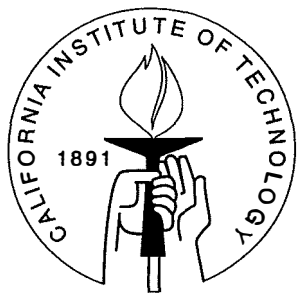
DIVISION OF THE HUMANITIES AND SOCIAL SCIENCES
CALIFORNIA INSTITUTE OF TECHNOLOGY
PASADENA, CALIFORNIA 91125

ESTIMATING THE PARTISAN CONSEQUENCES OF REDISTRICTING PLANS – SIMPLY

J. Morgan Kousser



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Abstract

Although some judges and political scientists have recently doubted 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 1992, and compare it to logit results and to more complicated equations that contain incumbency and socioeconomic variables. Since information on socioeconomic variables is often not available early in a redistricting cycle, and since incumbency in a district is often difficult to determine precisely after a reapportionment, I rely on the simplest equation, which correctly predicts 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 predict outcomes with considerable accuracy.

Using the party registration 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, which would have insured the GOP a majority of the Congressional seats even if Democrats won a landslide of the votes. I conclude by introducing a new graphical representation of redistricting plans, which strongly implies that in 1991, Republican and Democratic line drawers in California agreed on the registration margins necessary for party control and drew their plans with these in mind.

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 Indiana case of *Davis* v. *Bandemer* in 1986: "[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) Finally, legal scholars Daniel Polsby and Robert Popper assert that only a rigid adherence to a test for the geographic compactness of electoral districts can combat partisan gerrymandering, and that the actual partisan consequences of imposing such a test do not matter. Exalting procedures over substance, they view compactness as by definition a politically neutral criterion that should be employed to evaluate competing redistricting plans at least partly because of the difficulty of making substantive decisions about the effects of those plans. (Polsby and Popper 1991, 336)

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, because a simple, unequivocally politically neutral test that uses widely available data is quite reliable. As ever more powerful personal computers and off-the-shelf redistricting software have become available, the number of plans proposed has multiplied and will no doubt expand further in the millennial reapportionments. Consequently, it is more and more important for participants and outside observers to be able to compare the partisan effects of suggested schemes. Unlike other measures of partisan bias (Grofman, 1983; Niemi, 1985; King and Browning, 1987; King, 1989; Gelman and King, 1990), 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 and into just how those intentions were put into effect. The test is not only more intuitively meaningful and easier to explain to judges and the public than sophisticated variants of seats-votes ratios (Gelman and King, 1990, 1994a, and 1994b), but the simulations that it suggests are more clearly tied to the specific electoral history in a jurisdiction than are those in more general, abstract schemes. In particular, I suggest ways to project quite easily the consequences of two kinds of possible shifts in voter behavior under proposed plans and to compare plans from one decade to the next. To determine how reliable the test is, I analyze registration and election data from California Congressional and State Assembly districts from 1970 to 1994, as well as

unpublished data from various plans proposed in 1991.[1] Finally, I introduce a new, revealing, and easy method of graphical 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 "It 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)

Ninety percent 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,[2]
%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.

The estimates of the parameters for these equations are given in Table 1 -- Panels A and C for the Democrats and Panels B and D for the Republicans.[3]

(Table 1 about here)

The equations fit the data rather well. For one thing, the R^2s are fairly high for social science, indicating that party registration generally explains about two-thirds of the variance in the vote, and that the trend in the 1970s toward crossover voting seemed to have reversed in the 1980s.[4] For another thing, suppose we apply

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

[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. It is also unclear whether a higher registration of Libertarians can be expected to help or hurt Republican candidates, and vice-versa for the other two parties and the Democrats. Because of this theoretical indeterminacy, the effects of all non-major registration are subsumed in the intercept term.

[3]Since we are trying to predict seats won, the proper unit here is the congressional district. Consequently, the regressions are not weighted. Since in California, but not everywhere, the major parties generally contest nearly every seat, I have made no correction for uncontested seats. For a discussion of how to handle large numbers of uncontested seats, see Gelman and King, 1994a, Appendix A.

[4]Signs on the Republican coefficients for the Assembly are statistically significant for every year except 1978 and on the Democratic coefficients for the Assembly, are statistically significant *only* in 1974. The changes in the signs of the insignificant Democratic coefficients have no substantive importance. The patterns are somewhat more complex in the congressional equations, but the same basic generalizations apply: Coefficients for the Republican registration are significant and in the same directions from 1978 on, with the exception of the quiet 1990 election, while coefficients for the Democratic

equations (1) and (2) at the district level, so that the actual party registration percentages in each district for each year are inserted in order to calculate expected vote percentages for the Democrats and Republicans in each district. That is, for each district in a particular year, we multiply the actual Democratic and Republican percentages in the district by the B's from the equations for that year for each legislative body. For instance, if a 1970 Assembly district were 55% Democratic and 39% Republican, its predicted Democratic vote would be

2.55 + (55 * -0.015) + (39 * -0.030) = 55.5%,

its predicted Republican vote would be

-1.80 + (55 * 0.017) + (39 * 0.033) = 42.2%,

and we would expect the Democratic candidate to win. In an actual contest in 1970 in the 16th Assembly district, which had exactly these registration percentages, Democrat Ken Meade of Berkeley beat Republican Caucus Chairman Don Mulford by 58.5% to 41.5%, which is quite close to the prediction and calls the winner correctly.

Table 2 compares the actual winners to those predicted by equations 1) and 2). Although the idiosyncrasies of particular contests throw the predictions slightly off each year, the overall predictions are quite accurate. For instance, in the 1970 Assembly contests in Panel A of Table 2, the party regression equations predict that the Democrats would win 47 seats, but they actually won only 43 - losing six where party registration alone should have made them winners, but carrying two where they enjoyed a smaller margin in registration than they generally needed to carry an Assembly district in 1970. 72 of the 80 contests, or 90%, are correctly predicted in this instance. 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.

(Table 2 about here)

Of the factors that account for the other 10% of the results and the other third of the variance in vote percentages, unquestionably the most important is incumbency. Politicians and newspaper writers agree with political scientists that incumbency is potent, and both formal and informal estimates of the effect of redistricting often take account of incumbency. (Cain 1985; Gelman and King 1994a, and 1994b) Why does incumbency appear so powerful, and how much better can we predict results if we take it into account?

Incumbents are often reelected for many reasons. Compared to most challengers, incumbents are better known and further build up reputations and obligations through constituency service; incumbents are also more experienced in campaigning, more familiar with their districts, can raise funds more easily, and, as Table 3 shows, occupy inherently safer seats. In thirteen years that span five different redistricting arrangements, the margin of Democratic registration over Republican registration in the district of the Democratic incumbents averaged 30.8% in Congress and 31.8% in the Assembly. By contrast, Republican

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regression are never significant and vary in no discernible pattern. The important thing about the equations is that they capture shifts in both party registration and defection rates from election to election quite well.

Assembly incumbents occupied seats in which Republican registrants, on average, equaled Democrats, 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. 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 of least use predictively, moreover, during an election year just after a redistricting, because that is when there are the most open seats. Table 3 validates that common observation for California. In the four 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%. The analogous figures for the Assembly are 25.3% and 14.9%, respectively. Models that include incumbency will not generally fit post-redistricting elections or alternative redistricting plans as well as they do pre-redistricting contests, and using pre-redistricting elections to estimate the characteristics of the error structures of such models, as Gelman and King, 1994a, do, will probably be misleading for prediction purposes.

(Table 3 about here)

To test for the added effect of incumbency, we merely add to equations 1) and 2) another term I, where
I = 1 if the incumbent is a Democrat,[5]
= 0 if the seat is open, and
= -1 if the incumbent is a Republican.

Table 4 shows that appending such a term to the equations predicting the Democratic vote explains an additional 11-13% of the variance in that vote and increases the proportion of winners predicted correctly from 89-92% without the term to 93-96% with it. 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 which incumbents will run for particular seats in plans that have not yet been put into place or may never be

[5]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.

adopted.[6] Table 4 makes us more confident that we can go ahead and project winners even before we know who is running.[7]

(Table 4 about here)

Some political scientists have suggested using logit analysis instead of OLS to make such estimates because some elections are uncontested or nearly unanimous or because small changes in reelection probabilities are more important when those probabilities are close to 50-50 than when they are much more lopsided. (Gelman and King 1990, 297; Glazer *et al.* 1987, 692) Table 5 compares the percentages of winners predicted correctly by OLS (taken from Table 2) with those predicted by a logit equation estimated by an iterative procedure in which the dependent variable is the logit of 1 if a Democrat carried the district and 0 if a Republican won, and the independent variables are the same registration percentages as in the OLS estimation. In the case of California, at least, the more complicated logit technique predicts winners no better than OLS does, the procedure does not converge in four instances, and the estimates of the probability of victory that can be derived by a transformation of the logit results seem less intuitively meaningful than the estimates of the victory margins obtainable from OLS.[8] Moreover, visual inspections of the residuals from the OLS equations do not suggest a regular nonlinear pattern, and there are few completely uncontested elections in the state. Finally, plots of the probabilities derived from the logit results for the California data for selected elections seem much too steep in the center of the graph. For instance, in the 1992 election for Congress, a shift in the Republican registration percentage from 36% to 36.7% is associated with a decline in the probability of a Democratic victory from 92% to 62%, according to the logit estimates. If the Republican registration rises to 41.7%, the probability of a Democratic win then drops to 30%, and if Republican registration nudges up to 42.5%, the Democratic prospect is almost hopeless --4%. Perusal of a series of such graphs for successive election years suggests that registration and loyalty shifts over a reapportionment

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[6]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.

[7]Gelman and King, 1994a, 525, suggest using "party control" -- i.e., 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 during a redistricting. 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.

[8]In the 1982 through 1988 Congressional elections, registration was such an accurate predictor of voting patterns that the probabilities of Democratic or Republican victories in safe seats were packed near one or zero. In such cases, logit estimates do not always converge. In other words, the better OLS is at predicting, the less useful or necessary logit is.

cycle can cause very exaggerated changes in probability curves. I therefore used OLS, rather than logit or probit, as an estimation procedure.

(Table 5 about here)

As a final indication of how well party registration predicts the vote, consider how much better we or someone who was drawing district lines could do at guessing the results of particular plans if we had a great deal more socioeconomic data available, in addition to party registration. (Although politicians and consultants often have an extremely good "feel" for the socioeconomic composition of various areas, much of the relevant decadal census data only becomes available long after redistricting must occur.) Consider, for instance, the congressional elections of 1984. If we add to equations (1) and (2) eleven more variables that are plausibly related to voting - ethnic percentages, 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 of housing that is owner-occupied, and the percentage urban - the increase in the percentage of variance explained, corrected for degrees of freedom, is only four percent for the Republicans and five percent for the Democrats.[9] Similar calculations for the 1976 and 1980 elections yield less than one percent in additional percentages of variance explained, controlling for the additional degrees of freedom.

If we regress vote percentages on all of these socioeconomic variables, 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 1984 than we do if we use only registration in our prediction equation. 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 eleven attributes of socioeconomic status for each district. The conclusion is that on the level of congressional districts, partisan registration is a good shorthand for a set of socioeconomic and attitudinal variables that produce outcomes.

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 and particularly in ones 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?

The answers to these questions provided by Tables 6, 7, and 8 are comforting. Table 6 applies the same simple models used to produce Tables 1 and 4, above, to similar data for North Carolina congressional contests from 1980 to 1992. Registration alone predicts, on average, three-fourths of the eleven or twelve contests correctly; by adding 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.

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[9]Incumbency is not included in this equation.

(Table 6 about here.)

Table 7 shows that "down-ticket" races in Texas, which does not compile party registration figures, predicted 1992 congressional contests quite well, and that 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. Table 7 demonstrates that 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 90% or more of them correctly. Returns from the 1988 Senate and 1990 Governor's races, similarly regressed on the 1992 congressional returns, also produce accurate estimates of the victors.

(Table 7 about here.)

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. Table 8 shows that returns for Senate and Governor do almost as well at predicting Assembly and congressional returns as the Assembly and congressional returns predict themselves, but that 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 from 2% to 9% fewer of the Assembly and congressional contests than self-regressions, but predictions based on the 1978 property tax limitation, 1982 handgun control, and 1990 legislative term limits initiatives are generally less good predictors. 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. And on this evidence, at least, regressions based on the offices, minor or major, will provide reliable predictions.

(Table 8 about here.)

Now that we have validated this simple technique, we can estimate 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 that Democrats most strongly preferred, and with the party registration percentages that were in effect at the time the final choices between plans were being made, those of November, 1991. To project these results, one merely multiplies the percentages in each district under the Democratic plan by the parameters in the last rows of Panels C and D of Table 1, which are based on the actual 1992 results.

In 1992, a Democratic candidate for Congress in an average district[10] 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.[11] 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.[12] The difference between the hypothetical outcomes under the preferred plans of the two parties, nine seats, was only one less than the net national swing in Congressional seats in the 1992 election. And some political scientists doubt that redistricting usually makes a difference![13]

Another sort of hypothetical 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 parameters from the penultimate rows of Panels C and D of Table 1 by the party registration percentages in each district for their preferred plan? Democrats won 55.7% of the two-party vote in an 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%.[14] That is to say, an

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[10]See Appendix B for a discussion of using the average district percentage, rather than the statewide proportion of votes win by each party, as a criterion.

[11]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, e.g., Rae 1967.

[12]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."

[13]For a similar view of the political efficacy of redistricting, see Squire, 1995.

[14]This calculation uses the registration proportions as of November, 1991 for the Masters' plan, as well as the other two plans, while the actual outcomes described in the previous paragraph used the November, 1992 registration percentages for the Masters' plan. During the year 1992, California Democrats launched a surprisingly effective registration drive, especially

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." (Hager, 1986.)[15] In the 1980 election, which was conducted under the Masters' plan, Democratic candidates received 50.1% of the 2-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 the regression relationships between party registration and voting had been those of 1982, but the boundaries had been those of 1980, Democrats would have won 27 of 43 seats, which works out to be exactly the same percentage of seats (62.2%) as they actually received under the Burton plan in 1982. If the court-drawn boundaries that were 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 those of 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%) that they actually won. Putting both hypotheticals 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.[16]

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

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targeting their efforts at marginal districts. This marked the first presidential election year in 16 years in which Democrats had surpassed Republicans in signing up new voters in the state. At the time that the State Supreme Court accepted the Masters' plan, therefore, the expectation based on past experience would have been that the state would have become more Republican, not more Democratic before election day, skewing the seats-votes ratio further.

The higher number of hypothetical Democratic victories under the Republican plan using the behavioral equations based on the 1990 election, rather than the 1992 election is an anomaly, rather than a misprint. One district that was 48.8% Democratic and 42.5% Republican in registration in the Republican plan is projected to produce a 1.5% Democratic margin if voters had behaved as in 1990, but a 0.7% Republican margin if voters had behaved as they did in 1992.

[15]For a more tempered scholarly view, see Robertson, 1983.

[16]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, 1995.

anticipated by redistricters, although they would not expect them to be so geographically uncomplicated.[17] 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 party 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, the exercise will at the least deepen our understanding of each plan's effects. It may allow us to eliminate particularly unfair plans or even to choose one or more as unambiguously superior according to explicit criteria.

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.

Calculations outlined above based on registration patterns at the time 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 one percent more Democratic and one percent less Republican by November, 1992 -- a shift that, for instance, would raise the registration in a 53% Democratic, 39% Republican district to 54% to 38%. Then, as Table 9, 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.

(Table 9 about here)

Alternatively, starting from the same baseline, suppose that the party registration stayed the same in 1992 as it had been in November, 1991, but that the parameters in the last rows of Panels C and D of Table 1 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 change were in their favor, but only 24, 27, and 32 seats if the change 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 mirror images of each other. If their plan had been adopted, Democrats would have stood to lose 2-4 seats in case the electorate shifted sharply towards the Republicans. 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

[17]If one had special knowledge, as redistricters often do, about differential party growth rates expected in particular districts, this knowledge could be incorporated into the projections merely by projecting partisan rates of registration to increase or decrease by different rates in different districts in the calculations of hypothetical outcomes.

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 Democrats did and only about half as many as the Republicans, if a change in patterns similar in magnitude to that in each of the two previous decades were to occur in the 1990s. Under any plan considered by the legislature, a Governor's Commission, and the courts in California in 1992, it would take a very sizable electoral earthquake to shake loose a fifth of the Congressional seats from the party that would control each district. 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.[18]

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. It also allows one to determine whether opposing redistricters appear to agree on the partisan margin necessary to elect candidates of each party.

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 1 and 2 show, the comparisons can be very revealing. While the left tail of Figure 1 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 about a 0% and 20% Democratic registration margin. Republicans kept as many districts as possible below about an 8% Democratic registration margin, and then jumped abruptly to 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. Obviously, neither side liked marginal districts and 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%. Neither side was so risk-accepting as to include in their plans many districts that they could expect to win barely, and each side was sufficiently crafty that it did not need to make risky bets to gain a substantial partisan advantage. But as Table 6 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 1 about here)

Figure 2 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 2, the ascent of the Masters' plan is

[18]Some commentators (Weber, 1995), without presenting any evidence, assert that the Masters' Plan was "politically neutral."

somewhat smoother than that of the Republicans -- 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. (Weintraub, 1991).

(Figure 2 about here)

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 she has 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, which are measured quite accurately in the state by party registration percentages. While it is true that the party registration equations err about 10% of the time, such a rate would make a bettor on horses or stocks extremely wealthy. It therefore 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 confidently 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.

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APPENDIX A: THE NOTION AND MEASUREMENT OF PARTISAN BIAS

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 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 numbers 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 barely 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.

An illustration will demonstrate the point. Suppose party R creates several districts that it expects to win barely if its overall statewide average is at least 48%, while party D creates several districts that it will probably win unless its statewide average falls below 44%. Suppose that an outside analyst defines the relevant range of outcomes as from 45% to 55% for each party. Then she will discover that at 45-47% R, party R loses a large number of seats, while at 45-47% D, party D gets about the same number of seats that it gets at 48-50% D. Thus, the system seems asymmetric *in favor of party D*, while in fact, in normal times, it is *party R* that has manipulated the system in its favor. Moreover, extending the range to include party D's tipping point is self-defeating, because there is no natural general stopping point for the range, and eventually, any system may be judged unbiased, because every district will shift party control at some level of core support. It might be possible to attach weights to each shift proportional to the probabilities that a change in core support of that magnitude was likely to occur and then average over the range from zero to 100%, but it is difficult to see immediately how to calculate such weights.

At least without term limits, this is a quite realistic situation. If incumbents can strongly influence the redistricting of their own seats, they are likely to demand high levels of safety for themselves, and to get their wishes, as Table 3 above shows. The party that has more incumbents is therefore likely to be induced to be more risk averse. In recent times, this has been the Democrats in most states. Thus, incumbents' effects on redistricting may partly account for the Gelman and King finding of a shift to a pro-Democratic bias in the

redistricting of non-southern congressional seats after 1960. (Gelman and King 1994, Figure 1) But what they may actually be measuring is greater incumbent-induced Democratic risk aversion.

Even if both parties have the same degree of risk aversion, setting an arbitrary range for every state and every legislature may distort the results (especially if the legislature knows that its handiwork will be judged on that range). Suppose both parties expect party D to get 52% of the vote in an average district in a typical election and that both parties decide to cluster their marginal districts at about 4% from the expected mean. Then any counterfactual model would find that at less than 49% D, party R would be expected to win a large number of seats, and it would conclude that the system was biased in favor of party R, or, more sophisticatedly, that either the system was biased or party R was less risk averse than party D. But, in fact, the example is constructed so that neither is the case.

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; 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 9 or Figures 1 and 2, 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.

APPENDIX B: SHOULD SEATS BE COMPARED TO THE STATEWIDE AVERAGE OF VOTES OR TO VOTES IN THE AVERAGE DISTRICT?

The fact that Section Two of the Fourteenth Amendment to the U.S. Constitution apportions members of Congress to the states by total population, not 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.) Scholars should follow suit not only for constitutional, but also for normative reasons.[19] 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 33rd 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 33rd. 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. Of course, it is unlikely that this was the case in California in 1992, since a Republican-dominated court-designed plan was put into effect. But in fact, nearly every plan proposed for the Assembly or Congress would could have been expected to produce about the same outcome in the average district.

Table B-1 was computed using equations for 1990 and 1992 from Table 1. To obtain the first entry in the first row in Panel A, for instance, the parameters from the equations for the Democratic and Republican percentages for Congress in 1990 (from Table 1, Panels C and D) were multiplied by the relevant party registration figures in each district in "Plan A." The resulting estimated outcomes were then averaged, and the proportions for other parties were eliminated. The result, 53.5%, should be interpreted as giving the estimated Democratic percentage of the two-party vote for Congress in the average district if the electorate behaved as it did in 1990, and if the districts were configured as in Plan A, the most Democratic plan. As the table shows, if the configuration was that of the "Jones" (Republican) plan or the Masters' Plan,[20] the average

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[19]Note that Gelman and King, 1994a, also compute seats/votes ratios on the basis of district level statistics.

[20]To make the plans comparable, I have used registration figures from November, 1991 for all of them, including the Masters' plan. A large and unexpected registration shift toward the Democrats over the next year produced a much more pro-Democratic final outcome in November, 1992. This accounts for the discrepancy between the estimated and the actual district averages under the Masters' plan. I calculated estimates based on equations for two different years because the pro-Democratic shifts in the relationships between party registration and voting from 1990 to 1992 might have interacted with details of districting plans to distort the comparative estimated district averages. That proved not to be the case.

district percentage would have been almost exactly the same. In fact, all eight of the plans on which information is available yield virtually identical estimates for the 52 congressional districts and the 80 Assembly districts, although the more fine-grained Assembly districts increasingly approach the overall statewide average. Only the plan of Governor Pete Wilson's informal Commission, which was so insensitive to minority concerns that it had to be amended into the "Shumate Plan" before it was made public, produces a markedly different district average for Congress. All of the estimated Assembly averages are within four-tenths of one percent of each other. The conclusion is that it was the constraints of the equal population cases and the Voting Rights Act, not biases in proposed district configurations, that accounted for any likely deviations between the statewide and district-level averages. Using the district average as a measure of fairness imparts only legally-required distortions.

Table B-1: Estimated District Mean Percentages of the Two-Party Vote for Democratic Candidates in California, 1992, Under Eight Proposed Plans

Plan	% Congress	% Assembly
Panel A: Estimated from 1990 Party Equations		
Democratic Plans		
Plan A	53.5	56.5
Plan B	53.4	56.7
Plan C	53.5	56.6
Jones (Republican)	53.7	56.7
Governor's Commission	53.2	56.3
Shumate (revision of Governor's Commission)	53.6	56.5
MALDEF 2 (revision of technically flawed plan)	53.7	56.7
Masters (based on Nov., 1991 registration)	53.6	56.4
Panel B: Estimated from 1992 Party Equations		
Plan A	54.5	54.3
Plan B	54.4	54.5
Plan C	54.5	54.5
Jones	54.7	54.5
Commission	52.5	54.1
Shumate	54.6	54.3
MALDEF	54.7	54.3
Masters	54.6	54.2

Source: Computed from data supplied by Pactech Data Research.

APPENDIX C: DO VARIATIONS WITHIN DISTRICTS MAKE REDISTRICTING UNPREDICTABLE?

Rush's attack (Rush 1993) on the predictability of electoral outcomes has both conceptual and methodological problems, and data on California casts doubt on it.[21] Rush 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^2s, 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 in Appendix B, 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, as discussed in Appendix A. They are evidence that redistricting *does* make a difference, rather than the contrary.

There are also three possible flaws in Rush's methods.[22] First, his R^2s may be lowered by his inclusion of consecutive elections that are not always contested. He does not always explain how he treats such pairs. Excluding them is likely to delete units that vote overwhelmingly in one direction, while including them would also reduce the correlation because a shift from, say, a 70-30 pattern in one election to a 100-0 vote in the next leaves a good deal of (rather meaningless) variation unaccounted for. Second, his exclusion of some city wards -- generally the most consistently Democratic units in the state -- in Connecticut because of boundary changes may also reduce his correlations. Third, it may be that correlations of elections for Congress and the state senates for presidential years only and non-presidential years only would be higher than between on-years and off-years, which are the only ones that he prints. (Gubernatorial contests, which he does examine in four-year cycles, are much more dependent on the personalities and issue positions of particular candidates, and these varied widely, particularly in Massachusetts in the 1970s and 80s.)

California data aggregated at the most relevant level -- i.e., the district -- casts doubt on Rush's contention that the effects of redistricting are unpredictable because voters often "change their partisanship when

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[21] Since no election returns for identifiable localities within state legislative or congressional districts in California are published by the Secretary of State's office, and since localities are regularly split by district lines, often in different ways from one redistricting to the next, it is very difficult to test Rush's hypothesis on data exactly analogous to his. In any case, it is the predictability of *districts*, not of smaller areas, that is most important in assessing whether redistricting makes a difference.

[22] Since Rush does not fully discuss his methodological decisions or print figures that are necessary to evaluate them, such as the number of towns involved in each of his regressions or which towns were excluded because of boundary changes, it is not possible to tell whether all of these criticisms are valid or not.

redistricted." (p.39) In this view, redistricting makes all the difference, because voters are quite plastic, easily remolded by different electoral stimuli, but redistricters can only be frustrated by the voters' fickleness.

But the fact that voters are less likely to change their party registration, as Rush stresses (p.82), than they are to vote for candidates of different parties from one election to the next gives us a way to test this assertion. If registration is relatively constant, while voting patterns fluctuate wildly, if one is stable while the other can be largely refashioned by alterations in districts and candidates, then the relationship between registration and voting ought to differ considerably between two elections separated by a redistricting. Table C-1 shows that this was not true in California from 1970 to 1992.[23]

Table C-1 uses the regression coefficients from the election before a redistricting (from Table 1) to estimate the number of seats that each party would win in the newly configured districts after redistricting, when the party registration percentages were often quite different in each district. I then compare these estimates with the actual outcomes in the election, total up the correct and incorrect predictions of winners, and contrast the errors in these estimates with the number of errors, given in Table 2, above, that result from using the post-redistricting election equations. The basic point is that the predictions based on the previous elections are quite good. Overall, 87.9% of the election outcomes are predicted correctly, compared to 89.7% if the post-redistricting elections are used, in a sense, to predict themselves. And this is true for very dramatic reconfigurations of districts between court-ordered and overtly partisan plans, and over election pairs that included the 1974 Watergate election, with its massive shift to the Democrats.

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 any stable opinions. If democracy works, redistricting can change outcomes.

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[23]Tables 1 and 2 above also show no consistent pattern of differences in the correlations or reliability of the estimates of seats won between pairs of elections when a redistricting intervenes and pairs when it does not. For instance, the proportions of variance explained by party registration rise from 1990 to 1992 in all four equations in Table 1.

Table C-1: How Well Can We Predict the Outcomes of Post-Redistricting Elections From Pre-Redistricting Regressions?

Parameters From/Results From	Democratic Winners		Republican Winners		Errors Using Second Election to Predict
	Correct	Additional	Correct	Additional	
Panel A: Assembly					
1970/72	51	0	14	15	15
1972/74	50	5	17	8	13
1980/82	47	3	29	1	4
1990/92	48	0	22	10	6
Panel B: Congress					
1970/72	21	2	18	2	4
1972/74	25	3	11	4	5
1980/82	26	2	17	0	1
1990/92	25	5	19	3	6
Average Percentage Correctly Predicted Using First Election = 87.9%					
Average Percentage Correctly Predicted Using Second Election = 89.7%					

Table 1: Statistics for Party Registration Regressions*

Year	Intercept	Democratic Registration	Republican Registration	R^2
Panel A: Assembly Democratic Vote Percentages				
1970	2.55 (2.41)	-0.15 (-1.35)	-0.030 (-2.71)	0.73
1972	2.04 (2.16)	-0.010 (-0.98)	-0.025 (-2.48)	0.61
1974	2.29 (3.84)	-0.013 (-2.10)	-0.026 (-4.07)	0.65
1976	1.40 (1.87)	-0.004 (-0.44)	-0.018 (-2.24)	0.52
1978	0.79 (1.23)	0.002 (0.34)	-0.011 (-1.55)	0.39
1980	1.04 (2.35)	0.001 (0.13)	-0.017 (-3.18)	0.60
1982	0.93 (1.97)	0.002 (0.44)	-0.015 (-2.76)	0.70
1984	1.58 (2.45)	-0.005 (-0.65)	-0.022 (-2.99)	0.63
1986	1.70 (3.48)	-0.007 (-1.23)	-0.022 (-4.04)	0.76
1988	1.30 (2.50)	-0.002 (-0.38)	-0.017 (-2.90)	0.73
1990	1.63 (2.52)	-0.006 (-0.88)	-0.020 (-2.73)	0.59
1992	0.91 (2.77)	0.001 (0.35)	-0.012 (-3.04)	0.72
1994	1.35 (5.49)	-0.004 (-1.40)	-0.018 (6.14)	0.85
Panel B: Assembly Republican Vote Percentages				
1970	-1.80 (-1.72)	0.017 (1.59)	0.033 (2.94)	0.73
1972	-1.55 (-1.69)	0.015 (1.55)	0.031 (2.94)	0.62
1974	-1.43 (-2.38)	0.015 (2.29)	0.028 (4.23)	0.65
1976	-0.61 (-0.79)	0.006 (0.69)	0.020 (2.40)	0.49
1978	0.19 (0.30)	-0.002 (-0.34)	0.011 (1.55)	0.39
1980	-0.23 (-0.51)	0.001 (0.15)	0.019 (3.50)	0.61
1982	-0.09 (-0.19)	-0.001 (-0.16)	0.017 (3.02)	0.70
1984	-0.77 (-1.15)	0.007 (0.89)	0.024 (3.16)	0.62
1986	-1.01 (-2.10)	0.010 (1.83)	0.026 (4.73)	0.78
1988	-0.72 (-1.26)	0.006 (1.00)	0.022 (3.40)	0.72
1990	-1.00 (-1.76)	0.010 (1.57)	0.024 (3.69)	0.66
1992	-0.26 (-0.78)	0.002 (0.51)	0.016 (3.94)	0.73
1994	-0.62 (-2.44)	0.006 (2.21)	0.021 (7.02)	0.86

Year	Intercept	Democratic Registration	Republican Registration	R^2
Panel C: Congressional Democratic Vote Percentages				
1970	-1.79 (-0.87)	0.030 (1.42)	0.15 (0.71)	0.65
1972	-0.46 (-0.33)	0.017 (1.15)	0.000 (0.05)	0.63
1974	-0.80 (-0.70)	0.022 (1.78)	0.004 (0.35)	0.69
1976	0.28 (0.28)	0.009 (0.85)	-0.006 (-0.59)	0.63
1978	1.13 (1.52)	-0.000 (-0.04)	-0.017 (-2.04)	0.68
1980	1.13 (2.05)	-0.000 (-0.10)	-0.018 (-2.82)	0.67
1982	0.92 (1.66)	0.001 (0.24)	-0.014 (-2.14)	0.71
1984	1.49 (2.06)	0.004 (-0.50)	-0.022 (-2.66)	0.72
1986	1.52 (2.17)	-0.004 (-0.52)	-0.021 (-2.58)	0.75
1988	2.06 (2.86)	-0.010 (-1.23)	-0.026 (-3.20)	0.75
1990	-0.18 (-0.25)	0.014 (1.64)	0.000 (0.00)	0.66
1992	0.83 (2.29)	0.002 (0.45)	-0.011 (-2.48)	0.77
1994	1.27 (3.77)	-0.003 (-0.82)	-0.017 (-4.28)	0.86
Panel D: Congressional Republican Vote Percentages				
1970	2.59 (1.23)	-0.028 (-1.29)	-0.014 (-0.62)	0.62
1972	1.00 (0.72)	-0.013 (-0.85)	0.004 (0.28)	0.64
1974	1.64 (1.30)	-0.020 (-1.50)	-0.003 (-0.22)	0.64
1976	0.49 (0.53)	-0.007 (-0.70)	0.009 (0.92)	0.68
1978	-0.25 (-0.34)	0.001 (0.18)	0.018 (2.22)	0.69
1980	-0.49 (-0.84)	0.004 (0.64)	0.022 (3.24)	0.66
1982	-0.16 (-0.23)	0.001 (0.17)	0.016 (1.91)	0.57
1984	-0.56 (-0.81)	0.005 (0.61)	0.022 (2.69)	0.72
1986	-0.71 (-0.91)	0.006 (0.24)	0.023 (2.71)	0.73
1988	-1.30 (-1.76)	0.012 (1.47)	0.029 (3.44)	0.76
1990	0.52 (0.88)	-0.005 (-0.90)	0.006 (0.85)	0.69
1992	0.07 (0.18)	-0.002 (-0.34)	0.011 (2.24)	0.72
1994	-0.10 (-0.32)	0.007 (0.19)	0.015 (3.78)	0.87

*t statistics in parentheses

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

Table 2: Comparison of Actual Winners and Winners Predicted from Party Registration Regressions

Year	Dem. Winners Predicted Correctly	Additional Dem. Winners	Rep. Winners Predicted Correctly	Additional Rep. Winners
		Panel A: Assembly		
1970	41	2	31	6
72	45	6	20	9
74	51	4	16	9
76	54	3	15	8
78	45	5	23	7
1980	39	8	27	6
82	47	1	29	3
84	46	1	30	3
86	43	1	31	5
88	47	0	28	5
1990	47	1	28	4
92	44	4	30	2
94	35	4	35	6
		Panel B: Congress		
1970	19	1	16	2
72	23	0	16	4
74	28	0	10	5
76	28	1	9	5
78	24	2	14	3

80	19	3	19	2
82	28	0	16	1
84	27	0	18	0
86	27	0	15	3
88	27	0	16	2
90	25	1	16	3
92	27	3	19	3
94	22	5	23	2

Table 3: Incumbency is Less Useful in Predicting the Results of Immediate Post Redistricting Than of Other Election

Election	Number of Incumbents Running in General Election			Average Democratic Registration Margin		
	Dem.	Open	Rep.	Dem. Inc.	Open	Rep. Incum.
Panel A: Assembly						
1970	38	6	36	29.5	13.2	4.4
1972	39	12	29	32.2	22.1	5.6
1974	36	21	23	31.9	22.7	6.1
1976	46	15	19	33.3	19.7	10.6
1978	50	17	13	31.3	18.2	10.5
1980	42	15	23	32.4	14.3	8.2
1982	34	22	24	36.2	19.8	-1.0
1984	46	3	31	32.8	-2.7	-7.1
1986	40	12	28	31.6	17.7	-9.4
1988	44	3	33	32.3	-2.7	-7.1
1990	43	9	28	28.7	13.6	-10.0
1992	34	26	20	30.3	8.8	-5.6
1994	33	27	20	30.4	11.8	-8.9
Panel B: Congress						
1970	18	3	17	29.8	24.7	1.3
1972	20	7	16	29.2	17.5	5.9
1974	20	7	16	36.3	19.9	5.3

1976	27	3	13	34.1	10.0	9.8
1978	25	8	10	32.3	24.3	6.2
1980	24	3	16	31.0	27.7	6.4
1982	20	10	15	33.8	20.7	1.5
1984	28	0	17	30.8	---	-5.0
1986	27	3	15	33.8	20.7	1.5
1988	26	4	15	29.4	-2.7	-8.4
1990	25	3	17	27.2	29.7	-10.4
1992	21	16	15	30.0	11.3	-5.8
1994	28	4	20	28.4	12.3	-6.3

Table 4: How Much Better Can One Predict Assembly and Congressional Results by Adding Incumbency to the Equation?

Year	R^2, Party Only, Democratic Equation	Additional R^2 Incumbency Added	%Outcomes Predicted Correctly, Party Only	Increased % Outcomes Predicted Correctly, Incumbency Added
		A. Assembly		
1970	.732	.131	90.0	3.75
1972	.608	.171	81.25	12.5
1974	.653	.107	83.75	7.5
1976	.519	.177	86.25	6.25
1978	.391	.116	85.0	3.75
1980	.600	.162	82.5	6.25
1982	.702	.069	95.0	1.25
1984	.633	.136	95.0	3.75
1986	.763	.124	92.5	5.0
1988	.733	.059	93.75	1.25
1990	.589	.066	93.75	1.25
1992	.718	.045	92.5	1.25
1994	.853	.030	87.5	-1.25
Average	.653	.107	89.13	4.04
		Panel B: Congress		
1970	.647	.261	92.1	7.9
1972	.630	.199	90.7	6.9
1974	.691	.123	88.3	4.7
1976	.629	.186	86.0	11.7
1978	.677	.065	88.3	2.5

Year	R^2, Party Only, Democratic Equation	Additional R^2 Incumbency Added	%Outcomes Predicted Correctly, Party Only	Increased % Outcomes Predicted Correctly, Incumbency Added
1980	.673	.167	88.3	0
1982	.714	.105	97.8	0
1984	.717	.127	100	-2.2
1986	.749	.149	97.8	2.2
1988	.755	.131	95.6	2.2
1990	.664	.039	91.1	2.2
1992	.767	.055	88.5	3.8
1994	.858	.057	86.5	9.7
Average	.699	.128	91.6	3.97

Table 5: Percent Correctly Predicted Using OLS and Logit Models

Year	OLS, 2-Equation Model		Logit	
	Congress	Assembly	Congress	Assembly
1970	92.1	90.0	92.1	92.5
72	90.7	81.25	90.7	80
74	88.4	83.75	83.7	86.25
76	86.0	86.25	88.4	85
78	88.3	85	88.4	85
80	88.3	82.5	86	83.75
82	97.8	95	N.C.*	96.25
84	100	95	N.C.	97.5
86	93.3	92.5	N.C.	92.5
88	95.6	93.75	N.C.	92.5
90	91.1	93.75	91.1	93.75
92	88.5	92.5	90.4	90
94	86.5	87.5	90.4	91.25
Average	91.3	89.1	89.0	89.7

*N.C.= did not converge

Table 6: How Well Does the Technique Work for Other States?

North Carolina Congressional Contests as a Test

Year	R^2 (Democratic Equation)		% Predictions Correct	
	Party Registration	+ Incumbency	Party Registration	+ Incumbency
1980	.711	.793	.727	.818
1982	.437	.559	.636	.818
1984	.696	.787	.727	.727
1986	.907	.930	.727	.818
1988	.737	.793	.727	1.000
1990	.619	.933	.818	.909
1992	.885	.914	. 917	1.000
Average	.713	.816	.754	.870

Source: North Carolina Secretary of State (1980-90); North Carolina State Legislature (1992), obtained in connection with *Shaw* v. *Hunt* litigation.

Table 7: How Well Do Other Electoral Contests Predict the 1992 Texas Congressional Contests?

Year	*Predictor*	*Democratic*		*Republican*		*Overall %*
		Correct	*Additional*	*Correct*	*Additional*	*Correct*
1988	Ct. Crim. Appeals	19	2	8	1	90.0
1990	Ct. Crim. Appeals	20	1	8	1	93.3
1992	Ct. Crim. Appeals	20	1	8	1	93.3
1990	Governor	18	3	9	0	90.0
1988	Senator	18	3	8	1	86.7

Source: Data from Texas State Legislature, produced for *Vera* v. *Richards*.

Table 8: How Well Do Other Contests Predict California Congressional and Assembly Elections?

Year	Contest	Democratic		Republican		Overall %
		Correct	Additional	Correct	Additional	Correct
			Panel A: Congress			
1978	Congress	24	2	14	3	88.3
1978	Governor	23	3	14	3	86.0
1978	Prop. 13	17	9	10	7	62.8
1982	Congress	28	0	16	1	97.8
1982	Governor	24	4	16	1	88.9
1982	Senator	26	2	17	0	95.6
1982	Prop. 15	22	6	11	6	73.3
1990	Congress	25	1	16	3	91.1
1990	Governor	24	2	16	3	88.9
1990	Prop. 140	23	3	16	3	96.7
			Panel B: Assembly			
1978	Assembly	45	5	23	7	85.0
1978	Governor	45	5	19	11	80.0
1978	Prop. 13	46	4	21	9	83.75
1982	Assembly	47	1	29	3	95.0
1982	Governor	48	0	23	9	88.75
1982	Senator	45	3	29	3	92.5
1982	Prop. 15	36	12	10	22	57.5
1990	Assembly	47	1	28	4	93.75
1990	Governor	43	5	29	3	90.0
1990	Prop. 140	45	3	23	9	85.0

Source: *California Secretary of State*, Statement of the Vote, relevant years.

Table 9: Projected Number of Congressional Seats That Would Be Won by Democrats in California If Registration or Crossover Behavior Shifted

Plan as of Nov. 1991	None	+2%D	+6%D	+2%R	+6%R
Panel A: Registration Shifts					
Democratic	33	35	35	32	29
Masters	28	29	32	27	24
Republican	24	27	33	24	24
Panel B: Crossover Shifts					
Democratic	33	34	35	32	31
Masters	28	28	30	27	25
Republican	24	27	32	24	24

Figure 1: Registration Margin, Congress
Republican Plan vs. Democratic Plan

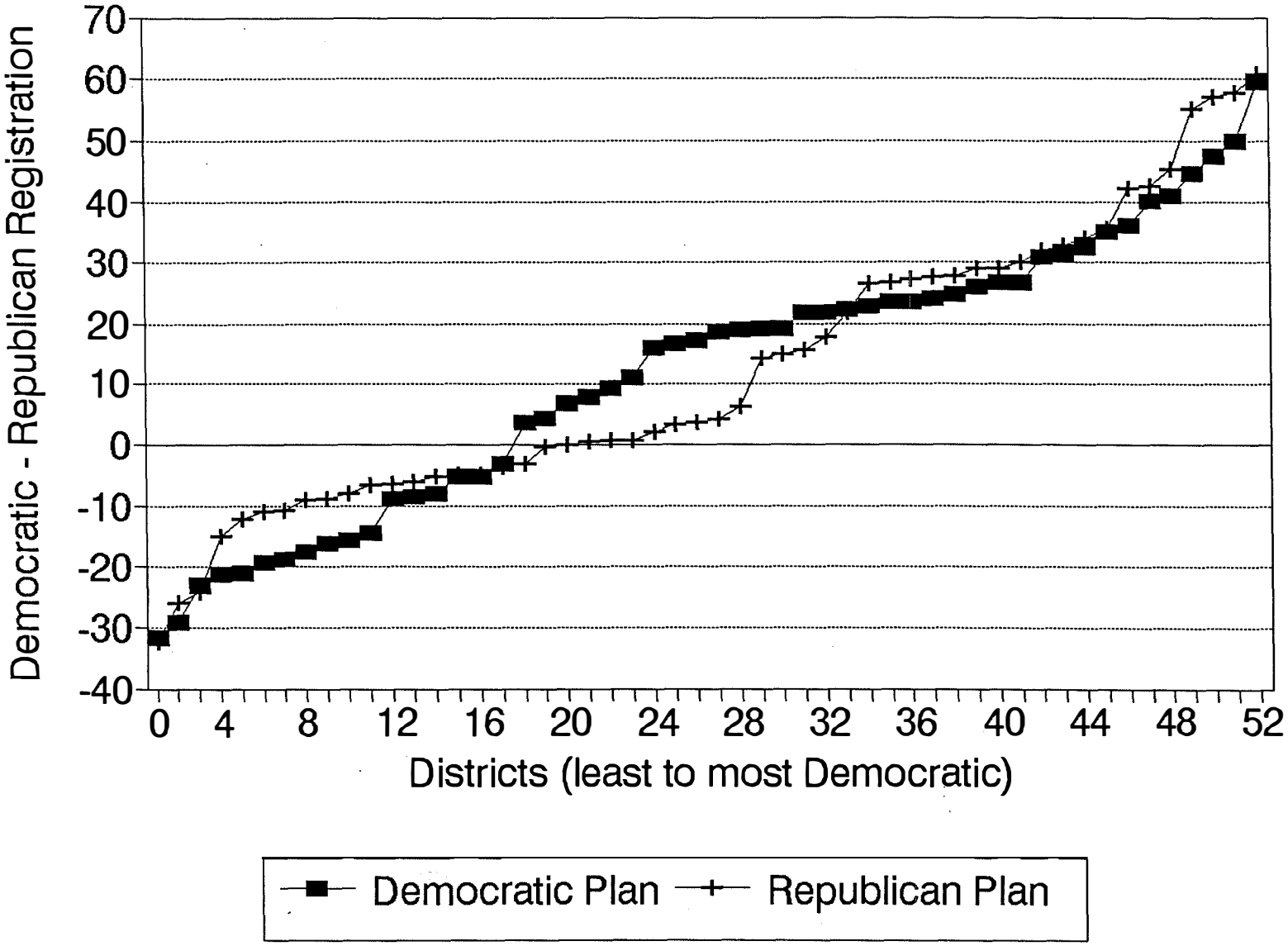

Figure 2: Registration Margin, Congress
Masters' Plan vs. Republican Plan