

## *Estimating the Partisan Consequences of Redistricting Plans—Simply*

Although some judges and political scientists have recently questioned the idea that it is possible to predict the partisan consequences of redistricting plans, I demonstrate that it is simple to do so with a pair of OLS equations that regress voting percentages on major party registration percentages. I test this model on data for all California Assembly and congressional elections from 1970 through 1994, and compare it to more complicated equations that contain incumbency and socioeconomic variables. The simplest equations correctly predict nearly 90% of the results. I show that analogous equations using registration or votes for minor or even major offices in California, North Carolina, and Texas can also predict outcomes with considerable accuracy. Using these equations, I show that the so-called "Burton Gerrymander" of 1980 had minimal partisan consequences, while the nonpartisan plan instituted by the California Supreme Court's Special Masters in 1992 was nearly as biased in favor of the Republicans as the proposal of the Republican party. I also introduce a new graphic representation of redistricting plans and conclude with a discussion of some seemingly methodological choices that have important substantive implications for assessing the fairness of redistricting plans.

### **Can Gerrymandering Be Quantified?**

Is it possible to measure partisan gerrymandering directly and reliably? Can it be done even before an election takes place under a proposed redistricting scheme, or even if a plan is never put into effect? Although politicians have generally believed that they could quite accurately determine the partisan consequences of redistricting plans, some judges and political scientists have recently scorned this belief, while others have implicitly cast doubt on it by focusing on the intricacy of lines between districts as an indirect indication of an intent to gerrymander. For example, in 1992, California Chief Justice Malcolm Lucas, a Republican and former law partner of the Republican governor who appointed him to the state's highest court, curtly rejected extensive evidence that a redistricting plan drawn under the auspices of three judges who had been appointed by Republican governors was meant to

damage Democrats: “[P]redictions of future election contests are quite obviously speculative and imprecise, involving the weighing of countless variables” (*Wilson v. Eu* 1992, 727). Similarly, in his provocative analysis of town-level registration and election statistics from Massachusetts and Connecticut, political scientist Mark E. Rush contends that voters’ allegiances to parties are too weak and shifting for redistricting to have very determinable consequences. Consequently, Rush concludes, courts should abandon the attempt to adjudicate partisan gerrymandering announced by the U.S. Supreme Court in the 1986 Indiana case of *Davis v. Bandemer*: “[I]f we cannot determine a town’s partisan profile, we cannot make the claim that a districting system is unfair to one of the parties, because we cannot say with certainty where the parties-in-the-electorate are located” (Rush 1993, 96).

This paper rejects the contentions of Lucas and Rush and suggests that we do not have to resort to such indirect measures of partisan gerrymandering as compactness (Polsby and Popper 1991, 336), because a simple, unequivocally politically neutral test that uses widely available data is quite reliable. Unlike other measures of partisan bias (Gelman and King 1990; Grofman 1983; King 1989; King and Browning 1987; Niemi 1985), the index of party strength presented here may be computed before an election has been held and it offers strong insights into the intentions of the redistricters (as indicated by the partisan consequences of their plans) and into just how those intentions are put into effect. The test is not only more intuitively meaningful, less dependent on complex statistical theory, and therefore easier for political activists, reporters, judges, and other interested parties to use than sophisticated variants of seats-votes ratios (Gelman and King 1990, 1994a, 1994b), but the simulations it suggests are more clearly tied to the specific electoral history of a jurisdiction than are those in more general, abstract schemes. I test the method on extensive data from California and more limited information from North Carolina and Texas.<sup>1</sup> I also introduce a new, revealing, and simple graphic representation of redistricting plans. It may be that courts should avoid partisan political thickets, but if so, it is not because they cannot find their way. Justice Byron White was right when he wrote that “[I]t requires no special genius to recognize the political consequences of drawing a district line along one street rather than another” (*Gaffney v. Cummings* 1973, 752–53).

### The Basic Model

Nearly 90% of the winners in California Assembly and congressional contests from 1970 through 1994 can be predicted correctly with two elementary equations estimated by ordinary least squares regression:

$$(1) \%D = B_{01} + (B_{11} * \%Dreg) + (B_{21} * \%Rreg) + u_1$$

$$(2) \%R = B_{02} + (B_{12} * \%Dreg) + (B_{22} * \%Rreg) + u_2$$

where

$\%R$  = Republican percentage of the total (not just two-party) vote, by district;<sup>2</sup>

$\%D$  = Democratic percentage of the total vote, by district;

$\%Rreg$  = Republican percentage of the total (not just two-party) registration, by district;

$\%Dreg$  = Democratic percentage of the total registration, by district;

the B's are the relevant OLS regression coefficients; and

u = an error term.<sup>3</sup>

I estimated these equations separately for each election year for each legislative body. Thus, for instance, the 1970 Democratic percentage of the votes for all Assembly candidates was regressed on a constant term and on the October 1970 percentages of the total registration (including minor parties and independents) for the Democrats and for the Republicans in each Assembly district. From this, one can calculate predicted Democratic percentages of the total vote for each Assembly district. I then performed a regression of the Republican percentage of the vote on the same independent variables, which enabled me to calculate a predicted Republican percentage in each district.<sup>4</sup> Overall, 52 regressions were run—one for each year from 1970 through 1994 for Congress and for the Assembly. The results of these equations are listed in Table 1.

Besides the R<sup>2</sup>s and standard errors of the regressions, which indicate a fairly highly predictive model by social scientific standards, two assessments of the uncertainty of the predictions were performed.<sup>5</sup> The first focuses on whether the relationships between partisan registration and the vote were constant across all the districts in the state. If they were not, then many of the winners predicted by the equations in Table 1 would not be the actual winners. Yet when the predicted winners are calculated, and those calculations are compared to the actual victors, they agree 90% of the time (89.1% for the Assembly and 91.6% for Congress).<sup>6</sup> The second assessment takes the coefficients from Table

TABLE 1  
 Statistics for Party Registration Regressions  
 (t statistics in parentheses; SER = standard error of the regression)

Year	Intercept	Democratic Registration	Republican Registration	R <sup>2</sup>	SER
<i>Panel A: Assembly Democratic Vote Percentages</i>					
1970	2.55 (2.41)	-0.150 (-1.35)	-0.030 (-2.71)	0.73	.10
1972	2.04 (2.16)	-0.010 (-0.98)	-0.025 (-2.48)	0.61	.12
1974	2.29 (3.84)	-0.013 (-2.10)	-0.026 (-4.07)	0.65	.09
1976	1.40 (1.87)	-0.004 (-0.44)	-0.018 (-2.24)	0.52	.14
1978	0.79 (1.23)	0.002 (0.34)	-0.011 (-1.55)	0.39	.16
1980	1.04 (2.35)	0.001 (0.13)	-0.017 (-3.18)	0.60	.14
1982	0.93 (1.97)	0.002 (0.44)	-0.015 (-2.76)	0.70	.13
1984	1.58 (2.45)	-0.005 (-0.65)	-0.022 (-2.99)	0.63	.16
1986	1.70 (3.48)	-0.007 (-1.23)	-0.022 (-4.04)	0.76	.11
1988	1.30 (2.50)	-0.002 (-0.38)	-0.017 (-2.90)	0.73	.11
1990	1.63 (2.52)	-0.006 (-0.88)	-0.020 (-2.73)	0.59	.13
1992	0.91 (2.77)	0.001 (0.35)	-0.012 (-3.04)	0.72	.10
1994	1.35 (5.49)	-0.004 (-1.40)	-0.018 (6.14)	0.85	.07
<i>Panel B: Assembly Republican Vote Percentages</i>					
1970	-1.80 (-1.72)	0.017 (1.59)	0.033 (2.94)	0.73	.10
1972	-1.55 (-1.69)	0.015 (1.55)	0.031 (2.94)	0.62	.12
1974	-1.43 (-2.38)	0.015 (2.29)	0.028 (4.23)	0.65	.09
1976	-0.61 (-0.79)	0.006 (0.69)	0.020 (2.40)	0.49	.14
1978	0.19 (0.30)	-0.002 (-0.34)	0.011 (1.55)	0.39	.16
1980	-0.23 (-0.51)	0.001 (0.15)	0.019 (3.50)	0.61	.14
1982	-0.09 (-0.19)	-0.001 (-0.16)	0.017 (3.02)	0.70	.13
1984	-0.77 (-1.15)	0.007 (0.89)	0.024 (3.16)	0.62	.16
1986	-1.01 (-2.10)	0.010 (1.83)	0.026 (4.73)	0.78	.10
1988	-0.72 (-1.26)	0.006 (1.00)	0.022 (3.40)	0.72	.12
1990	-1.00 (-1.76)	0.010 (1.57)	0.024 (3.69)	0.66	.12
1992	-0.26 (-0.78)	0.002 (0.51)	0.016 (3.94)	0.73	.10
1994	-0.62 (-2.44)	0.006 (2.21)	0.021 (7.02)	0.86	.07
<i>Panel C: Congressional Democratic Vote Percentages</i>					
1970	-1.79 (-0.87)	0.030 (1.42)	0.150 (0.71)	0.65	.12
1972	-0.46 (-0.33)	0.017 (1.15)	0.000 (0.05)	0.63	.12
1974	-0.80 (-0.70)	0.022 (1.78)	0.004 (0.35)	0.69	.11
1976	0.28 (0.28)	0.009 (0.85)	-0.006 (-0.59)	0.63	.11
1978	1.13 (1.52)	-0.000 (-0.04)	-0.017 (-2.04)	0.68	.11
1980	1.13 (2.05)	-0.000 (-0.10)	-0.018 (-2.82)	0.67	.11
1982	0.92 (1.66)	0.001 (0.24)	-0.014 (-2.14)	0.71	.10
1984	1.49 (2.06)	0.004 (-0.50)	-0.022 (-2.66)	0.72	.12
1986	1.52 (2.17)	-0.004 (-0.52)	-0.021 (-2.58)	0.75	.11
1988	2.06 (2.86)	-0.010 (-1.23)	-0.026 (-3.20)	0.75	.11
1990	-0.18 (-0.25)	0.014 (1.64)	0.000 (0.00)	0.66	.11
1992	0.83 (2.29)	0.002 (0.45)	-0.011 (-2.48)	0.77	.08
1994	1.27 (3.77)	-0.003 (-0.82)	-0.017 (-4.28)	0.86	.07
<i>Panel D: Congressional Republican Vote Percentages</i>					
1970	2.59 (1.23)	-0.028 (-1.29)	-0.014 (-0.62)	0.62	.12
1972	1.00 (0.72)	-0.013 (-0.85)	0.004 (0.28)	0.64	.12
1974	1.64 (1.30)	-0.020 (-1.50)	-0.003 (-0.22)	0.64	.12
1976	0.49 (0.53)	-0.007 (-0.70)	0.009 (0.92)	0.68	.10
1978	-0.25 (-0.34)	0.001 (0.18)	0.018 (2.22)	0.69	.10
1980	-0.49 (-0.84)	0.004 (0.64)	0.022 (3.24)	0.66	.12
1982	-0.16 (-0.23)	0.001 (0.17)	0.016 (1.91)	0.57	.13
1984	-0.56 (-0.81)	0.005 (0.61)	0.022 (2.69)	0.72	.12
1986	-0.71 (-0.91)	0.006 (0.24)	0.023 (2.71)	0.73	.11
1988	-1.30 (-1.76)	0.012 (1.47)	0.029 (3.44)	0.76	.11
1990	0.52 (0.88)	-0.005 (-0.90)	0.006 (0.85)	0.69	.09
1992	0.07 (0.18)	-0.002 (-0.34)	0.011 (2.24)	0.72	.09
1994	-0.10 (-0.32)	0.007 (0.19)	0.015 (3.78)	0.87	.07

Source: Computed from registration and vote percentages in *California Journal* and reports of the California Secretary of State.

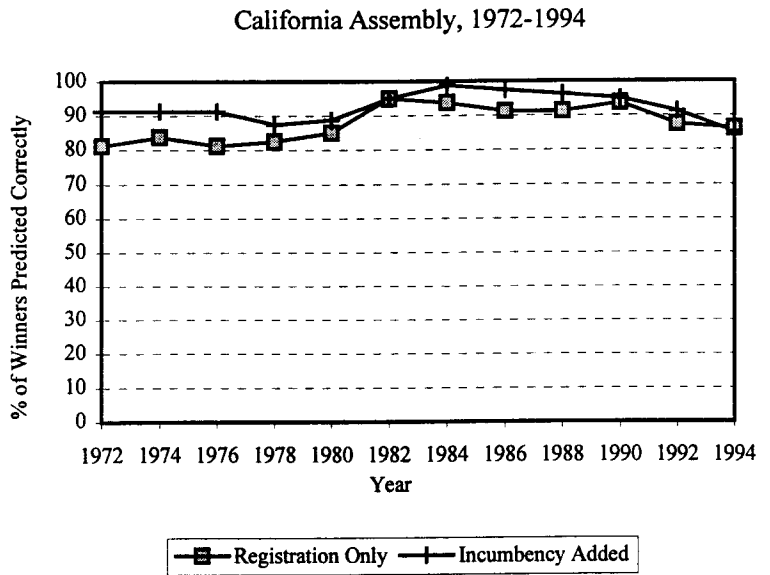
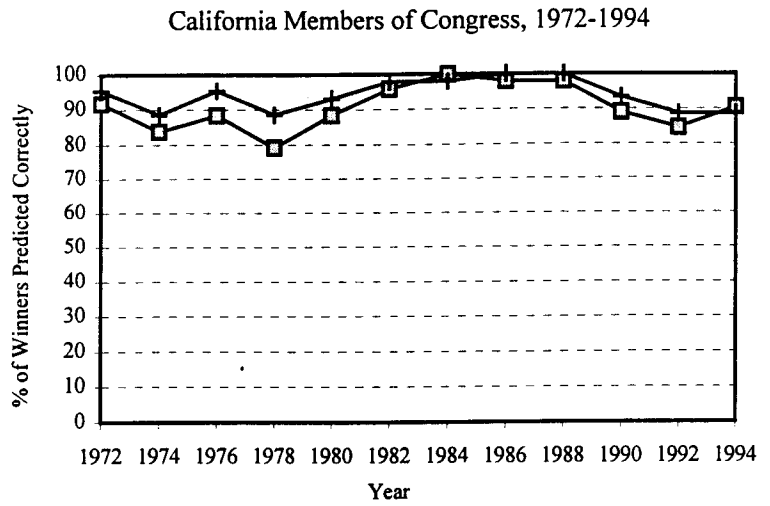
l and combines them, for each year separately, with the registration data from the succeeding election, calculates the expected winners, and compares them with the actual winners. Thus, the coefficients for the 1970 Assembly election are multiplied by the relevant registration percentages for each district in 1972 to predict 1972 winners, and so on for other years. Despite the repeated political upheavals in California during this period (Watergate, Proposition 13, the 1982 recession, the 1990–92 recession, the 1994 Republican surge—see Kousser, 1995b), these out-of-sample predictions correctly call the winners 88.7% of the time—only slightly less than if they are used to predict the elections in each district in the years they are based on.

Figure 1, which gives the proportions of winners predicted correctly by the equations for the previous election (as well as for an equation that includes incumbency, to be explained below), also shows that the predictions are approximately as good in years spanning redistrictings as in pairs of years within the same redistricting regime. If, as Rush argues, partisan gerrymandering is practically impossible because voters' loyalties are shattered when the boundaries of their home districts are redrawn, then the predictions for the 1974 Assembly or 1982 congressional contests, for instance, ought to be less accurate than those for the 1972 Assembly or 1980 congressional races.<sup>7</sup> In fact, they are more accurate. As a glance at the graphs show, the reliability of the predictions of the first post-redistricting years, with all the shuffling of lines and the increase in the number of open seats that generally takes place, is not noticeably different from that in other elections. And for the purposes of assessing the effects of proposed or actual redistricting plans, winning, not the percentage of variance explained or the results in subsections of a district, is the best test of predictability, for in elections in single-member districts, it is finishing first that counts.<sup>8</sup>

#### **How Important Is It to Add Incumbency and Other Variables?**

Of the factors that account for the other 10% of the results and the other third of the variance in vote percentages, probably the most important in the past has been incumbency. Politicians and journalists agree with political scientists that incumbency is potent, and both formal and informal estimates of the effect of redistricting often take incumbency into account (Cain 1985; Gelman and King 1994a, 1994b). Incumbents are better known than most challengers and have more opportunity to build their reputations and obligations through constituency

**FIGURE 1**  
**Speculative and Imprecise?**  
**Predictions from Basic Model and With Incumbency Added**



service; incumbents are also more experienced in campaigning, more familiar with their districts, can raise funds more easily, and, as is sometimes overlooked, occupy inherently safer seats. Over a 13-year period that spans five different redistricting arrangements in California, the margins of Democratic registration over Republican registration in the districts of Democratic incumbents averaged 30.8% in Congress and 31.8% in the Assembly. By contrast, Republican Assembly incumbents occupied seats in which the number of Republican registrants, on average, equaled Democratic registrants, while Republicans enjoyed very slight registration margins in the congressional districts of Republican incumbents. The same margins in open seats fell almost exactly between the party extremes, with means of 15.1% for Congress and 13.6% for the Assembly.<sup>9</sup> In equations predicting election outcomes, therefore, incumbency should not be expected to add a great deal to explanations that already include party registration, because there is so much collinearity between the independent variables.

Incumbency is least useful in predicting overall results during an election year just after a redistricting, because that is when there are the most open seats. In four California cases after a redistricting (1972, 1974, 1982, and 1992), 21.4% of the congressional districts had no incumbent; in the other nine contests, only 8.0% did not have an incumbent. The analogous figures for the Assembly are 25.3% and 14.9%, respectively. Furthermore, term limits, which force state legislators out and often up in a maximum of 6–8 years in California, are sure to reduce the proportion of seats occupied by incumbents in the Assembly and, because of more frequent challenges from unemployed state legislators, in Congress as well. Incumbency therefore seems likely to be less and less important in predicting the effects of a redistricting, as suggested by the convergence of the lines after 1990 in both parts of Figure 1.

To test for the added effect of incumbency, we merely add to the previous equations another term  $I$ , where  $I = 1$  if the incumbent is a Democrat,<sup>10</sup> 0 if the seat is open, and  $-1$  if the incumbent is a Republican. The lines connecting the crosses in Figure 1 show that appending such a term to the equations predicting the Democratic vote increases the proportion of winners predicted correctly from 88–90% without the term to 91–94% with it.<sup>11</sup> Incumbency improves our ability to predict outcomes then, but not by very much. This is a convenient result, for it is impossible for outsiders to predict with certainty which incumbents will declare their candidacies for particular seats in plans that have not yet been put into place or that may never be adopted.<sup>12</sup>

The closeness of the two sets of lines in Figure 1 makes us more confident that we can go ahead and project winners even before we know who is running.<sup>13</sup>

As a final indication of how well party registration predicts the vote in California, consider how much more successfully we or someone who was drawing district lines could guess the results of particular plans if we had a great deal of socioeconomic data available, in addition to party registration. If we regress vote percentages on 11 socioeconomic variables,<sup>14</sup> plus party registration and incumbency, and we use the resulting regression coefficients to predict the outcomes in each district, we actually make one more mistake in prediction for California congressional elections in 1984 than we do if we use only registration in our prediction equation.<sup>15</sup> For 1980, we make exactly the same number of errors—five—whether the prediction equation includes only party registration, or party registration and incumbency, or party registration and incumbency and the socioeconomic variables. For 1976, we improve our results a good deal—making five fewer errors—if we take incumbency into account, but we gain nothing by this measure when we add 11 attributes of socioeconomic status for each district. The conclusion is that on the district level, partisan registration is a good shorthand for a set of socioeconomic and attitudinal variables that produce outcomes.

#### **Other States and Other Indicators of Partisanship**

Is the apparent power of this simple model merely due to an extraordinary level of partisan division in the California electorate? Does it work well in other states, particularly in states in which party registration figures aggregated at the appropriate levels are not readily available? Which, if any, proxies are best to use in lieu of registration?

To answer these questions, I applied the same basic model as above to similar data for North Carolina congressional contests from 1980 to 1992.<sup>16</sup> Registration alone predicts, on average, three-fourths of the 11 or 12 contests correctly; when we add incumbency, we increase the accuracy to seven-eighths—a very respectable level in a state with relatively few seats, two of them, the fifth and the eleventh districts, quite marginal in the 1980s.

In Texas, which does not compile party registration figures, returns from down-ticket races from 1988 to 1992 predicted 1992 congressional contests quite well, and gubernatorial and senatorial returns were also good predictors. Texas insiders consider the statewide partisan elections for the Court of Criminal Appeals good measures of baseline



partisanship. If one regresses the 1988, 1990, and 1992 Democratic percentages for these contests separately on the 1992 Democratic congressional returns, one can predict the winners in 91.1% of them correctly. Returns from the 1988 Senate and 1990 governor's races, similarly regressed on the 1992 congressional returns, produce accurate estimates of the victors in 88.4% of the cases.

While the State of California does not publish returns for lesser statewide offices aggregated by legislative or congressional districts, it does provide totals for Senate and governor and ballot propositions at those levels. Regressions based on returns for senatorial and gubernatorial races do almost as well at predicting Assembly and congressional returns as those computed from Assembly and congressional returns themselves, but supporters and opponents of prominent ballot propositions have not divided along party lines nearly so reliably. In particular, regressions involving the races for governor in 1978, 1982, and 1990 and for U.S. Senator in 1982 correctly predict 88.8% of the Assembly and congressional contests for these three years, compared to 91.8% for the self-regressions and 89.1% for the lagged predictions. In contrast, predictions based on the 1978 property tax limitation initiative (Prop. 13), 1982 handgun control initiative (Prop. 15), and 1990 legislative term-limits initiative (Prop. 140) are generally less accurate predictors of congressional and Assembly election results, averaging only 76.5% correct. If one is forced to rely on returns from other contests to make estimates of the partisan consequences of a redistricting, then, one should first choose minor statewide offices, then major statewide offices, and finally ballot propositions. On this evidence, at least, regressions based on the offices, minor or major, will provide reliable predictions.<sup>17</sup>

#### **The Nonpartisan 1992 Plan and the Burton Gerrymander**

Now that we have validated this simple technique, we can illustrate its usefulness by estimating interesting counterfactuals and projections that bear on the intents and effects of various districting plans. Suppose that the 1992 congressional election in California had not been run under the plan adopted by the state court-appointed Special Masters, but under the plan Democrats most strongly preferred, and with the party registration percentages that were in effect when the final choices between plans were being made in November 1991. To project these results, one merely multiplies the percentages in each district under the Democratic plan by the parameters in the regression equations (from Table 1) that are based on the actual 1992 results.

In 1992, a Democratic candidate for Congress in an average district in California won 57.1% of the two-party vote, and the Masters' plan rewarded the party with 30 of the 52 seats, or 57.7%—a very small bonus for a single-member district plan.<sup>18</sup> Under the Democrats' favorite plan, there would have been 33 Democratic victories (63.5%), while under the Republican proposal, Democrats would have received but 24 seats (46.2%)—that is, a Democratic landslide would have been transformed into a substantial Republican victory through the magic of line drawing.<sup>19</sup>

Another sort of hypothetical prediction that can be calculated from the OLS results can be applied to plans even before any elections have been held under any of them. This is particularly important because Justice Byron White's plurality opinion in *Davis v. Bandemer* specifically sanctions the use of "projected election results" to determine whether an "electoral system is arranged in a manner that will consistently degrade a voter's or a group of voters' influence on the political process as a whole," a determination that, four members of the court held, is necessary to a finding of unconstitutionality (*Davis v. Bandemer* 1986, 2810, 2814, n. 17). This method provides a readily computable means of making such projections and one that has been extensively validated on real data.

Suppose that the redistricters combined parameters for the immediate pre-reapportionment election (or, in principle, for any other election) with the party registration figures under their plans to project results. What would they find if they did so for the 1990 California election, multiplying the relevant regression parameters by the party registration percentages in each district for their preferred plan? Democrats won 55.7% of the two-party vote in the average congressional district in California in 1990 and received 57.7% of the seats, a modest winner's bonus, under the plan in effect during the 1980s, the so-called "Phil Burton gerrymander." Under the 1991 Democratic plan, they would have won 61.5% of the seats; under the Masters' plan, 50%; under the Republican plan, 48.1%. That is to say, an objective observer who relied on the patterns of voter behavior in the election preceding reapportionment would have expected Democratic candidates to fall significantly short of proportional representation if they competed in districts drawn by the Masters or Republicans, but to gain more seats than their share of votes under the Democratic plan.

Another interesting comparison is between the districts drawn by another group of Special Masters in 1973 in California and those of the now legendary Burton gerrymander, which a Republican lawyer once denounced as "the most egregious partisan gerrymander, not only of

this decade but any other decade as well.”<sup>20</sup> In the 1980 election, which was conducted under the Masters’ plan, Democratic candidates received 50.1% of the two-party vote in the average district and won 22 of 43 congressional seats (51.1%). In the 1982 elections, they received 53.6% of the votes and 62.2% of the seats. If in 1980 the boundaries had remained the same, but the regression relationships between party registration and voting had been those of 1982, Democrats would have won 27 of 43 seats, which works out to be exactly the same percentage of seats (62.2%) that they actually received under the Burton plan in 1982. If the court-drawn boundaries in effect in 1980 are taken as a criterion of partisan fairness, then by this measure there was no partisan bias in the Burton plan. The trends in 1982, a year of Republican recession, were simply more favorable to the Democrats than trends in 1980, a year of Democratic stagflation. In the opposite case, in which the behavior is that of 1980 and the lines are those of 1982, Democrats would be estimated to win 26 of 45 seats (57.8%), instead of the 28 (62.2%) they actually did win. Putting both hypothetical situations together suggests that in a bad year for the Democrats, such as 1980, the party could expect to gain two more seats under the Burton plan than under the previous Masters’ plan. In a good year for the Democrats, such as that of the “Reagan recession” of 1982, the party could expect to do equally well under either plan. The Burton partisan gerrymander was largely a fiction.<sup>21</sup>

#### **Using Recent History to Assess the Fairness of Redistricting Plans**

Two other types of hypotheticals illustrate the range of probable outcomes if voters shifted their registration or their degree of partisan loyalty uniformly across the state—changes like those that must be anticipated by redistricters, although they would not expect them to be so geographically uncomplicated. Judges, journalists, political scientists, and other observers might use the results of these simulations to assess various facets of the fairness or other characteristics of different plans: Do different plans treat reasonably foreseeable pro-Democratic or pro-Republican shifts symmetrically? How do the plans compare in the number of seats that are expected to switch parties when voters’ loyalties vary as much as they did over the previous decade? While it is possible that simulations based on different guesses about partisan trends may yield slightly different judgments about the comparative fairness of different plans, they may at least allow us to eliminate particularly unfair plans.

TABLE 2  
 Projected Number of Congressional Seats  
 that Would Be Won by Democrats in California  
 if Registration or Crossover Behavior Shifted

Plan as of November 1991	None	+2%D	+6%D	+2%R	+6%R
<i>Panel A: Registration Shifts</i>					
Democratic	33	35	35	32	29
Masters	28	29	32	27	24
Republican	24	27	33	24	24
<i>Panel B: Parameter or Crossover Shifts</i>					
Democratic	33	34	35	32	31
Masters	28	28	30	27	25
Republican	24	27	32	24	24

Between February and November 1992, the difference between the percentage of registered Democrats and Republicans in the average district in California increased by 2.6% in a Democratic direction. From 1972 to 1976, the same margin rose by 6.5%; whereas from 1982 to 1990, it dropped by 6.8%. This suggests that redistricters might want to allow for registration swings of approximately 2% to 6% over the decade-long natural life of a reapportionment plan.

The calculations outlined above, which are based on registration patterns noted when the plans were being compared to each other publicly between November 1991 and late January 1992, project Democratic seat totals of 24 under the Republican plan, 28 under the Masters' plan, and 33 under the most Democratic plan. Starting from this baseline, assume that every district became 1% more Democratic and 1% less Republican by November 1992—a shift that, for instance, would change the registration in a 53% Democratic, 39% Republican district to 54% and 38%. Then, as Table 2, Panel A shows, Democrats would win 27, 29, and 35 seats, respectively, under the Republican, Masters', and Democratic designs. If the shift went the other way, increasing net Republican registration by 2% in each district, Democrats would win 24, 27, and 32 seats under the three plans.

Alternatively, starting from the same baseline, suppose that the party registration in 1992 was the same as it was in November 1991, but that the relevant regression coefficients changed by a net of 2%, first in a Democratic direction, then in a Republican. Democrats would then win 27, 28, and 34 seats under the Republican, Masters', and

Democratic plans if the changes were in their favor, but only 24, 27, and 32 seats if the changes were against them. If the changes in either registration or crossover were 6% instead of 2%, the shifts in seats according to the partisan plans would probably be distorted mirror images of each other. Had their plan been adopted, Democrats would have stood to lose 2–4 seats if the electorate shifted sharply towards the Republicans. However, had the Republican plan taken effect and had there been substantial Democratic trends in the electorate, Republicans would have been likely to lose 8–9 seats, because Republicans sacrificed more safety than the Democrats did, apparently in order to maximize their number of victories if registration or voting patterns stayed roughly constant. Although the authors of nonpartisan plans often claim to foster competitiveness, the Masters' plan actually created no more marginal seats than the Democratic plan would have and only about half as many as the Republican plan, if a change in patterns similar in magnitude to that in each of the two previous decades were to occur in the 1990s. In the simulations as well as the point estimates, the effects of the 1992 Masters' Plan are considerably closer to those of the Republicans than to those of the Democrats.

#### **A Graphic Method for Comparing Redistricting Plans**

The close relationship between partisan registration and electoral outcomes suggests a graphic means of comparing plans that demonstrates their patterns of packing opposing partisans into a small number of districts and stacking their opponents in districts just below an expected threshold of victory—the classic stratagems of redistricting. For each plan, subtract the Republican from the Democratic percentage of registration in each district, and then rank order the districts (independently for each plan) from the least to the most Democratic. Displaying the margins on the vertical axis and arraying the districts, in their partisan order, on the horizontal axis, put two (or possibly more) plans on the same graph. As Figures 2 and 3 show, the comparisons can be very revealing. While the left tail of Figure 2 shows that Democrats packed a higher proportion of Republicans into heavily Republican districts, the right tail demonstrates that Republicans did the reverse to Democrats. The consequences of this packing, as well as of clever and careful line-drawing by each party, are highlighted in the middle of the graph, in the districts that had between a 0% and 20% Democratic registration margin. Republicans kept as many districts as possible below an 8% Democratic registration margin, and then jumped abruptly to

FIGURE 2  
Registration Margin, Congress  
Republican Plan vs. Democratic Plan

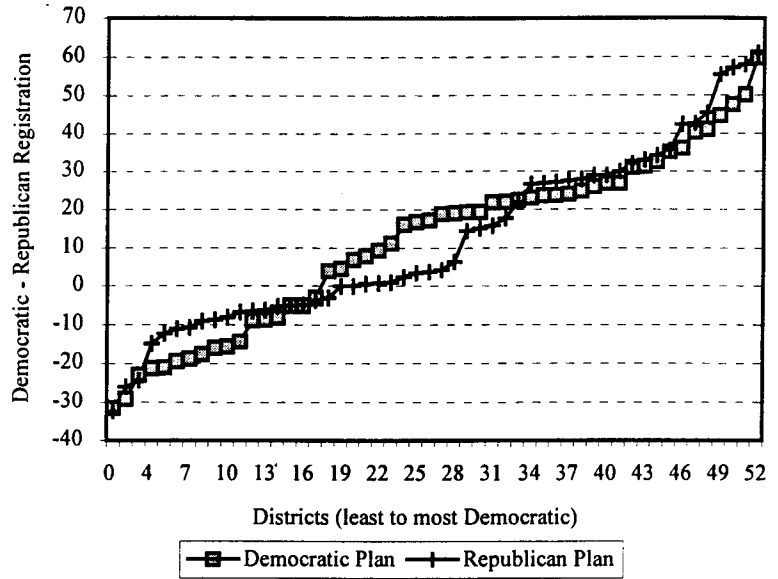
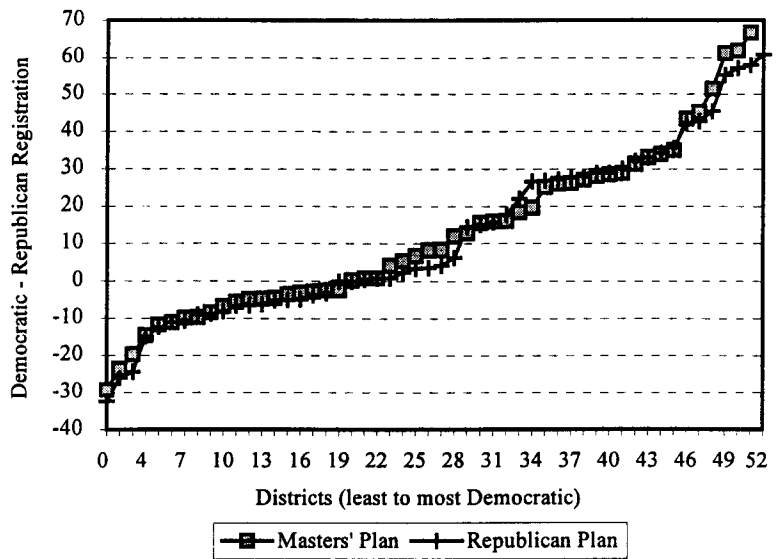


FIGURE 3  
Registration Margin, Congress  
Masters' Plan vs. Republican Plan



districts that were about 15% more Democratic than Republican. Conversely, Democrats created as many districts as possible that had a 15% Democratic margin and only two that had between an 8% Democratic margin and a slight Republican registration advantage. Neither side liked marginal districts—both seemed to agree that the definition of a marginal district was one that had a Democratic registration advantage of between about 8% and 15%—and each was sufficiently crafty that it did not need to make risky bets to gain a substantial partisan advantage. But as Table 1 demonstrates, the Republicans were somewhat more optimistic in late 1991 than the Democrats were, drawing 4–5 more districts that they apparently thought had just enough of a partisan advantage in their favor to be safe.

Figure 3 shows that the pattern of registration in the supposedly nonpartisan Masters' plan differed from that of the Republicans only in minor details. In the middle of Figure 3, the ascent of the Masters' plan is somewhat smoother than that of the Republican plan—enough to account for a 2–3 seat difference in expected outcomes under varying conditions—but the dominant impression is of the similarity between the registration patterns in the two plans. It is not surprising that Republican leaders greeted the unveiling of the Masters' plan with barely concealed glee.<sup>22</sup>

#### **Should Partisan Bias Be Defined as a Deviation from Symmetry?**

Gelman and King (1994a) are only the most thorough of those recent scholars who define partisan bias as a “deviation from partisan symmetry” over an arbitrary range of jurisdiction-wide vote percentages centering on 50% for each of the two major parties. There are three problems with this definition. First, rather than partisan bias, they may be uncovering different degrees of risk aversion and/or different proportions of incumbents (who often press successfully for very safe seats) in the major parties. Second, averaging these figures over standardized ranges may distort, as well as blur our picture of the nature of competing redistricting plans. Third, if what we are trying to capture in our notion of bias is the practical manipulation of a particular electoral structure, then we should take account of the specifics of expected behavior, not just the abstract characteristics of a generalized system. Measuring symmetry around 50% is illogical if that is not the partisan balance expected by those who struggle over redistricting.

Suppose both parties want to maximize their number of seats in a legislature that is redistricting itself, but that party R is willing to accept

a good deal more risk than party D is. Both parties will try to pack as many opposing partisans in as few districts as possible, but party R will draw more districts in which it expects to win by a bare margin than party D will. Call the percentage of core partisan support at which each party expects to win by a very small margin that party's tipping point. If there is a dramatic shift across the electorate toward party D, then party R will lose a great many seats. A corresponding shift toward party R will not, we assume, cost party D so dearly. But in more normal times party R will win more seats for a given vote than its more risk-averse opponent. If the range over which simulated results are calculated is so small that it includes the tipping point for party R but not for party D, then the Gelman-King measure may find the system biased in favor of party D.

In light of these difficulties, it seems preferable to speak of comparative, rather than absolute bias among competing plans; to distinguish risk aversion from bias by comparing the plans at several points rather than averaging, as in Gelman and King's Figure 4 (1994a, 547); and to use the recent history of shifts in party registration or exemplary elections, as well as regression parameters based on them, to project the range of likely variations over the life of a redistricting plan (as in Table 2 or Figures 2 and 3 above). In a word, bias should be measured more comparatively, concretely, and specifically. There is no such thing as bias in redistricting in general. Since gerrymandering is always specific to a particular regime of political behavior, attempts to measure it should be as well.

#### **Should Fairness Be Gauged by Statewide Vote Totals?**

The fact that Section Two of the Fourteenth Amendment to the U.S. Constitution apportions members of Congress to the states by total population, rather than by voting age population, registration, or turnout would seem to imply that states should do likewise, and courts have often so held (e.g., *Calderon v. City of Los Angeles* 1971; *DeWitt v. Wilson* 1994). Scholars should follow suit not only for constitutional, but also for normative reasons.<sup>23</sup> Turnout varies widely from district to district and is especially low among poorer ethnic voters, the core of the Democratic constituency. For instance, in 1992 only 8.4% of the population in the overwhelmingly Latino, heavily noncitizen 33d Congressional District in Los Angeles County voted in the contested general election for Congress, while at the same time 41.8% of the population in the 36th Congressional District, an affluent Anglo area, turned out.



Those who would assess the “fairness” of the distribution of seats by the statewide average, rather than the proportion averaged by districts implicitly take the position that the residents of the 36th should be counted five times as heavily as those of the 33d. Such a standard would disproportionately disadvantage poorer people and Democrats.

It might be, however, that the difference between the statewide average and the average computed by district was a function not only of differential turnout, but of how the various plans sorted people into districts. Democrats might waste as many Republican votes as possible by packing high-turnout Republican areas into as few districts as possible, thereby creating more low-income, low-turnout districts that Democrats could carry. But in fact, if we use parameters from the 1990 and 1992 regressions to simulate results under eight different proposed plans from both parties and outside groups and then calculate the means of the district vote percentages under each plan, there is almost no variation between the resulting averages.<sup>24</sup>

### **Do Shifts in Sub-Districts Prove that Redistricting Makes No Difference?**

Rush (1993) criticizes seats/votes ratios and other measures of the effects of reapportionment because year-to-year shifts in voting behavior in Massachusetts and Connecticut towns are not uniform, and because ratios of changes in seats/votes ratios measured at the state level are not always the same from one election to the next. Both criticisms concentrate on the wrong level of aggregation. The first is too low, overemphasizing idiosyncratic factors within state legislative or congressional districts that are rarely large enough to change election outcomes. Small shifts one way or the other may lower  $R^2$ s, but not push an otherwise losing candidate over the threshold of a plurality *of a district*, which is the much more relevant statistic for actual politics. The second is too high, for, as explained above, seats are allocated by population, not votes. Furthermore, differences in the responsiveness of seats to votes at different levels of vote percentages are evidence of partisan bias and differences in redistricters’ risk aversion. They are evidence that redistricting *does* make a difference, rather than the contrary.

### **Conclusion**

By emphasizing the predictability of election outcomes, I do not mean to imply that there is no art involved in redistricting or campaigning. Clever drawing of lines can certainly affect which candidates run and

win, and the more unconstrained the designer of the boundaries is, the more leeway there is to affect the partisan balance. Hardworking, attractive, well-spoken, well-funded candidates can sometimes prevail in spite of poor odds, while lazy, poor, inarticulate, or scandal-plagued candidates or aspirants whose views are too far from those of their constituents can, from time to time, overcome their party's natural advantages. But in the Darwinian world of politics, parties will eventually nominate fitter candidates, and the genius of reapportionment lies in rearranging people of known political proclivities. While it is true that the party registration equations err about 10% of the time, it seems improper to lay too much emphasis on the uncertainty of political predictions about election outcomes.

Using the simple methods outlined in this paper, anyone can compare the partisan effects of different systems of districting. If the most important aspect of reapportionment is who wins and who loses under alternative plans, not whether the districts conform to some geographer's mathematical model of compactness or whether the process by which they are drawn is formally partisan or nonpartisan, the validation of techniques for projecting partisan biases may help restore a proper focus to scholarly and popular evaluations of redistricting.

No doubt candidates and campaigns affect voters' decisions. If they did not, democracy would be impossible because voters would be immovable. But democracy would also be impossible, or rather, meaningless, if elites could manipulate voters at will, changing their behavior radically by slightly altering the stimuli to masses who had neither interests nor stable opinions. If democracy works, redistricting can change outcomes.

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## NOTES

Micah Altman and Gary King improved this paper, but should be exempted from any responsibility for the errors and infelicities that remain.

1. For an extensive discussion of the facts of the California redistrictings from 1971 through 1991, see Kousser 1995b.

2. In contemporary California, about 13.5% of the eligibles register with the Libertarian, Peace and Freedom, or Green parties or decline to state a party registration. The percentages vary widely from district to district and over time with a standard deviation in November 1992 of 2.7% and a range from 5% to 20%. The number and strength of minority party candidates also differ considerably across space and time. To test whether it would be worth typing in much more data, rather than, in effect,

subsuming the effects of nonmajority party registrants in the intercept term, I ran regressions similar to equations 1 and 2 but explicitly included the registrants from all but one nonmajority party (to avoid multicollinearity) for three election years: two excellent years for the Democrats, 1974 and 1992, and one very good year for the Republicans, 1978. Corrected for degrees of freedom, the  $R^2$ s usually decreased or barely increased when the minor parties were added to the equations. In only one case, the 1974 Assembly, was there a marked increase (four seats) in the ability to predict the winners, in the sense described below in the text, by adding minor parties. I therefore did not explicitly add minor parties to equations 1, 2, and more complicated equations discussed in the rest of the paper.

3. Since there were few completely uncontested seats in California during this period, I included all seats in every equation. Of the 582 total congressional races, only 10 were absolutely uncontested, and only 24 lacked a Democratic and a Republican candidate. In the Assembly, the analogous numbers were 1040 total, 57 uncontested by anyone, and 82 uncontested by one major party. For a discussion on how to handle large numbers of uncontested seats, see Gelman and King 1994a, Appendix A.

4. A single-equation model using as a dependent variable the Democratic percentage of the two-party vote and as an independent variable the Democratic percentage of the two-party registration yields predictions which are almost as good, in the senses discussed in the text below, as the two-equation model.

5. For more sophisticated measures of uncertainty in related models, see Gelman and King 1994a.

6. Details of these calculations are in Kousser 1995a.

7. A court order provided that the 1972 state legislative contests would be held in the districts established in the previous decade. Thus, 1974 was the first year in revamped Assembly districts.

8. I show in Kousser 1995a that substituting logit analysis (with a dependent variable equal to one if the winner was a Democrat, and zero, otherwise) fits the California data no better than OLS does and sometimes does not converge to give any results at all. The simpler OLS is therefore preferable to logit or probit estimation for this problem.

9. For more details, see Kousser 1995a.

10. By incumbent, I refer to a candidate who was elected two years before. Thus, those occupying seats won in special by-elections are not considered incumbents. In elections immediately after reapportionments, judgment as to whether one is an incumbent is sometimes required. Although it would be preferable to have statistics on the proportion of people in a district who were represented by the incumbent in a previous legislature, such figures are not easily available.

11. That adding this term explains an additional 11–13% of the variance in the vote percentages is less important.

12. In a particularly pertinent example, Congressman Phil Burton in 1981 designed a district to help his brother John win reelection to Congress in 1982, but John instead dropped out of Congress. Contrary to the assertion of Gelman and King (1994a, 525), even ultimate insiders may not always be able to predict what incumbents will do.

13. Gelman and King (1994a, 525) suggest using party control—that is, the party of the sitting incumbent—when incumbency is unavailable. But if district lines are considerably scrambled by the redistricting process, it may not be possible or

meaningful to compute such a variable. Moreover, most demographic variables from the census will typically not be available at a fine-grained census level during the summer of years ending in 1, when redistricting plans are being formulated and assessed. Therefore, it will generally be impossible to calculate with much precision Gelman and King's error (what they refer to as  $\gamma$ ) or proportion of the total error ( $\lambda$ ) due to omitted variables and measurement problems prospectively, because too many of the values of the independent variables will be unknown. In these conditions, their model reduces to one very similar to mine (Gelman and King 1994a, 528–29).

14. I added to equations 1 and 2 eleven more variables that are plausibly related to voting—the percentages of the voting age population that were African-American and Latino, median incomes, median values of housing and rents, the percentage who graduated from college, the percentage who lived in the same house from 1975 to 1980, the percentage who moved but stayed within the county, the percentage of housing that is owner-occupied, the percentage of families below the poverty line, and the percentage urban.

15. Predictions here are for the same-year equations, not the lagged equations, but the results are so similar for both that it makes little difference.

16. Again, the predictions are for same-year elections, but lagged predictions are almost equivalent.

17. For details of the North Carolina, Texas, and additional California regressions, see Kousser 1995a.

18. This was the smallest ratio of the percentage of total seats won to the percentage of the two-party vote received in the average district in California from 1970 to 1992. On the general tendency of electoral systems to reward first-place finishers, see, for example, Rae 1967.

19. As a negotiating tactic, the Democrats actually proposed and the legislature passed three separate plans—one that they hoped courts might adopt if negotiations broke down, and the other two designed to appeal to conservative and moderate Republicans, respectively. The plan discussed in the text is the first of these, which was referred to as Plan A.

20. Philip Hager, "Judges Question GOP's Bid to Dump California Remap Plan," *Los Angeles Times*, 6 December 1986, sec. II, p. 1.

21. Burton and his ally Michael Berman did tailor several congressional seats for their friends and families, but these were all such safely Democratic seats that, after setting aside these areas and making other Democratic incumbents somewhat more comfortable, Burton and Berman had too few extra Democratic voters to shift around to affect the party balance of the state's seats very much. For much more detail on these developments, see Kousser 1995b.

22. Daniel M. Weintraub, "Remap Could Bring Major Gains for GOP," *Los Angeles Times*, 4 December 1991, sec. A, p. 1.

23. Note that Gelman and King (1994a) also compute seats/votes ratios on the basis of district level statistics.

24. For specifics, see Kousser 1995a.

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