

# Does Gerrymandering Cause Polarization?

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*Both pundits and scholars have blamed increasing levels of partisan conflict and polarization in Congress on the effects of partisan gerrymandering. We assess whether there is a strong causal relationship between congressional districting and polarization. We find very little evidence for such a link. First, we show that congressional polarization is primarily a function of the differences in how Democrats and Republicans represent the same districts rather than a function of which districts each party represents or the distribution of constituency preferences. Second, we conduct simulations to gauge the level of polarization under various “neutral” districting procedures. We find that the actual levels of polarization are not much higher than those produced by the simulations. We do find that gerrymandering has increased the Republican seat share in the House; however, this increase is not an important source of polarization.*

Contemporary politics in the United States is historically distinctive in at least two respects. The first is the ever-increasing polarization of political elites. As McCarty, Poole, and Rosenthal (2006) have documented, partisan differences in congressional voting behavior have grown dramatically to levels not seen since the early twentieth century. The second distinction is the historically low levels of competition in congressional elections. This is especially true of the House of Representatives, where 99% of incumbents standing for reelection were successful in the 2002 and 2004 elections. In the swing to the Democrats in 2006, no individual Democrats were defeated and even 89% of standing Republicans were reelected.

Given the conjunction of these two patterns, it seems natural to draw a link; namely, the increased polarization of Congress is a direct result of the increasing ease of reelection. Presumably in an era of declining competition politicians no longer feel the need to reach out to moderate and independent voters. Instead politicians are free to pander to their base. Politicians who do not pander may face primary challenges by ideologically purer candidates.

Is there a link between increased polarization and declining competition? Scholars have yet to establish a com-

elling causal relationship. Some scholars (as well as pundits) claim that the link between polarization and declining competition is rooted in the increasingly sophisticated techniques deployed during the congressional redistricting that follows each decennial census. Pundits proclaim that we are in “the age of gerrymandering” (Hulse 2006). Many observers argue that redistricting increasingly produces districts that are homogeneous with respect to partisanship and voter ideology (Carson et al. 2007; Eilperin 2006; Fiorina, Abrams, and Pope 2004; Issacharoff 2004; Macedo et al. 2005; Theriault 2008; Toobin 2003). Consequently only conservative Republicans can win in conservative Republican districts just as liberal Democrats dominate liberal Democratic districts. Because redistricting no longer produces moderate, bipartisan, or heterogeneous districts, moderates cannot win election to the House.

This narrative is attractive not only because of analytical elegance, but also because it suggests a single, perhaps even feasible, solution to what ails the American polity: take the politics out of redistricting. Districts drawn by neutral experts and judges would be heterogeneous and politically moderate. Appealing to independents would become the key to winning election, and polarization would become a thing of the past.

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Unfortunately, although elegant in description and prescription, the story may not be true. There are a number of reasons to be skeptical. Certainly individual politicians desire more electoral security. Yet it is not clear that these individual desires lead to more security for all politicians or that the resulting manipulation of districting exacerbates polarization. Despite the increased ingenuity and sophistication of gerrymanders, numerous constraints and obstacles impede using redistricting as an “incumbency protection” plan. The requirements of equal population, compactness, and contiguity reduce the scope of such manipulation. Because many states have relatively few districts, gerrymanderers often lack the flexibility to create distorted districting plans. Legal requirements such as majority-minority districts may exacerbate polarization. But such requirements would be adhered to under other districting mechanisms.

Politicians, moreover, have, in addition to the incumbent protection incentive, a partisan incentive. This was most recently illustrated by Tom DeLay’s gerrymander of Texas. The partisan incentive leads to a more basic reason that gerrymandering does not necessarily generate safe seats. Here the majority party in a state tries to maximize the number of seats it wins in future elections. Such a goal leads it to create as many districts where it constitutes the majority as possible. Doing so implies that the supporters of the minority party are packed into as few districts as possible. Ironically, this process leads to more electoral security (and presumably more extreme preferences) for the minority party and less for individual members of the majority party.<sup>1</sup> Consequently, partisan gerrymandering leads to more competitive districts than noncompetitive districts and has an ambiguous effect on polarization.

Not only does the theoretical case for a link between gerrymandering and polarization have holes, but there is also little empirical support for the claim. That the U.S. Senate has experienced an increase in polarization at the same time as the House suggests that gerrymandering plays at best a modest role. This fact has not deterred writers from arguing either that gerrymandering-induced polarization from the House spilled over into the Senate (Eilpern 2006; Theriault 2008) or that gerrymandering has an additional contribution to polarization beyond the common factors that led to the increase in both the House and Senate. In this article, we find that gerrymandering has not contributed to polarization in the House. This finding undermines both of the claims.

<sup>1</sup>Grofman and Brunell (2005) have coined the term “dummyman-der” for those situations when the majority spreads its voters so thin that it actually loses seats.

We have three primary findings. First, a very large fraction of the polarization in the House is the result of within-district divergence between the voting records of Democrats and Republicans. In other words, for a given set of constituency characteristics, a Republican representative compiles an increasingly more conservative record than a Democrat does. Gerrymandering cannot account for this form of polarization. Second, some of the increase in polarization is due to an increase in the congruence between a district’s characteristics and the party of its representative. Republicans are more likely to represent conservative districts, and Democrats are more likely to represent liberal ones (also see Ono 2005 and Mann 2006). Such an effect is consistent with the gerrymandering hypothesis, but it is also consistent with a general geographic polarization of voters along ideological and partisan lines. Moreover, we find that the timing of this sorting effect is inconsistent with the gerrymandering story. It occurs in the 1980s and early 1990s, relatively early in the upswing of polarization. This is well before the most recent decline in electoral competition in the House. In particular, the larger increases in the sorting effect precede the 1994 elections when 34 Democratic incumbents were defeated and the Republicans enjoyed a 54-seat swing. Third, using data for counties, we compute the expected polarization following various districting procedures. The difference between the actual polarization and these simulated procedures allows us to establish estimates of the upper bound of the gerrymandering effect. This upper bound is very small and realistically can account, at most, for 10–15% of the increase in polarization since the 1970s.<sup>2</sup> Because we use county-level data, this bound is almost certainly biased upward. But most damning, this upper bound does not increase substantially following redistricting as the gerrymandering hypothesis would suggest.

Gerrymandering may have partisan effects even if these effects do not produce increased polarization. Using the same techniques we use to study polarization, we find a moderate tendency for gerrymandering to have benefited the Republican party. This result is likely to reflect, as illustrated by the Tom DeLay redistricting in Texas, an increase in Republican control of state legislatures. The Republicans may well have been able to draw most of the benefits from their political success with more

<sup>2</sup>Although this estimate may appear similar to that reported by Theriault (2008), who employs quite different estimation strategies, it is important to distinguish the nature of the two sets of findings. Our estimate of 10–15% is a rather generous *upper bound* for the total effect of gerrymandering (as opposed to redistricting). Thus, our results are not directly comparable to Theriault’s *point estimates* for the effects of redistricting. Our point estimates for redistricting are considerably smaller and generally not statistically different from zero.

traditional redistricting methods. Moreover, as we indicated above, aggressive gerrymandering makes majority party seats less safe. The Republicans may have paid a price for gerrymandering when a national tide swung to the Democrats in 2006.

## Preliminary Evidence

Despite the conventional wisdom that incumbency-protection gerrymanders have exacerbated partisanship and polarization in the House, there has been remarkably little systematic study of the issue. Carson et al. (2007) find that members representing newly created or significantly redrawn districts have more extreme voting records than those representing districts that continue in their old form. Theriault (2008) conducts a similar analysis and reaches similar conclusions. While suggestive, these studies fail to account adequately for one important feature of the last three apportionment cycles. As McCarty, Poole, and Rosenthal (2006) report, the reapportionments since 1980 have shifted seats from the Northeast where polarization is moderate to more polarized regions, the South and Southwest, while the relatively unpolarized Midwest has neither lost nor gained seats. Consequently, new congressional districts and those significantly redrawn are not a random sample of all districts, but are heavily concentrated in polarized regions.<sup>3</sup>

Another approach to establishing a link between polarization and gerrymandering is to demonstrate that congressional districts become more homogeneous following reapportionments. Theriault (2008) reports that the number of congressional districts that a presidential candidate won by a large margin increased following the 1990 and 2000 reapportionments. He also notes, however, that the standard deviation of the presidential vote across congressional districts fell after the 1980 and 2000 reapportionments, suggesting less partisan packing of districts. In other words, a falling standard deviation shows that districts have become more, not less, similar. The standard deviation increases a trivial amount following the 1990 round. In sum, his findings are inconclusive.

Brunell and Grofman (2005) present evidence challenging a key premise of the gerrymandering hypothesis. They argue that while redistricting has produced greater homogeneity of districts and a decline in competition for House seats, they find no evidence that the winners of homogeneous and noncompetitive (measured by con-

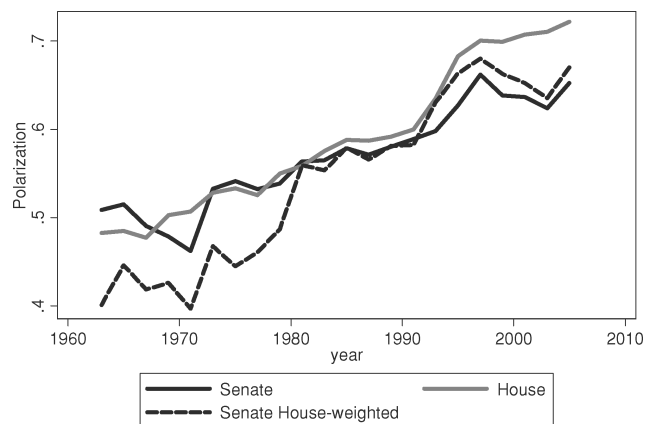
gressional vote shares) districts have more extreme voting records. But studies measuring competitiveness by presidential vote share, such as Ansolobehere, Snyder, and Stewart (2001), find a substantial correlation between extremism and lopsidedness.

McCarty, Poole, and Rosenthal (2006) look for direct evidence that the distribution of presidential voting is more bimodal in congressional districts than it is in other geographic units not affected by political districting. They find, however, that the distribution of presidential vote across congressional districts is very similar to the distribution of presidential vote across counties apart from differences attributable to majority-minority districting. Most district-level presidential vote margins are very similar to those of counties.

Another potential piece of evidence supporting the gerrymandering hypothesis is that during the 1990s the House of Representatives polarized at a greater rate than the Senate, presumably as a result of the 1990s redistricting. This claim is bolstered by comparing differences in party means or medians using common space NOMINATE (Poole 1998) or adjusted ADA scores (Groseclose, Levitt, and Snyder 1999) across chambers. Figure 1 provides a comparison of House and Senate polarization using common space scores. Indeed, there is evidence of faster House polarization following 1992, but the gap appears to close after 2002.

There are, of course, two problems in comparing polarization across chambers. The first is the well-known problem of comparability of voting scores estimated from disjoint sets of votes and legislators. Common space

**FIGURE 1 House and Senate Polarization Using Common Space Scores**



House and Senate Polarization

The House-weighted Senate polarization score is the mean partisan difference in the Senate where each senator's score is weighted by the number of House districts in his or her state.

<sup>3</sup>Carson et al. (2007) do conduct separate analyses for the South, but many new seats were created in polarized non-Southern states such as Arizona and California.

NOMINATE scores and adjusted ADA scores “solve” this problem by assuming that House members who later become senators maintain the same ideal point in both chambers. This identifying assumption is not directly testable, and there are many reasons why it may not hold.

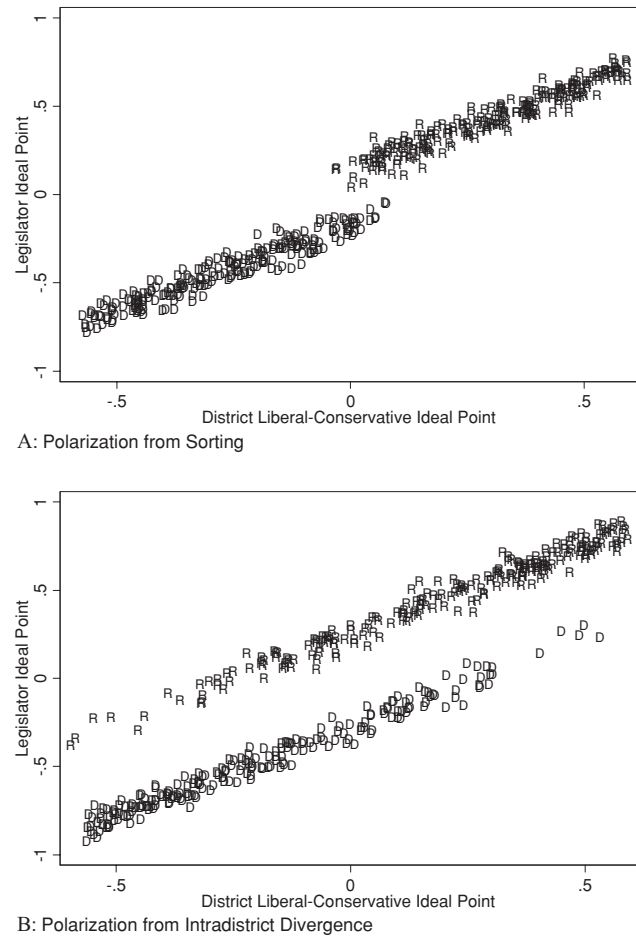
The second problem is a composition one. For example, Senate polarization measures weigh observations from California the same as those from Delaware while the House measure weighs California 53 times as much. So a more appropriate comparison would weigh Senate ideal points according to the number of House districts from that state. The thinner line in Figure 1 shows Senate polarization using this “House-weighting” scheme. Several features are noteworthy. First, the divergence between the House and House-weighted Senate measure begins after the 1998 elections. Second, the gap between these two measures closes after 2002 just as the gap in unweighted measures did. Third, in the 1960s and the 1970s, the polarization gap between the House and Senate-weighted series was not smaller than it has been in the past decade. Clearly, then, differences in polarization between the House and the Senate can occur for reasons that are totally unrelated to any recent spate of aggressive gerrymandering. In sum, the evidence for a gerrymandering effect based on House-Senate differentials is weak.

## Sources of Polarization

Polarization in Congress has two distinct manifestations. First, it can manifest itself in better *sorting* of legislators into districts so that Republicans are more likely to represent conservative districts and Democrats are more likely to represent liberal districts. Because gerrymandering is alleged to generate artificially safe districts for liberal Democrats and conservative Republicans, sorting is the form of polarization suggested by the gerrymandering hypothesis. The second manifestation is an increase in the *intradistrict divergence* of the parties. The difference in the voting records of Republicans and Democrats representing the same (or very similar) district has increased. Gerrymandering should have no direct effect on such differences. Both of these effects have increased the difference in mean or median voting scores of the two parties.<sup>4</sup>

<sup>4</sup>Our approach is somewhat similar to that employed by Ansolabehere et al. (2001), who illustrate that controlling for presidential vote, Democratic and Republican members compile quite different voting records. They do not, however, explicitly decompose polarization into divergence and sorting components as we do. Therefore, they are unable to say anything about the relative

FIGURE 2 Two Forms of Polarization



In the top panel, polarization occurs purely from sorting. Republicans represent the districts with the most conservative preferences. In the bottom panel, polarization occurs largely from intradistrict divergence. Republican legislators are more conservative than Democratic legislators even if they come from districts that have very similar or identical preferences.

This distinction between sorting and intradistrict divergence is illustrated graphically by examples shown in Figure 2. In both panels, we plot distributions of legislator ideal points against a hypothetical measure of district preferences. In panel *a*, the average Republican ideal point is much greater than the average Democratic ideal point because Republicans tend to represent all of the most conservative districts. But the difference between the Democrats and Republicans representing the moderate districts is quite small. In this scenario, polarization is primarily a product of sorting.

magnitudes of these two sources or their trends. Although they find that the responsiveness of roll-call voting to presidential vote is declining, their finding does not necessarily imply increasing intradistrict divergence.

In panel *b*, some Democrats represent conservative districts while some Republicans represent liberal ones. But Republican representatives compile much more conservative voting records than Democrats do for a given district preference. Consequently, polarization is due to intradistrict divergence. Although we have constructed both panels such that the difference in party means is 0.9, the two panels show sharply distinct forms of representation.

To formalize sorting and intradistrict divergence, note that we can write the difference in party means in DW-NOMINATE (hereafter simply NOMINATE, which we abbreviate *NOM*) as

$$\begin{aligned} & E(NOM | R) - E(NOM | D) \\ &= \int \left[ E(NOM | R, \mathbf{z}) \frac{p(\mathbf{z})}{\bar{p}} \right. \\ & \quad \left. - E(NOM | D, \mathbf{z}) \frac{1 - p(\mathbf{z})}{1 - \bar{p}} \right] f(\mathbf{z}) d\mathbf{z} \end{aligned}$$

where *R* and *D* represent Republican and Democratic representatives,  $\mathbf{z}$  is a vector of district characteristics distributed by density function  $f$ , and  $p(\cdot)$  is the probability that a district with characteristics  $\mathbf{z}$  elects a Republican member and  $\bar{p}$  is the average probability of electing a Republican. The difference between  $E(NOM | R, \mathbf{z})$  and  $E(NOM | D, \mathbf{z})$  reflects intradistrict divergence; variation in  $p(\mathbf{z})$  captures the sorting effect. When there is no sorting effect  $p(\mathbf{z}) = \bar{p}$  for all  $\mathbf{z}$ . Thus, without a sorting effect,

$$\begin{aligned} & E(NOM | R) - E(NOM | D) \\ &= \int \left[ E(NOM | R, \mathbf{z}) \right. \\ & \quad \left. - E(NOM | D, \mathbf{z}) \right] f(\mathbf{z}) d\mathbf{z} \end{aligned}$$

The right-hand side of this equation is the average intradistrict divergence between the parties. We abbreviate it as *AIDD*. When there is positive sorting such that more conservative districts are more likely to elect Republicans, then  $E(NOM | R) - E(NOM | D) > AIDD$  with the difference due to sorting. Thus, we can decompose polarization measured as  $E(NOM | R) - E(NOM | D)$  into the *AIDD* and sorting effects.

## Estimating the AIDD and Sorting Effects

Estimating the *AIDD* is analogous to estimating the average treatment effect of the nonrandom assignment of party affiliations to representatives. There is a large literature discussing alternative methods of estimation for this

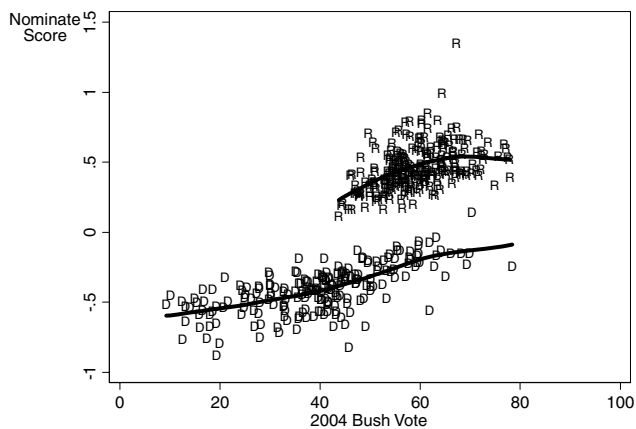
type of analysis. For now we assume that the assignment of party affiliations is based on observables in the vector  $\mathbf{z}$ .<sup>5</sup> If we assume linearity for the conditional mean functions, i.e.,  $E(NOM | R, \mathbf{z}) = \beta_1 + \beta_2 R + \beta_3 \mathbf{z}$ , we can estimate the *AIDD* as the OLS estimate of  $\beta_2$ . But following the suggestion of Wooldridge (2002), we include interactions of *R* with  $\mathbf{z}$  in mean deviations to allow for some forms of nonlinearity. Mean deviating  $\mathbf{z}$  before interacting with *R* ensures that the *AIDD* is the coefficient on *R*.

Because these functional forms are somewhat restrictive, we also use matching estimators to calculate the *AIDD*. Intuitively, these estimators match observations from a control and treatment group that share similar characteristics  $\mathbf{z}$  and then compute the average difference in *NOM* for the matched set. We use the bias-corrected estimator developed by Abadie and Imbens (2002) and implemented in STATA (Abadie et al. 2001).

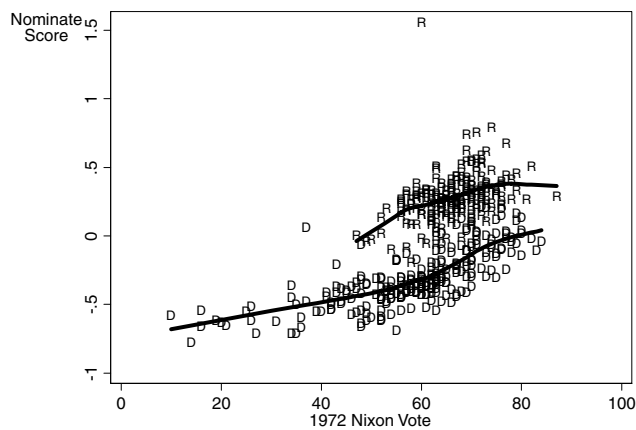
To visualize the extent of sorting and divergence in actual data, we plot the NOMINATE score for each member of the 108<sup>th</sup> (2003–04) House against the Bush vote in their districts in the 2004 election in the top panel of Figure 3. The presence of both sorting and intradistrict effects is evident. Clearly, Republicans are overrepresented in districts that Bush won by large margins and are absent from those he lost big. But holding Bush's vote share constant, there is a large gap between Republican and Democratic NOMINATE scores. The lowest lines plotted for each party show that the relationship between the NOMINATE score and the Bush vote is not exactly linear but the departure is not great. Importantly,  $E(NOM | R, \mathbf{z}) - E(NOM | D, \mathbf{z})$  does not vary much by  $\mathbf{z}$  (the Bush vote). So estimating *AIDD* by OLS (under the maintained assumption that assignment of party affiliations is based on observables) seems reasonable. Matching estimates are generally less efficient but are not biased by the nonlinearities. One problem is that many of the Democratic districts do not match with any Republican district. Most of these are majority-minority districts. Because the inclusion of "unmatched" districts may affect the matching estimates, we estimate the *AIDD* on districts whose propensity score for Republican representation lies between 0.1 and 0.9.<sup>6</sup>

<sup>5</sup>An unobservable factor only affects *AIDD* if it affects the probability of assignment and voting record of the members asymmetrically across parties.

<sup>6</sup>Crump et al. (2006) argue for the appropriateness of trimming the observations where the propensity of treatment is less than  $\alpha$  or greater than  $1 - \alpha$ . They provide an algorithm for estimating the optimal  $\alpha$ . In each sample, we find the optimal  $\alpha$  to be slightly lower than 0.1. But the results are quite insensitive to the exact threshold and changes in the sample size across congressional terms. The propensity scores are based on probit estimates using the same covariates as the matching estimator.

**FIGURE 3** NOMINATE versus Presidential Vote

a: NOMINATE vs. Bush Vote, 108th House



b: NOMINATE vs. Nixon Vote, 93rd House

The presidential vote in a congressional district is plotted against the NOMINATE score of the district's representative. Republican representatives from districts with a given presidential vote are much more conservative than are Democratic representatives from districts with similar presidential votes. The difference between the parties increased substantially from the 93<sup>rd</sup> to the 108<sup>th</sup> House.

The bottom panel of Figure 3 shows the relationship between NOMINATE scores and the 1972 Nixon vote in the 93<sup>rd</sup> House (1973–74). Here we see that the difference between lowess curves for each party is quite small. This suggests that there has been a major increase in the AIDD over the last 30 years. In addition, the sorting effect has increased as well. Although Nixon won in a landslide in 1972, still the number of Democratic districts on the liberal tail is much smaller than in 2004. In addition, the conservative Democratic districts are almost entirely gone by 2004. These districts in 1972 were overwhelmingly Southern and are now represented by conservative

Republicans (McCarty, Poole, and Rosenthal 2006; Poole and Rosenthal 2007).<sup>7</sup>

As discussed above, we estimate the sorting and intradistrict effect using both OLS and matching estimators.<sup>8</sup> Table 1 reports these results for the 107<sup>th</sup> and 108<sup>th</sup> Congresses. The results for the 108<sup>th</sup> are located in the upper panel. The first row lists the simple difference in party means (.867) as the benchmark measure of polarization. The second row provides the estimate of intradistrict divergence when we condition exclusively on the districts' presidential vote. The estimate of .667 suggests that 77% (.667/.867) of the contemporary level of polarization is accounted for by intradistrict differences, with the remaining 23% (.200/.867) due to sorting. The third row is an estimate generated by matching districts solely on the basis of the presidential vote. This estimate is slightly lower than that from OLS; divergence is still the much larger component of polarization. In the next two rows we add additional control variables to the OLS and matching models. These include income, region, and the racial and ethnic composition of the district.<sup>9</sup> The inclusion of these additional variables raises both the OLS and matching estimates. Based on the estimates from the more fully specified models, divergences account for almost 80% of total polarization.

In the lower panel of Table 1, the analysis is repeated for the 107<sup>th</sup> House (2001–02). These districts are based on districting following the 1990 Census. As suggested by the gerrymandering hypothesis, there is an increase in the overall level of polarization from the 107<sup>th</sup> House to the 108<sup>th</sup> of .021. In a comparison of the models based exclusively on presidential vote, the AIDD is larger in the 107<sup>th</sup> than the 108<sup>th</sup>. The estimates for the fully specified matching model are almost identical. This suggests that the overall increase was due to a large increase in the sorting effect, consistent with the gerrymandering hypothesis. But the fully specified OLS model tells a different story. These results suggest the AIDD increased by .011, which is more than 50% of the increase in polarization

<sup>7</sup>The patterns are almost identical if we plot the actual NOMINATE scores against the predicted NOMINATE scores from a regression on Bush vote, education levels, percent black, percent Hispanic, median income, and region.

<sup>8</sup>The sample includes only those districts represented by a Democrat or a Republican. When deaths or retirements cause multiple members to serve in the same district, we average the NOMINATE scores for same-party replacements, but discard opposite-party replacements. Thus, the sample sizes are occasionally less than 435.

<sup>9</sup>Each district is matched to the four closest districts in terms of the covariates. Observations are matched exactly on region. Varying the number of matches has little effect on the estimate.

TABLE 1 Estimates of AIDD

	Conditioning Variables	N	Estimate	95% Conf.
<b>108<sup>th</sup> House (2003–2004)</b>				
Total Polarization		433	.867	[.836, .897]
OLS	Pres Vote	269	.667	[.625, .708]
Matching	Pres Vote	269	.640	[.598, .682]
OLS	+demo, region	214	.678	[.628, .730]
Matching	+demo, region	214	.686	[.648, .723]
<b>107<sup>th</sup> House (2001–2002)</b>				
Total Polarization		435	.846	[.811, .879]
OLS	Pres Vote	296	.675	[.638, .712]
Matching	Pres Vote	296	.659	[.628, .690]
OLS	+demo, region	261	.667	[.625, .708]
Matching	+demo, region	261	.684	[.652, .716]

*Note:* Total polarization is the difference in the Republican mean DW-NOMINATE score and the Democratic mean score. Observations where more than one party represented the district in the Congress are dropped, leading to total Ns below 435. In the matching regressions, observations with propensity scores less than 0.1 or greater than 0.9 in magnitude are dropped. The OLS regressions are run for the same set of observations as used in the matching calculations.

from the 107<sup>th</sup> to the 108<sup>th</sup> House. This suggests a much smaller increase in the sorting effect following reapportionment.

### Does “Re”districting Cause Polarization?

Even if we accept the finding of the matching estimates that produce the larger increase in the sorting effect from the 107<sup>th</sup> to the 108<sup>th</sup>, it does not follow that the increase resulted from gerrymandering. Such an increase could occur for a number of other reasons, such as increases in partisan voting (Bartels 2000), realignment (Abramowitz, Alexander, and Gunning 2006), or greater geographic clustering of the like-minded (Bishop 2008). Therefore, we examine whether increases in the sorting effect following reapportionment are larger than those in other years. To test this implication of the gerrymandering hypothesis, Table 2 reports estimates of the AIDD and sorting effects for each congressional term since the 1970s, based on the fully specified OLS and matching models.

Both sets of estimates reveal that the sorting effect increased considerably over the 1990s between reapportionments. The matching estimates (columns 5 and 6) indicate that sorting actually decreased in 1993–94 following the reapportionment based on the 1990 Census. In contrast, sorting increased in the following two Con-

gresses. According to the matching estimates, the average biennial increase over the 1990s was .019, which is almost identical to the increase following the 2000 redistricting. Thus the causal effect of redistricting is approximately zero. While it is possible that the increases in 1995–96 and 1997–98 show a lagged effect of redistricting, the important result is that there is no particular year in each five Congress redistricting cycles that has a sharp increase in the sorting effect.<sup>10</sup> Rather, the increase in sorting appears to be a longer-term phenomenon whose origins predate the arrival of computerized gerrymandering.

The patterns for the earlier rounds of districting provide only a little more support for a gerrymandering effect. The OLS results (columns 3 and 4) show that the sorting effects increased more during the redistricting that followed the 1980 and 1990 censuses than in the surrounding years. The matching estimates also show an effect for 1980. But no such effect appears in the matching estimates for 1990. Further, it is important to note that even the largest of the year-to-year changes in the

<sup>10</sup>One objection to estimating the effect of gerrymandering from pre- and post-redistricting sorting measures is that incumbency and other frictions may preclude immediate resolution of the full impact. To test for such a lagged effect, we estimate a simple model of the change in sorting effect on indicators of whether redistricting occurred at  $t-1$  or  $t-2$ . The small number of observations precludes the inclusion of longer lags. Whether we use matching or OLS estimates, the coefficient on neither the  $t-1$  nor the  $t-2$  lag is statistically distinct from zero.

TABLE 2 AIDD and Sorting Estimates

Congress	Polarization	Average Intradistrict Divergence (OLS)	Sorting (OLS)	Average Intradistrict Divergence (Matching)	Sorting (Matching)	Number of Districts Used to Compute AIDD
108 <sup>th</sup> (2003–2004)	.867	.678	.189	.686	.181	214
107 <sup>th</sup> (2001–2002)	.846	.667	.179	.683	.163	261
106 <sup>th</sup> (1999–2000)	.827	.687	.140	.673	.154	271
105 <sup>th</sup> (1997–1998)	.816	.682	.134	.668	.148	278
104 <sup>th</sup> (1995–1996)	.797	.677	.120	.676	.121	260
103 <sup>rd</sup> (1993–1994)	.723	.600	.123	.614	.089	294
102 <sup>nd</sup> (1991–1992)	.678	.571	.107	.581	.097	322
101 <sup>st</sup> (1989–1990)	.664	.557	.107	.553	.111	305
100 <sup>th</sup> (1987–1988)	.668	.559	.109	.551	.117	296
99 <sup>th</sup> (1985–1986)	.660	.564	.096	.545	.115	290
98 <sup>th</sup> (1983–1984)	.636	.558	.078	.533	.103	319
97 <sup>th</sup> (1981–1982)	.605	.572	.033	.547	.058	404
96 <sup>th</sup> (1979–1980)	.583	.519	.064	.508	.075	389
95 <sup>th</sup> (1977–1978)	.561	.521	.040	.489	.072	387
94 <sup>th</sup> (1975–1976)	.573	.503	.070	.502	.071	364
93 <sup>rd</sup> (1973–1974)	.584	.495	.089	.499	.085	335

Note: Congresses that reflect the effects of reapportionment and redistricting are shaded.

sorting effect are not statistically significant given the level of estimation error of the AIDD. Given that much of the discussion about gerrymandering has focused on the use of sophisticated computer programs to draw boundaries, it is ironic that the largest effect we estimate occurred before the era of personal computing!<sup>11</sup>

Even if we accepted the pre- and postdistricting changes in sorting as the effect of gerrymandering, the effects are substantively quite small. Under this assumption, the gerrymandering effect is .07 for OLS and .06 for matching. These effects are less than 10% of the total level of polarization and less than 25% of the increase in polarization since 1973. If we “detrended” these estimates by subtracting the average increase in the sorting effect since the last round of districting, the total effects would be even smaller.<sup>12</sup>

Table 2 does provide some evidence for at least one aspect of the gerrymandering hypothesis: that political competition falls after redistricting. Recall that the AIDD is estimated from those districts with estimated probabilities of electing a Republican of at least .1 but no more

than .9. So the size of the sample used for estimating the AIDD is a rough measure of the number of competitive seats. The number of competitive districts fell by 83 in 1983, 28 in 1993, and 47 in 2003. The three rounds of redistricting account for 83% of the decline in competitive seats since 1980. Surprisingly, such dramatic declines in electoral competition have had very little impact on polarization.

## Does Districting Cause Polarization?

Although we have demonstrated that the sorting effect does not increase much following redistricting, it is still possible that polarization is greater than it would be if the districting process were more politically neutral. In other words, *districting* might cause polarization even if *redistricting* does not. To explore this possibility, we conduct a number of simulations designed to predict what polarization would be under various districting plans. The first step in these simulations is to estimate  $E(NOM | R, \mathbf{z})$  and  $E(NOM | D, \mathbf{z})$ . Given the results of the previous section, these can be adequately estimated by OLS with interactions of party and  $\mathbf{z}$ . Second, we estimate the probability that a Republican wins in a district with characteristics  $\mathbf{z}$ ,  $p(\mathbf{z})$ , using probit. To capture the effects of estimation

<sup>11</sup>Mainframe computing was used in districting before the 1980s, but not widely.

<sup>12</sup>Mid-decade redistricting has affected a small number of districts but is unlikely to have affected aggregate polarization to any large degree.



error across the simulations, we estimate  $E(NOM | R, z)$ ,  $E(NOM | D, z)$ , and  $p(z)$  on a bootstrapped sample.<sup>13</sup>

From these functions we can generate congressional districts from smaller (on average) fixed geographic entities for which we can observe  $z$ . After simulating an alternative districting plan, we compute  $\bar{z}$  for each new district. We then generate election outcomes  $\bar{R}$  or  $\bar{D}$  using  $p(\bar{z})$  and compute NOMINATE scores for each simulated district using  $E(NOM | \bar{R}, \bar{z})$  and  $E(NOM | \bar{D}, \bar{z})$ . Our simulated polarization measure is just the difference in means from the simulated data. We repeat this process 1,000 times for each simulation experiment.

We now describe the various districting experiments.

### Random Districting

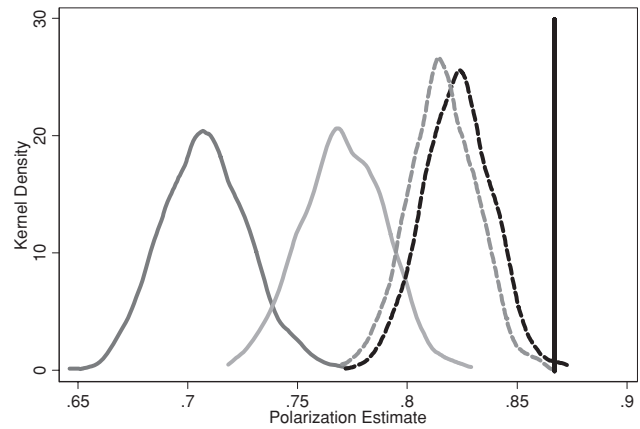
Due to data limitations, our underlying geographical data are from U.S. counties. A major limitation of this data is that there is tremendous variation in size, ranging from Loving County, Texas (pop. 179), to Los Angeles, California (pop. 9,545,829). To adjust for size differences and to rearrange these county units into new districts, we subdivide each county into 1,000-person blocks (and eliminate counties with lower populations). Unfortunately, we do not consistently observe  $z$  at the subcounty levels so we must assume that each of these county blocks is identical. As we discuss below, this homogeneity assumption biases towards finding a gerrymandering effect. Thus, our county block data set contains 10 observations for a 10,000-person county (remainders are dropped). Using this procedure, we created 275,584 county blocks.

To summarize, here is what was done in each of the 1,000 bootstraps for each experiment:

1. Draw a bootstrap sample from the actual congressional districts.
2. Estimate  $E(NOM | \cdot, z)$  and  $p(z)$  using the bootstrap sample.
3. Draw districts from county blocks and compute  $\bar{z}$  for each district.
4. Allocate each district to a Republican or Democrat from a random draw based on  $p(\bar{z})$ .

<sup>13</sup>One caveat is that these estimated functions are reduced-form estimates of the underlying relationships between district composition, member ideology, and partisanship. Because they are estimated for a particular set of districts, the relationships may not hold under alternative allocations of voters to districts. This problem, however, is one that is endemic to counterfactual analyses of districting because there is not enough data to estimate the structural relationship mapping any districting outcome to the distribution of partisanship and ideology.

**FIGURE 4** Bootstrap Estimates under Random Districting and Geographical Sorting



The darker solid density plot represents estimated polarization when congressional districts are created by random sampling of county blocks, without regard to state boundaries. Polarization is still substantial although it is significantly less than actual polarization, shown as the vertical bar. The lighter solid density plot represents estimated polarization when congressional districts are created by random sampling of county blocks within each state. Polarization is substantially increased by sampling within but remains significantly less than actual polarization. The figure indicates that polarization in large part reflects divergence of political preferences across states. The dashed line represents estimated polarization when contiguity and compactness of congressional districts are created by imposing a longitude (darker line) and latitude (lighter line) constraint on congressional district formation. The relationship of preferences to geography can produce nearly all of the polarization of the House of Representatives.

5. Assign a NOMINATE score to each district using  $E(NOM | \cdot, \bar{z})$ .
6. Compute polarization.

Our first districting experiment simply randomly allocates (without replacement) the county blocks into 435 districts, ignoring all legal, political, and geographic constraints (including state boundaries). Obviously, this produces 435 districts that are ex ante drawn from the same distribution. Differences between districts reflect only the random effects of the sampling process. Consequently, the simulated polarization approximately equals the AIDD. The darker solid curve in Figure 4 plots the kernel estimate of the distribution of the simulated polarization scores across the 1,000 iterations for the 108<sup>th</sup> House. For comparison, the vertical line is the actual level of polarization in the 108<sup>th</sup> House. The mean value of polarization is .708 with a 95% confidence interval of [.670, .748]. The results of all of the experiments are reported in Table 3.

TABLE 3 Simulation Results from the 108<sup>th</sup> (2003–2004) House

Experiment	House Polarization						
	Mean	St. Dev	Min	Max	Pr > .867	Maximum Districting Effect (108 <sup>th</sup> House)	Maximum Districting Effect (107 <sup>th</sup> House)
Random	0.708	0.020	0.646	0.773	0.000	.159	.159
Random by State	0.771	0.019	0.718	0.829	0.000	.096	.088
By Longitude	0.823	0.016	0.771	0.873	0.006	.044	.045
By Latitude	0.816	0.016	0.770	0.865	0.000	.051	.051
Racially representative	0.832	0.016	0.781	0.884	0.012	.035	.042
Partisan representative	0.853	0.015	0.798	0.892	0.198	.014	.012
Ideologically representative	0.856	0.015	0.800	0.906	0.204	.011	.011
<i>Actual Polarization (108<sup>th</sup> House)</i>	0.867						

In a second experiment, we simply add state boundaries to the experiment. Each state is assigned its actual number of congressional districts. Now districts are created from random sampling (without replacement) of county blocks within each state. The lighter solid curve in Figure 4 shows the distribution of the simulated polarization measure. Simply adding state boundaries raises the mean simulated polarization to .771. This implies that 33% of the sorting effect (Polarization – AIDD) is the result of demographic and political variation across states. And no more than a .096 difference in party means can be accounted for by how voters are allocated within states.

There are many reasons, however, to believe that even this small estimated effect is much larger than the actual effect. The first reason has to do with the limitations of the county data. Our procedure assumes that counties are demographically and politically homogeneous. In states with large counties, this homogeneity assumption makes it more unlikely that the simulations will produce either very conservative or very liberal districts. For example, the county blocks from counties that have sizeable minority populations but are less than 50% minority cannot be used by our simulations to generate very liberal majority-minority districts. Similarly, our simulations cannot put together the wealthy parts of Los Angeles County. Obviously, this reduces the chance of simulating high levels of polarization. The second reason why these random simulations overestimate the effects of gerrymandering is that they ignore a number of legal constraints on the districting process. Most importantly they ignore geographical constraints such as contiguity and compactness. Without geographic contiguity, there is less chance for similar areas to be paired together. Our simulations are unlikely to pair a block from relatively wealthy Nassau County, New York,

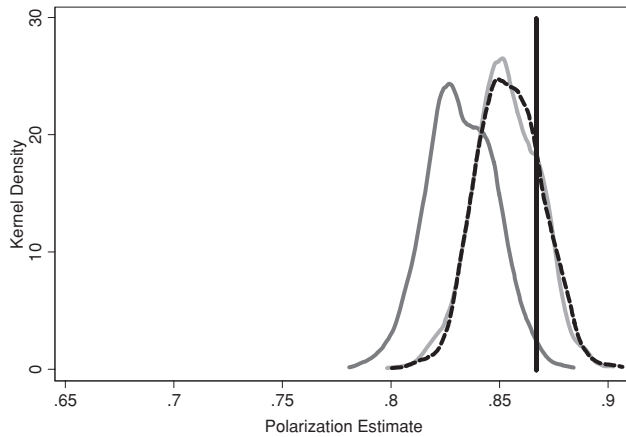
with a similar area from adjacent Suffolk County. Finally, random districts violate reasonable norms of representation. In the random districting scenario, all districts within a state are approximately microcosms of the state. Political and racial minorities have little opportunity to elect representatives who share their preferences. Districting systems that take such representation seriously will necessarily produce more polarization than the random districting benchmark.

### Geographical Constraints

We can roughly estimate the effects of imposing contiguity and compactness requirements. Because the county data are coarse, it is quite difficult to devise simulations of all districting plans that meet these requirements. Therefore, we use two different crude approximations. In the first, we rank order the blocks within each state by longitude of the county center. Then on the basis of this ranking we divide the state into districts from east to west so that district 1 is composed of the most eastern county blocks and district  $k$  is the most western. The second experiment is the same as the first except that latitude is used. Both of these districting schemes satisfy contiguity and compactness, but of course they represent just two of the many that do so.

Figure 4 also illustrates the simulated polarization results for districting based on longitude (darker dashed curve) and latitude (lighter dashed curve). The mean polarization score for longitude is .823, which suggests a gerrymandering effect of at most .044. Although it is substantively small, this difference is statistically significant at conventional levels as only 6 of the 1,000 simulations

**FIGURE 5 Bootstrap Estimates with Racially and Politically Representative Districts**



The solid black line is the density of estimated polarization when congressional districts are created by imposing a racial constraint on congressional district formation. County blocks are sorted on the basis of the percentage of their population that is African American. The solid gray line and dashed black line represent estimated polarization from partisan and ideologically representative districts, respectively. Polarization is substantially increased from that shown in Figure 4 but remains significantly less than actual polarization, shown as the vertical bar.

produce polarization scores exceeding the actual value. That is, even though the gerrymandering effect estimated using a simple geographic constraint is much smaller than the effect based only on purely random assignment within each state, the effect remains statistically significant. The results for latitude (see Table 3) are quite similar with a mean polarization score of .816.

### Minority Representation

Another consideration that random districting ignores is the representation of racial minorities. The random districts are very majoritarian and are likely to produce few African American or Hispanic representatives. To crudely, yet feasibly, capture the effects of majority-minority districting plans, we generate districts on the basis of their racial composition. The county blocks with the largest African American populations are placed in district 1, the second highest are placed into district 2, and so on.

The solid dark curve in Figure 5 reveals the distribution of polarization estimates. The mean score is .832 and the “p-value” with respect to the actual level is .012. Again while the difference is statistically significant, substantially the effect is only slightly more than 10% of

the increase in polarization since the 1970s. This result is hardly surprising. Given that African Americans represent only roughly 15% of the population, packing this population into as few congressional districts as possible can only explain so much of the national pattern of polarization. Simulations based on Hispanic population or African American plus Hispanic population generate slightly lower polarization scores.

### Political Representation

An undesirable feature of randomized districting is that the districts are unrepresentative of diverse interests in each state. Each district is approximately a microcosm of the state so that conservative and liberal interests are not well represented. These simulated districts are also extremely heterogeneous because they are microcosms.<sup>14</sup> Recent analyses of districting also question the desirability of random or majoritarian districting. In a model designed to examine the impact of various gerrymanders on policymaking in a majoritarian legislature, Gilligan and Matsusaka (2006) show that random districting only produces the policy desired by the median voter under the knife-edge case of a symmetric distribution of voter preferences. Moreover, they show that districting systems that maximize homogeneity of districts minimize the distance between the median voter’s ideal point and the legislative policy outcome. Coate and Knight (2006) characterize the “socially optimal” gerrymander. They show that the optimal gerrymander involves a very responsive seats-votes curve. Although our simulations do not produce a seats-votes curve, majoritarian districting systems such as those produced by our random simulations are not very responsive.

To establish districting benchmarks that avoid these concerns about random districts, we conduct two simulations that produce districts representative of the partisan and ideological diversity in each state. The first experiment attempts to replicate each state’s distribution of partisanship as measured by  $p(\mathbf{z})$  (simulations based on presidential vote share yield quite similar results). First, we use our probit estimates to calculate an estimate of  $p(\mathbf{z})$  for each of the county blocks. We then rank the county blocks on the basis of these estimates where ties are broken randomly. Then we create  $k$  districts using the first  $1/k$  percent of the blocks to form the first district, the second  $1/k$  to form the second, and so on. This procedure

<sup>14</sup>Buchler (2005) and Brunell (2008) make very similar arguments against majoritarian districting plans. Indeed they argue a positive case for the bipartisan, incumbency-protecting gerrymander. But as we point out below, such plans are not likely to be representative.

creates a distribution of districts that reflects the underlying distribution of partisanship of the county blocks.

It is important to note that the districts produced are quite different from what we would expect from incumbency-preserving gerrymanders. Under those plans, independent or swing districts (i.e.,  $p(\mathbf{z}) \approx .5$ ) would be underrepresented. In contrast, partisan representative districting produces many competitive districts.

A related criterion for politically representative districts is to produce districts where the distance from each representative's ideal point to those of her constituents is minimized. Unfortunately, we cannot implement this criterion directly because we do not observe the ideal points of voters or county blocks. We can instead rank county blocks on the basis of  $E(NOM | \mathbf{z})$ . However,  $E(NOM | \mathbf{z})$  is very highly correlated with  $p(\mathbf{z})$  so we do not report simulated districts based on it. We can alternatively rank on the basis of  $E(NOM | \mathbf{z}, R)$  or  $E(NOM | \mathbf{z}, D)$ . Because we estimate  $E(NOM | \mathbf{z}, R)$  and  $E(NOM | \mathbf{z}, D)$  with OLS, the rank correlation of the estimates is 1. So we report only simulations based on  $E(NOM | \mathbf{z}, R)$ . It is worth reiterating that, just as in the partisan case, this procedure produces moderate districts in the same proportion as moderate county blocks.

The solid gray line in Figure 5 reveals the distribution of simulated polarization scores based on partisan representative districts. The mean score is .853, a mere .014 less than the actual level. Almost 20% of the simulations produce polarization scores higher than the true level. So the effect is not statistically significant at conventional levels. As shown by the dashed curve in Figure 5, the results for ideologically representative districts are almost identical. The mean is .856 and 20% of the simulations produce higher polarization scores than the actual level.

The simulation results we have reported to this point are for a single Congress, the 108<sup>th</sup>. To see if *redistricting* has an effect on polarization, we need to compare a Congress that preceded redistricting with the one that followed. In particular, the estimated polarizing effect of biased districting should have increased after the round of districting following the 2000 Census. To test this hypothesis, we simulate the gerrymandering effect for the 107<sup>th</sup> House that preceded redistricting and compare it to our simulations of the effect in the 108<sup>th</sup> House. The last two columns of Table 3 show the simulated effect of districting for the 107<sup>th</sup> and 108<sup>th</sup> Houses for each of our experiments. The simulations are not statistically independent across Congresses because there is overlap in the samples of legislators used to estimate  $E(NOM | \cdot, \mathbf{z})$  and  $p(\mathbf{z})$ . Therefore, it is difficult to access the statistical sig-

nificance of the differences. But the substantive insignificance is quite apparent. The largest differential is .008 for random districting by state. Most of the experiments account for a much lower differential or, in the cases of longitude and racial sorting, even a negative differential. The average difference across all of the simulations is just .0003. Even if we were to accept the largest difference as the causal effect of the 2000 redistricting on polarization, it can only account for less than 3% of the increase in polarization since the 1970s.

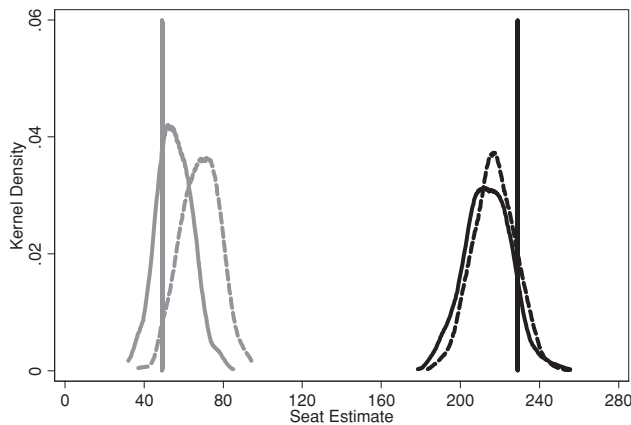
One might object to these results by arguing that the 2000 districting round had minimal effects because the sorting effect of gerrymandering is already so large that it could not have been increased by strategic districting. Casual inspection of Figure 3 seems to rule out this possibility as many conservative districts continue to be represented by Democrats just as many liberal districts continue to be represented by Republicans. But we can deal with this objection more systematically by estimating the predicted level of polarization under the counterfactual of perfect sorting using our estimates of  $E(NOM | \mathbf{z}, D)$ ,  $E(NOM | \mathbf{z}, R)$ , and  $p(\mathbf{z})$ . To generate perfect sorting, we assign each district a Republican representative if its propensity for electing a Republican is greater than the average Republican propensity. We then impute NOMINATE scores for each district using this deterministic assignment and calculate the resulting polarization. This exercise reveals that polarization would be as high as .884 if districts were perfectly sorted by party. So sorting could have increased as much as .27 following the 2000 redistricting, rather than the .10 estimated by OLS.

## Did Districting Solidify the Republican Hold on Congress?

We have seen that districting and, more specifically, redistricting are not major factors in the increase in polarization. The centers of the two major parties have drifted further apart as a result of other forces. On the other hand, it does appear that districting, abetted by the increased Republican hold on state legislatures, not only protected incumbents but also led to an increased Republican majority. This claim is supported by redoing our simulations for the 108<sup>th</sup> Congress.

To study competition and Republican shares, it sufficed to analyze only the  $p(\mathbf{z})$  part of our simulations. Simulated districts for which  $p(\mathbf{z})$  was in the interval [0.4, 0.6] were deemed competitive. For each simulated district, as in the polarization simulations, a binomial

**FIGURE 6 Republican and Competitive Seats under Longitude and Racial Sorting**



The solid curves are estimated republican seat share (black) and number of competitive seats (gray) when contiguity and compactness of congressional districts are created by imposing a longitude constraint on congressional district formation. The dotted curves are estimated Republican seat shares (black) and the number of competitive seats (gray) when congressional districts are drawn to be racially representative. The black vertical line is the actual number of Republican seats in the 108<sup>th</sup> House. The gray vertical line is an estimate of competitive seats based on probit estimates for the 108<sup>th</sup> House.

random draw using  $p(z)$  was used to determine whether the Republicans won the district.

When random districting at the national level was used, almost all simulated districts were competitive, reflecting the nearly 50–50 division of the electorate between the two parties. Under random districting, the Republicans had a 25% chance of winning even more districts than the 229 they actually won in the 2002 elections. This is because, with so many districts being competitive under random districting, the chances of a good Republican draw would be considerable.

The suggestion of a strong Republican advantage from gerrymandering is tempered when we respect geographic contiguity in the latitude and longitude levels. In both simulations, competitive seats fall to fewer than 80 and the Republicans can expect to win over 200 seats. The “p-values” for the Republicans winning more than 229 seats, rather than being zero, are a more modest .108 for longitude and .078 for latitude. The longitude results are shown as the solid curves in Figure 6.

Forming congressional districts by sorting by race within state (the solid curves of Figure 6) reduces competition to nearly the observed level but also slightly reduces the expected number of Republican seats over the geographic contiguity experiments. The “p-value” increases to .163. Finally, sorting along partisan and ideological

lines, in expectation, reproduces almost exactly the actual numbers of competitive and Republican congressional districts with “p-values” similar to those for longitude and latitude. Partisan sorting has a “p-value” of .069 and ideological, .058.<sup>15</sup>

In summary, gerrymandering within states has sharply increased the number of Republican congressional districts over what it would be if districts were randomly formed from county blocks. On the other hand, the increase is much less sharp if other constraints, such as respecting geographical contiguity or creating minority-majority districts, are imposed.

## Conclusion

Despite a lack of direct evidence, partisan gerrymandering has become one of the prime suspects in the investigation into what killed moderation and bipartisanship in American politics. The evidence just presented suggests that partisan gerrymandering has worked to the advantage of the Republicans in the 108<sup>th</sup> Congress although the same gerrymanders may have been detrimental once the tide switched to the Democrats in 2006.

Partisanship would appear to make a compelling circumstantial case for an increase in polarization. Politicians are observed engaging in raw power politics to draw districts for personal and partisan advantage. Simultaneously, electoral competitiveness declines in Congress. It seems reasonable to conclude that the two phenomena are related and that the consequence is greater polarization.

But in our search to uncover the smoking gun, the case has crumbled. True, the sorting effect has increased over time, as shown in Table 2. But the secular increase in sorting does not appear to be linked to census-triggered redistricting that would reflect gerrymanders. A good deal of the increase reflects the gradual disappearance of the one-party South (e.g., Abramowitz, Alexander, and Gunning 2006) and increased geographical sorting on political and social attitudes (e.g., Bishop 2008). Moreover, the secular increase in polarization is not primarily a phenomenon of how voters are sorted into districts. It is mainly the consequence of the different ways Democrats and Republicans would represent the same districts. Such a finding suggests that there may be considerably more payoff in efforts to link polarization to changes in social

<sup>15</sup>Our findings about competition are consistent with those of Abramowitz, Alexander, and Gunning (2006), who find that levels of competitiveness (based on presidential vote) change little following reapportionments. Using a regression-discontinuity design, Friedman and Holden (2006) find that redistricting increases political competition.

and economic structure (e.g., McCarty, Poole, and Rosenthal 2006; Stonecash, Brewer, and Mariani 2002), in the nature of the legislative agenda (Lee 2006), in the opportunities and strategies of party leadership (e.g., Roberts and Smith 2003; Rohde 1991; Theriault 2008), or ideological and organizational changes in the Republican party (Perlstein 2001).

Our simulations further demonstrate that the levels of polarization we observe are quite consistent with congressional districts representative of the states for which they are drawn. Thus, the scope of districting reform to eliminate polarization is extremely limited. Even if we eliminated districting altogether and elected candidates statewide, we could only roll polarization back to the level of the mid-1990s.

Indeed, if anything, we underestimate the ability of blind redistricting to reduce polarization. The relatively blind redistricting used in our simulations will create a large number of districts that are quite heterogeneous with respect to income, race, ideology, and other characteristics. To estimate how these districts would be represented, we have relied on linear models using “average” demographic characteristics of the simulated districts. Research by Gerber and Lewis (2004), however, indicates that legislators from these average, heterogeneous districts are likely to deviate, in a polarized fashion, from the “average” preferences of the constituents. That is, the AIDD is likely to be greater for a heterogeneous district than for a homogeneous one.

Nothing we say should be interpreted as contentment with congressional districting as it is currently practiced. The protracted political and legal battles over the boundaries cannot help but diminish the legitimacy of American democracy. And redistricting does appear to have a negative impact on electoral competition. There are many reasons to do something about gerrymandering. But reducing polarization is not one of them.

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