Changing Public Policy: The Impact of Public Opinion, Antiwar Demonstrations, and War Costs on Senate Voting on Vietnam War Motions

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A new approach to the study of legislative change enables us to deal directly and quantitatively with questions about how long-term changes in public policy come about. The approach is applied to the aggregate change of mind by the U.S. Senate as it moved from support of the Vietnam war to opposition from 1964 to 1973. Substantively, cumulative war costs, public opinion, and antiwar demonstrations all had significant effects on Senate roll call outcomes, but they were so highly intercorrelated that their separate effects could not be disentangled. In addition, demonstrations taking place in the months before a vote had a slight positive impact on the number of dovish votes received by motions. The 1970 invasion of Cambodia seems to have led to a significant turning point in the way the Senate dealt with the war. The general strengths and weaknesses of the new approach are assessed. It opens a new area to statistical inquiry and generates a number of novel questions that should lead to additional research.

In this paper, we present a new approach to the study of legislative change. This approach enables us for the first time to deal directly and quantitatively with questions about how long-term changes in public policy—as defined in legislation—come about. To demonstrate the utility of the approach for dealing with issues of genuine importance, we have chosen to apply it to the Senate's voting on the Vietnam war.

THE STUDY OF LEGISLATIVE CHANGE

The Problem

Like many other issues, this was one on which Congress voted many times over a period of years; some motions were passed, others were rejected, and the legislation on the books at the end of the period was substantially dif-

ferent from what it had been at the beginning. What happened in between, and why?

Anyone interested in democratic politics would like precise answers to many questions about legislative change, for it is a key link in the democratic process—the point at which political preferences are (or are not) translated into new policy directions for a society. What is the impact of public opinion, the actions of interest groups and elected officials, and significant events? Does the importance of such factors vary over time? Why do some motions pass and others fail in a given session, and why do some fail one year only to be passed later? Answers to these and related questions would greatly increase our understanding of democratic political change, but unfortunately they are not now available.

We claim that this is the case because no one has previously developed a satisfactory way to study statistically the determinants of roll call outcomes over time. Scholars have made noteworthy progress in studying public policy, political change, and legislatures, but the following review of the literature will show that none of the advances really allows us to confront directly a central question: What leads to changes in legislative outcomes over time?

Past Work

Even the two bodies of work most relevant here—first, work on how political "inputs" are transformed into "outputs" and, second, studies of legislatures—do not deal directly with legislative change.

1. Of central importance is the literature which attempts to show how inputs into the political process—social, economic, and political characteristics of states or nations, party activities, etc.—are related to political outputs, including government adoption of policies, expenditure levels, and support for various programs. Such work has greatly contributed to our understanding of how such characteristics of political units influence public policy, and controversies generated by the work have provided a substantial research agenda (see, e.g., Dye 1966; Fry and Winters 1970; Wilensky 1975; Silberman and Durden 1976).[2] But although this body of work is important and helpful, it has three weaknesses which make it less than satisfactory for dealing with legislative change.

First, the statistical work on the correlates of public policy does not

[2] Works dealing with the factors that determine aggregate election results can be considered part of the same input-output paradigm (see, for example, Kramer 1971; Tufte 1975; Bloom and Price 1975; see also Nordhaus 1975).

deal with the alternatives considered by the legislature. Typically, some consequences of legislation—most often expenditures—are studied; sometimes legislative provisions are studied as well. But almost never studied is the process by which the legislature, faced with many alternative proposals, accepts some while rejecting others.

Second, most available work on policy outputs is based on cross-sectional data rather than time-series data. The work is thus inherently static in design, and conclusions drawn from it must be treated very cautiously when trying to draw inferences about political change over time (see Hofferbert 1974, chap. 6, for a discussion). Several recent articles show, in fact, that the substantive conclusions that can be drawn from time-series analysis will often be different from those drawn from cross-sectional analysis (see Erikson 1971; Gray 1976; Winters 1976).

Third, as many have noted, past works have often employed unsatisfactory measures of public policy. Most often, policy outputs are measured in terms of expenditures (e.g., Dye 1966; Jacob and Vines 1971; Hofferbert and Sharkansky 1971), although there are a significant number of exceptions (e.g., Dye 1969; Walker 1969; Ginsberg 1976). Prominent scholars in the area have long been aware of the problems involved in using expenditure data, however (e.g., Hofferbert 1974, chap. 6): for example, many types of legislation involve primarily regulative activities and are only poorly gauged by expenditure data. In addition, one recent article shows that substantive conclusions about the determinants of public policy can be very different if nonexpenditure data rather than expenditure data are used (LeMay 1973).

2. Similarly, past work on Congress is both important and helpful here, but it has several deficiencies for those concerned with how legislation changes over time. A great deal of work, for example, focuses on the determinants of voting by individual legislators, assessing the relative importance of constituency, party, the president, committee chairmen, and so on (e.g., Turner 1970; Kingdon 1973; Clausen 1973; Jackson 1974; Froman 1963; Matthews and Stimson 1975; cf. Mayhew 1974; Fiorina 1974). Unfortunately, since the emphasis in past work has been on predicting the behavior of individuals—nearly always at one point or over a short period —the work bears only indirectly on change in (aggregate) roll call outcomes over time. This is especially true when substantial changes in personnel must be taken into account.

Also common are case studies of the passage of particular bills and types of bills (e.g., Bauer, Pool, and Dexter 1963; Berman 1962; Nadel 1971). Case studies can be informative with regard to the passage of particular items and can point to factors generally important in the passage of legislation. But they tend to focus on the specifics of particular bills rather than

developing general analytic approaches which would be useful in studying enough motions simultaneously to determine the causes of long-term legislative change. In addition, there is little attempt in such studies to quantify the relationships among the variables, so conclusions are imprecise and not easily translated into more formal models (on these points, see Jackman 1975, chap. 1; and Hofferbert 1974, chap. 4).[3]

Thus past statistical work on policy does not deal with alternatives considered by the legislature, does not consider changes in policy over time, and has not developed satisfactory measures of policy. Past work on Congress has tended to focus on the cross-sectional analysis of individual behavior. What is needed, and what we attempt to provide in this paper, is an approach that deals with the collective voting of a legislative body over time, using satisfactory quantitative measures of policy.

DESIGN

If we want to explain why a legislative body votes the way it does on a series of roll calls on an issue, we must be clear about what we are explaining—the dependent variable—and about the independent variables and their effects. In attempting to specify the dependent variable, however, we immediately confront a problem no one has satisfactorily handled to date. If we want to deal with legislative outcomes, we need to consider both the content of a bill and the number of votes it receives. The problem is that these two factors—content and number of votes—are generally intertwined in our intuitive way of thinking about legislation. If someone were to say, "The Senate is more dovish this year than last year," that might mean that equally dovish motions were getting more votes or that increasingly dovish bills were getting the same number of votes, or both. To analyze legislative change, however, content and number of votes must be treated separately.

In principle, our solution is simple, though perhaps counterintuitive: the dependent variable will be the number of votes motions receive on a given

[3] Two other bodies of work on Congress will come to mind but are not directly relevant. Studies of the internal operation of Congress are numerous but will be ignored for the most part. The approach to be developed is most closely tied to the statistical work on public policy, which deals relatively little with the details of legislative activity. It is not claimed that the internal operations of Congress are unimportant; it is just that the approach will focus on the links between certain variables, without considering all the intervening mechanisms. The literature on roll call analysis is voluminous but not helpful for understanding legislative change. One of the leading roll call analysts, Aage Clausen, implies that the dimensions of voting discovered in such analyses are likely to be of little use in studying changes in policy (Clausen and Cheney 1970, p. 139; on related points, see Leik and Matthews 1968).

side of an issue—in this paper, the number of dovish votes.[4] The content of the motions will be among the variables predicting the number of votes. It certainly seems reasonable to suggest that the number of votes a motion gets depends to some extent upon the content of the motions; legislators respond to content when deciding how to vote, as they respond to other stimuli.

This focus on aggregate roll call outcomes and their determinants over time represents a major reconceptualization of the study of voting in legislatures. The utility of the design is best shown in applying it to a specific issue.

ENDING THE VIETNAM WAR: SOME HYPOTHESES

The Vietnam war was an extremely important and divisive issue in recent American political history. Although a list of the causes of the end of American involvement could be quite long, the various factors were important only insofar as they affected the decisions of Congress and the president: American involvement ended after Congress "changed its mind" and cut off all funding for the war. The Senate played a particularly important role in ending the war (Bernstein and Anthony 1974; Kanter 1972; Laurance 1976; cf. Hinckley 1971, pp. 138–48), and it is the Senate's change of mind, from almost unanimous support to strong opposition, that is the focus of this paper.

Our basic idea is extremely simple: we assume that, in the aggregate, members of Congress are rational political actors who often calculate costs and benefits in simple ways. Specifically, we predict that Congress will turn against past policies as the costs of the policies rise without compensating benefits and that members of Congress will respond to public opinion. We are not sure how they will respond to protest demonstrations. By taking these few factors into account, we expect to explain a fairly high proportion of the variance in Senate voting on the Vietnam issue.[5]

4 The reduction of voting to a single pro-anti dimension may seem like an oversimplification but is a fair representation of reality: senators are actually faced with complex choices reduced to yea-nay-abstain on given roll calls, but it is nevertheless fairly clear on which side of a conflict a given choice falls.

5 Given the importance of the Vietnam war issue, one would think that we would know with some accuracy what caused the Senate to change its mind and turn against the war. But we do not. Very little quantitative work has been done on the end of the Vietnam war, and most of what exists concentrates on public opinion or the interplay of personalities. See Page and Brody (1972); Verba et al. (1967); Converse and Schuman (1970); Hahn (1970); Brody and Verba (1972); Verba and Brody (1970). There are two attempts to predict aggregate public opinion from the war situation, but they do not deal with congressional voting; see Milstein (1973); Mueller (1973, chap. 3); cf. Sullivan and O'Connor (1972). One study deals with Senate voting but does not deal

Public Opinion

Given the amount of discussion devoted to public opinion and represen-
tative government, we know very little about the relationship between
public opinion and legislative change. Relatively few works empirically
relate public opinion to legislative activity on specific issues, and even
fewer attempt to gauge precisely the impact of public opinion on changes
in legislation over time. The only firm conclusion one can draw from the
literature is that sometimes legislatures are responsive to public opinion and
sometimes they are not.[6]

Thus, with regard to Vietnam, we have less than we would like to draw
on in formulating hypotheses about the relationship between public opinion
on the war and Senate voting. Congress can be expected to be responsive
to public opinion in general, given the assumption that legislators are moti-
vated by the desire to be reelected and that, everything else being equal,
responsiveness to public opinion is perceived as a good way to enhance
one's electoral prospects (see Mayhew 1974; Downs 1957). Whatever the
truth, congressmen believe that their stands on issues influence their elec-
toral chances (Mayhew 1974, pp. 70–71). Not all issues are equal, of
course. In general, the public is not very attentive to foreign policy ques-
tions, and legislators are not likely to pay much attention to public opinion
on issues that are not salient to the public (Miller and Stokes 1966). It
is possible, therefore, that the Senate paid little attention to public opinion
very early in the war. The cost of the war increased fairly quickly, how-
ever, and the issue became more important. It became clear fairly soon
that voters could be influenced by candidates' stands on the war.[7] In addi-
tion, voluminous data on public opinion at the national level were available
to legislators, who would therefore have information to guide their votes,
should they choose to pay attention to it.[8] Given that the issue was salient

with public opinion or war costs; see Burstein and Freudenburg (1977). Relevant stud-
ies will be cited below, but we have found none examining how war costs, public
opinion, and demonstrations, either separately or together, affected policy change in
Congress. For a theoretical discussion, see Andrews (1976).

[6] On public opinion and legislation or legislators, see Devine (1970); Miller and Stokes
(1966). Cohen (1973, chap. 1) provides an excellent review of the literature on public
opinion and foreign policy, which explicitly notes our ignorance about the relationship
between public opinion and Vietnam policy. He emphasizes that, although we often
think we know about the impact of public opinion on policy, the literature does not
support such a claim.

[7] See Page and Brody (1972); for discussions of the circumstances under which foreign
policy and defense issues enter domestic politics, see Zimmerman (1973) and Rosenau
(1967).

[8] Senators would be primarily concerned with their own states but would probably
still be interested in national trends. On the difficulties senators have in gauging constit-
uency opinion, particularly in foreign policy, see Jewell (1962, chap. 8).

and that some information about public opinion was available, we would expect that, in the aggregate, the Senate would respond positively to public opinion.

How strong is the relationship likely to be? This is an uncharted area, and there is little theory to draw on. The simplest viewpoint is probably that, although the Senate would be inclined to follow public opinion, everything else being equal, the body was also under a variety of other pressures, including strong proinvolvement pressures from Presidents Johnson and Nixon. Some pressures might reinforce public opinion, but others would oppose it and tend to weaken its influence. Thus the relationship between public opinion and Senate voting can be expected to be moderate, not strong.[9]

Antiwar Protest Demonstrations

If senators saw antiwar protest demonstrations as representing the opinions of large numbers of constituents, Senate voting could be expected to be positively related to the incidence and magnitude of such demonstrations. We do not really know what senators thought about the demonstrations, however, nor do we have much idea about the impact of such demonstrations.

Gamson (1975) has recently provided some evidence that organized social protest can be effective in producing political change, despite the disfavor with which protest is often viewed. Despite the magnitude of anti–Vietnam war protest, however, we do not know whether it was effective; some thought it helped end the war, others thought it was counterproductive, while still others thought the demonstrations might have had an indirect effect by helping to get the antiwar point of view before the public.[10] There have been no studies of the impact of demonstrations on congressional voting.

Given the lack of past evidence and the diversity of hypotheses, we make no predictions about the impact of demonstrations on Senate voting. The attempt to assess the impact, however, will contribute to our knowledge about a phenomenon much discussed at the time.

[9] We know that here and elsewhere we are making predictions about aggregate-level outcomes which depend upon assumptions about individual behavior not tested here. The findings are merely expected to be consistent with the assumptions.

[10] Converse and Schuman (1970) and Berkowitz (1973) discuss these possibilities. As far as we can tell, Berkowitz's study is the only one which attempted a quantitative assessment of the impact of demonstrations. After examining the impact of a number of demonstrations on public opinion and American troop strength and munitions expenditures, he concluded that demonstrations might have had small, short-term counterproductive effects, but that they could easily be said to have had no impact in the long run.

War Costs

Our hypothesis is extremely simple and obvious: there should be a strong positive relationship between war costs—defined in terms of battle deaths and monetary costs—and Senate opposition to the war.

Although the hypothesis is obvious, it has not been tested. What we are really testing is the possibility that legislators assess the effectiveness of policies they voted for in the past and adjust future votes accordingly. This possibility has seldom been considered explicitly in past work on voting in legislatures, perhaps because researchers so seldom analyze change over time.[11] The impact of the past upon the present is considered explicitly in work on budgeting; a major controversy, in fact, revolves around whether legislators are often concerned with the substantive content of programs when they vote on budgets.[12] But the actual impact of past policy decisions on present voting has not been examined empirically, so we have little sense of the impact of past success or failure on present voting.

Change over Time

We were also interested in seeing whether the hypothesized relationships remained the same throughout the war. For example, the Senate might have been slow to respond to public opinion, or it might have responded to antiwar demonstrations only late in the war, when it became relatively clear that participants were not merely part of a radical fringe.

Our main focus was on 1970. Popular and congressional response to the American invasion of Cambodia in May was widely seen as a turning point in the history of American involvement. Senate outcomes on Vietnam-related roll calls certainly underwent a crucial change during 1970. Before the invasion of Cambodia, almost no dovish proposals received a majority of votes in the Senate, and the mean number of dovish votes received on proposals was 32. During 1970, however, dovish proposals began to receive a majority of votes fairly often, and the mean number of dovish votes

[11] This is not to imply that it has not occurred to others that legislators examine past policy in an area when deciding how to vote; it merely seems that such considerations are seldom incorporated explicitly into statistical analysis. In analyzing the influences on voting in Congress, the literature most often examines the impact of various political actors, such as constituents, lobbyists, party leaders, the president, and so on, often attempting to weigh the relative impact of various kinds of actors; see, for example, Jackson (1974). Ideology and party are often examined as well; in the defense realm, see Bernstein and Anthony (1974).

[12] The seminal work in the area claims that the congressional focus is mostly fiscal; see Wildavsky (1974); Davis, Dempster, and Wildavsky (1966). More recent work tries to show that programmatic concerns are often crucial; see Natchez and Bupp (1973) and Kanter (1972).

received by motions was 53; for the entire period from 1970 through 1973, the mean was 48. We decided, therefore, to see whether the furor over the invasion of Cambodia led to a significant transition point, by determining whether the relationships examined were the same after 1970 as they were in 1970 and before.

DATA

Our dependent variable is the total number of dovish votes received by each of 91 bills and amendments related to the Vietnam war voted on by the Senate between the passage of the Gulf of Tonkin resolution in 1964—the vote commonly associated with the beginning of major American involvement—and the passage of the War Powers Act in 1973, which is taken to be symbolically and concretely an important point in the end of American involvement.[13] The roll calls included are those dealing with American military forces in Vietnam; military and economic aid to Vietnam; American military forces in, and military aid to, other nations in Indochina; and those defining the war-making powers of the president. Dovishness was defined within the context of each series of roll calls.

Our independent variables are measures of the content of the motions, war costs, public opinion, and antiwar demonstrations.

The content of the motions was measured along three dimensions: (1) a dimension gauging the hawkishness of motions; (2) a funding dimension, with the motions most generous with regard to funding at one extreme; and (3) a war-powers dimension, with motions favoring presidential initiative at one extreme and those limiting the president at the other.[14]

Our basic measures of war costs were total battle deaths and the incremental monetary cost of the war through the fiscal year during which each vote was taken.

Public opinion was measured in terms of the only questions asked consistently by polling organizations over the whole course of the war. One measure gauged the proportion of the population which felt our entry into the war had been a mistake, while the other was the proportion of the population that approved of the way the president was handling the situation in Vietnam.[15]

[13] In a preliminary analysis, we tried using dovish votes as a proportion of votes actually cast; since the results were virtually identical with those that follow, however, we will use only the more straightforward dependent variable—the number of dovish votes —throughout this paper.

[14] Information about the construction of these and other variables can be found in the Appendix.

[15] Though neither of these questions is a satisfactory measure of position on a hawk-dove dimension, they do give some indication of American reaction to our Vietnam policy, and, as stated, they were the only questions asked throughout the war. A major

The basic measure of antiwar demonstrations was a monthly count of demonstrations reported in the *New York Times*. Although this is a crude measure, the number of demonstrations reported in the *Times* is probably highly correlated with the number actually taking place, and the number of demonstrations is probably highly correlated with the number of participants.[16] Given that we are concerned with national trends and their impact upon Senate voting in the aggregate, rather than upon individual senators, our measure should not be biased in any significant way.

For the time-series analysis, the data were organized on a monthly basis for all variables except monetary cost of the war, for which fiscal-year data were used. When data were not available on a monthly basis, values of the variables were estimated for the months needed for the analysis. The procedures are specified in the Appendix.

We decided to include the following variables in our equations: DOVE-YEA, the dependent variable, is the number of dovish votes cast in each of the 91 roll calls; DIM1 is the dimension which gauges the hawkishness of motions; DIM2 is the funding dimension; DIM3 is the war-powers dimension; COST is the incremental monetary cost of the war through the fiscal year of the vote, in billions of current dollars; DEATHS is the total number of battle deaths up to the time of the vote, in hundreds; RCNTDETH is the number of deaths during the six months preceding the vote, in hundreds; MISTAKE is the percentage thinking entry into the war was a mistake; APPROVE is the percentage approving presidential handling of the Vietnam situation; DEMONS is the total number of antiwar demonstrations up to the time of the vote, in tens; and RCNTDEMS is the number of antiwar demonstrations during the six months preceding the vote, in tens.

We included RCNTDETH and RCNTDEMS because we thought that recent events might have an especially great impact on Senate voting; the use of a six-month period is somewhat arbitrary, but changing the length of time involved did not materially affect the results when we tried it.

STATISTICAL MODELS

We are interested in examining the relationships between the independent variables, including content, and our dependent variable, the number of

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problem with the "mistake" question is that a significant proportion of those who thought our entry was a mistake were hawks rather than doves; see Converse and Schuman (1970). Nevertheless, the question has proven useful in previous analyses; see Mueller (1973).

[16] Snyder and Tilly (1972, p. 523) found a correlation of 0.84 between the number of demonstrations and the number of participants in their much more elaborate study.

dovish votes:

$$\text{DOVEYEA} = b_1 + b_2\,\text{DIM1} + \ldots + b_i\,\text{DEATHS} + \ldots$$
$$+ b_n\text{RCNTDEMS}\,. \tag{1}$$

The analysis is complicated, however, by the fact that we want to determine whether the relationships were the same in the 1971–73 post-Cambodia period as they were earlier. To do so, we define a dummy variable with a value of zero for roll calls taking place through the end of 1970 and a value of one for roll calls taking place after that. This procedure is described in detail in Johnston (1963, pp. 221–28; for a political application, see Jackson 1974, chap. 5); in essence, it allows us to see whether there is a turning point between 1970 and 1971 at which some of the relationships change. Equation (1) is rewritten, using the same variables, but adding a series of variables, each of which is a variable from equation (1) multiplied by the dummy variable D:

$$\text{DOVEYEA} = b_1 + \ldots + b_n\text{RCNTDEMS} + a_1 D + a_2\text{DIM1}\cdot D + \ldots$$
$$+ a_n\text{RCNTDEMS}\cdot D\,. \tag{2}$$

Because of the dummy variable in each of the a coefficient terms, the as are only computed for roll calls taking place after 1970, when $D = 1$. If an a coefficient fails to reach statistical significance, this means simply that there was no significant difference (between the 1964–70 and the 1971–73 periods) in the relationship between the independent variable and DOVEYEA; the b coefficient for that variable is the best estimate of the relationship for the entire war. In cases where an a coefficient is significant, the hypothesis of "no turning point in 1970" will be rejected. In such a situation, the a coefficient gauges the change in the relationship occurring between the earlier and later periods. The relationship between the independent variable and DOVEYEA is gauged by the b coefficient for the earlier period, and by the sum of the respective a and b coefficients for the later period.

FINDINGS

Zero-order correlations are presented in table 1, and we can immediately see some findings of interest, findings which also cause problems for multivariate analysis. MISTAKE, COSTS, DEATHS, and DEMONS are very highly intercorrelated;[17] each is a simple monotonic increasing function of time. It is perhaps not surprising that costs of the war and battle deaths were so highly correlated. Of more substantive interest is the fact that

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[17] See Mueller (1973, chap. 3) and Hibbs (1974), for detailed discussions of the relationship between MISTAKE and DEATHS.

TABLE 1

ZERO-ORDER CORRELATIONS, MEANS, AND STANDARD DEVIATIONS

	1	2	3	4	5	6	7	8	9	10	11	M	SD
1. DOVEYEA.....												40.9	24.0
2. DIM1.........	208											56.3	41.4
3. DIM2.........	−394	−007										7.2	35.5
4. DIM3.........	−478	−331	101									5.1	34.5
5. COST.........	587	440	−472	−214								87.0	31.1
6. DEATHS.......	629	453	−454	−195	977							386.4	141.6
7. RCNTDETH....	−256	−186	331	322	−435	−357						14.2	17.3
8. MISTAKE.....	580	417	−454	−223	980	935	−480					59.4	12.8
9. APPROVE.....	452	108	−373	−171	435	400	−260	464				53.2	9.2
10. DEMONS......	579	427	−483	−228	940	903	−611	954	513			139.1	56.8
11. RCNTDEMS....	243	133	−007	107	182	285	202	088	129	232		21.4	20.0
12. TIME*........	583	410	−501	−240	974	927	−611	990	504	971	080	78.6	25.7

NOTE.—$N = 91$; decimals implied; correlations greater than .17 are significant at .05, greater than .24 at .01, and greater than .31 at .001.

* Time measured in months since Gulf of Tonkin resolution.

public opinion and demonstrations are so highly correlated with war costs broadly defined. The most obvious interpretation is that the general public, as well as the antiwar demonstrators, were quite conscious of the costs of the war; as costs escalated without compensating benefit, people turned against the war.[18]

We had initially hoped to assess the relative impact of war costs and public opinion on Senate voting as well as to produce estimates of the effects of each of our independent variables. Unfortunately, the extremely high correlation between the measures of public opinion and war costs made satisfactory estimation impossible.

As a consequence of the pattern of correlations we discovered, we decided to leave COST out of the multivariate analyses. DEATHS, the other "war costs" variable, is more highly correlated with DOVEYEA and was a slightly better predictor in trial equations we ran in which we included first one and then the other. In addition, DEATHS is a more sensitive indicator, since we can gauge it accurately on a monthly basis, whereas COST is available only on a yearly basis and is less sensitive to specific events in Vietnam. But even after we omitted COST, the high degree of multicollinearity among MISTAKE, DEATHS, and DEMONS would produce unreliable parameter estimates were we to include them all simultaneously in our equations.[19]

We ultimately decided to estimate the parameters in the three equations presented in table 2: an equation including measures of content alone, an equation including content and war costs, and an equation including content, public opinion, and recent demonstrations.

The simplest potentially appropriate estimation procedure is ordinary least squares (OLS). When the models presented in table 2 were estimated using OLS, however, the Durbin-Watson d-statistic showed that serial correlation was a potential problem in every equation. In such a situation, OLS is likely to produce exaggerated estimates of t- and F-statistics and of R^2. To overcome this problem, we used an appropriate variant of generalized least squares (GLS), which is well described in the econometrics literature, to estimate the models.[20]

[18] Mueller hints at such an interpretation for his more limited data set but does not make it explicit (1973, chap. 3).

[19] As noted by the econometricians, little can be done statistically in such a situation, where we have no independent way to estimate parameters. We can make predictions using these data but are very limited in the causal interpretations we can legitimately make, especially given the fact that several of the independent variables increase monotonically with the passage of time.

[20] Problems concerning serial correlation and the proper use of ordinary least squares and generalized least squares are described in Wonnacott and Wonnacott (1970, chap. 6; the procedure employed here is described on pp. 331–32; see Theil [1971, chap. 6]). Hibbs (1974) provides a good explanation of GLS with sociological and political exam-

TABLE 2

MODELS AND DATA, SENATE DOVISH VOTES, 1964–73
(N of Roll Calls = 91)

UNSTANDARDIZED COEFFICIENT	EQUATION		
	1	2	3
CONSTANT:			
1964–70...............	35.65(4.99)**	...	−27.50(−2.01)*
Change, 1971–73.......	8.52(.99)	...	9.63(.18)
DIM1:			
1964–70...............	...	−.20(−2.88)**	−.21(−2.95)**
Change, 1971–73.......	...	.21(2.34)*	.23(2.54)**
DIM2:			
1964–70...............	−.03(−.55)	.05(.92)	.00(.02)
Change, 1971–73.......	−.35(−2.44)**	−.42(−3.03)**	−.35(−2.34)*
DIM3:			
1964–70...............	−.20(−2.95)**	−.27(−4.33)**	−.28(−4.44)**
Change, 1971–73.......	.04(.32)	.03(.23)	.06(.48)
DEATHS:			
1964–70...............	...	.15(10.87)**	...
Change, 1971–73.......	...	−.06(−3.23)**	...
MISTAKE:			
1964–70...............	...	...	1.23(3.48)**
Change, 1971–73.......	...	...	−.28(−.33)
RCNTDEMS:			
1964–70...............	...	...	.48(2.68)**
Change, 1971–73.......	...	...	−.70(−2.73)**
R²	.30	.57	.61
F	7.1	16.1	11.2
Estimated serial correlation	.65	.29	.24

NOTE.—t-statistics in parentheses.
* Significant at the .05 level, one-tailed test.
** Significant at the .01 level.

ples, including one rather similar to the analysis presented here. The program used is in Peck (1975). Another way to deal with serial correlation is to employ the method of first differences. This is most appropriate where theoretically required or where the serial correlation approaches one; neither condition applies here. In addition, there are conceptual (if not technical) problems in using first differences in a data set such as this, where the time intervals between data points vary so greatly and are often effectively zero with regard to possible change in many of the independent variables. Technically, since some of the independent variables are highly correlated with a variable measuring the passage of time, results from the use of first differences would be extremely similar to those presented. Table 2 presents GLS estimates. We also ran the same equations using OLS, and the differences in results were those expected theoretically: the relative magnitudes of coefficients were the same in both sets of results, but R^2 and F- and t-statistic estimates from OLS were higher than those from GLS; in a number of cases, we would have concluded from OLS that coefficients were significantly different from zero, whereas we could not so conclude from the GLS results. Thus our substantive interpretations do depend to some extent upon our choice of procedure. The GLS results are clearly more conservative. Finally, in order to avoid specification error (e.g., Duncan 1975, chap. 8), the equations were first estimated including all independent variables; then they were reestimated including only those variables that had significant coefficients in the first estimation, for either the 1964–70 period or the "change" variables. Variables with t-statistics of less than 2.0 in both cases were eliminated from the equations presented.

We see from equation (1) that content determines roll call outcomes to a significant extent. The F-statistic shows that the equation as a whole is significant at the .01 level.[21] In part, this is not a finding about voting but a test of the validity of the content measures. Senators voting on how dovish policy is to be must necessarily be affected by the dovishness of the motions being considered. If the correlation between content, as measured, and voting were zero, we would have reason to doubt the validity of the content measures (more on this point can be found in Burstein [1978]).

At the same time, however, the relationship between content and roll call outcomes still must be gauged; ours is the first work we know of which yields a quantitative estimate of the relationship. This is important for two reasons. First, as will be shown below, the relative importance of different content dimensions can vary over time; by examining the changes we can learn something about how the Senate operates. Second, given the results shown in equation (1) and the correlations of the content dimensions with other variables in table 1, it is clear that inclusion of the content measures in the analysis is essential; were they left out, the models would be seriously misspecified.

Equation (2) gauges the impact of war costs on DOVEYEA, while equation (3) focuses on the impact of public opinion. DEATHS had a significant impact on DOVEYEA, but RCNTDETH did not. The Senate seems to have responded to the cumulative cost of the war, but not to costs incurred during the period immediately before voting. MISTAKE and RCNTDEMS had a significant impact on DOVEYEA, but APPROVE did not. The fact that attitudes about presidential handling of the war are not related to Senate voting is perhaps not surprising, given the twists and turns of presidential policy and the fact that we are looking at Senate voting rather than presidential behavior. The "mistake" question, which refers to the war itself, is clearly the better predictor of Senate behavior.

As mentioned above, we cannot, unfortunately, satisfactorily separate the effect of MISTAKE from that of DEATHS because they were so highly intercorrelated. War costs seem to be the more basic variables, in that we would expect them to affect public opinion, rather than the reverse, in the short run. At the same time, the costs would have political meaning for the senators mostly insofar as they affected public opinion. We really cannot say that the Senate was responding to war costs rather than public opinion or the reverse.[22] We can say that the data are consistent with

[21] The use of significance tests where the relationship between the sample and the universe is unclear raises interesting questions; it is standard procedure among economists, who have developed an elaborate rationale for their procedures; see Johnston (1963, chap. 1).

[22] By using appropriate leads and lags, we should be able to establish causal ordering among the variables in a way not possible with the usual cross-sectional data; see Sims

the Senate's being responsive to public opinion and/or war costs. The fact that both costs and opinion were moving in the same direction might have reduced the degree of uncertainty felt by senators, and so have hastened their response, but at this point emphasis on one variable or another depends upon one's purpose and point of view.

Whereas table 2 presents changes between the two periods, table 3 offers the actual coefficients for dovish votes in each period. Focusing on equations (2) and (3) of table 2 and on the second and third sections of table 3, we can see that DIM3, the issue of presidential war powers, turns out to have had a consistent importance throughout the war, regardless of how the remainder of the equation is specified. In the light of that fact,

TABLE 3

COEFFICIENTS FOR SENATE DOVISH VOTES

	1964–70	1971–73
Content:		
DIM 1...................	...ᵃ	...ᵃ
DIM 2**................	−.03	−.38
DIM 3...................	−.20	−.16
Content and war costs:		
DIM 1*..................	−.20	.01
DIM 2**................	.05	−.37
DIM 3...................	−.27	−.24
DEATHS**..............	.15	.09
Content and public opinion:		
DIM 1**................	−.21	.02
DIM 2**................	.00	−.35
DIM 3...................	−.28	−.22
MISTAKE...............	1.23	.95
RCNTDEMS**...........	.48	−.22

ᵃ Variable significant at the .05 level in neither period.
* Difference between time periods significant at the .05 level.
** Difference significant at the .01 level.

(1972). Because several of the variables are strongly correlated with time, however, leads and lags tell us little. Correlations among DEATHS, COST, MISTAKE, DEMONS, and DOVEYEA are highest in all cases where the variables are all measured for the same months, and the correlations decrease monotonically but very slowly as six-month, one-year, and two-year lags are introduced. We considered examining the impact of distributed lags, but there were major conceptual and practical problems in doing so; in ordinary time-series analyses, variables are measured at regular intervals —annually, quarterly, etc.—but the intervals between roll calls were extremely variable. On one hand, the nature of the relationships among the independent variables makes the Vietnam issue less than perfect for demonstrating the utility of a new approach to studying legislative change; we would prefer to be able to come to grander and more elegant conclusions. On the other hand, the issue was an extremely important one, well worth looking at for anyone concerned with democratic political change. It would be silly not to study an issue merely because theoretically relevant independent variables were highly intercorrelated. In addition, discovering the correlations teaches us something about the issue and would probably not have occurred if we had not developed the approach being demonstrated in the paper.

the varying impact of the other two dimensions is somewhat surprising; DIM2, the funding dimension, becomes important only after 1970, with DIM1, the main hawk-dove dimension, being important only up until 1970.

DEATHS had a significant impact on Senate voting, but one that varied over the course of the war. Translating the coefficients into concrete terms, we can say that every 700 battle deaths produced another dovish vote, on the average, during the earlier part of the war (as far as we can estimate given the set of variables included in these equations). From 1971 on, however, the impact of deaths declined, with about 1,100 deaths being required to produce another dovish vote.

The situation with regard to measures of public opinion was mixed. As mentioned, APPROVE had no significant impact on Senate voting. MISTAKE was related significantly and consistently to Senate voting; the greater the proportion of the public that thought our entry into the war had been a mistake, the greater the number of dovish votes in the Senate, for the entire war.

Finally, the results for RCNTDEMS are striking. Contrary to what many have speculated, and also somewhat paradoxically, as the demonstrations became more widespread and acceptable in later years, they became less productive as well. Recent demonstrations seem to have moved the Senate in a dovish direction up through the time of the Cambodian invasion; after that, their impact became negative. In other words, those who claimed that the Senate responded positively to antiwar demonstrations and those who claimed exactly the opposite were evidently both partly right. Early demonstrations may have indeed had the consciousness-raising impact that their supporters claimed, but later demonstrations may also have had the alienating effect that many of their opponents charged.

If 1970 was a turning point, what kind of turning point was it? Our findings on the content dimensions—that the hawk-dove dimension was salient only up through 1970, with the funding dimension becoming significant only thereafter—were initially surprising to both authors, yet they make considerable sense after a moment's thought. The power of the purse is in fact a time-honored source of congressional control over public policy and over the executive branch. The data may imply that up through 1970 the Senate was not inclined to treat American participation in the war as an ordinary issue to be dealt with by usual means. The 1970 turning point may have been the time when the war issue came to be treated like other issues, with debate and effective control over policy revolving around funding.

In the same context, our initially puzzling findings on the importance of antiwar demonstrations also begin to make sense. As the 88–2 margin on the Gulf of Tonkin resolution (along with the consistently lopsided ad-

ministration victories and the small proportion of Americans opposing the war during the first several years) indicates, opponents of the war had to do a great deal of consciousness raising before they could expect to be taken seriously and begin to debate the merits of their stance from a position of even moderate strength. It appears possible that demonstrations may be fruitful at an early stage—when the aim is to make an issue salient and the subject of serious debate—but ineffective later, when the Senate doves were more concerned with more subtle problems of drafting legislation that could gain a majority of votes. Demonstrations are more effective for attracting attention than for providing detailed guidance on coalition building.

CONCLUSIONS

In this paper we have shown, by means of a single significant and substantive example, that it is now possible to deal directly and quantitatively with the causes and correlates of major legislative changes over time. The method is nonarbitrary, relatively straightforward, and easily replicable. Probably more important, however, the findings are sensibly and substantively interpretable, and they have provided us with knowledge about an important topic that has simply not been available before. We have seen that content alone, not surprisingly, can explain a reasonable proportion of the variance in Senate voting on Vietnam but that the addition of a very small number of other variables boosts the proportion explained substantially. As far as we can tell from our data, our hypotheses about Senate voting have been supported in a general way: although public opinion and war costs (measured either in dollars or in human lives) were so tightly correlated that it simply was not reasonable to include both types of variables in our equations at the same time, either type individually had a strong and consistent predictive impact on Senate voting throughout the war. The Senate did respond to war costs and public opinion.

Our findings are all the more encouraging in view of the lack of precedents in this area and the fact that we have not yet included a wide range of other factors that might reasonably be expected to lead to a further improvement in R^2. On the other hand, at this point we have not been able to explain many of the results in other than an ad hoc way: our speculations about the impact of demonstrations may or may not be correct, for example, and we are also not sure why the impact of public opinion as measured by the "mistake" question remained constant, while battle deaths appear to have had a declining marginal impact—although we could clearly offer speculation on these points as well. The crucial point to be made here, however, is this: we do not have a satisfactory theoretical framework in

which to explain the results, in part because it has not previously been possible to arrive at such results. That is, the development of a new approach to the study of legislative change—one which enables us to make relatively precise estimates about the impact of various factors on legislatures—may be producing a new body of data which we do not yet have the theories to explain.

We have outlined a new approach to the study of legislation and tried to demonstrate some of its strengths and weaknesses by applying it to a particularly important issue in recent American history. The approach needs to be applied to many more issues before its full range of strengths and weaknesses can be known; other types of variables must be included in the models as well. Yet even in this obviously preliminary paper, we have been able to see things we had not been able to see before, and that, perhaps, is the clearest test of value for any new perspective.

APPENDIX

Data Sources and Definitions of Variables

Senate voting results.—*Congressional Quarterly Almanac* (1964–73). Each vote was treated as if it took place on the first day of the month in which it took place, except for *CQ* roll call 136, 1970, which was placed in June with other votes on Cambodia.

Incremental costs of the war.—For fiscal years 1965–74 (estimated), from Fried et al. (1974).

Battle deaths.—U.S. Senate Committee on Foreign Relations (1971, pp. 5–6); U.S. Department of Defense (1974, p. 61). Summed to the beginning of the roll call month where feasible or estimated where only quarterly or annual data were available; from 1971 on, only annual figures are available.

Public opinion.—Gallup results (and estimates—see below) for the month of the roll call were used.

A. Mistake to have entered the war: results from two questions were merged; cf. Mueller (1973, chaps. 1, 3); $N = 23$: "In view of the developments since we entered the fighting in Vietnam, do you think the U.S. made a mistake in sending troops to fight in Vietnam?" and "Some people think we should not have become involved with our military forces in Southeast Asia, while others think we should have. What is your opinion?"

Because the data showed a strong linear trend over time ($R^2 = .84$ for the "mistake" response), the percentages were regressed on time to provide estimates for months when the question had not been asked. For months for which observations were available, they were used; for other months, regression estimates (ordinary least squares) were used.

B. Approval of handling of war, $N = 47$: "Do you approve or disapprove of the way the [Johnson, Nixon] administration is dealing with the situation in Vietnam?"

Sometimes there are slight variations in wording. Because the percentages showed only a weak trend over time, simple linear interpolation was used to estimate values for months when the question was not asked. Results from the first and last asking of the question were used for earlier and later months, respectively.

Data are taken from Gallup International Inc. (1964–1973) and Gallup (1972, vol. 3).

Antiwar demonstrations.—*New York Times Index,* 1964–73. Domestic antiwar demonstrations mentioned each month in the *Index* were simply tabulated; when the *Index* referred to numbers in general terms, the *Times* itself was consulted for specifics.

Measuring legislation.—In order to analyze legislative outcomes, we must be able to conceptualize and measure them adequately. Several methods have been employed in the past to deal with legislation quantitatively, but unfortunately none was adequate for this paper without extensive modification: (*a*) dummy variables, often used to indicate whether particular provisions have been adopted, are too crude (e.g., Jackman 1975; Walker 1969); (*b*) sophisticated content analysis is best used where there is a sound theoretical basis for defining dimensions, which was lacking here; in addition, there are few models to draw on (e.g., Ogburn [1912] 1964; Pomper 1968: chaps. 7–8); (*c*) expenditure data do not adequately describe all legislative activity; (*d*) roll call analysis has not incorporated ways of dealing with a single, fairly specific issue over time, nor of dealing with personnel turnover; roll call analysts do not deal with situations in which a legislative body as a whole shifts its position on an issue (see MacRae 1970; Clausen and Cheney 1970, p. 149). To overcome these problems, roll call analysis techniques were modified as briefly described below; an extensive discussion is provided in Burstein (1978).

Essentially, the aim was to develop a technique that would make it possible to determine whether the motions voted on could be described in terms of a small number of dimensions and, if so, to place each motion on the dimensions, so that locations on the dimensions could be treated as variables used in statistical analysis.

We concluded that our intuitive way of thinking about dimensions, when we are concerned with change, is in terms of a Guttman-like scale of items. For example, we might think of motions as being on a hawk-dove dimension, with motions calling for withdrawal at one end, those maintaining the status quo in the middle, and those calling for great escalation at the other end. If we want to measure change in legislation over time,

we want to be able to form dimensions in terms of the relationships of the motions to each other. Each dimension should be a function of similarities and differences among the items—similarities in that the items all reflect part of a single underlying dimension and differences in that each item represents a different point on the dimension.

The conceptual framework for such an approach was outlined by Guttman (1954), who suggested defining dimensions in terms of rank orders among variables called "simplexes." A set of variables forms a simplex if the matrix of correlations among them can be arranged so that the largest correlations are all next to the main diagonal and decrease monotonically as one goes away from the main diagonal. The relations among the variables (motions) can be interpreted in a manner comparable to the interpretation of relations among items in a Guttman scale. Defining dimensions of legislation in terms of simplexes enables us to define dimensions in terms of the relationships among the items, while arraying the items along the dimensions.

The procedure was as follows: the seat, rather than the senator, was chosen as the unit of analysis. (When roll call analysts study voting over time, they usually consider only seats continuously occupied by the same person for the entire period. This is simply not an acceptable procedure when a long period is being considered and turnover is high [over 50% in the period studied].) A vote on the dovish side of a motion gave a seat a score of one and a vote on the hawkish side a score of zero; then the correlations among the 91 roll calls were calculated. Using seats as the unit of analysis, when newly elected legislators perceive the dimensions of legislation the same way as incumbents, the same dimensions should be detected as if no replacement had occurred. If newcomers see things differently, there will be no interpretable dimensional structure.

The correlation matrix was subjected to Smallest Space Analysis (SSA), the Guttman-Lingoes version of nonmetric multidimensional scaling, which can be used to discover simplexes in correlation matrices (unlike factor analysis; see MacRae 1970, p. 47; on SSA, see Guttman 1968; Lingoes 1973; Roskam and Lingoes 1970). A good solution (coefficient of alienation of 0.16) in three dimensions was found; the dimensions were interpretable as a hawk-dove dimension, a funding dimension, and a war-powers dimension. After rotating the axes, which are initially arbitrary, each motion was given a set of scores, each score being the position of the item on each dimension. The scores were then treated like other variables in the analysis (for an example of a similar procedure, see Laumann and Pappi 1973). The three variables thus became the measures of content. To control for changes in context, it is necessary to control for time in the statistical analysis (a fairly common procedure where trends are involved); the

equivalent control in the analysis here is the variable very highly correlated with time—DEATHS.

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