The Theory of the Midterm Referendum

The midterm is neither a mystery nor an automatic swing of the pendulum; the midterm vote is a referendum.
—Edward R. Tufte (1975, 862)

Referenda theories of midterm elections stand in sharp contrast to the theories centered on presidential elections. Rather than focusing on the circumstances of the presidential election, referenda theories attempt to explain midterm losses by examining conditions at the midterm itself. The results of midterm elections can be interpreted, from this perspective, as the voting public's judgment of the current administration's effectiveness on the job.

Since its introduction in the mid-1970s the referenda theory has replaced the theory of surge and decline as the accepted wisdom regarding national forces in midterm elections. In reviewing the literature on congressional elections, Barbara Hinckley comments that the presidential-election-centered explanations of midterms are "no longer widely accepted by political scientists" (1981, 115), and Gary Jacobson and Samuel Kernell observe that "the surge and decline view of midterm elections has been eclipsed by the more fashionable economic voting theory" (1981, 64)—the economic voting theory being one aspect of the more general referenda theory. In another overview of the field, Herbert Asher, after briefly reviewing the surge-and-decline theory, concludes that "today the dominant portrait of congressional elections is far different" (1983, 368). Assessments of individual midterm elections, such as the 1982 midterm (Mann and Ornstein 1983, 138-40), are now almost entirely conducted from the referenda theory's viewpoint. The theory has even been applied successfully outside the American context. In a study of government vote losses in British midterm by-elections from 1950 to 1983, Anthony Mughan concludes that "by-elections can quite legitimately be interpreted as national referendums on government performance" (1986, 773). The final testimony to the displacement of surge and decline is, as we will shortly see, the sheer volume of research devoted to testing, refining, and otherwise modifying the basic referenda model.
The Basic Referenda Theory

Edward Tufte (1975, 1978) authored the seminal works espousing the referenda theory of midterm elections. Tufte first presented his referenda model in "Determinants of the Outcomes of Midterm Congressional Elections" (1975) and shortly thereafter extended his analysis in Political Control of the Economy (1978, chap. 5). Together these reports constitute the centerpiece of work in the referenda strain of research on midterm elections.

Two aspects of Tufte's analysis require discussion. First, I address the specification of the referenda model itself, the initial evidence marshaled on its behalf, and the more recent analysis raising questions about the model. Second, and just as importantly, I examine Tufte's claims about his model and its relationship to surge and decline.

Tufte succinctly states the rationale for the referenda theory as follows: "Because there are no other targets available at the midterm, it is not unreasonable to expect that some voters opposed to the president might take out their dissatisfaction with the incumbent administration on the congressional candidates of the president's party" (1975, 813). The extent of voter displeasure with the incumbent administration may of course vary from one midterm to the next, thus accounting for variation in the extent of midterm losses.

What might indicate the extent of public satisfaction or dissatisfaction with the president at the midterm? Tufte proposes two indicators. The first indicator is quite straightforward: the public's expression of approval for the incumbent president's handling of his job as measured in national surveys. Since the late 1930s the Gallup Poll on nearly a monthly basis has asked a national sample of citizens whether they approve of the president's job performance. The question's standard wording since 1945 is "Do you approve or disapprove of the way (the president) is handling his job as president?" The percentage of the sample indicating approval of the president's job performance in September before the November midterm election, his approval rating, is the aggregate indicator of midterm satisfaction. The second indicator of sentiment for or against the administration is an economic index, the annual change in real disposable personal income per capita between the year of the midterm and the previous year (1975, 817). Following the work of Gerald Kramer (1971) and others, Tufte hypothesizes that a midterm verdict would be at least partly a verdict on the adequacy of economic progress made under the administration. With these two indicators of public satisfaction, Tufte offers a joint hypothesis: "The lower the approval rating of the incumbent president and the less prosperous the economy, the greater the loss of support for the president's party in midterm congressional elections" (1975, 817).
Two aspects of Tufte's construction of the dependent variable in his analysis deserve note. First, he elects to conduct his analysis in two steps. The first step seeks to explain the congressional vote in terms of presidential popularity and economic conditions. The second step translates the congressional votes into seats via a swing ratio. This is somewhat problematic since the swing ratio fluctuates a good bit over time. Second, Tufte's measure of the congressional vote is a "standardized vote loss by president's party" in the midterm. He computes the standardized-vote-loss measure as the difference between the party's national congressional vote and the party's normal congressional vote, and he computes a party's normal congressional vote as the average of its vote in the preceding eight elections. Tufte examines deviations from the normal vote to control for the varying strength of the Democratic and Republican parties over time. Some control for the normal vote is required so that judgments at the midterm are not confused with standing commitments to the parties. Without this standardization or deviation measure, the majority party would appear to receive a greater vote than it deserves from the public's referenda judgment and the minority party would appear to receive a smaller vote than the referenda judgment would merit. The standardized vote loss predicted by the economy and popularity variables can be translated into a predicted vote for the party by adding the normal vote to the standardized vote loss. ¹

With the referenda equation of midterm vote losses specified, Tufte proceeds to conduct a very thorough analysis yielding findings strongly supportive of the referenda theory. He produces five findings in support of the referenda explanation: (1) The two referenda independent variables—presidential popularity and economic change—account for 91 percent of the variance (adjusted R² = .88) in the standardized vote loss in midterms between 1938 and 1970, excluding the 1942 midterm for lack of a presidential-popularity reading (Tufte 1975, 818, table 2). The equation estimates are presented in table 4.1. (2) The coefficients for both variables are statistically significant (at the .01 level) and consistent with estimates independently arrived at by Kramer (in the case of the economic variable) and Kemell (in the case of the presidential-popularity variable). (3) Tufte shows these coefficients and the overall fit of the model not to be overly sensitive to particular midterms in the analysis. Tufte's reestimate of the equation with different midterms omitted from the analysis shows the model to be quite stable (819). (4) The postdictions, or after-the-fact expected votes based on the equation, prove to be quite accurate. The average absolute error of the postdictions was only .6 percentage points (819, table 3), a good deal more accurate than Gallup's direct measure of the national congressional vote. (5) The equation also yields quite accurate predictions. Tufte generates predictions for the five more recent midterms in his study from
Table 4.1. Referenda Equations of Midterm Losses, 1946-1994

<table>
<thead>
<tr>
<th>Independent variables</th>
<th>(1) Std. vote loss (1946-70)</th>
<th>(2) Std. vote loss (1946-74)</th>
<th>(3) Std. vote loss (1946-94)</th>
<th>(4) Cong. vote loss (1946-94)</th>
<th>(5) Seat loss (1946-94)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Constant</td>
<td>-11.08</td>
<td>-10.74</td>
<td>-10.73</td>
<td>2.93</td>
<td>-92.05</td>
</tr>
<tr>
<td>Presidential popularity</td>
<td>.13</td>
<td>.13</td>
<td>.14</td>
<td>.11</td>
<td>1.17</td>
</tr>
<tr>
<td></td>
<td>(4.03)</td>
<td>(3.00)</td>
<td>(2.36)</td>
<td>(1.57)</td>
<td>(2.72)</td>
</tr>
<tr>
<td>Economic conditions</td>
<td>.04</td>
<td>.62</td>
<td>.45</td>
<td>.56</td>
<td>1.92</td>
</tr>
<tr>
<td></td>
<td>(5.83)</td>
<td>(3.75)</td>
<td>(1.94)</td>
<td>(2.26)</td>
<td>(1.18)</td>
</tr>
<tr>
<td>Normal vote (8 election ave.)</td>
<td>—</td>
<td>—</td>
<td>—</td>
<td>.76</td>
<td>—</td>
</tr>
</tbody>
</table>

Number of cases: 8 8 13 13 13
R^2: .91 .83 .54 .77 .51
Adjusted R^2: .88 — .45 .69 .42
Standard Error: — — 2.14 2.11 15.01

Source: Equation 1 is from Tufte 1975, table 2. This equation also includes the 1938 midterm. Equation 2 is from Tufte 1978, table 5.2. Equations 3, 4, and 5 are estimated by the author.

Note: The figures in parentheses are t-ratios. Presidential popularity is the Gallup approval rating for the president at the time of the election. Economic conditions are the percentage change in real disposable income per capita, as reported in the U.S. Bureau of Economic Analysis, National Income and Product Accounts of the United States (the volumes for 1929-58 and 1959-88) and in the Survey of Current Business, July 1994.

Since Tufte's initial exposition of the referenda theory there has been some erosion in the evidence supporting it. The first indication that the referenda equation is not as complete an explanation of midterm elections as Tufte's original analysis suggests is in Tufte's own reanalysis of the equation (1978, 112). In this reanalysis Tufte drops the 1938 midterm case (because the wording of the Gallup presidential-popularity question deviated from the standard wording that year) and adds the 1974 midterm case. After substituting the 1974 case for the 1938 case, the fit of the equation drops considerably (see equation 2 in table 4.1), from accounting for 91 percent to 83 percent of the variance. The percentage of the variance in...
the standardized vote loss accounted for by the economy and presidential-popularity measures declines still further with the addition of the 1978, 1982, 1986, 1990, and 1994 midterms (equation 3 in table 4.1). The equation accounts for about half of the variance for midterms from 1946 to 1994 (adjusted $R^2 = .45$). The results are substantively similar if we exclude the unusual 1994 midterm.

Tufte's equation appears somewhat stronger over the period from 1946 to 1994 when the actual congressional vote is examined as the dependent variable (as opposed to standardized vote loss or vote change) and the normal vote is included separately as an independent variable. This equation, including presidential popularity, economic change, and the normal vote, is estimated as equation 4 in table 4.1. As specified, this equation accounts for 77 percent (adjusted $R^2 = .69$) of the variation in the midterm congressional vote for the president's party, though presidential popularity surprisingly falls short of conventional levels of statistical significance.

In its focus on the vote loss at the midterm, Tufte's analysis stops short of explaining variance in seat losses. The analysis examines how votes translated into seats, noting a highly erratic and generally declining swing ratio, but does not attempt to explain seat losses directly. The referenda equation constructed by Tufte has been used, nevertheless, to generate seat-loss predictions. These predictions in several recent midterms have been significantly off the mark (see Jacobson 1983b, 6; Mann and Ornstein 1983, 140; Witt 1983, 49). The strength of the referenda equation in explaining midterm seat losses can be assessed more systematically by respecifying the equation so that it accounts directly for seat losses rather than for the standardized vote loss. I have constructed and estimated such an equation, and the results appear as equation 5 in table 4.1. The referenda equation adapted to account for seat losses directly seems to be a modest success. It explains about half the variance (adjusted $R^2 = .42$) in midterm seat losses from 1946 to 1994. However, in six of the thirteen midterms the equation is more than 14 seats in error. In the 1958 midterm, for instance, the equation yields an expected seat loss that is 22 seats less than the actual loss (27 lost seats expected vs. 49 actual). The average absolute error over these thirteen midterms is 11.5 seats.

Despite these recently apparent shortcomings, the point emerging from the reanalysis of Tufte's referenda theory is not that the theory now fails to be supported by the data. On the contrary, the evidence continues to show that midterms are, to some significant degree, referenda on the administration's performance. However, the theory as originally operationalized does not appear quite as strong as it did in Tufte's original analysis. While considerable effort has been devoted to strengthening the
specification of the referenda theory, I will argue that, however revised or modified, a purely referenda explanation of midterm losses is at best a seriously incomplete explanation.\(^2\)

What is the relationship between Tufte's referenda theory and Angus Campbell's surge-and-decline theory of midterm elections? Obviously, they both explain midterm outcomes in terms of national political forces, but they disagree as to whether the relevant forces are at the midterm or at the prior on-year election. But should these theories be seen as competing with one another, as complementing one another, or as basically addressing different questions about the nature of midterm-election outcomes? Tufte offers some interesting observations about the relationship of his referenda theory to the theory of surge and decline.

According to Tufte, the two theories address two essentially separate and distinct questions about midterm elections. Surge and decline, as he interprets it, explains why the president's party consistently loses votes and seats in midterm elections. In Tufte's own words, "It explains why the president's party should almost always be operating in the loss column, it does not account for the number of votes and seats lost by the president's party. In statistical parlance, the adjustment model [surge and decline] of midterm congressional elections explains the location of the mean rather than variability about the mean" (1975, 813). In contrast, the referenda theory seeks to explain the variability of midterm losses, though in his conclusion Tufte speculates that the referenda theory is also a partial explanation of the consistency of presidential-party midterm losses. Tufte explains that two reasons presidents do not do well in midterm elections is that their popularity generally "has declined since the prior on-year election and because the economy is performing less well at the time of the midterm than it was two years earlier during the presidential election" (Tufte 1975, 826). He goes on to speculate that "a satisfactory explanation of why the president's party always operates in the loss column in off-years will grow from a combination of the midterm [referenda] model and a revised version of Campbell's 'surge and decline' model (which, in revision, might place more emphasis on the surge and decline of coattail effects and less on turnout effects" (1975, 826). In effect, Tufte suggests that the two theories might well be considered complementary in answering the consistency question about midterm losses.

While his speculation about the complementary nature of the two theories with respect to explaining the consistency of midterm losses is an important insight, there is one major flaw in Tufte's analysis of the relationship between the two theories. Tufte mistakenly interprets the theory of surge and decline as speaking only to the question of the consistency of midterm losses and not to the question of their variability. But all surges are not equal. There are surges, and then there are surges. Losses
following a narrow presidential victory should be small compared to those following a landslide victory.

The Development of the Referenda Theory

Tufte’s seminal works on the referenda theory of midterm elections laid the foundation for a great deal of research. The character of this research has varied. Some scholars have attempted to reconfirm and extend the initial findings or have offered minor refinements. Others have attempted to elaborate or to amend more significantly the original theory. In this section I address five efforts to improve upon the original referenda model.

The negative-voting hypothesis. Writing at about the same time as Tufte, Samuel Kernell (1977) offers an intriguing amendment to or elaboration of the referenda theory. Based on the findings of The American Voter (Campbell et al. 1960, 554-56) and more generally on social psychology, Kernell argues that negative impressions are more salient to voters than positive impressions. As applied to midterm elections, this means that the referendum on the administration’s performance in office is not an unbiased one; it is biased against the president’s party. This negative bias plays itself out in three ways: (1) The negativity thesis hypothesizes, as John Mueller (1973) and James Stimson (1976) find and as Tufte (1975, 826) also observes, that the popularity of the president tends to decline by the time of the midterm. It is generally suspected that the public becomes disenchanted when the high expectations raised in the administration’s initial honeymoon period are not met. In addition, Kernell hypothesizes that the public has a tendency to focus on incumbents’ failures. Achievements by incumbents are taken for granted. (2) The negativity thesis also hypothesizes that disgruntled potential voters are more likely to turn out at the midterm than potential voters who are satisfied with the administration’s performance. Displeasure with the current administration activates voters, while satisfaction engenders complacency. (3) Finally, the negativity thesis hypothesizes that the greater salience of negative evaluations affects defection rates. Partisans of the president’s party who disapprove of the president’s performance are more prone to defect than partisans of the opposition party who approve of the president’s performance.

Kernell finds evidence generally supporting these negativity hypotheses in an analysis of six midterm-election polls from 1946 to 1966. As expected, those disapproving of the president at the midterm turned out and defected at greater rates. Further research, however, has not been so supportive. Albert Cover (1986b) performed a multivariate test of the negativity hypotheses with NES data from the 1974, 1978, and 1982 midterm
surveys. Although Kernell discusses a multivariate analysis in an appendix to his article, most of the evidence he presents in support of the hypotheses is in the nature of contingency-table analysis. Contrary to Kernell’s analysis, Cover finds that “negative evaluations of the president do not appear to motivate voters to turn out in disproportionately higher numbers in midterm congressional elections” (1986b, 800) and finds only “some support for the notion that disapproving partisans are less likely to vote for the in-party’s congressional candidates than are other partisans” (1986b, 795–96).

Although Cover’s analysis fails to strongly support the negativity hypotheses, Robert Erikson’s analysis (1988) concludes in favor of the negative-voting theory. Erikson examines a variety of theories about the relationship between the congressional votes in succeeding on-year and midterm elections. In each of the ten midterms from 1946 to 1982, Erikson estimates that negative voting cost the president’s party between nine and ten percentage points of the national congressional vote. Extending the analysis back to the 1904 midterm (n = 21), he calculates that negative voting cost the president’s party about 7.7 percent of the vote in each midterm. The equation explains the Democratic midterm congressional vote as a function of the prior on-year Democratic congressional vote and a dummy variable for the president’s party (equal to one for Democratic administrations). Erikson interprets the coefficient for party dummy variable as the in-party penalty attributable to negative voting. The equation accounts for 70 percent of the variance in the midterm congressional vote for Democrats.

There are two major problems with Erikson’s analysis. First, the significance of the simple dummy variable for the president’s party is consistent with several different interpretations of midterm elections besides the negative-voting interpretation. The dummy only reveals the average midterm vote loss for the president’s party. Erikson’s equation links the party dummy to a decline in the midterm vote. Or, in other words, the party holding the presidency loses votes at the midterm. But this is just what all the various theories, surge and decline as well as the negative-voting theory, seek to explain. The equation does not indicate in any way whether the average midterm vote loss is due to negative voting or to any other cause or set of causes, including those suggested by surge and decline. In fact, as one might well expect, the presidential vote—the principal indicator and independent variable in the aggregate model of surge and decline—is strongly and positively related to Erikson’s control party dummy variable (r = .84 from 1900 to 1984).

Second, while Erikson specifies the impact of negative voting as a constant by using a dummy variable for the in-party penalty, Kernell’s theory of negative voting suggests that the negative-voting penalty is variable.
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The extent to which the in-party is penalized should depend on the popularity of the president, and presidents are not equally popular or unpopular with midterm electorates. As Kernell explains, "High presidential popularity reduces the deleterious consequences largely to the degree that it limits the number of voters who are dissatisfied (or at least attribute blame and are thus likely to engage in negative voting). . . . Low popularity, according to this model, indicates that a large proportion of the electorate may be disposed toward negative voting" (1977, 52). That is, the negative-voting thesis, according to Kernell, is not an alternative to the presidential-popularity/economy referendum interpretation of midterm losses, as Erikson perceives it, but is instead a variant of the basic referendum model. 6

Although still in dispute, the negative-voting theory is an interesting elaboration of the referendum theory of midterms. Like surge and decline, the referendum theory with the negative-voting thesis traces the developments of the midterm election to turnout decisions as well as to partisan-defection decisions. Perhaps more importantly, although the negative-voting thesis does not alter the referendum model's explanation of why some midterm losses are greater than others, it does help the referendum theory explain why the president's party consistently operates in the loss column in midterms. In a presidential election the public reacts to both parties and the greater salience of negative impressions affects the parties about evenly. In midterms the focus is only on the president's party, and whatever shortcomings the president is perceived as having are magnified by the greater salience of the negative. 7

The strategic politician. Gary Jacobson and Samuel Kernell (1981) offer a very interesting and quite different revision to the referendum theory. They argue that the referendum theory as originally formulated "is deficient for failing to recognize the prior, independent role of politicians in systematically structuring voters' choices" (1981, 65). That is, the original referendum theory—and for that matter the theory of surge and decline—explains midterm-election outcomes entirely in terms of the voters' behavior. The decisions of politicians are ignored. Jacobson and Kernell argue that this is a major oversight. Politicians make decisions that influence midterm outcomes, and there is reason to suspect that the strategic behavior of potential congressional candidates systematically affects aggregate midterm-election results. In essence, Jacobson and Kernell offer an elite version of the referendum theory.

The core of their argument concerns candidate recruitment and the decisions of potential congressional candidates on whether or not to throw their hats into the ring. Given the tremendous investment of time, energy, and money needed to run a serious congressional campaign, potential
candidates, as a group, are quite deliberate in making the decision to run. It is not a decision to be made lightly or without a careful reading of the political climate. While rationality and information assumptions may apply broadly to potential candidates, all potential candidates are not equal and will not necessarily make the same decision to run under the same circumstances. Some are more savvy than others. Some have more to lose than others. Some would run a stronger campaign than others. Jacobson and Kernell (1981) hypothesize a pattern: “Ambitious career politicians looking to enter or move up the hierarchy of elective offices are likely to be the most formidable challengers. But they also have the most to risk in the attempt; defeat is at best a setback, at worst ends the career. Thus the best candidates will also be the most cautious in deciding when to run for higher office. They will be most sensitive to the odds on winning and most aware of the factors that affect those odds” (424).

When conditions appear favorable for a party, more of the stronger potential congressional candidates from that party may be tempted to take the plunge. The prospect that their party label may provide more of a help than a hindrance, or more of a help than it would in other election years, may entice some candidates to run who wouldn’t take the gamble under other circumstances. Campaign activists and contributors might make similar calculations, choosing to work harder or contribute more when there is reason to be optimistic about the party’s chance of victory. Conversely, when conditions look bleak for a party, strong potential candidates may be more inclined to postpone making the run until the partisan climate appears more favorable. Similarly, campaign workers and contributors may be reluctant to invest their time and money in a campaign fighting such an uphill battle. In short, the political climate of a campaign may encourage or discourage strong candidates from entering the field.

What are the measures of the political climate in midterm elections? What national factors might potential candidates believe would affect their odds of winning the election? Like Tufte, Jacobson and Kernell claim that the political climate can be measured by the popularity of the president and the condition of the economy. When all things are equal—such as the efforts and financing of the national parties (1982, 428-30)—and when the president is popular and the economy is healthy, the president’s party will find it easier to recruit strong candidates to the ticket and the opposition party will find it more difficult. As Jacobson and Kernell put it, “The party expected to have a good year will field a larger proportion of high-quality challengers with well-financed campaigns, while the opposing party is stuck with a disproportionate number of feeble challengers lacking the resources for a serious campaign” (1982, 425).
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Initially, Jacobson and Kernell offer only an indirect test of their thesis. Since potential candidates must decide whether to run well before voters must decide for whom they will vote, they hypothesize that if the real referendum is being conducted at the candidate rather than the voter level, earlier rather than later indicators of the political climate should be more strongly associated with midterm results. They construct and examine three referendum equations: one with political-climate variables (economic change and presidential popularity) in the fall just before the midterm; one with the political climate in the spring, when candidates would be deciding whether to run; and one with both spring and fall indicators. Although they conclude that the analysis "presents strong evidence for the counterintuitive prediction that the more distant spring political environment will have a greater effect on the election" and therefore supports the strategic-politician revision, the evidence is ambiguous. While the spring model accounts for a greater portion of the variance in the standardized-vote-loss measure than the fall model, the difference is not very large (adjusted $R^2 = .72$ and .65, respectively). Moreover, though the economy matters more in the spring than in the fall, just the reverse is true of presidential popularity. The fall measure of presidential popularity is stronger than the earlier spring measure, contrary to the strategic-politician hypothesis but in accord with the voter-referenda hypothesis.

Richard Born's examination of the strategic-politician thesis reaches a very different conclusion. Examining aggregate data from 1946 to 1982 and individual-level data from 1966 to 1982, Born concludes that "the theory does not stand up to empirical investigation" (1986, 599). Born's adjustment to Tufte's original referendum model, looking at economic change over eighteen months rather than a year and averaging presidential popularity over the final three months of the campaign, accounts for a greater percentage of variance in the standardized vote loss than the strategic-politician referendum model (adjusted $R^2 = .643$ and 594, respectively). His individual-level analysis also fails to support the strategic-politician thesis. The quality of the challenger, as measured by his having previously held elective office, does not significantly affect the vote choice in any of the five midterms Born examines.

After his original analysis with Kernell (1981) and again after Born's counterfindings, Jacobson has produced additional support for the thesis (1982, 1989). Measuring candidate quality by whether or not the candidate had ever held elective office, he finds evidence of two critical links in the strategic-politician thesis in both aggregate and district analyses of elections from 1946 to 1986. First, as in Jon Bond, Cary Covington, and Richard Fleisher's (1985) analysis of challenger quality in the 1980 elections, Jacobson (1989, 779-80) finds that the national political climate
affects a party’s attraction of quality challengers, though this effect is not significant for Republicans. Second, he finds that the relative quality of the parties’ challengers significantly affects both vote margins and election outcomes (Jacobson 1982; 1989, 781-87).

Although the status of the strategic-politician thesis is still open to question, the evidence on balance suggests that candidate recruitment is sensitive to political conditions and has an impact on election results. As such, and as specified in Jacobson’s analysis (1989), the thesis would appear to supplement the original referendum theory based on voter reactions. It is important to bear in mind that the decisions of strategic politicians magnify the consequences of the political climate rather than entirely displacing voters as judges of midterm conditions. If candidates behave in ways that anticipate voter reactions that never materialize, the strategic-politician thesis amounts to a fragile self-fulfilling prophecy of supposedly well-informed and strategic politicians systematically making career decisions on the faulty premise that midterm conditions really matter to voters. If voters don’t react to the political climate, there is little reason for well-informed strategic politicians to enter anticipated reactions into their deliberations. If politicians are not fools, they will only anticipate voter reactions that ultimately will affect the vote.8

**Evaluations of party competence.** In a series of articles, Albert Cover and his colleagues (Cover 1986a, 1986b; Abramowitz, Cover, and Norpoth 1986) suggest that the referenda model as originally specified omitted an important variable, public evaluations of party competence, the party’s relative ability to handle the nation’s most serious problems. These evaluations, according to Abramowitz, Cover, and Norpoth, “are the linchpin of the midterm election model” (570).

In Abramowitz, Cover, and Norpoth’s revised referenda model, evaluations of party competence mediate the effects of the two principal exogenous variables, the economic-growth variable and the presidential-popularity variable, on the ultimate dependent variable, the standardized-vote-loss measure employed by Tufte (1975). There is considerable empirical support for the party-competence referenda model. In examining the nine midterms from 1946 to 1982 (excluding 1954 because of missing data), Abramowitz, Cover, and Norpoth find (1) that the exogenous variables of the economy and presidential popularity strongly affect evaluations of party competence (betas of .54 and .52, respectively) and (2) that party-competence evaluations are a very strong influence on the standardized vote loss (beta of .88). They also find that party-competence evaluations help to explain the greater losses that have been sustained by the parties of presidents serving in their second terms. In an examination of individual-level survey data, they find evidence supporting the impor-
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tance of party-competence evaluations. They examine determinants of the congressional-vote choice by a probit analysis of NES data for 1974, 1978, and 1982 and find that party-evaluation effects on the vote choice are statistically significant in two of the three elections (1974 and 1982). Cover (1986a) extended the analysis to include presidential-year elections and found further evidence of the impact of party-competence evaluations. Party competence continued to have a strong direct effect on the standardized vote loss in the aggregate-time-series analysis and was significant in five of the six (1972 to 1982) individual-level, cross-sectional probit analyses of the vote choice (the sole exception again being 1978).

In evaluating the party-competence amendment to the original referenda model a few points should be kept in mind. First, the addition of party-competence evaluations to the model does not alter the model in any basic sense. That is, since party-competence evaluations are inserted as an intervening variable between the exogenous variables and the election-outcome variable, their role is to elaborate the mechanism by which economic growth and presidential popularity are translated into congressional-vote choices. Second, the causal standing of party-competence evaluations is open to question. Advocates for the incorporation of these evaluations in the referenda model assume that these evaluations affect the vote choice and are not affected by the vote choice. It is quite plausible, however, that party-competence evaluations may in part be rationalizations of the vote. That is, voters who have decided to vote for a party’s congressional candidate may be inclined to report, simply out of a desire for consistency, that that party is the most able to solve the most important national problem. Last, although the inclusion of party-competence evaluations improves the fit of the referenda model, the improvement is not dramatic. Over the nine midterms considered, party-competence evaluations directly account for 78 percent of the variance (adjusted $R^2 = .74$) in the standardized vote loss. As a point of comparison, over the same set of nine midterms from 1946 to 1982 (also excluding 1954), the original version of the referenda model with the state of the economy and presidential popularity accounts for 71 percent of the variance (adjusted $R^2 = .61$).

Despite these reservations about the value of introducing party-competence evaluations into the midterm-referenda model, the party-competence variable may still shed some light on the public’s referendum judgment. In particular, it may help to illuminate the role of the economy in that judgment. In a separate piece of research and using somewhat different terminology, John Petrocik and Frederick Steeper (1986) reach a conclusion about the need for an overall subjective evaluation of performance that in many ways is similar to the Cover (1986a) argument. Examining the 1982 midterm election, Petrocik and Steeper argue that many voters
did not fully attribute the blame for the 1982 economic recession to President Reagan and the Republican Party. Some voters believed that Reagan had inherited an economic mess from the Carter administration. Any analysis that just examined the cold economic figures in 1982 would miss how they were being politically translated by the public. A model using only objective economic-change data would incorrectly assume that a first-term Reagan presidency would be punished as much for the 1982 recession as a second-term Carter presidency.¹⁰

The exposure thesis. Bruce Oppenheimer, James Stimson, and Richard Waterman (1986; Waterman 1990; Waterman, Oppenheimer, and Stimson 1991) offer another modification to the referenda theory: the exposure thesis. The exposure thesis adapts and uses the referenda theory rather than the standardized vote loss to explain the loss of seats at the midterm by the president’s party and introduces a new variable into the referenda equation, the exposure variable. The exposure variable is a measure of the extent to which a party’s pre-election seat holdings exceed or fall short of its normal share of seats. If a party typically has held 254 of the 435 House seats, as Oppenheimer, Stimson, and Waterman calculate the Democrats have held since 1938, then its holding fewer than 254 seats going into an election would constitute an underexposure of the difference between the normal share and the actual number of seats. Conversely, if the party held more than the 254 seats going into an election, it would be overexposed.

According to the exposure thesis, all things being equal, a party that is underexposed should find it easier to gain seats than a party overexposed. An underexposed party has a number of seats to gain back that it normally holds. This is a much easier job than that faced by the overexposed party, which would do well simply to preserve the status quo. Generally, the overexposed party should expect to lose some of its surplus at the next election as the system returns to equilibrium.

Oppenheimer, Stimson, and Waterman (1986) conduct an aggregate analysis of the impact of a party’s exposure on its seat change in both on-year and midterm elections from 1938 to 1984. They examine equations with the exposure variable alone, in conjunction with a midterm dummy variable, and incorporate it into both Tufte’s and Jacobson and Kernell’s (spring measures) referenda equations. In each case the exposure variable proves strongly negative and statistically significant, as expected by the thesis. The exposure variable alone accounts for nearly one-half of the variance in seat changes in the twenty-four congressional-election years they examine from 1938 to 1984 (adjusted R² of .46). Moreover, it substantially improves the fit of the two differently timed referenda equations. The addition of the exposure variable to the Tufte equation, using fall measures of the political climate, improves the proportion of explained variance in seat
Table 4.2. Seat Changes Explained by the Exposure Thesis, 1938-1994

<table>
<thead>
<tr>
<th>Independent variables</th>
<th>Presidential- and midterm-election years</th>
<th>Only midterm elections (1946-94)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>1938-84</td>
<td>1944-94</td>
</tr>
<tr>
<td>Constant</td>
<td>-8.05</td>
<td>-6.86</td>
</tr>
<tr>
<td>Exposure</td>
<td>-.71</td>
<td>-.64</td>
</tr>
<tr>
<td></td>
<td>(4.47)</td>
<td>(2.91)</td>
</tr>
<tr>
<td>Presidential popularity</td>
<td>-</td>
<td>-</td>
</tr>
<tr>
<td>Economic conditions</td>
<td>-</td>
<td>1.17</td>
</tr>
<tr>
<td></td>
<td>(2.72)</td>
<td>(3.01)</td>
</tr>
<tr>
<td>Number of cases</td>
<td>24</td>
<td>26</td>
</tr>
<tr>
<td>R²</td>
<td>.48</td>
<td>.26</td>
</tr>
<tr>
<td>Adjusted R²</td>
<td>.46</td>
<td>.23</td>
</tr>
<tr>
<td>Standard error</td>
<td>-</td>
<td>27.01</td>
</tr>
</tbody>
</table>

Source: The estimates for the regression in the first column are from table 2 of Oppenheimer, Stimson, and Waterman 1986, 237. The other regressions are calculated by the author. Note: The figures in parentheses are t-ratios.

Although the exposure thesis is plausible, there are real questions about the extent and nature of its actual contribution to explaining midterm losses. The explanatory power of exposure is less impressive when more recent elections are considered. The second equation in table 4.2 updates the "exposure alone" equation for elections from 1944 to 1990. While the estimated effects of exposure remain negative and statistically significant, exposure now accounts for a bit less than one-quarter of the variation in seat change rather than the one-half estimated in the original analysis of the elections from 1938 to 1984.

While the evidence in support of the exposure thesis was never particularly strong and is even weaker in recent elections, the thesis fares even more poorly in explaining the extent of seat losses for the president's
party in midterm elections. This was apparent in Oppenheimer, Stimson, and Waterman's original test of the thesis. Their examination was conducted on all congressional elections without distinguishing those held in on-years from those held in midterms, except for the inclusion of a midterm dummy variable in one version of the equation. The implicit assumption is that exposure matters equally in on-year and midterm congressional elections. But this need not be and apparently is not the case. In their analysis of the equation explaining seat changes in terms only of the party’s exposure, Oppenheimer, Stimson, and Waterman examine the residuals for the twelve midterm elections in their series (1986, table 3). The absolute magnitude of these errors indicates that, at least in this bivariate analysis, the exposure variable contributes little, if anything, to explaining variation in midterm seat losses in the president’s party. In one midterm, 1946, the exposure-alone equation was nearly fifty-five seats in error. In six of the twelve midterms the error in the exposure-alone equation exceeded nineteen seats. The average absolute error for the equation over the twelve midterms from 1938 to 1982 was nearly nineteen seats, and even though its prediction for 1986 of a seven-seat loss was only two seats off the mark, the average error for the last six midterms (1966-86) was still thirteen seats. As a benchmark, consider that the average absolute error produced by simply predicting the average midterm seat loss would be just eighteen seats (from 1938 to 1982).

The weakness of the exposure thesis is also evident in the reanalysis of the “exposure only” model of seat change for only midterm elections from 1946 to 1994 in the third equation in table 4.2. The analysis indicates that exposure alone actually fails to account for any variance in midterm seat losses ($R^2 = .00$). Moreover, when the exposure variable is added to the conventional referenda equation, the variable’s effects fall short of statistical significance, though the explanatory power of the equation is increased a bit (the adjusted $R^2$ increases from .42 to 45). These results are substantively similar whether the unusual 1994 midterm is included or excluded from the analysis.

Where the exposure variable may contribute to understanding midterm elections is in the consistency, rather than the variability, of presidential-party losses. Although Oppenheimer, Stimson, and Waterman do not mention it in their analysis, the president’s party was overexposed and therefore more vulnerable to seat losses in eleven of the twelve midterm elections they considered. Why is the president’s party regularly overexposed going into a midterm? The answer would seem to reside with the events of the prior presidential election. The president’s party is overexposed going into the midterm because the political climate in the prior presidential election favored that party. Favorable short-term forces in the prior election determined which party won the presidency.
and spilled over to help that party win more than its normal share of congressional seats. In essence, the exposure thesis amounts to a partial elaboration of surge and decline or, perhaps, some other presidential-election theory of midterm-election losses.16

The lagged referendum. Michael Lewis-Beck and Tom Rice (1984) have constructed a version of Tufte’s referenda equation with the purpose of making it useful for forecasting election outcomes. Since the two political-climate variables are not available until very close to the midterm election or, in the case of the economic measure, actually well after the midterm, the original referenda equation is not useful for forecasting midterm-election outcomes before the fact. Lewis-Beck and Rice modified the original equation to make it useful for forecasting by taking earlier measurements of the political-climate independent variables (measuring the president’s popularity and the growth rate in the economy six months prior to the midterm) and by directly predicting seat losses rather than by converting standardized vote losses into seat losses with an estimated swing ratio. The early measurement resulted not only in an equation yielding forecasts but also in an equation somewhat more accurate in its predictions than the original version modified to predict seat losses directly. Whereas the original equation erred on average by about eleven seats from 1950 to 1982, the lagged equation erred on average by just eight seats over this same period. Still, an average error of plus or minus eight seats is a sizable spread, and on occasion the equation has been off by eighteen and nineteen seats. Although the lagged-referendum equation is more accurate than the original, there is certainly room for further improvement.

An Assessment of the Referenda Theory

Although the particulars of several of the suggested revisions to the referenda theory are still matters in dispute and although the evidence of recent midterms has not been as kind to the theory as the evidence of prior midterms examined by Tufte, the basic argument and operationalization of the theory seem now to be well confirmed. As Jacobson and Kernell and Lewis-Beck and Rice have separately shown, the timing of referenda decisions occurs somewhat earlier than originally envisioned, but there is little doubt that the decisions expressed in the midterm at least partially reflect the voters’ and, most probably, potential candidates’ judgments about the performance of the president. As Tufte claims, “The midterm vote is a referendum” (1975, 862).

Despite the significant evidence on its behalf, the referenda theory is flawed. In concentrating exclusively on the midterm itself and the
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short-term forces of that campaign, the theory fails to take into account the full context of the election. With the single exception of the exposure-thesis variation on the theory (a variation that has its own problems), the referenda theory ignores the political events of the preceding election. While this is a more glaring omission for those versions of the theory explicitly looking at change from the presidential election to the midterm (e.g., Lewis-Beck and Rice 1984), it is a problem inherent in the narrow focus of the referenda theory. The outcomes of midterm elections reflect referendum-like judgments by the voting public, but this is not all that they entail. The referenda story of midterm elections is an incomplete story. The story of a midterm election really begins in the prior presidential election.