Multi-Election Regression of Seats on Votes:

A Sensitivity Analysis

Dr Adrian Blau

Lecturer in Governance

Politics, School of Social Sciences

University of Manchester

Manchester M13 9PL

Email: Adrian.Blau@manchester.ac.uk

— DRAFT —

— **NOT FOR CITATION** —

— Comments and criticisms welcome —

Abstract

Analysis of single member district electoral systems often involves regression of seat shares on vote shares over multiple elections, to estimate the degree of responsiveness and partisan bias. This paper tests the accuracy of multi-election regression, using a sensitivity analysis. Elections are simulated to compare known levels of responsiveness and bias with estimates from regression. This shows that large errors can result from regression if bias varies over time. Standard statistical indicators do not pick up these errors. Further flaws with multi-election regression are illustrated using American and British elections. Single-election methods should therefore be used where possible.

Analysts often try to measure two key aspects of the translation of votes to seats in single member district electoral systems: responsiveness and partisan bias. Responsiveness refers to the extent to which the winning party's lead in votes is exaggerated into a larger lead in seats. Bias involves a partisan asymmetry in this relationship: for example, a party may come first in votes and second in seats. Accurately quantifying these indices is normatively important: some critics argue that declining responsiveness has insulated Congress from shifts in public opinions and reduced electoral accountability in Britain, while many writers decry the apparent inequity of biased results where one party wins more seats than its main competitor despite having less votes, as in the American presidential election of 2000, and the British general elections of 1951 and February 1974 (Mayhew 1974; Curtice and Steed 1982, Powell 2000).

This paper critically assesses multi-election regression, a method that has often been used to estimate the levels of responsiveness and bias in American and British elections. Multi-election regression plots parties' shares of votes and seats over a series of elections: the gradient of the best-fit regression line indicates the rate at which votes are translated into seats (responsiveness), while asymmetry indicates partisan inequities in the relationship (bias).

Multi-election regression has been used in at least thirty studies. But in the 1980s and 1990s, certain scholars questioned its validity, in terms of its sensitivity to input values (Niemi and Fett 1986, 79-9; Jackman 1994, 330), its statistical properties (Monroe 1998, 9-10), and the meagre information that it provides about change over time (King and Gelman 1991, 118; Garand and Parent 1991, 1021; Jackman 1994, 328-31; Monroe 1998, 10). Along with the growing sophistication of single-election methods, multi-election regression has thus been less common in recent years. However, a sustained analysis of its deficiencies has still not been conducted, and the most detailed analysis (Monroe 1998, 9-10) remains unpublished. So the method continues to be used in normatively important debates, such as whether the responsiveness index has declined in America and Britain (Dunleavy 1991; Norris and Crewe 1994; Friedrich 2001), whether congressional elections are biased to the Democrats (Campbell 1996; Friedrich 1999), and whether redistricting affects bias (Stokes 1993; Cox and Katz 2002).

This paper therefore assesses the suitability of multi-election regression by conducting a sensitivity analysis. In a sensitivity analysis, we use simulations to artificially vary one parameter while holding the other(s) constant, before repeating this with the other parameter(s). This approach, like control experiments in the natural sciences, indicates the absolute and relative importance of each parameter.

In this paper, simulated series of election results are produced, using already-known indices of responsiveness and bias. Multi-election regression is then applied to the same elections, to see how accurately it estimates the actual indices of responsiveness and bias. The simulations allow levels of responsiveness and bias to be held constant or to vary. This suggests that multi-election regression is much more sensitive to changes in bias than responsiveness. The resulting errors can be worryingly large. Other problems are then considered, using American and British elections as examples. Overall, fundamental doubts are cast on the use of multi-election regression in analysing the translation of votes to seats in single member district elections.

Responsiveness and bias: equations and methods

Equation 1 shows the standard equation for responsiveness and bias:

$$\frac{S}{100-S} = b\left(\frac{V}{100-V}\right)^{k} \qquad (1)$$

where S and V are party A's share of the two-party seats and votes respectively (measured on a 0-100 scale), b is the bias index, and k is the responsiveness index. (The two-party seats and votes refer to seats and votes won by the Democrats and Republicans in American elections, the Conservatives and Labour in British elections.) A responsiveness index of 3 represents the 'cube law' or 'cube rule' that once applied in US congressional elections in non-southern states, and in British general elections (Kendall and Stuart 1950). A bias index of 1.0 means no bias, while a bias index of 1.5 means that at 50 per cent of the two-party vote, party A will win 60 per cent of the two-party seats and party B will win only 40 per cent.

There are two main families of methods for estimating responsiveness and bias: single-election and multi-election methods. Single-election methods estimate responsiveness and bias for each separate election. For example, Gelman and King (1994) chart the level of responsiveness and bias for each twentieth century congressional election. Multi-election methods estimate overall levels of responsiveness and bias by examining two or more elections simultaneously. For example, King and Browning (1987) use regression to estimate the levels of responsiveness and bias in post-war congressional elections.

The simplest multi-election method is the 'two-election swing ratio'. This divides the change in seat shares by the change in vote shares. For example, if a 4 percentage point swing in votes produced a 6 point change in seats, the two-election swing ratio would be 1.5. Two-election swing ratios have been used in several studies (Burnham 1975; Tufte 1975; Abramowitz 1983; Brady 1985; Niemi and Jackman 1991; Niemi and Winsky 1992; Niemi and Abramowitz 1994). But two-election swing ratios have significant defects, especially an inability to distinguish responsiveness and bias; this method has now been convincingly refuted (Niemi and Fett 1986, 78; Browning and King 1987, 310; Campagna and Grofman 1990, 1245-6). I therefore focus on the main multi-election method: multi-election regression.

Multi-election regression plots a best-fit line through a graph of vote and seat shares over a series of elections. The responsiveness index is the gradient of the line over a given area, which is usually a normatively important area such as the central region of the graph. The responsiveness index represents the expected change in seats for a one point swing in votes. Bias involves an asymmetry in translating votes into seats. For example, if the best-fit line does not pass through 50 per cent votes 50 per cent seats, then one or other party is advantaged at this point, indicating that it could or did win a majority of seats on a minority of votes. Responsiveness and bias can be measured at different points, and different types of regression may be used, such as linear versus loglinear techniques (for a summary, see Monroe 1998). However, such details are not necessary to this paper, as all such methods share the same basic assumption: that by regressing seat shares on vote shares, we can uncover the rate at which votes are translated into seats (responsiveness), and find asymmetries in this relationship (bias). Over30 studies of seats-votes relationships have used multi-election regression (Dahl 1956, 147-9; March 1958; Spafford 1970; Erikson 1971; Kuklinski 1973; Tufte 1973; Burnham 1974; Tufte 1974; Linehan and Schrodt 1977; Clubb, Flannigan and Zingale 1980, 172-5; Schrodt 1981; Jacobson 1987; King and Browning 1987; Browning and King 1987; Jacobson 1990, 83-94; King 1990; Dunleavy 1991; Niemi and Jackman 1992; Stokes 1993; Jackman 1994; Norris and Crewe 1994, 206-7; Campbell 1996, 74-5, 80-1; Swayze 1996; Friedrich 1999; Grofman and Reynolds 2001, 133; Blais 2002; Cox and Katz 2002, 55-65; Maloney et al. 2003; Beasley 2007, 1490-2; Zucco 2007, 307-8; Vowles 2008, 169-72). This list includes two ‘classic’ papers (Tufte 1973; King and Browning 1987).

Method: sensitivity analysis

To test the robustness of multi-election regression, two-party elections are simulated in a control experiment which tests the sensitivity of multi-election regression to variations in responsiveness and bias. This test has two stages. First, I simulate randomized election results (vote share and seat share) where the indices of responsiveness and bias are known for each election. We can then compute mean indices of responsiveness and bias for a series of such simulated elections. Second, multi-election regression is applied to the same series of elections, to see how close the regression-based estimates of responsiveness and bias are to the mean figures. The two stages are repeated one hundred times. This whole process can be applied while holding responsiveness constant and letting bias vary, or vice versa, or varying both at the same time. This not only tests multi-election regression's sensitivity to input values, but also shows which input value – responsiveness or bias – is more important here.

Why use two stages? The aim is to see how accurately multi-election regression calculates responsiveness and bias, so we must compare regression-based *estimates* of responsiveness and bias against *known* levels of responsiveness and bias at the same elections. We can easily stipulate the level of responsiveness and bias at a given election. Say that we stipulate an election with a responsiveness index of 3.0 and a bias index of 1.0 (the so-called 'cube law' or 'cube rule'). Assuming two-party competition, a party with 51 percent of the vote would get 53 percent of the seats, and a party with 52 percent of the seats would get 56 percent of the seats. Now that we have two elections (i.e. two points on a graph of votes against seats), we can apply multi-election regression. In this example, it would rightly indicate a responsiveness index of 3.0 and a bias index of 1.0. In many other examples, as we will see, it is less accurate.

The first stage of the simulation therefore requires us to create indices of responsiveness (k) and bias (b) at elections where we also know the vote share (V) and seat share (*S*) for a party. In each simulated election, *V* is a random number between 45 and 55 per cent. (All random numbers in this paper are drawn from a uniform distribution.) The chosen values of *k* and *b* will be explained shortly. Once we know *V*, *k* and *b*, the seat share *S* can easily be calculated, following Garand and Parent's (1991, 1018) method:

$$S = 100\left(\frac{b\left(\frac{V}{100-V}\right)^{k}}{1+b\left(\frac{V}{100-V}\right)^{k}}\right) \qquad (2)$$

To calculate *b* and *k*, four different simulations are used:

Simulation 1	Constant responsiveness (2.0)	Constant bias (1.0)
Simulation 2	Variable responsiveness (1.75 to 2.25)	Constant bias (1.0)
Simulation 3	Constant responsiveness (2.0)	Variable bias (0.9 to 1.1)
Simulation 4	Variable responsiveness (1.75 to 2.25)	Variable bias (0.9 to 1.1)

Such variations in responsiveness and bias are not unusual for American congressional and state elections (Gelman and King 1994a, 540-1; Gelman and King 1994b, 556-7). Responsiveness usually changes less in recent British general elections but bias sometimes changes more (Johnston et al. 2001; Blau 2004, 434, 443). Each simulation has one hundred sets of five election results: five is the longest set of general elections without redistricting in post-war Britain, and the usual number of congressional elections between redistricting in America. Overall, then, the assumptions provide reasonable simulations of British and American election results.

For example, in simulation 2, we randomly generate five vote shares between 45 and 55 percent, and each of these points has a different responsiveness index, randomly situated between 1.75 and 2.25; but each point has a bias index of 1.0. Given the vote share, the responsiveness index and the bias index, we can calculate a seat share for each election.

The second stage of the test is to apply multi-election regression to these sets of simulated elections. Because curved relationships between votes and seats cannot be modelled accurately using linear regression, the regression equation to be applied is a logarithmic transformation of equation 1, following Tufte (1973, 545):

$$\ln\left(\frac{S}{100-S}\right) = \ln(b) + k\ln\left(\frac{V}{100-V}\right) \quad (3)$$

The estimated responsiveness index is the regression coefficient for the log-odds ratio of two-party vote shares, and the estimated bias index is the antilog of the intercept term. (For further details, see Garand and Parent 1991, 1013-8.) Note that all election results lie between 45 and 55 per cent of the vote, and I measure responsiveness and bias over the same range. This is the normatively important area of the graph where the level of responsiveness and bias can particularly affect the election outcome.

Results

The results of the four simulations are depicted in Table 1. Simulation 1 (constant responsiveness and bias) produces perfect estimates of responsiveness and bias. Any plot of party A's vote and seat shares sees all points sitting exactly on a straight line that has a gradient of 2 and passes through 50 per cent votes, 50 per cent seats. Even two elections would produce the same line.

– Table 1 about here –

Simulation 2 (variable responsiveness, constant bias) produces fairly solid results. The mean absolute difference between the average and regression-based responsiveness indices is only 0.05 units, a tiny discrepancy. The largest difference is about 0.2 units. The regression-based responsiveness index never falls below 1.75 or above 2.25, the known limits for this index. Estimates of bias are very accurate: the mean error is only 0.005 units, equivalent to roughly a 0.15 percentage point error in seats at 50:50, which is negligible.

However, simulation 3 (constant responsiveness, variable bias) produces far less accurate estimates of responsiveness. The mean error in estimating responsiveness is 0.25 units, the largest error is a mammoth 1.87 units, the estimates of the responsiveness index range between 1.05 and 3.87 (a surprisingly large spread), and 40 of the 100 simulations imply a responsiveness index of under 1.75 or over 2.25. These are remarkable errors given that the actual responsiveness index was 2.0 at every single election, with the only deviations in the expected seat share coming from modest fluctuations in bias. These results should greatly concern those who use multi-election regression. Clearly, variable bias can seriously contaminate estimates of responsiveness. However, the mean error in estimating bias remains low (0.01 units, equivalent to 0.25 percentage points in seats at 50:50), and while the largest divergence (of 0.1 units) would involve a 2.5 point error in seats at 50:50, bias estimates were usually accurate.

Simulation 4 (variable responsiveness and variable bias) produces similar results to simulation 3. The mean error in estimating responsiveness is 0.22 units, the largest error is 0.70 units, and 37 of the 100 simulations imply responsiveness indices below 1.75 or above 2.25. The mean error in estimating bias was again low (0.01 units), and while the largest divergence (of 0.07 units) is equivalent to more than a 1.5 point error in seats at 50:50, bias estimates were again usually accurate.

Evidently, variable responsiveness has little effect on the accuracy of multi-election regression: variable bias is the problem. And it is a big problem. These errors could lead to invalid normative conclusions, for example implying static rather than changing responsiveness, or vice versa.

Curiously, indicators of statistical significance say little or nothing about these errors in estimating responsiveness and bias. Table 2 shows that even simulations 3 and 4 typically produced high R^2 and low standard errors of the regression. Unsuspecting analysts might thus infer that the tests of responsiveness and bias are reliable. But we know this is not necessarily the case.

– Table 2 about here –

Equally worrying is the fact that the size of the errors in responsiveness and bias rarely correlates as expected with indicators of statistical significance. Table 3 shows that a high R^2 and a low standard error usually say little about how accurate estimates of responsiveness and bias are, except in simulation 3 where the errors in estimating responsiveness and bias generally increase as R^2 falls, as we would expect, and in simulation 4 where the errors in estimating responsiveness and bias generally increase as the standard error of the regression *falls*, which is the opposite of what we would expect.

– Table 3 about here –

Indicators of statistical significance have thus given multi-election regression a spurious veneer of respectability. Understandably, scholars often accept multi-election estimates of responsiveness and bias if they pass tests of statistical significance (for example, Spafford 1970). But these simulations once more highlight the truth of the well-known adage that statistical significance need not mean substantive significance.

Grofman and King (2007, 10), who rightly criticize multi-election regression, write that it ‘works fine in principle, except that there are usually five or fewer elections between redistrictings, which is too few to pin down the seats-votes curve with much certainty.’ The sensitivity analysis in this paper suggests a more fundamental problem. If responsiveness and bias can change from election to election, then the precise relationship between votes and seats will change from election to election. The search for a single seats-votes curve, based on a single seats-votes relationship, becomes problematic. Having more data points is not a solution – indeed, the more data points there are, the more likely it is that bias will vary, and the more likely it is that regression-based estimates of responsiveness and bias will mislead us. Standard assumptions about statistical inference do not thus apply to multi-election regressions of seats on votes. I now explain why.

Analysis: why multi-election regression is unreliable

Why does variable bias affect estimates of responsiveness, and why do statistical tests not reveal this? The first clue is that for each simulation in Table 3, errors in responsiveness correlate positively with errors in bias: on average, the less accurate the estimate of bias, the less accurate the estimate of responsiveness.

To understand this correlation, consider Figure 1. The unbroken line runs through a series of five elections marked with squares, each of which has a responsiveness index of 2.0

and a bias index of 1.0. As in Simulation 1, regression produces the correct indices: a multi-election responsiveness index of 2.0 and bias index of 1.0.

Let us add an election on the right-hand side, marked with a circle, which has a responsiveness index of 2.0 but a bias index of only 0.9. Multi-election regression is now applied to the set of six elections (the five squares and the circle). The new best-fit line, the dotted line, alters the estimates of bias *and* responsiveness for the series as a whole, implying a bias index of 0.99, and an overall responsiveness index of 1.6 – even though each individual election had a responsiveness index of 2.0.

– Figure 1 about here –

This explains why Simulations 2 to 4 saw correlations between the error in estimating bias and the error in estimating responsiveness. Even though the deviant election in Figure 1 only saw a change in bias, *the regression equation also changes the responsiveness index to produce a better fit*. (Line 2 would not have as good a fit if it were forced to have a slope of 2.)

This is why statistical indicators are not always reliable guides to the accuracy of multi-election estimates of responsiveness and bias: to generate a higher R^2 or a lower standard error, multi-election regression may have to produce less accurate estimates of responsiveness and/or bias. As with two-election swing ratios, criticized above, multi-election regression may confuse responsiveness and bias – a significant and previously overlooked problem that markedly weakens the method's usefulness in analysing seats-votes relationships.

Figure 1 also explains why variable bias affects multi-election regression more than variable responsiveness does. The deviant election (the circle) represents a party getting 53 percent of the vote and 53.4 percent of the seats. In the above example this result was reached by lowering the bias index to 0.9 while keeping the responsiveness index at the same level as the other five elections (2.0). Of course, we could have achieved the same result by keeping bias at the same level as the other five elections and instead lowering the responsiveness index. However this would have required a major fall in the responsiveness index, to just 1.1. Evidently, given the levels of variability of responsiveness and bias seen in American and British elections, multi-election regression will be more sensitive to the observed levels of changing bias than to the observed levels of changing responsiveness.

We should also note that in Figure 1, multi-election regression could even suggest that line 1 has a responsiveness index of 2.0 and a bias index of 1.0 despite this not being true at *any* individual election. For example, a party could receive 51 per cent of the vote and 52 per cent of the seats from a responsiveness index of 2.0 and bias index of 1.0 – or from a responsiveness index of 1.0 and a bias index of 1.04. A party could receive 52 per cent of the vote and 54 per cent of the seats with a responsiveness index of 2.0 and a bias index of 1.0 – or from a responsiveness index of 3.0 and a bias index of 0.92. Both elections sit on a line with a responsiveness index of 2.0 and a bias index of 1.0, but neither figure need apply at the individual elections.

Multi-election regression is fundamentally weakened by this inability to provide clear inferences about individual elections. Niemi and Fett's (1986, 82) well-known analysis of multi-election regression suggested that '[i]f what happened in a particular year is the focus, some sort of residual analysis … would be useful'. The sensitivity analysis in this paper suggests otherwise, however. If a point deviates from the multi-election best-fit line, we cannot tell whether the residual error reflects responsiveness and/or bias – or even neither, given that the original estimates of responsiveness and bias may be misleading.

Consider Erikson's study of the partisan impact of redistricting, which used multi-election regression on thirty-eight states for the nine House elections between 1952 and 1968; each regression equation included a dummy variable distinguishing elections before and after redistricting (Erikson 1971). This tells us if deviation from the best-fit line occurred after redistricting, but we cannot tell whether the deviation involved a change in responsiveness, for example by incumbent protection, and/or a change in bias, for example, by gerrymandering. This is the very information we need, and only single-election analysis gives the answer. Moreover, if redistricting indeed altered bias, as probably occurred in many states, then a number of Erikson's multi-election estimates of responsiveness may also have been affected – and if the slope and position of these best-fit lines is questionable, so are the values of the 'residuals'. Multi-election regression cannot uncover the information that we need here.

A further problem relating to analysis of residuals, and change over time more generally, arises from Dunleavy's (1991) multi-election analysis of British general elections. Dunleavy criticizes Curtice and Steed's (1982) thesis that the cube law has declined, and uses linear regression to examine the relationship between the Conservative lead over Labour in seats against the Conservative lead over Labour in votes, for elections from 1918 to 1987. His regression suggests that the average percentage lead in seats is equal to 2.3 times the Conservative percentage lead in votes, minus 1.4 (R^2 is 0.97). Analysis of residuals indicates no systematic trend over time, so Dunleavy concludes that the electoral system 'has clearly not been exaggerating winners' majorities any less than in the past'.

However, the translation of votes to seats is unlikely to be constant over the period studied, given the marked changes in party competition and electoral geography. Curtice and Steed's argument about the cube law's decline, moreover, focuses on elections from 1955 onwards, not from 1918. If we replicate Dunleavy's regression for elections between 1955 and 2001, the residuals do in fact show a clear and statistically significant shift over time. (The correlation coefficient between time since 1955 and the size of the residuals is –0.92, significant at $p < 0.05$; $N = 13$.) Plotting the residuals against time shows a gradual fall in their size over time: the regression equation initially underestimates the likely Conservative lead over Labour by some 5 to 10 per cent of the seats, with this situation gradually reversing itself by 2001. However, a basic problem with multi-election regression is that the changing residuals cannot themselves confirm Curtice and Steed's conclusion about the cube law's decline: we do not know whether the changing residuals reflect changes in responsiveness, or bias, or both. In any case, scholars have found significant shifts in bias over this period (Curtice and Steed 1998; Johnston et al. 2001; Blau 2004), and given the sensitivity of multi-election regression to variable bias, we must doubt the accuracy of the regression-based estimates of responsiveness, and thus the meaningfulness of the residuals.

This brings us to another problem with multi-election regression mentioned at the start of this paper: by aggregating several elections, multi-election regression provides little reliable information about temporal change in responsiveness and bias. Gary King once used multi-election methods (King and Browning 1987; King 1990), but now prefers single-election methods precisely because multi-election regression ignores short-term changes that we need to understand, such as the effects of redistricting (King and Gelman 1991, 118). Other scholars express similar concerns (Garand and Parent 1991, 1021; Jackman 1994, 328-31; Monroe 1998, 10). The above simulations also suggest that when comparing two different periods of elections, multi-election regression could wrongly imply a significant change in the responsiveness index, or could even suggest that the index has not changed when actually it has.

Temporal change means that we cannot be too comforted by the fact that the simulations above usually saw fairly reliable estimates of bias. Tufte asks 'is there a significant bias?' in British general elections from 1945 to 1970, and concludes from multi-election regression that there was 'no bias' (Tufte 1973, 546). We now know this to be incorrect: all scholars using single-election methods have found sizeable pro-Conservative bias in this period (Curtice and Steed 1998; Johnston et al. 2001; Blau 2004). But even if Tufte's claim had been true on average, it overlooks change over time, and most importantly it misses the 1951 election, when the Conservatives won four percentage points more seats than Labour despite having one point less in votes. This is an empirically large and normatively important bias; any method which overlooks such results is open to criticism. Note too that if bias varies over time, it may mislead to say that 'an electoral system is unbiased' (Tufte 1973, 547), or that '[t]he U.S. system is moderately biased' to the Democrats (King 1990, 172). Multi-election regression should not be used to talk about what happens at individual elections as if this is a property of 'an electoral system'. (For a similar criticism of some accounts of disproportionality indices, see Monroe 1994, 175.)

Indeed, multi-election regression does not really estimate responsiveness and bias. Strictly speaking, it merely predicts the seat share for any given vote share, and indicates the variability around this seat share. Consider line 1 in figure 1 once more. All that the regression equation really says is that over these elections, a one percentage point change in votes produced a two percentage point change in seats, on average, and that the best-fit line passes through 50 per cent votes 50 per cent seats.

Multi-election regression cannot therefore give us some of the most important information that we need about the translation of votes to seats at individual elections and about change from one election to the next. Single-election methods have their own problems, of course. Some methods assume uniform swing, a questionable assumption (King 1989, 796-8; Blau 2001, 46-55), but the non-uniform-swing methods used by Gelman and King (1994a) and Campbell (1996, 114-7) do not take account of the difference between distribution and size bias (Grofman, Koetzle and Brunell 1997, 464, 468), while Blau (2001, 57-8) admits imperfections with his own alternative to uniform swing.

But if we want to understand responsiveness and bias, multi-election regression is not the solution. Rather, we must continue to develop single-election methods with more robust hypothetical assumptions than uniform swing (see especially Gelman and King 1994a), which estimate responsiveness and bias at actual election results rather than under hypothetical conditions (Campbell 1996, 114-7; Blau 2004), and which deal more effectively with third parties (Borisyuk et al 2008). Multi-election regression may appear to be ‘more strictly empirical’ than single-election methods (Friedrich 1999, 7-8). But appearances can be deceptive.

Conclusion

This paper has shown that multi-election regression is an unreliable tool for investigating the translation of votes to seats in single member district electoral systems. If bias changes between elections, multi-election regression can wrongly suggest that responsiveness too has changed. The resulting estimates can be strikingly inaccurate, even when statistical tests imply robust results. The potential inaccuracies revealed by this paper’s simulations, and problems with uncovering temporal change, militate against the use of multi-election regression. Of course, single-election methods have problems too, but their ability to provide empirically and normatively important information on the translation of votes to seats in single member district electoral systems is far greater than that of multi-election methods. It is thus on single-election methods that future studies should focus.

References

Abramowitz, Alan. 1983. “Partisan Redistricting and the 1982 Congressional Elections.” *Journal of Politics* 45(3): 767-70.

Ansolabehere, Stephen, David Brady, and Morris Fiorina. 1992. “The Vanishing Marginals and Electoral Responsiveness.” *British Journal of Political Science*, 22(1): 21-38.

Beasley, Timothy, and Ian Preston. 2007. “Electoral bias and policy choice: theory and evidence”, *Quarterly Journal of Economics* 122.4, 1473-1510.

Blais, André. 2002. “Mode de Scrutin et Système de Partis: qu’en est-il du Scrutin à Deux Tours?” Paper delivered at Assocation Française de Science Politique conference, Lille.

Blau, Adrian. 2001. “Partisan Bias in British General Elections.” *British Elections and Parties Review*, 11: 46-65.

Borisyuk, Galina, Ron Johnston, Michael Thrasher, and Colin Rallings. 2008. “Measuring bias: Moving from two-party to three-party elections.” *Electoral Studies* 27(2): 245-56.

Brady, David. 1985. “A Reevaluation of Realignments in American Politics: Evidence from the House of Representatives.” *American Political Science Review* 79(1): 28-49.

Browning, Robert, and Gary King. 1987. “Seats, Votes, and Gerrymandering: Estimating Representation and Bias in State Legislative Redistricting.” *Law and Policy* 9(3): 305-22.

Burnham, Walter. 1974. “Communications.” *American Political Science Review*, 68(1): 207-11.

Burnham, Walter. 1975. “Insulation and Responsiveness in Congressional Elections.” *Political Science Quarterly*, 90(3), 411-35.

Campagna, Janet, and Bernard Grofman. 1990. “Party Control and Partisan Bias in 1980s Congressional Redistricting.” *Journal of Politics*, 52(4): 1242-57.

Campbell, James. 1996. *Cheap Seats: The Democratic Party’s Advantage in U.S. House Elections*. Columbus: Ohio State University Press.

Clapham, Christopher. 1996. *The Concise Oxford Dictionary of Mathematics*. Oxford: Oxford University Press.

Clubb, Jerome, William Flanigan and Nancy Zingale. 1980. *Partisan Realignment: Voters, Parties, and Government in American History*. London: Sage Publications.

Cox, Gary, and Jonathan Katz. 2002. *Elbridge Gerry's Salamander: The Electoral Consequences of the Reapportionment Revolution*. Cambridge: Cambridge University Press.

Curtice, John, and Michael Steed. 1982. "Electoral Choice and the Production of Government: the Changing Operation of the Electoral System in the United Kingdom since 1955." *British Journal of Political Science*, 12(3): 249-98.

Curtice, John and Michael Steed. 1998. "Neither Representative nor Accountable: First-past-the-post in Britain." Paper delivered at Elections, Public Opinion and Parties annual conference, University of Manchester.

Dahl, Robert. 1956. *A Preface to Democratic Theory*. London: University of Chicago Press.

Dunleavy, Patrick. 1991. "Democracy in Britain: a Health Check for the 1990s." *British Elections and Parties Yearbook*, 2: 26-47.

Erikson, Robert. 1971. "The Partisan Impact of State Legislative Reapportionment." *Midwest Journal of Political Science,* 15(1): 57-71.

Friedrich, Robert. 1999. "Seats and Votes Revisited: New Perspectives on a Century of Congressional Elections." Paper delivered at American Political Science Association annual meeting, Atlanta.

Friedrich, Robert. 2001. "Methodological Choices in the Study of the Seats-Votes Relationship." Paper delivered at American Political Science Association annual meeting, San Francisco.

Garand, James, and Wayne Parent. 1991. "Representation, Swing, and Bias in U.S. Presidential Elections, 1872-1988." *American Journal of Political Science,* 35(4): 1011-31.

Gelman, Andrew, and Gary King. 1994a. "A Unified Method of Evaluating Electoral Systems and Redistricting Plans." *American Journal of Political Science*, 38(2): 514-54.

Gelman, Andrew, and Gary King. 1994b. "Enhancing Democracy Through Legislative Redistricting." *American Political Science Review*, 88(3): 541-59.

Grofman, Bernard, and Gary King. 2007. "The Future of Partisan Symmetry as a Judicial Test for Partisan Gerrymandering after *LULAC v. Perry*." *Election Law Journal* 6.1, 2-35.

Grofman, Bernard, and Andrew Reynolds. 2001. "Electoral Systems and the Art of Constitutional Engineering: an Inventory of the Main Findings." In *Rules and Reason: Perspectives on Constitutional Political Economy*, ed. Ram Mudambi, Pietro Navarra and Giuseppe Sobbrio. Cambridge: Cambridge University Press, 125-64.

Jackman, Simon. 1994. "Measuring Electoral Bias, Australia. 1949–93." *British Journal of Political Science*, 24(3): 319-57.

Jacobson, Gary. 1987. "The Marginals Never Vanished: Incumbency and Competition in Elections to the U.S. House of Representatives, 1952-82." *American Journal of Political Science*, 31(1): 126-41.

Jacobson, Gary. 1990. *The Electoral Origins of Divided Government: Competition in U.S. House Elections, 1946-1988*. Boulder: Westview Press.

Johnston, R.J., Charles Pattie, Danny Dorling, and David Rossiter. 2001. *From Votes to Seats: The Operation of the UK Electoral System since 1945*. Manchester: Manchester University Press.

Kendall, Maurice, and Alan Stuart. 1950. "The Law of Cubic Proportion in Election Results." *British Journal of Sociology*, 1(3): 183-96.

King, Gary. 1989. “Representation Through Legislative Redistricting: a Stochastic Model.” *American Journal of Political Science*, 33(4): 787-824.

King, Gary. 1990. “Electoral Responsiveness and Partisan Bias in Multiparty Democracies.” *Legislative Studies Quarterly*, 15(2): 159-81.

King, Gary, and Robert Browning. 1987. “Democratic Representation and Partisan Bias in Congressional Elections.” *American Political Science Review*, 81(4): 1251-73.

King, Gary, and Andrew Gelman. 1991. “Systemic Consequences of Incumbency Advantage in U.S. House Elections.” *American Journal of Political Science*, 35(1): 110-38.

Kuklinski, James. 1973. “Cumulative and Plurality Voting: an Analysis of Illinois’ Unique Electoral System.” *Western Political Quarterly*, 26: 726-46.

Linehan, William, and Philip Schrodt. 1977. “A New Test of the Cube Law.” *Political Methodology*, 4: 353-67.

Maloney, John, Bernard Pearson, and Andrew Pickering. 2003. “Behind the cube rule: implications of, and evidence against a fractal electoral geography.” *Environment and Planning A* 35.8, 1405-14.

March, James. “Party Legislative Representation as a Function of Election Results.” *Public Opinion Quarterly*, 21(4): 521-42.

Mayhew, David. 1974. “Congressional Elections: the Case of the Vanishing Marginals.” *Polity*, 6: 295-317.

Monroe, Burt. 1994. *Electoral Science: The Analysis of Voting Systems.* D.Phil. thesis, University of Oxford.

Monroe, Burt. 1998. “Bias and responsiveness in multiparty and multigroup representation.” Paper presented to the Political Methodology Summer Meeting, University of California, San Diego.

Niemi, Richard, and Alan Abramowitz. 1994. “Partisan Redistricting and the 1992 Congressional Elections.” *Journal of Politics*, 56(3): 811-7.

Niemi, Richard, and Patrick Fett. 1986. “The Swing Ratio: an Explanation and an Assessment.” *Legislative Studies Quarterly*, 11(1): 75-90.

Niemi, Richard, and Simon Jackman. 1991. “Bias and Responsiveness in State Legislative Districting.” *Legislative Studies Quarterly,* 16(2): 183-202.

Niemi, Richard, and Laura Winsky. 1992. “The Persistence of Partisan Redistricting Effects in Congressional Elections in the 1970s and 1980s.” *Journal of Politics*, 54(2): 565-72.

Norris, Pippa, and Ivor Crewe. 1994. “Did the British Marginals Vanish? Proportionality and Exaggeration in the British Electoral System Revisited.” *Electoral Studies*, 13(3): 201–22

Powell, Guy Bingham. 2000. *Elections as Instruments of Democracy: Majoritarian and Proportional Visions*. New Haven: Yale University Press.

Schrodt, Philip. 1981. “A Statistical Study of the Cube Law in Five Electoral Systems.” *Political Methodology* 7: 31-53.

Spafford, Duff. 1970. “The Electoral System of Canada.” *American Political Science Review,* 64(1): 168-76.

Stokes, Donald. 1993. *Legislative Redistricting by the New Jersey Plan*. New Jersey: The Fund for New Jersey.

Swayze, Mikael. 1996. “Continuity and Change in the 1993 Canadian General Election.” *Canadian Journal of Political Science,* 29: 555-66.

Tufte, Edward. 1973. “The Relationship Between Seats and Votes in Two-Party Systems.” *American Political Science Review,* 67(2): 540-54.

Tufte, Edward. 1974. “Communications.” *American Political Science Review* 68(1): 211-3.

Tufte, Edward. 1975. “Determinants of the Outcomes of Midterm Congressional Elections.” *American Political Science Review,* 69(3): 812-26.

Vowles, Jack. 2008. “Systemic failure, coordination, and contingencies: understanding electoral system change in New Zealand.” In *To Keep or To Change First Past The Post? The Politics of Electoral Reform*, ed. André Blais. Oxford University Press, 163-83.

Zucco, Cesar. 2007. “Where’s the bias? A reassessment of the Chilean electoral system.” *Electoral Studies* 26(2): 303-14.

TABLE 1: *Exaggeration and bias (actual and regression-based) in simulated elections*

	k (actual)	*k* (regr.)	*b* (actual)	*b* (regr.)	Absolute error in *k*	Absolute error in *b*
Simulation 1: constant exaggeration, constant bias						
mean	2.00	2.00	1.00	1.00	0.00	0.00
Std Dev	*(0.00)*	*(0.00)*	*(0.00)*	*(0.00)*	*(0.00)*	*(0.00)*
min	2.00	2.00	1.00	1.00	0.00	0.00
max	2.00	2.00	1.00	1.00	0.00	0.00
Simulation 2: variable exaggeration, constant bias						
mean	2.00	2.00	1.00	1.00	0.05	0.005
Std Dev	*(0.07)*	*(0.09)*	*(0.00)*	*(0.01)*	*(0.04)*	*(0.004)*
min	1.83	1.75	1.00	0.99	0.00	0.00
max	2.17	2.21	1.00	1.01	0.19	0.01
Simulation 3: constant exaggeration, variable bias						
mean	2.00	2.01	1.00	1.00	0.25	0.01
Std Dev	*(0.00)*	*(0.36)*	*(0.03)*	*(0.03)*	*(0.25)*	*(0.01)*
min	2.00	1.05	0.94	0.88	0.00	0.00
max	2.00	3.87	1.07	1.08	1.87	0.10
Simulation 4: variable exaggeration, variable bias						
mean	2.00	2.03	0.99	0.99	0.22	0.01
Std Dev	*(0.06)*	*(0.28)*	*(0.03)*	*(0.03)*	*(0.16)*	*(0.01)*
min	1.84	1.26	0.94	0.92	0.00	0.00
max	2.16	2.64	1.07	1.06	0.70	0.07

N is 100 for each simulation. *k* is the responsiveness index, *b* is the bias index. 'Actual' refers to the simulated election results, 'regr.' refers to the multi-election regression estimates applied to the same results.

TABLE 2: *Statistical indicators for simulations 2 to 4*

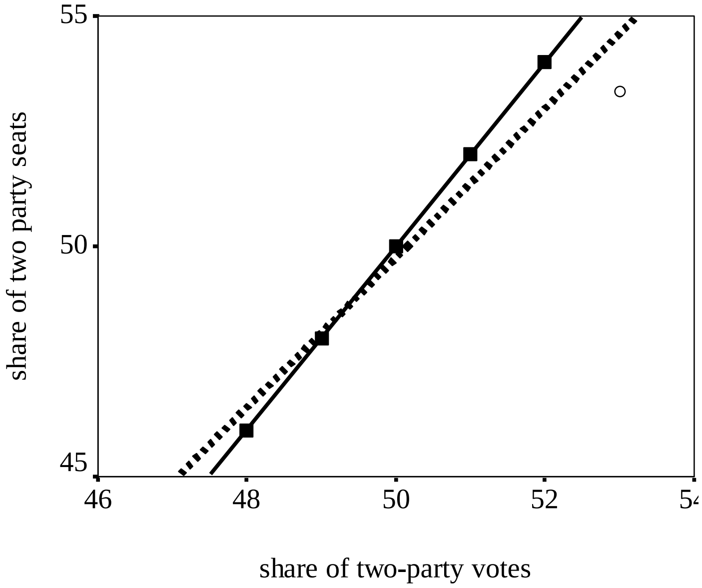
	R^2	standard error of the regression
Simulation 1: constant exaggeration, constant bias		
mean	1.00	0.00
Std Dev	*(0.00)*	*(0.00)*
Simulation 2: variable exaggeration, constant bias		
mean	0.996	0.014
Std Dev	*(0.005)*	*(0.007)*
Simulation 3: constant exaggeration, variable bias		
mean	0.94	0.05
Std Dev	*(0.09)*	*(0.02)*
Simulation 4: variable exaggeration, variable bias		
mean	0.94	0.06
Std Dev	*(0.09)*	*(0.02)*

TABLE 3: *Correlations between statistical indicators and errors in estimating exaggeration and bias*

	Correlation with				
	b error	R^2	s.e. regr	*k* Std Dev	*b* Std Dev
Simulation 2 (variable exaggeration, constant bias)					
k error	0.27*	–0.004	–0.02	0.24*	–
(s.e.)	*(0.007)*	*(0.97)*	*(0.85)*	*(0.02)*	
b error	–	–0.16	0.20*	0.31*	
(s.e.)		*(0.88)*	*(0.05)*	*(0.001)*	
Simulation 3 (constant exaggeration, variable bias)					
k error	0.77*	–0.30*	–0.17	–	–0.04
(s.e.)	*(0.00)*	*(0.03)*	*(0.09)*		*(0.68)*
b error	–	–0.35*	–0.02	–	–0.04
(s.e.)		*(0.00)*	*(0.84)*		*(0.70)*
Simulation 4 (variable exaggeration, variable bias)					
k error	0.74*	0.02	–0.25*	0.02	0.13
(s.e.)	*(0.00)*	*(0.81)*	*(0.01)*	*(0.82)*	*(0.21)*
b error	–	0.01	–0.27*	0.08	–0.02
(s.e.)		*(0.95)*	*(0.007)*	*(0.46)*	*(0.83)*

* $p < 0.05$

FIGURE 1: *How changing bias contaminates estimates of exaggeration*



Line 1 (squares) $k = 2.0$, $b = 1.0$

Line 2 (squares + circle) $k = 1.6$, $b = 0.99$