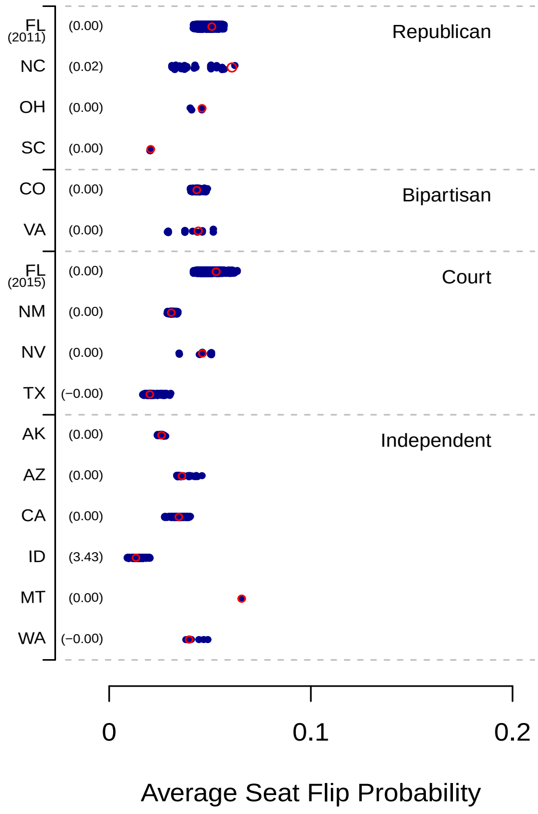
As seen in Table A2, states differ substantially in the size of their maximal party vote swings. Consequently, we doubt that using some fixed competitiveness threshold is appropriate. For instance, researchers will often set some pre-determined value, like 5% or 10%, and then examine which seats are 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

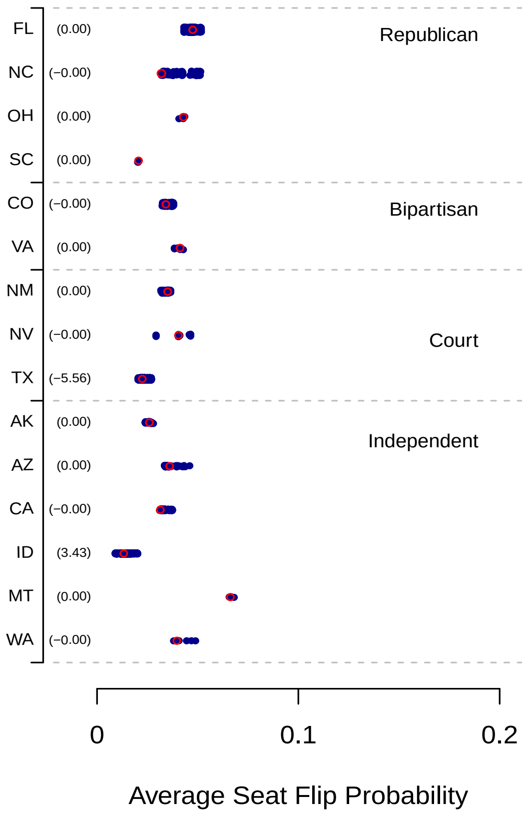
We focus on the 2010 redistricting cycle in the main paper because we have simulated maps and alternative maps available to compare to the actual redistricting plans in a large number of states. Chen and Rodden (2013), however, also have made available simulated data for the 2000 redistricting cycle 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

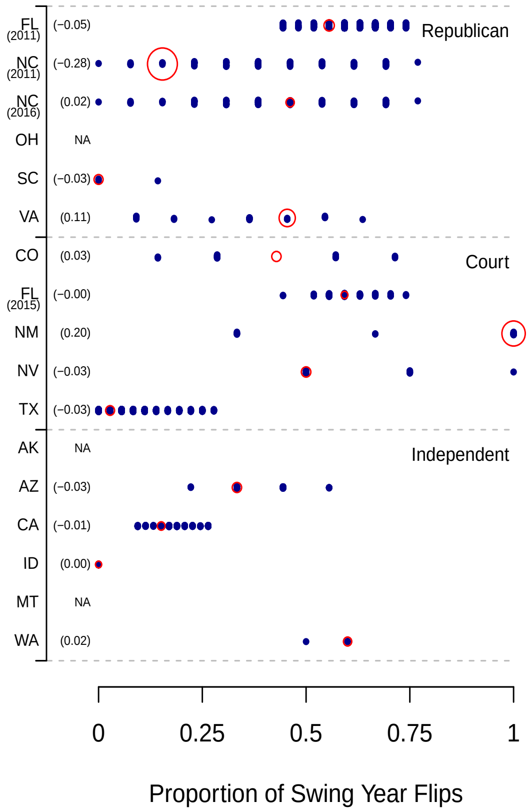


(b) State Senate Alternatives Only

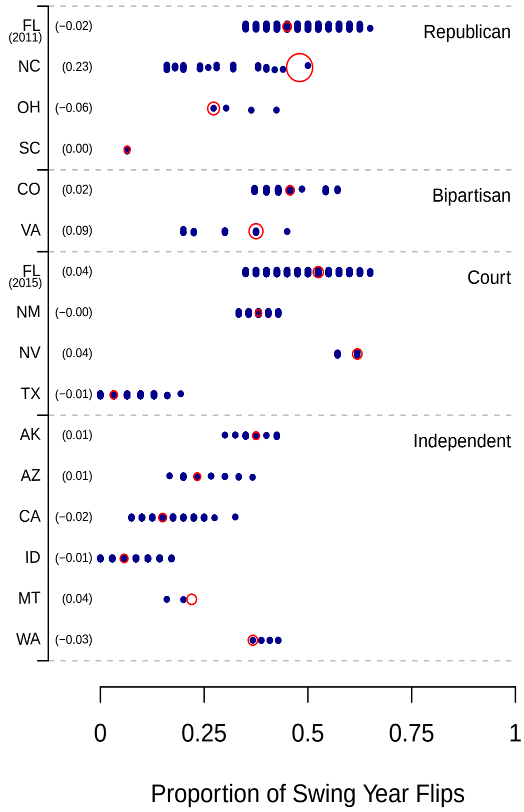


(c) State Assembly Alternatives Only

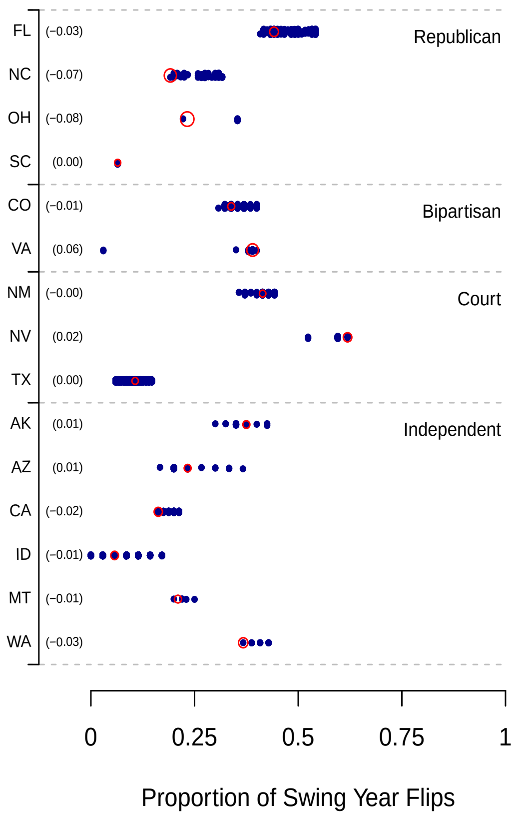
Figure A1: **Incumbent Loss Probabilities of U.S. House, State Senate and Assembly Districts for Enacted, Simulated and Alternatives Plans in 2010**



(a) House Alternatives Only

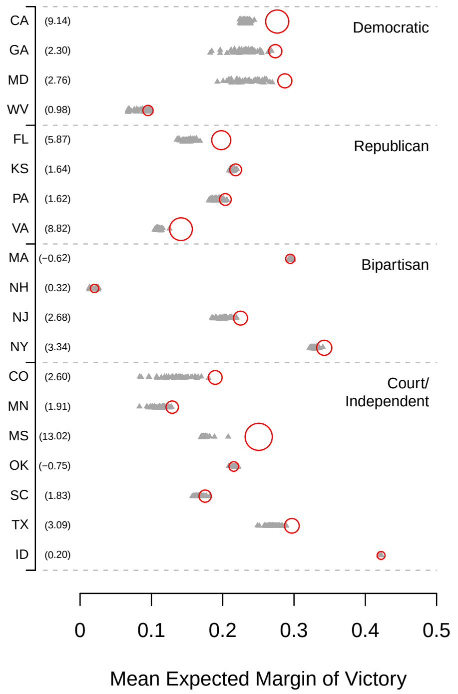


(b) State Senate Alternatives Only

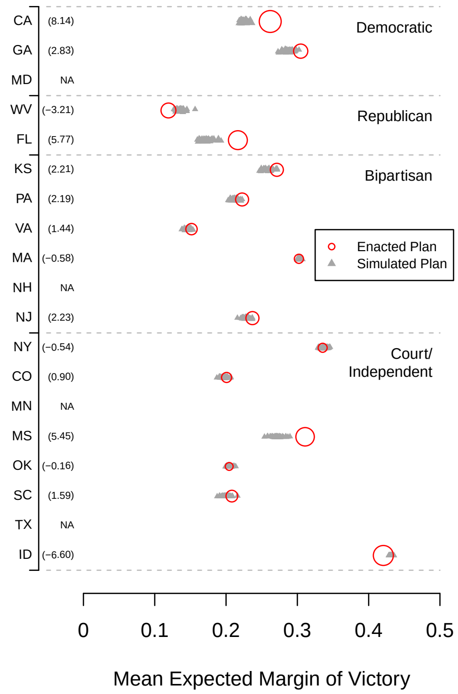


(c) State Assembly Alternatives Only

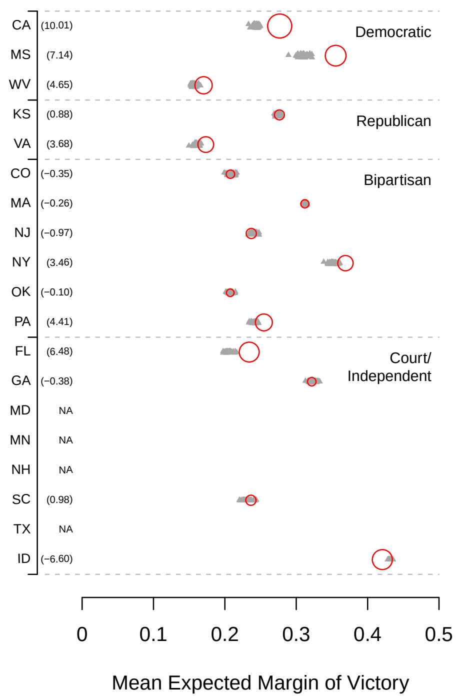
Figure A2: **Counts of Incumbent Seat Losses of U.S. House, State Senate and Assembly Districts for Enacted, Simulated and Alternatives Plans in 2010**



(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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