Sprague, John. 1984(1981). "One-Party Dominance in Legislatures." In *The Research Process in Political Science*, W. Phillips Shively (Editor), pp. 225-67. Itasca, Illinois: F. E. Peacock Publishers, Inc. Also reprinted from *Legislative Studies Quarterly*, Vol. VI, No. 2, May 1981.

CHAPTER SEVEN

One-Party Dominance in Legislatures

7. One-Party Dominance in Legislatures

This study is different from the others you have read in this book. Where each of the other studies is primarily an attempt to ascertain some set of facts, this one develops a theory that would be consistent with some facts that are already known. Both types of study deal with facts and theory. But whereas the other studies you have read seek out facts to test a theory, Sprague's study starts from an observation that had struck him as odd and attempts to develop a theory that could account for that observation.

The puzzle that starts Sprague off is the deceptively simple question: How is it that since World War II, while Republicans and Democrats have shared the presidency almost equally, the Democrats have nevertheless held almost exclusive control of the House of Representatives? Since approximately the same electorate is involved in both sorts of election, why have the Democrats done so much better in controlling the House than in controlling the presidency?

Sprague attempts to account for this paradox by developing a theory that would be consistent with it. He uses a mathematical theory—a frequently used device, since the precision and formality of mathematical language are well adapted to the needs of theory. In reading his paper, it is quite possible that you will strike some mathematical technique that is new to you. Should this happen, please don't feel that you must stop at that point; you may be able to continue to follow the basic argument of the paper, even if you must for now take it on faith that Sprague has in fact done something mathematically that he says he has done. Eventually, if this kind of theory building appeals to you, you will want to acquire enough mathematical training to approach work like Sprague's confidently and critically. For our present purposes, however, it is permissible if you occasionally are forced to "suspend disbelief" and continue through the paper.

Formal theoretic work of the sort represented here has become increasingly popular in political science in the last decade or so. In a useful and readable article, Morris Fiorina lists four advantages to developing theories in this way:

1. Developing a formal model forces precision in the terms of one's argument. One cannot use words with multiple or ambiguous meanings in a mathematical equation.

2. Developing a formal model requires that all of our theoretic assumptions be stated fully and explicitly. In casual arguments, assumptions may be left unstated and unexamined. In mathematical statements, however, they must be included; any inconsistencies among them will become obvious because the mathematics will fall apart.

3. A related point is that it is easier to check the validity of the logic involved in a formal model than in a more casual argument.

4. Finally, formal models should lead on to a richer set of further conclusions and applications, because the varied results that logically follow from a given set of assumptions show up readily from the mathematical statements. Without these, we might have to depend much more on the inventiveness and thoroughness of the researcher. As Fiorina puts it: "Formal models greatly facilitate carrying an argument to its logical end—bleeding a set of assumptions dry, so to speak."[1]

As you read Sprague's article, you might bear these possible advantages in mind. Does his formal modeling in fact benefit from these advantages? Are there compensating disadvantages (other than the fact that one must, of course, read mathematical work slowly)? Another question to consider—even though Sprague works from observation to the development of theory, rather than from theory to an investigation of facts—is how different are the tasks that Sprague is doing from the tasks that faced the other investigators whose accounts you have read? In his narrative, did he face different problems from them? Does he seem to have had different feelings about what he did?

NOTE

1. Morris P. Fiorina, "Formal Models in Political Science," *American Journal of Political Science* 19, no. 1 (February 1975): 133–59.

RESEARCH ARTICLE

7A. One-Party Dominance In Legislatures

JOHN SPRAGUE

Democratic party domination of the United States House of Representatives coupled with competitive presidential elections is mildly paradoxical. The same voters participate in presidential elections as in congressional elections, yet the partisan distribution of voters across constituencies is such that control of the U.S. House has been consistently Democratic in the modern era, a period in which the Presidency has shifted frequently between the parties. It is reasonable to inquire into the determinants of that persistent condition in our national representative institutions. While the U.S. presidential election is in many respects a national election, the election of the House of Representatives is of course the aggregate result of elections in 435 district constituencies. In the article which follows, the consequences of the particular distribution of votes which exists in a given set of legislative constituencies over an electoral epoch are explored. Employing a simple mathematical model, I attempt to demonstrate that a number of significant consequences for partisan control of a legislature are logically entailed by the inevitably unequal distribution of safe seats between two parties. The argument is broadly applicable to two-party legislatures but empirical reliance is placed on the experience of the U.S. House of Representatives.[1]

SOME EVIDENCE

The history of partisan control in the House of Representatives is displayed in Figure 7.1 where the measure of control is the proportion of all House seats controlled by the Democratic Party from 1900 through 1970 in the session prior to a general election. The magic number of .5 is imposed on the figure to aid the eye in seeing this history (Przeworski and Sprague, 1971). The figure

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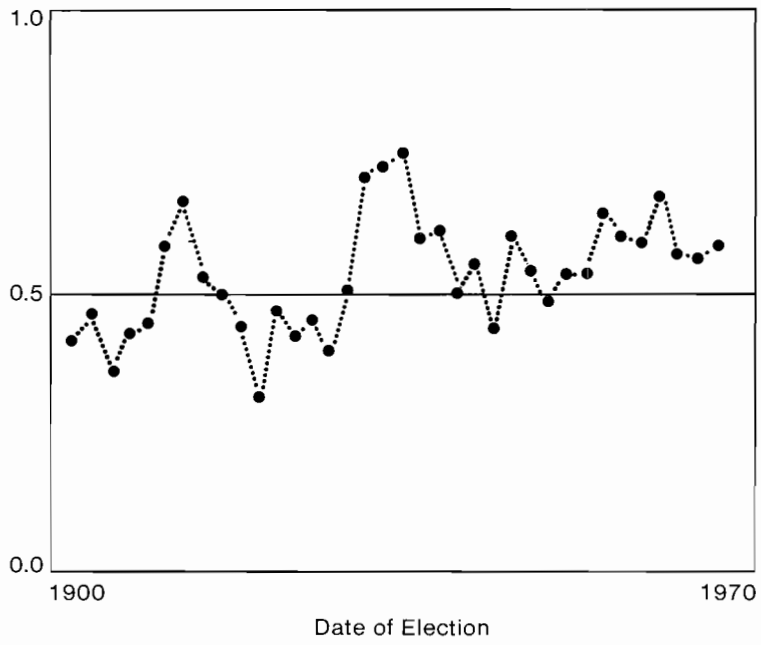

FIGURE 7.1
Partisan Control of the U.S. House of Representatives, 1900 – 1970 (in proportions of all seats by date of election)

reveals the truly extraordinary control majorities received by the Democrats in 1932, 1934, and 1936—they exceed all other Democratic control proportions in the 20th century, including the results of the 1964 elections—and visual inspection strongly suggests that the *system* of partisan control is different before and after these great electoral events. Prior to 1932 the pattern of control follows the presidential election results and perhaps exhibits more variation. After those remarkable three elections not only are the Democrats typically above the magic number, i.e., in control of the House, but there appears to be less variation in this control. After 1936 the points appear to move within a narrower range. These features of the data urge the wisdom of distinguishing the earlier from the later period.

To evaluate further the reasonableness of making a division of the data into periods, it is useful to remove some of the short-term variation by some smoothing of the observations in Figure 7.1. This is done in Figure 7.2, which

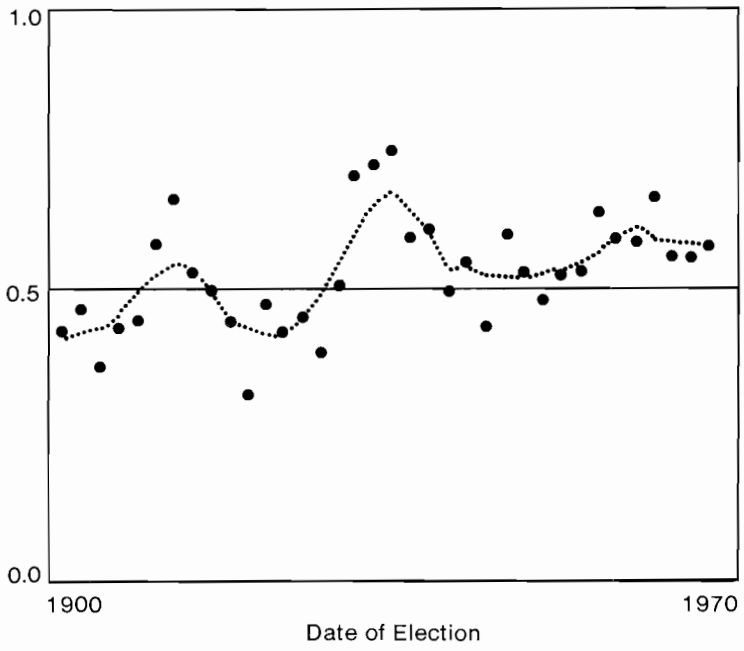

FIGURE 7.2
Partisan Control of the U.S. House of Representatives, 1900-1970
(In proportions of all seats by date of election. Dotted line is moving average[a])

[a]The moving average is unweighed, centered, and includes five values.

repeats the data of Figure 7.1 but imposes a line computed as a centered five-election equally-weighted moving average. With some short-run noise, i.e., short-run political conditions, thus suppressed, Figure 7.2 supports the notion that the epoch before 1930 should be distinguished from the years following. At no time since 1930 has the moving average taken on a value less than .5, which is in sharp contrast with the pattern before that date. The earlier moving average crosses the magic number line twice before the final crossing that ushers in the modern era.

The patterns in Figures 7.1 and 7.2 justify breaking out the period from 1930 onwards as distinct. It is characterized by an average control that is decidedly biased in favor of the Democratic party, a moving average of control which never dips below .5, an average value a considerable (statisti-

cal) distance away from the .5 value of pure competition, and less over-time variation. We shall return to these properties after a brief examination of the presidency in a comparable period.

The (same) voters who returned all but two Houses to Democratic control managed to award the White House to Republicans occasionally. Table 7.1 sets forth the observed pattern for the period 1936–1972, a convenient ten elections, and also displays the results of ten simulations of ten elections each, using a simple Bernoulli trials model.[2] The probability of a Democratic victory at any election was set equal to .65, which was determined by the belief that there are about twice as many Democrats as Republicans in the presidential electorate so that some probability around two-thirds is a good estimate of Democratic presidential election probability of success or voting support. Nevertheless, under that hypothesis there was one run of ten simulated elections in which the Republicans managed seven victories (simulation 7). Table 7.1 establishes two facts. First, presidential elections have been competitive between the parties in the modern era (the Democrats won six times and the Republicans four). Second, a totally naive model will adequately reproduce that pattern. It is clear that the same totally naive model interpreted on House partisan control history will substantially underpredict the frequency of Democratic control. That is, the Democrats have controlled the House more often than we should expect, simply on the basis of the number of Democrats in the electorate.

Now reconsider the partisan control patterns in the House in the later time period. What keeps the moving average of Figure 7.2 bounded above .5? The advantages of incumbents might explain the persistence of a pattern once established, but they do not immediately provide an explanation for the pattern of Democratic dominance—the bias in the system in favor of Democratic control. Nor do they account for the return to Democratic dominance after the two instances of Republican partisan control.

A REFORMULATION OF THE PROBLEM

If the probability of successfully reelecting members to the House by a party is proportional to the vote division, then a frequency distribution of seats by partisan vote shares gives at least a partial picture of partisan competitiveness or noncompetitiveness. When the deciles are treated as categories of competitiveness for each party, the within-party distributions may be computed and compared. This is done in Table 7.2, which immediately reveals the advantage for the Democrats in safe seats in the 1970 election.

It is apparent that the congressional districting map falls distinctively across the distribution of partisanship in the electorate. In the remainder of this

TABLE 7.1
Partisan Control of the Presidency: Observed 1936–1972, and Simulations

		Simulation Trials (P = .65 = Probability of Democratic Victory)									
Election	Observed	1	2	3	4	5	6	7	8	9	10
1	D	D	D	D	R	D	R	D	R	R	D
2	D	R	R	D	D	D	D	R	D	R	R
3	D	D	D	D	R	R	D	R	D	R	R
4	D	D	D	D	D	D	D	R	D	R	D
5	R	D	D	D	D	D	D	D	D	D	R
6	R	D	D	R	D	D	D	R	D	D	D
7	D	D	D	D	R	R	D	D	D	D	R
8	D	D	R	D	R	D	R	R	D	R	R
9	R	R	R	D	R	D	D	R	R	D	R
10	R	D	D	D	R	R	D	R	D	D	D
Total D =	6	8	7	9	4	7	8	3	8	5	4
Total R =	4	2	3	1	6	3	2	7	2	5	6

Means across all ten simulations: $\bar{D} = 6.3$, $\bar{R} = 3.7$.

TABLE 7.2
Seat Safety Distributions in the U.S. House of Representatives by Party for the 1970 Election (based on Democratic proportions of the major party vote by vote proportion deciles)

		Proportion of Party Seats	
Vote Proportion Deciles		Democrats	Republicans
Safe	I	0.21	0.03
	II	0.08	0.01
	III	0.21	0.13
	IV	0.30	0.48
Competitive	V	0.20	0.35
Total		1.00	1.00

article, I will demonstrate that this particular overlay of districting on partisan preferences produces a pattern of Democratic control of Congress that is different from the pattern of party competition for the presidency. A different overlay, in a different electoral epoch, or in a different country, would produce different consequences.

The Importance of Seat Safety

In the argument which follows, seat safety plays a central role. This notion will be given a precise definition. It is used to explore some logical properties of the joint system of one-party dominance, seat safety differentials, and institutionalized political behavior.

The elaboration of these interrelationships sheds light on the way in which relatively fixed structural characteristics—in this case the distribution of seat safety—bias political outcomes. The problem may be reformulated now in metatheoretic fashion. What are the consequences of committing oneself to the sentence "The Democrats have more safe seats than the Republicans" as an explanation for the observed differences in partisan control in the presidency and Congress?

The moving average plot shows that the dynamic system, whatever it is, underlying the Democratic dominance of the House since 1930, tracks to some value above .5 Democratic. Ultimately, it will be possible to calculate a precise value toward which the system moves, a system telos that behaves much like the desired setting on a thermostat, but that calculation requires an explicit model. For the time being, note that after big wins and narrow wins, and even after Republican wins, the system returns to the neighborhood of the small partisan bias in favor of the Democrats.

It is equally worth noting that this bias does not operate to push the 1936 Democratic margin any higher. This suggests in turn that the telos of the system lies somewhere between .5 and the .77 proportion of seats the Democrats controlled after the 1936 election. It is argued below in addition, following Stokes and Iversen (1967), that certain other system forces also work to reduce the Democratic advantage.

The evidence from the seat and vote share distribution in 1970 leads to the hypothesis that the telos toward which the system tracks is a function of differential seat safety between the two parties. In the modern electoral epoch it must be the case that seat safety is higher on average for the Democrats. Intuitively, this differential in seat safety is a parameter of the underlying process continually driving the partisan control of the House into Democratic hands. Thus, we seek the form of the dynamic process overlaid on the short-run factors influencing electoral outcomes which determines the system control bias in favor of the Democrats as a function of Democratic seat safety advantage. Before attempting to specify the form of this underlying process, the notion of seat safety is rendered with somewhat greater precision.

Estimating Seat Safety

I believe there is a number, a pure theoretical quantity, which gives the probability that my Congressional district will continue in Democratic hands

after the next election if it is in Democratic hands before the election. Similarly, there is a number which gives the probability that my district will continue in Republican control after the next election if it is in the control of Republicans before the election. These conditional probabilities are seat safeties.

No good method of estimating seat safeties for individual districts is at hand, so we will seek some average for all districts. The focus is on Democratic advantage in control in the modern era, and for this reasonable time period it may be that seat safety is roughly constant. The stability of the moving average of control in Figure 7.2 after 1936 is consistent with this assumption. What matters, really, is not that average seat safety is constant over time but that it stays biased and bounded in a fairly narrow range so that the assumption that it is constant over time does no great harm. In fact, this assumption does very well as it turns out. The resulting measures of seat safety, d and r, once they are obtained, can be thought of as estimates of average values for each party for reasonable electoral epochs.

How can the seat safety probabilities be interpreted at the twice aggregated level of parties and electoral epochs? We defined d as the average conditional probability that an average House seat (selected at random from all House seats) remains in Democratic control after an election, given that it was in Democratic control before the election, for all elections throughout an electoral epoch. A similar definition holds for r (substitute "Republican" for "Democratic" here). These average values, d and r, are independent.

How can numerical estimates of the theoretical seat safety quantities d and r be obtained? Before any given election, the Democrats and Republicans hold known seats which do or do not continue in the control of each party after the election. The average seat safety conditional probabilities for each party at a given election can be estimated by forming the ratio of the seats controlled after an election that were also controlled before the election to the total controlled before the election. Once such a series of d and r values for particular elections are computed they may be averaged over time and the doubly aggregated seat safety values then estimated for an electoral epoch.

A numerical example will help to fix ideas. Prior to the election of 1970 the Democrats controlled 243 House seats. In the 1970 election, although they won 18 seats formerly controlled by Republicans, the Democrats lost 8 seats to the Republicans. The 18 new seats are ignored in the calculation of d, but the 8 seats lost from those formerly controlled are sufficient information along with the original seats controlled to compute a seat safety estimate. A total of 235 seats were retained in Democratic control out of the 243 originally in Democratic control. The ratio of these two numbers produces an estimate of seat safety of approximately .97. Under the interpretations which have been developed and subject to the idiosyncratic error associated with the 1970 election, this number may be interpreted to assert that in 1970 the average

conditional probability that a Democratic House seat chosen at random would remain in control of the Democratic Party after the election was .97. For the same election using the same procedure, r—estimate of seat safety for the Republican party—is approximately .91. Consistent with our expectations, the Democratic seat safety is larger than the Republican seat safety. Note also the very high value of these numbers and their obvious independence. It turns out in the model developed below that these particular seat safety values for 1970 imply a long-run expected Democratic control proportion (if these values were fixed for all time) of .74, i.e., about three out of four House seats in Democratic hands. The value of .74 is between .5 and the 1936 observed Democratic control proportion of .77.

The procedure of the numerical example can be repeated for each election in the 20th century. When this is done the estimates give a portrait of the dynamics of seat safety subject to error due to short-run idiosyncratic effects associated with each particular election. A time plot of seat safety values estimated in this way is displayed in Figure 7.3. Figure 7.3 is consistent with the earlier conclusions based on partisan control with respect to the division of House partisan control history into two epochs. Seat safety in the modern era exhibits two features. First, in the modern era the mean value of seat safety for each party is higher than in the earlier era. Second, in the modern era the over-time variation in the seat safety estimates is smaller.

Aggregated over individual seats and over time, seat safety may thus be empirically estimated. Such summary numbers for electoral or party system epochs provide an empirical handle for the abstract pure theoretical construct seat safety. The price that is paid is that argument must proceed at the level of the system—a collection of districts—rather than at the level of each individual district; and at the level of time intervals—collections of elections—rather than at each individual election. In the argument that follows, average seat safeties for each party, d and r, are taken as parameters in a dynamic representation of the influence of seat safety on partisan control.

The Connection Between Seat Safety and Partisan Control

The substantive problem may now be cast as a modeling problem. How does seat safety determine partisan control? Determine is meant in a mediated sense, for clearly seat safety is itself determined by a complex of other considerations. Nevertheless, it is instructive to proceed from this fairly high level of abstraction seeking insight into the problem of one-party domination in the modern history of the House.

A slight addition to our notational baggage is useful. Let D_t denote the proportion of all House seats which are controlled by the Democratic Party immediately prior to election t. Similarly, let R_t denote the proportion of all

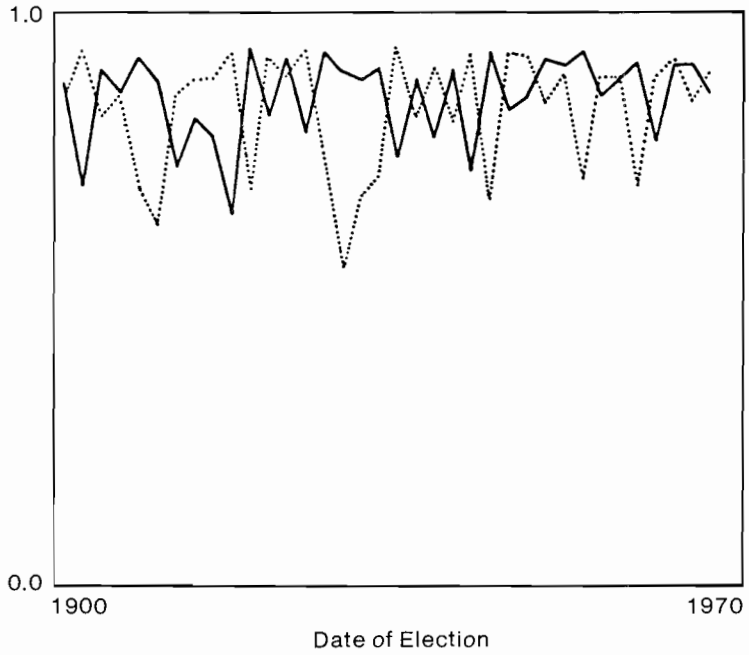

FIGURE 7.3
Seat Safety Estimates for Democrats (solid line) and Republicans (dotted line) in the U.S. House of Representatives, 1900-1970 (in proportions of original seats retained after each election by date of election)

House seats which are Republican at election t. The phenomenon to be explained, or accounted for, or at least with which we wish to come to grips, is the time path of D_t. The graph of D_t in the 20th century was set forth above in Figure 7.1.

The simplest model that specifies partisan control as a function of party seat safeties in an era is a slight generalization of the accounting relationship which must obtain between any two sequential Congresses. The seats the Democrats control after an election are composed of those seats the party successfully defended plus seats gained from other parties. It is assumed here that the only other party is the Republican party; i.e., a two-party system is assumed for purposes of argument. Hence, the Republican seats after the election are similarly composed of the seats they defended successfully plus those won from the Democrats. Suppose a legislature of 150 seats with 100

seats in Democratic control and 50 seats in Republican control prior to election t. Suppose further that the Democrats retain 80 seats and the Republicans retain 35 seats. In a two-party system of constant total seats these facts are sufficient to determine completely legislative control at $t + 1$ and to provide estimates of d_t and r_t. Symbolically in terms of proportions this accounting relationship for two elections may be written:

$$D_{t+1} = d_t D_t + (1 - r_t)R_t. \tag{1}$$

The implied values for seat safety are $d_t = 0.80$ and $r_t = 0.70$. Eq. (1) may be read as asserting that the level of Democratic control at $t + 1$ arises from seats retained from those originally controlled at t, i.e., total retention of seats in the amount of $d_t D_t$; plus those originally controlled by the Republicans at t lost to the Democrats at a rate $1 - r_t$, i.e., total Democratic party gains of $(1 - r_t)R_t$. Now in a two-party system with seat shares measured as a proportion of total seats, the following identity holds for any t:

$$D_t + R_t = 1.0. \tag{2}$$

Thus, R_t may be expressed in terms of D_t and Eq. (1) may be rewritten as:

$$D_{t+1} = d_t D_t + (1 - r_t)(1 - D_t). \tag{3}$$

Two more steps and the elementary model of party control will be in hand. First, following the discussion of seat safety and especially relying on the fact that the moving average of Democratic control never fell below .5 in the modern era (see Figure 7.2 above), assume that seat safety is fixed for both parties in electoral epochs; hence, suppress the index t on r_t and d_t. Substituting these average values, r and d, into Eq. (3) produces:

$$D_{t+1} = d D_t + (1 - r)(1 - D_t). \tag{4}$$

The form specified in Eq. (4) can be thought of as determining the underlying effect of seat safety in an epoch after the short-run effects of particular elections are removed from d_t and r_t. It specifies, for each election, a direction of movement or stability for partisan control independent of short-run electoral forces. Eq. (4) gives the law connecting seat safety in the system as a whole to partisan control in the system as a whole. It expresses the institutional consequence, the logic, of a relatively fixed system of stable areal partisan seat continuity.

Each election, of course, has overlaid upon it the results of the particular conditions obtaining in that election, whatever they may be—Watergate or

job scarcity or gasoline prices. Thus, the second step required to complete the model is to represent these short-run forces. This second step is postponed until some properties of the model in the form given by Eq. (4) have been examined.

PROPERTIES OF THE DETERMINISTIC MODEL

The model set forth in Eq. (4) can be manipulated algebraically in various ways to see what can be learned from it—what it implies. The model is a linear difference equation with constant coefficients, and it is true, happily, that the theory of such mathematical objects is complete (Goldberg, 1958) and may be exploited.

Over-Time Behavior of Model

The first question to be asked is: What is the over-time behavior of the model? What qualitative pattern does it generate? Put another way, with short-run electoral conditions suppressed, what does the model dictate in terms of partisan control? These questions may be studied both numerically and analytically.

In order to study the model's behavior numerically some explicit quantitative parameter assignments are required. In particular, the process must be started somewhere, some initial condition D_0 (the Democratic proportion of seats held at time zero, when we start looking at the process) must be chosen, and values must be assigned to d and r. Now the entire structure of the argument up to now, and the entire structure of the argument to come, rests on one fundamental relationship between Democrats and Republicans. To wit, the Democrats have more safe seats than do the Republicans. In terms of the parameters of the model of Eq. (4), this may be expressed by the inequality:

$$r < d. \tag{5}$$

Ineq. (5) is only a hypothesis. Numbers—estimates—were computed above which are consistent with Ineq. (5) but it is an assertion of an empirical regularity. In other epochs it may be false or it may fluctuate. We take Ineq. (5) as true of the House since 1930.

If the relationship between r and d is to reflect bias in favor of the Democratic Party, no such case can be made, based on the model, for biasing the initial condition D_0 in similar fashion. Thus, choose $D_0 = .5$, which is essentially what its value was following the election of 1930—a convenient congruence between history and our technical requirements. Some estimates for r and d were given above in the discussion of seat safety and others are

discussed below. For a first look at model behavior set $d = .89$ and $r = .79$—an assignment that certainly satisfies Ineq. (5).

With these particular parameter assignments and with no additional short-run electoral effects imposed on the model, a history of Democratic party control is generated and displayed in Figure 7.4 for ten elections in sequence. The future is indeed deterministic and indeed highly favorable to the Democrats under these particular numerical hypotheses. From a completely competitive initial condition the level of Democratic control moves steadily upward, evidently at an ever decreasing rate, approaching a dotted line on the graph of the figure that is labeled D^*. One can interpret the sequence of large black dots as describing a hypothetical history absent all other events—in short, absent politics.

The quantity D^* gives an equilibrium value for the process—if D_0 is set equal to D^* the process never moves from that value—toward which the process moves but never quite reaches. In fact, D^* is the only equilibrium value of the system in the example and it is stable. If the system is displaced from D^* (by short-run electoral events perhaps?) it will move back toward D^*. This is true whether the displacement is below or above the equilibrium value; i.e., it is globally stable.

The quantity D^*, the global equilibrium of the system, can be expressed analytically in the form:

$$D^* = (1 - r)/(2 - d - r). \tag{6}$$

Eq. (6), defining D^*, shows that the equilibrium value, the telos of the system toward which the system moves, depends only on the seat safety parameters d and r. Thus, the underlying logic of the seat safety system is to move the level of Democratic control toward a value determined by Eq. (6). For the particular numerical assignments underlying Figure 7.4, D^* takes the value of .656. In the long run, if the process is left undisturbed, the Democratic Party will come to control about two-thirds of the seats in our numerical example.

This systematic and biased process is hidden by the short-run fluctuations that result from politics as usual. Each of the points on the plot in Figure 7.4 can be thought of as some initial condition, and there could be others at intermediate values or below .5, and also still others above the equilibrium value. Any election result determines a new status quo ante for a dynamic thrust toward equilibrium according to the law given in Eq. (4). Some short-run forces may speed up the approach toward equilibrium, i.e., enhance the underlying system bias in favor of the Democratic Party. Other short-run forces may slow the approach to equilibrium or even reverse it. The point to be made is that the underlying systematically-biased dynamic is always present, no matter what other short-run conditions obtain, provided only that Ineq. (5) holds, $r < d$.

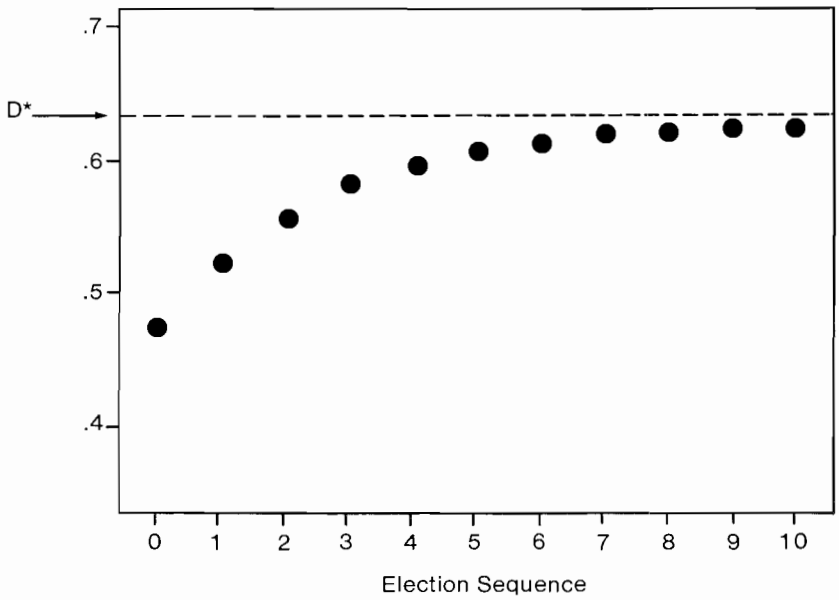

FIGURE 7.4
A Model of Democratic Control Over Time: Proportion of House Seats Controlled by the Democratic Party in the Absence of Short-Term Effects
($D_0 = .50$; $d = .89$; $r = .79$)

The equilibrium value of the party dominance model of Eq. (4) defined in Eq. (6) is a candidate for characterizing the system consequences of differentials in inter-party seat safety for system partisan control. With conditions normal and politics as usual, the model moves toward an equilibrium in monotonic fashion following behavior similar to that exhibited in Figure 7.4. This normal condition may be written as:

$$1 < d + r. \tag{7}$$

The principal import of Ineq. (7) is that average seat safety in the system as a whole exceeds .5. When that is true, oscillation in the time path of partisan control is precluded. For the major parties in the United States (and it turns out to be true also for major parties in at least two non-American systems) seat safety probably exceeds .5 always. Thus, the normal condition of modern House electoral politics is d and r each greater than one-half and $r < d$, Ineq.

(5). The joint consequence of these empirical facts is that the underlying institutional party habit tracks smoothly, monotonically, and inexorably toward an unknown equilibrium of Democratic control larger than .5.

I have urged the view that D^* is a likely candidate for measuring or representing the bias in the overall system in favor of the Democrats. It is clear that D^* is a measure of the long-run expected level of Democratic legislative control, but is it, however, a reasonable measure of the system bias arising from seat safety? The answer is yes and may be justified by examining three special cases of the consequences on D^* of specific relationships between d and r.

If there is no bias in the seat safety relationship in the system, then a measure of system bias in partisan control terms should be neutral. This condition obtains for D^* and it can be shown:

(8) If $d = r$, then $D^* = .5$.

Thus, D^* is a neutral measure of system bias just in case the competitive status of the parties is equal.

Whenever asymmetry in the competitive status of seat safety between the parties obtains, a measure of system bias in partisan control should reflect that fact. Is this true of D^*? It is and the following propositions hold:

(9) If $d > r$, then $D^* > .5$;
(10) If $d < r$, then $D^* < .5$.

I conclude on the basis of propositions (8), (9), and (10) that D^* is a reasonable measure of system bias in partisan control arising from party differentials in seat safety. More can be shown analytically. D^* does move in the correct direction for marginal changes in the values of seat safety for the two parties. Second, the effect of each seat safety on D^* becomes more pronounced as the seat safety parameters move away from zero.

Because asymmetry between r and d exists empirically, $r < d$, a plot of D^* as a function of the seat safety parameters will trace out a set of curves in some appropriately chosen space. To obtain a representation in two dimensions, which is technically convenient, we turn to an elaboration of the notion of institutionalized political behavior, which is substantively important (Huntington, 1965; Przeworski and Sprague, 1971).

INSTITUTIONALIZATION

Overall institutionalization of partisan seat safety may be described by the average of d and r. This number may be interpreted as giving the probability that political control conditions in the legislature will be the same after an

election as they were before. It is intuitively a description of the probability that all political actors behaving in a current election reproduce the same behavior as the occupants of those same statuses did at the previous election. Institutionalization of politics means, in this formulation, the probability that all actors continue to act the same way at time t as they did at time $t - 1$ with respect to some relevant behavior.

Now if everyone acts the same way in election t that they did in election $t - 1$, with high probability, the system can be considered highly institutionalized. Defining institutionalization by the average of d and r is on all fours with this idea. If everyone acts the same way the same distribution of party control will result. What is of interest is how does D^*, our measure of system bias, vary as institutionalization varies? Define institutionalization by:

$$(11) \qquad I = (d + r)/2.$$

In order to obtain a representation in the (I, D^*) plane we generate curves for fixed differences between r and d. The results of this manipulation are set forth in Figure 7.5 for the area of the plane of empirical interest for the House in the modern era.

The curves displayed in Figure 7.5 deserve close study. First note that if a level of institutionalization is fixed, then increasing the magnitude of the difference between d and r in a direction favorable to the Democrats (moving upward through the family of curves in a vertical direction) increases the system bias. This is perfectly reasonable. Holding I fixed requires moving d upward and r downward in this case. For example, fix I at the .65 level with $d = .7$ and $r = .6$—a difference of .1 in seat safety. Then keep I fixed at .65 and set $d = .75$ and $r = .55$—a difference of .2. What happens to the value of D^*? It moves from a value of .57 to a value of .65. Increasing the difference in seat safety at a fixed level of overall institutionalization has a correct consequence.

Second, consider fixing D^* and moving across the curves in a horizontal direction from left to right, i.e., increase institutionalization while maintaining some fixed long-run advantage for the Democrats. The figure shows and numerical calculations confirm that this results in a decreasing difference between the seat safety values for Democrats and Republicans. But this means that as institutionalization increases, a smaller and smaller difference in seat safety is required to maintain the same long-run system advantage. In fact, an even stronger statement may be maintained, for it is possible that the difference between d and r may be decreased and yet D^* is increased; i.e., lowering the relative seat safety advantage of the Democrats can result in an increase in their long-run system advantage! This requires traversing the curves in Figure 7.5 in a direction from lower left to upper right. Some numerical examples are set forth in Table 7.3.

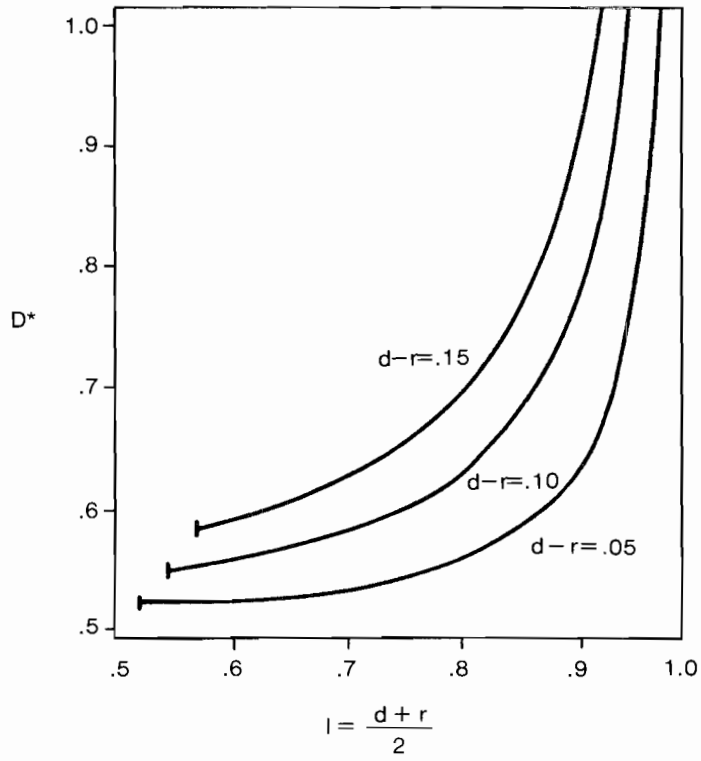

FIGURE 7.5
System Bias in the Long-Run for Fixed Differences in Partisan Seat Safety as a Function of Average Seat Safety for Both Parties

The last four entries in Table 7.3 are especially instructive. A fixed advantage in seat safety of .05 yields a high system advantage just in case overall institutionalization is high, but almost no advantage when institutionalization is low. Furthermore, tracing out the curve while keeping the seat safety difference fixed results in large changes in system advantage just in case the system is already highly institutionalized, but virtually no change in system advantage at low levels of institutionalization. Those particular results characterize the curve in Figure 7.5 closest to the bottom and farthest to the right. And that curve is not very far away from the most likely empirical curve characterizing the relative position of Democrats and Republicans in the period 1930–1970. For the 1930–1970 period average seat safeties are $d = .917$ and $r = .873$. Using these values the index of institutionalization,

TABLE 7.3
Various Examples of Seat Safety, Long-Run System Bias, and Institutionalization (numerical examples in the (I,D*) plane)

Seat Safety (d)	Seat Safety (r)	Institutionalization (I)	Seat Safety Advantage (d − r)	Long-Run System Bias (D*)
.70	.60	.650	.10	.57
.75	.55	.650	.20	.64
.85	.70	.775	.15	.67
.95	.85	.900	.10	.75
.90	.85	.875	.05	.60
.95	.90	.925	.05	.67
.60	.55	.575	.05	.529
.65	.60	.625	.05	.533

I, takes on the value .895, the difference between d and r is .044, and the long-run system advantage, D^*, has the value .60, which compares favorably with the mean Democratic control since 1936 of .57.

The relative difference in seat safety between the parties in the House is very small but it is consequential precisely because the system is highly institutionalized. The curves in Figure 7.5 show that small differences in relative seat safety advantage are translated into large long-run system advantage in seat control only when the overall level of continuity in seat control is high for both parties. But that is the empirical condition of House electoral politics in the United States. Democratic control of Congress is to be expected because a small advantage in seat safety is embedded in a highly institutionalized system of electoral politics. These results may be summarized in the following strategic property of the system:

Principle I: Small differences in political resources lead to large differences in political advantage whenever the system is highly structured.

Consider achieving influence in the decision process in a court of law compared with influencing the decision process in a legislature. In the court of law small differences in attorneys' skills may be very important, whereas small differences in the skills of lobbyists in the legislature are not likely to be consequential. The essential difference in the two situations is the level of institutionalization, or structuring, of the influence process.

Before moving on to a model elaboration including short-run electoral conditions, one last consequence of institutionalization for the behavior of the model is considered. The question is simple: How rapidly do the Democrats approach the long-run expected advantage they have in the modern system?

Since every election can be viewed as a new initial condition or a new displacement from equilibrium, what is at issue here is the rate of first-period recovery toward equilibrium after some disturbance. The difficulty is that the rate of movement toward equilibrium is a function not only of the parameters r and d but also of the magnitude of the displacement; hence, this must be corrected for in some fashion. A trick that works is to express the first period change toward equilibrium—the first period change is the rate of movement in this discrete time formulation, relative to the displacement from equilibrium. The quantity may be written as:

$$\frac{\Delta D_0}{D^* - D_0} = 2 - d - r. \tag{12}$$

Inspection of the result in Eq. (12) shows that when $r = d = 0$, i.e., when institutionalization is zero, the first period recovery is two times the displacement. The process wildly overshoots D^*. This represents the extreme point in the realm of oscillation where there is undamped but nonexplosive change with the parties exactly recovering their original partisan strength every other election. The interested reader should turn back to the model of Eq. (4) and reconstruct the pattern of D_0, $1 - D_0$, D_0, . . . , and so forth. As r and d move from zero through average values less than one, the first period recovery still overshoots, but this is in the realm of damped or convergent oscillation in the model. Finally, the realm of smooth system response is for average values of seat safety above one, i.e., politics with at least some non-pathological institutionalization, and within this range the first period recovery toward equilibrium is some (decreasing) proportion of the displacement. And that is the essential point as the graph in Figure 7.6 shows. As institutionalization approaches its maximum of one, the rate of proportional recovery goes toward zero and we have another strategic property of the system, which is summarized in the following:

Principle II: Under conditions of highly institutionalized politics the price of system advantage in the long run is a slow rate of approach to that advantage in the short run.

The system, then, "exhibits a contradiction Comrade," for the properties that give a long-run expected advantage to the Democrats—a slight advantage in seat safety rate coupled with a high level of system institutionalization—also imply that the approach or recovery will be slow. Now every election is subject to short-run electoral effects, and hence this short-run slow recovery rate is just like the system bias in the long run—ever present. We will return to this slow response feature of the House partisan control system under the guise of system memory after elaborating short-run effects.

Pardon my intrusion into this article, but let me add a note to help explain equation 12.

First note that

(a) $D_{t+1} = dD_t + (1-r) + rD_t - D_t$

which is a mild simplification of equation 4 as found on page 236 of the article. Then note that $\Delta D_t = D_{t+1} - D_t$. Now, subtract D_t from both sides of equation (a) above and you have

(b) $\Delta D_t = dD_t + (1-r) + rD_t - 2D_t$

which can be re-arranged as

(c) $\Delta D_t = D_t(d+r-2) + (1 - r)$.

Now let us return to equation 12. Let us substitute our equation for ΔD_t (equation c) as well as the formula for the equilibrium point, D* (see equation 6 on page 238), into equation 12, and we have (after noting the time subscript t=0),

(d) $$\frac{D_0(d + r - 2)) + (1 - r)}{[(1-r)/(2-d-r)] - D_0}$$

which (after multiplying the top and bottom by -1) is the same as

(e) $$\frac{D_0(2-d-r)) - (1 - r)}{D_0 - [(1-r)/(2-d-r)]}$$

which (after multiplying the top and bottom by 2-d-r) is the same as

(f) $$\frac{(2-d-r)[D_0(2-d-r) - (1 - r)]}{D_0(2-d-r) - (1-r)}$$ which equals 2 - d - r.

This is where we want to arrive in equation 12.

Now, back to the article.
Courtney Brown

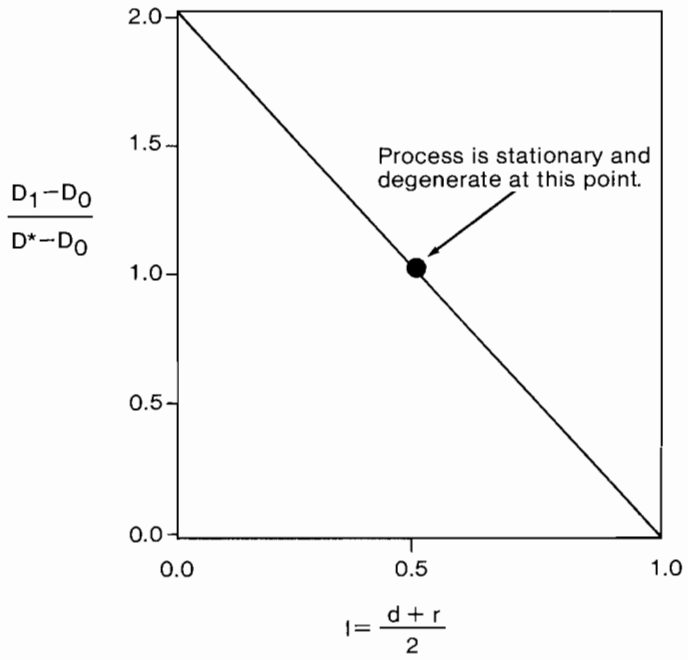

FIGURE 7.6
First Period Recovery as a Function of Institutionalization (measured in proportions of displacements from equilibrium)

The deterministic model, elementary in form as it is, nevertheless turns out to be quite rich analytically. At least one qualitative result is surprising, Principle I, and the general behavior of the model shows a consistent set of relationships between system partisan control and the driving parameters of seat safety. It is time to add some short-run politics.

A LONG-RUN STRUCTURE FOR SHORT-RUN ERROR

To simulate realism, a stochastic component can be added to the model as an additive random shock. If the shock is too large it will overwhelm the deterministic component, and if the shock is too small no realism will be achieved. The magnitude of shock and its variance are in part a function of voter habit, and this is represented in simulations as a fixed probability interpreted as the likelihood of not switching between parties on the average.

A value of about .75 for voter habit has been assumed. This is the principal regulator on shock magnitude, although the variance of the distribution also can be controlled independently.

In addition to voter habit, which must be reasonably high in a highly institutionalized system, a result due to Stokes and Iversen (1967) is exploited. Stokes and Iversen utilize a random walk model of competition to simulate voting between the parties and conclude that if the model were correct, then either the Democratic Party or the Republican Party would have been out of business by now. The inference is that as partisan control margins (vote proportions) become extreme, forces arise in the American political system working against the fortunes of the dominant party. This suggestion may be exploited in constructing the error term. It is implemented by making the sign of the random shock depend inversely on the magnitude of D_t. Thus, the probability of a negative shock is made proportional to $1 - D_t$ and, conversely, the probability of a negative shock is made proportional to D_t. But in a system that is biased toward D^* larger than .5, the probability of a negative shock will be on average more likely than a positive shock. The seat safety advantage pushes, deterministically, the level of Democratic partisan legislative control steadily upward. The Stokes-Iversen error term works against this tendency, and the more so the higher D_t becomes—a long-run structure for short-run error.

Making the sign of the influence of short-run political fortunes depend (probabilistically) on the magnitude of D_t is the essence of the Stokes-Iversen error conception. As D_t approaches high values, like the 1936 value for House Democratic partisan control, the probability of a negative counterforce becomes larger and larger. This gives the push and pull so characterisic of the 1936 to 1938 drop or the 1964 to 1966 drop. When short-run forces (by accident or design) push the partisan control level to an extreme value, the return movement is nearly certain, and the Stokes-Iversen error term tries to capture that reality. The bigger the aberrant displacement the more certain the system retaliation. This logic also may be used to ape off-year on-year Congressional voting turnout effects (the presence of the President is a random shock that is positive) but, most importantly for our purposes, it copies the off-year fall-back in House seat control when some President (Roosevelt in 1936 or Johnson in 1964) has particularly long coattails. And so the push and pull in the model copies in a simple abstract fashion the interaction of institutional seat safety biases and short-run electoral effects.

Will this elementary and now stochastic model reproduce the essential pattern of legislative control in the modern House? The answer is yes if pattern is used to mean expected frequency of partisan control in an epoch. The relevant history and two simulation runs are set out in Table 7.4. In the

TABLE 7.4
Simulated and Observed Time Paths of Democratic Proportions of the House of Representatives

Congress	First Simulation		Second Simulation		Observed, 1930–1970
	D_t	Shock	D_t	Shock	
0	.50	—	.50	—	.50
1	.55	.00+	.46[a]	−.09	.72
2	.55	−.03	.51	−.02	.74
3	.59	0	.55	0	.77
4	.53	−.08	.61	.03	.60
5	.57	.00+	.64	.01	.61
6	.60	0	.65	.00+	.51
7	.51	−.11	.65	0	.56
8	.53	−.03	.56	−.10	.43[a]
9	.47[a]	−.10	.49[a]	−.10	.60
10	.53	0	.49[a]	−.05	.54
11	.61	.04	.57	.02	.49[a]
12	.58	−.05	.50	−.09	.53
13	.49[a]	−.11	.59	.03	.54
14	.56	.01	.64	.03	.65
15	.58	−.01	.54	−.10	.60
16	.60	.00+	.58	0	.59
17	.62	0	.60	0	.68
18	.56	−.07	.59	−.03	.57
19	.64	.04	.63	.01	.56
20	.52	−.12	.64	0	.59

For Simulations: $D_0 = .5$; $d = .89$; $r = .79$; probability of shock at $t = .8$; $D^* = .656$; probability shock is positive is $1 - D_t$.

[a]House controlled by Republicans.

two simulations displayed in the table, the Republicans controlled two Houses and three Houses, which is in close agreement with the observed frequency. The model, with the addition of a Stokes-Iversen error representing short-run electoral effects, will produce the basic descriptive facts which motivated its development. An inspection of the magnitudes of partisan control as they pass through time in the two simulations also have an intuitively reasonable appearance. Some results, varying r and d, with other conditions the same, are shown in Table 7.5. I conclude that the model will represent adequately the dynamics of partisan control at the level of the system for the special but important pattern of frequency of partisan control.

In terms of the model, the elections of 1932, 1934, and 1936 must have included very rare short-run events. Events of such magnitude have some very long-run consequences in highly institutionalized systems, as I will now try to show.

TABLE 7.5
Frequency of Republican Control of House of Representatives, In Simulations with Varying Parameters

	Simulations												
Parameters	1	2	3	4	5	6	7	8	9	10	11	12	13
d	.77	.97	.97	.97	.93	.93	.93	.87	.89	.89	.89	.89	.89
r	.69	.91	.91	.91	.77	.77	.77	.79	.79	.79	.79	.79	.79
Frequency of Republican Control	1	6	3	0	0	0	0	8	2	0	3	1	2

SYSTEM MEMORY

There is a particular representation of the output of a linear system such as that specified by Eq. (4) that provides insight into the long-run consequences of particular extraordinary events like the election of 1936. The current level of Democratic partisan control can be decomposed into its components from some origin in terms of a sum of weighted system inputs, e.g., the particularly large positive shock in 1936. Define U_t as the system input at time t and W_i as its weight. The weighting sequence representation of current system output, i.e., the current level of Democratic partisan control, can be written as:

$$D_t = \sum_{i=0}^{i=t} W_i U_{t-i}. \tag{13}$$

For $t = 3$, the definition in Eq. (13) produces, when written out:

$$D_t = W_3U_0 + W_2U_1 + W_1U_2 + W_0U_3. \tag{14}$$

It can be shown (Cortes *et al.*, 1974) that the W_i for the system in the representation of Eq. (13) are given by:

$$W_i = (d + r - 1)^i. \tag{15}$$

The intent of Eq. (13) and its significance can now be seen by recalling that both r and d are relatively high; hence, the W_i are proportions close to one raised to successive powers. Because they are close to one they die out slowly, and Eq. (14) shows that each input is weighted by a proportion to a power of i with the power increasing as the age of the input increases. Old inputs, that is to say old short-run effects, are discounted many times and thus contribute a smaller component to current output than more recent short-run

effects or inputs. At least they would contribute less if all shocks were of the same magnitude. However, a 1936-style shock is of uncommon magnitude, and this occurs in a highly institutionalized system, i.e., one in which the average of d and r approaches one. If the institutionalization of the system is about .9, more than 10 percent of an input (a short-run electoral effect) still survives after 40 years (20 elections)! Short-run events have long-run consequences under conditions of highly institutionalized politics. This interpretation of the system consequence of highly institutionalized politics is consistent with common views of the importance of incumbency (but see Mann, 1978).

CONCLUSIONS

Static facts like Democratic legislative hegemony in the modern Congress may be explained or at least investigated by means of dynamic representations. The strategy of attack used was to construct the property of Democratic partisan control as a process in time.

The theory asserts three propositions. (1) There is a fixed structure of areal seat safety which determines partisan seat control. (2) This connection is dynamic. (3) There is asymmetry in seat safety between the parties.

The theory was modeled in the most elementary manner. A representation of partisan control was defined, D_t, two pure theoretical quantities were defined for seat safety, r and d, and these were connected by an accounting relationship which specified a dynamic rule. The model could not have been constructed of simpler elements.

The elementary model turns out to lend itself to manipulation, however, and a number of analytic quantities of interest were derived from it. Elementary as the model is, its deductive consequences are quite rich. Ease in obtaining these consequences arises from the ability to exploit the rather complete mathematical theory available for the particular model form.

The most important quantity to emerge from the deductive manipulation is D^*—a measure of the bias in the system of partisan control given by the fixed structure of the seat safety system. The behavior of this quantity as institutionalization varies was surprising, but the number itself emerges as a useful summary of an important underlying tendency of electoral systems measured in the coin that counts, partisan control of legislatures.

The substantive results depend on the model, jointly with the data, and also depend for much of their force on two empirically tested hypotheses. First, central to the argument is the hypothesis that r is less than d. Second, equally central to the more surprising results, is the empirical hypothesis that r and d tend toward high values. These empirical hypotheses find their summary in the statement that there is asymmetry in seat safety between the parties, and

that the institutionalization of electoral behavior is high in the congressional electoral system.

Two propositions emerged from the analysis that seemed of sufficient generality to set out as principles. First, small differences in seat safety between the parties under conditions of high levels of institutionalization lead to large long-run partisan seat advantages. The language in which the principle is stated above is more general. The second principle asserts that high levels of institutionalization enforce a trade-off between short- and long-run system advantages.

There were a few other assertions that emerged along the way that are worthwhile recalling. The long-run system advantage of the Democratic Party in the House of Representatives in the modern era is $D^* = .60$. That is a number to remember. It summarizes a fundamental property of the modern era. The analysis of the logical structure of the model considered as a linear system produced a result based on the memory of the partisan dominance system—the persistence of the influence of past events. Under typical modern conditions in the House, i.e., under conditions of high institutionalization of politics, some effect from short-run electoral disturbances survives for a very long time. Probably the effects of the 1936 election were still felt 40 years later in 1976.

Why do the Democrats dominate the modern House? Because the Democrats have more safe seats then the Republicans. And we now have some insight into the logical consequences of that explanatory sentence. But these results may be interpreted more generally. The basic result is that small seat safety biases produce one-party dominance under conditions of highly institutionalized behavior. If the seat safety biases are stable over time, as they must be, then one-party dominance in legislatures is not at all surprising. One-party dominance in legislatures is typical, not atypical. It is the behavioral norm. It is what should be expected. It should be expected because politics in the electoral-legislative system is highly institutionalized and because symmetry in seat safety between the parties must occur with probability zero. Under these conditions, long periods of one-party dominance in legislatures are to be expected.

NOTES

Roger Robert Huckfeldt, now of the Department of Political Science of Louisiana State University-Baton Rouge, collected a major portion of the data used for the analyses. Accordingly, I am happy to hold him responsible for errors in these data. The opportunity to present the lecture on which this paper is based was provided by the Department of Political Science and the Benjamin F. Shambaugh Fund of the University of Iowa. I wish to thank G. R. Boynton for the opportunity and Gerhard Loewenberg for his ruthless editorial advice.

1. One-party dominance of the House as a phenomenon of substantive interest is closely related to several other scientific concerns by investigators of both congressional behavior and mass electoral behavior. Indeed, my original interest in this problem was first motivated by the difficulty of reconciling, to my own satisfaction, arguments and results by Stokes (1967) on the nationalization of electoral forces with the apparent imperviousness of incumbent congressmen to electoral attack reported by Cummings (1966). This issue has been reopened and forcefully presented to the scientific community most recently by Mann (1978). A related set of issues turns on incumbency effects, i.e., the electoral advantages of incumbency in House elections and, also related but not identical, the time pattern of the frequency distribution of electorally marginal seats, vanishing or not. The *opus classicus* on marginals is Mayhew (1974) which should be read in conjunction with Ferejohn (1977) and Fiorina (1977). Incumbency effects have been studied intensively from a variety of perspectives (Erikson, 1971; Kostroski, 1973; Abramowitz, 1975; Cover, 1977; Nelson, 1978; Born, 1979). There has also been the long-standing and continuing concern of political scientists with the drawing of district lines (Erikson, 1972; Bullock, 1975; Niemi and Deegan, 1978). Finally, it should be mentioned that political scientists have been concerned with the behavioral consequences of some of these empirical conditions (Burnham, 1975; Fiorina, 1974; Sinclair, 1976; Kuklinski, 1977; Sullivan and Uslaner, 1978; Brady *et al.*, 1979). The development below is probably most closely related to the scientific literature on incumbency advantages, although no direct use is made of results in that area.

2. The results in Table 7.1 for the simulation amount to the experiment of 100 trials of flipping a biased coin where the probability of heads, say, interpreted as a Democratic victory, is set equal to .65. The model is deliberately totally naive.

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PERSONAL ACCOUNT

7B. The Origin and Tortuous Progress of One-Party Dominance in Legislatures

JOHN SPRAGUE

The natural history of "One-Party Dominance in Legislatures" extends embarrassingly long in time. From a first recognition and statement of the problem through a tentative solution to a final finished written product covered more than a decade of my professional life—perhaps twelve or thirteen years—which surely provides a bad model of scientific productivity. On the other hand, the finished article is mature and self-contained just as is the motivating problem, and the argument had the benefit of occasional public airings in academic settings before finally being cast in written form. The final written result, in a first finished draft, which even so omitted reporting a considerable development of the analytic structure of the error term of the model, ran just shy of ninety pages. The time rate of production then amounted to eight pages a year when amortized across the period of creation. Such a production rate certainly would never lead to favorable tenure decisions if that was all one did.

There is probably nothing special about the history of this research except the length of its period of gestation and two eureka experiences along the way—one early, one late. Indeed, the origin of the problem emerged from a discrepancy or puzzle in existing literature and the effort from the outset was to somehow satisfactorily resolve the puzzle. This pattern follows textbook descriptions. Thought, theory, and modeling came first. Then came model manipulation. Finally, data collection and analysis followed in three stages, each occasioned by the necessity of making public presentations of the problem, argument, model, and deductive model results—always made more believable if empirically embedded.

We work in a profession of empiricists who dearly love to see the descriptive facts. I rely on this tendency when asked to give a colloquium or a lecture and always try to have some data-based tables and graphs to accompany my presentations. Thus, data collection is a necessity of the public nature of our discipline, though of course it may also be intrinsically

rewarding. It remains true, nevertheless, that my collection of data relevant to the model was motivated by the public nature of science rather than a desire to "test" (dreary word) the model. As it turned out, the data were somewhat more satisfying than I had any reason to hope or expect.

What follows is an attempt to reconstruct retrospectively the conditions obtaining at the time of discovery, the possible informational inputs, and a sketch of the chronology leading to final publication of a portion of the results. The most elegant result, unappreciated by reviewers and editors, remains unpublished, but perhaps some of it can be smuggled in here past hawkeyed (no pun intended) Shively.

PROBLEM ORIGIN

In the late 1960s I was a beginning assistant professor engaged in two programs of professional upgrading. First, I was in the middle of a long-run program to improve my technical, particularly mathematical, understanding. This program continues, if with less investment of time, today. Second, I was casting about intellectually in an attempt to escape the field of public law, judicial behavior, and things mostly legal, to some alternative focus on American politics, especially in its institutional manifestations. This latter activity coincided with the desirability of a departmental offering of a graduate seminar in legislative politics and thus the public interest turned out, as usual, to be consistent with my private interest—on this occasion an institutionally focused program of personal study. Ken Shepsle had not yet joined our department, and for a year or so it looked as if legislative politics would be an interesting line of investigation to pursue. Although my personal history ultimately did not go in that direction the reading for, and teaching of, that one seminar *did* furnish the basis for the puzzle of persistent one-party dominance of Congress.

At about this time a number of graduate students at Washington University developed interests that touched on some of my own—Karl Kurtz, Virginia Gray, Louis P. Westefield, Chuck Bullock, Rich DeLeon, and Dan Mazmanian. Some of these students were in that seminar (I refuse to dig up the old grade book to determine which ones), and discussions with them no doubt were important. Two other influences are easier to isolate—one a book and the other a person. The book was William McPhee's collection of essays *Formal Theories of Mass Behavior* (1963), which remains the single most intellectually exciting book I have ever read. The person was Adam Przeworski, who taught at Washington University before leaving for that pernicious institution, the University of Chicago, and with whom I still maintain some collaborative research interests.

It was in this context that the problem or puzzle was developed. My reading program was geared in a loose fashion toward the phenomenon of underlying, persistent, broad-gauge patterns of behavior—institutionalized behavior or practices. In reading for the seminar I juxtaposed Cummings (1966) with Stokes (1967), and it was clear that something was amiss. If Stokes's thesis was correct, the congressional constituency was increasingly being nationalized. How then could this thesis be reconciled with Cummings's data which showed lack of sensitivity to national politics when measured by the yardstick of incumbent reelection success, which apparently was on the increase? This led, in turn, to some desultory data collection so that election survival rates for the House could be plotted, varying conditions at entry for different cohorts (the most interesting condition was the artificial cohort of those elected at special elections). What strikes one from such plots is the overwhelming stability of House membership, and hence it seemed to me that if voting in congressional elections was becoming more nationalized (Stokes's result), the power of incumbency was still more powerful. This was not yet the problem or puzzle of one-party dominance, but it was bordering on it in terms of empirical materials.

Reflecting on these first empirical forays and on the conflicting—or so it seemed to me—analytical results of Stokes in contrast with Cummings's data, it was a very short step to the actual problem of legislative partisan dominance. The key fact to be recognized was that when Republicans gained control of the House in the modern era (twice), incumbency was not sufficient to insure their prolonged control. Why not? The problematic nature of this observation was particularly compelling when coupled with the close electoral margins in presidential elections and the notable success of the Republican party in securing control of the presidency. Thus the same electors produced a noncompetitive House and a competitive presidency. How could that be? The problem was in hand. Evidently the two electoral systems were distinct in some interesting ways. At the same time that I saw the problem *as a puzzle* the idea occurred that the long-run pattern of partisan dominance resulted from fixed underlying habit, that is, as institutionalization commonly labeled seat safety.

With the problem thus formulated there were two components for analysis. First, why did the presidency exhibit competitiveness? Second, why did the House show partisan persistence or continuity? Although ultimately profoundly substantively interesting, the first question at the abstract level of the system can be simply modeled, and in any event Dean Burnham was a colleague at the time. The second question was the one that attracted my attention and commanded my time. It is worth pointing out that some of this discovery of the problem and its formulation was worked out in the context of that seminar with active participation by captive graduate students.

THE MODELING ATTEMPT

Although the dominance of the House by the Democratic party is a big, fat static fact of modern political experience, it was clear from the outset that the central feature of the phenomenon was that it was a process in time, that is, it was a dynamic phenomenon. Hence, the modeling strategy consisted of attempts to connect observed cross-sectional one-party dominance with its presumed source in the distribution of seat safety and *its* dynamics. How soon or quickly results came I can no longer remember; however, some features of the process bear emphasis.

In this situation my style of work is to run through yellow writing tablets in a great chaos of sentences, brief notes, algebraic expressions, references that come to mind, tactical hypotheses, and so on. Pages judged worthless or wrong-headed are immediately discarded while those that are judged to have promise are numbered. All work is also dated, which turns out to be very useful when going back to notes that have been set aside because of the press of other obligations or other opportunities. At the end of a morning or a day or an evening one has a record of activity and also some sense of whether one has gotten any further with the problem at hand. After one session or thirty as the case may be, one either has results, however partial, or one has been stumped. If I have results, my habit is then to consolidate them carefully into a systematic (dated) set of notes suitable as a basis for presenting to colleagues or students the essentials of the argument and model results.

For a few brief beautiful years when Adam Przeworski was a colleague (and when he happened to be in residence), intermediate results would have been shared with him and useful progress would usually result. We had a remarkable ability to interact profitably over modeling problems, which I now understand was a truly rare condition. The key, I think, was a mutual willingness to suspend disbelief in discussion in order to see what the kernel of insight was that so interested the other. In any event, Przeworski's role for this particular modeling attempt was straightforward in another regard. He provided me with my first opportunity to trot out the results in a seminar he was conducting. This privilege was extended in spite of his frequently chiding me for studying anything so narrow as Congress.

My program of personal study and particularly the influence of McPhee's book (1963) entered the modeling strategies I pursued. In the course of my self-improvement I had worked through the marvelous difference equations text by Goldberg (1958), motivated by a reference to it in one of McPhee's essays. The lasting lesson of the McPhee essays for me was that mathematics could be used informally and without pomposity to attack successfully fundamental and broad-gauge questions of social science, that is, with genuine insight while remaining readable and empirical. I know of no better

counterargument to the charge that to quantify or model in social science is to trivialize human behavior than to read these splendid essays of McPhee. Frequently his models include some dynamic components. I had recently studied a dynamics text. These were both tools I had at hand and inspiration for the possibilities of such analysis. It was natural, then, to attack the problem as one in dynamic modeling both for general substantive reasons and for reasons of personal intellectual experience.

After I made an initial presentation to Przeworski's seminar, the model and a few crude plots were consigned to the files (after all, the problem was now solved) and stayed there for perhaps three years. Occasionally some version might be shared with my own graduate students to make an instructional point but no serious *research* effort was undertaken for some time. Here matters stood until it was time to travel.

EMPIRICAL DEVELOPMENT AND SIMULATION

The initial opportunity to go professionally public with this model, albeit in an early version, was provided by an invitation to visit the University of Kentucky and present a colloquium. This was arranged by Virginia Gray, long since gone to Minnesota, and provided immediate stimulus to tidy up my thesis concerning one-party dominance. All in all, the presentation itself went very poorly; it was badly attended and not well understood. That is of minor consequence in light of the industry to which I was inspired, for the invitation led to a hurried though systematic data collection, an explicit programming of the naive presidential model for purposes of simulation, and serious work on the model itself as I prepared a program for simulation.

Of these three tasks the first and second were routine and necessary to provide motivation for the problem as I had formulated it. The third task, developing the simulator for the model, that is, adding to the beautiful and clear deterministic logic some stochastic realism, was both a major task and the occasion for much discovery. Those readers who have never written computer code for a set of logical ideas they thought they understood well have still ahead of them one of their potentially more instructive personal experiences. The code was written for an early precursor of today's desk-top mircocomputers, which I had managed to wheedle from the dean (a political scientist) ostensibly for the use of the Department of Political Science. To get some notion of how much better life has become, that machine cost about $12,000 in 1970 currency and could be matched in performance today for perhaps a tenth of that amount and by tomorrow for an insignificant fraction.

The challenge of the coding was developing a sensible yet well-behaved error term. This is briefly discussed in the article as Stokes-Iversen error,

which references a paper (1967) by them. The brief discussion in the article gives no hint of the amount of time and effort that went into developing what amounts to a simple heuristic rule. The importance of the stochastic component is that it must bear the theoretical burden of representing *all* of short-run politics. An explanation of the long-run institutional bias in favor of the Democratic party is what I sought, but the instrumentality for showing that bias and its presumed source in the seat safety distribution is to show that it emerges in a model world that also includes other factors, that is, politics as usual. Hence, the development of a publicly justifiable error term was crucial to a defense of the entire mechanism of the model and the thesis that seat safety was the key determinant of dominance.

I am no statistician, but it was clear at the time even to me that the error could not be simply additive and independent—the error could not be Gauss-Markov. Although the occurrence of a random shock could be made independent of the state of the system (level of Democratic dominance), its magnitude needed to be controlled, obviously, and hence at a minimum the distribution would be truncated. But if the insight of Stokes and Iversen (1967) was to be used as a basis for modeling short-run electoral forces, it was equally clear that the sign of the stochastic disturbance had to depend on the state. To wit, when the Democrats did really well, the probability of a contrary short-run force should go up, and conversely.

This nonadditivity of the error has a twofold implication. First, constructing the error term for the simulation was an important and time-consuming task. It required thought and a little subtlety. Second, the possibility of an empirical test of the reasonableness of the error structure assumptions being made for the simulation was presented. If the error structure assumptions were closer to reality than a standard Gauss-Markov hypothesis then it could in principle be shown by an appropriate experiment. What was required was a demonstration that an estimation procedure that used the information in the Stokes-Iversen formulation would defend itself more adequately than a procedure making the Gauss-Markov assumptions. I developed such a procedure and ran some experiments, which are partially reported in the original long manuscript written some years later (Sprague, 1980a: 34–35). One result of these manipulations was to strengthen my belief in the reasonableness of the short-run stochastic structure used in the simulations. Of course the material was not the sort of thing for a substantive colloquium but it did shore up my confidence.

The technically inclined reader might like to attempt a development of the expected value of the error and its variance. I would be interested in seeing such results. In particular, the behavior of the error as a function of both seat safety party difference and average seat safety magnitude (institutionalization) would be interesting to plot.

Working through the empirical material for this first professional presentation provided the first eureka experience of dealing with this problem: the nonlinear behavior of the limiting system bias as a function of average seat safeties set out in the article in Figure 7.5 and Table 7.3 and summarized as Principle I. Lovely! Here was a payoff in substantive insight growing directly out of the model *and* the empirical situation being studied. Those plots reveal the system level mechanism by which the Democrats translate a modest advantage in seat safety into a persistent bias in legislative control and on reflection show how incumbency effects enhance the dominance of the Democrats in the House by increasing overall safety for both parties.

After this flurry of activity was completed the model went back to the files for further maturing. Once more it may have come out from time to time and was occasionally inflicted on students for instructional purposes but mainly it lay fallow. Another generation of graduate students came along including Tom Likens, Carol Kohfeld, Michael Wolfe, and Bob Huckfeldt. The first two had occasion to use the model in teaching a special small freshman course for students interested in quantitative social science and it taught successfully in that context. Huckfeldt and Wolfe were in the unfortunate position of being my research assistants for several years and hence were mercilessly exploited in several respects both by being subjected to this and other models on my mind and also by gathering various appropriate data when occasion demanded.

Sometime in the late 1970s, perhaps academic 1977–1978, an opportunity to give some colloquia at the University of Minnesota was presented. Bill Morris arranged the invitation (he has since left academic life for practical politics), and one of the seminars I offered was organized around the model of one-party dominance. This provided an opportunity to commission Huckfeldt to gather data anew. At this point I was also seriously considering going to the trouble of translating the notes into writing. If that were to be done, the data collection would have to be done from scratch and very systematically. These new data were not actually used in the Minnesota presentation, but that visit furnished the occasion for the construction of a small but carefully developed data set. The occasion also provided the stimulus to work through the model once more, to elaborate further some of the algebra, and to reconsider the overall motivation for the problem. Others may testify to the contrary but my recollection of the seminars as a whole is that they went very well, were well attended by both faculty and graduate students, and provided me with both sharp criticism and new suggestions. I believe both Stan Feldman and John Freeman were graduate students there at the time and had (no doubt skeptical) worthwhile comments to make. (The editor of this collection was also in attendance.)

The aftermath of this second elaborated development of the research on

one-party dominance was not, unfortunately, to forge ahead and write the thing up. Other research took priority, and once more the materials were filed although now the file section was getting quite fat. One further stimulus was needed to get the matter into written form. It came in the form of a phone call from G. R. Boynton at the University of Iowa, who invited me to spend a week with his department and give a series of lectures sponsored by the Shambaugh Fund. I agreed, and we scheduled for the spring of 1980.

WRITING

One of the advantages of a systematic set of notes, which have served as the basis for lectures and seminars and have had the benefit of the resulting comments, is that they are more than adequate as an outline for writing. It was natural to include the substance as one of the Shambaugh lectures, since the site was the University of Iowa with its specialists in legislative politics. It was natural to seize on the occasion as an opportunity finally to put it all together in one written piece for two reasons. First, Boynton wanted the lectures written down and furnished to his faculty ahead of time (I furnished him a book's worth ultimately). Second, there would be no page constraints, and I was of the belief then (and still am now) that it was important to have a very carefully developed motivation, and page space is necessary for that purpose. In the finished article it takes two figures, two tables, and six pages to motivate the problem.

So finally I wrote it all down. Well, not *quite* all and not *just* from my notes. One of the great difficulties with writing is that it is invariably stimulating to the intellect and typically produces more questions to pursue than satisfactions with what is going down on paper. It also has another important property. It forces putting in all the logical connections in a line of argument and hence allows one to exhibit one's real argument. This is not just useful for potential readers, for its greatest virtue is in how transparent one's thoughts are made to oneself. This process produced a draft document that excluded one important section of the notes—a section that develops the Stokes-Iversen error term as a problem in estimation by analyzing geometrically the error bias induced by differential seat safety—and that added a section based on the analysis of some comparative materials that constituted a generalization of the model which was suggested in the process of writing. I will comment on each, but this latter generalization was the second eureka experience encountered in the long history of this small project. The finished draft included, after preliminaries, forty-three pages of text, thirteen pages of tables, eleven pages of figures, and twenty-one pages of technical appendices. All in all, I was very pleased with the finished paper and after one redrafting, I sent it off to Boynton.

A good example of the serendipitous stimulation that came from the act of writing itself and the knowledge that space was unconstrained was included in the original paper but not in the published (much shorter) version. In the course of writing the problem motivation and developing the definition of seat safety, some scatterplotting of Republican seat safeties on Democratic seat safeties was done. This suggested, in the middle of writing, a formulation of the frontier of interparty competition as analogous in form to a Cobb-Douglas production function. At that point writing stopped temporarily and the necessary analyses were done. This involved transforming seat safety measures for each party into interparty competition measures by subtracting them from unity. Geometrically this moved the scatterplot close to the origin and changed the probable convexity of the functional form to be fitted. Substantively it changes the focus to *losses* to the other party rather than seat retention, that is, it focuses attention on the margin or area of competitive interchange. The form

$$C = (1 - r_t)^a(1 - d_t)^{(1-a)}$$

was then estimated for the constants C and a and the resulting curve imposed on the scatterplot—no eureka experience but very satisfying. These manipulations are detailed in the original Shambaugh lecture draft (Sprague, 1980a: 13–15, 57–58).

What the ability to fit to those data demonstrated was the empirical dependence of the logically independent seat safety measures. The plots and fitted curve also suggested quite forcefully the increasing impenetrability of each party's electoral base as election swings become extreme. Each party does have a large relatively safe base. On the other hand, immediately vivid substantive interpretations of the parameters did not jump forward, and the analysis was fundamentally static. What the episode illustrates is how far from finished work may be, even when it is familiar territory, as certainly the model, its motivation, and the notion of seat safety were to me at the time of writing. It was a worthwhile and intellectually satisfying digression in the course of writing up the overall results but one which could be sacrificed when it came to publication without loss of continuity in the flow of the narrative.

The original notes had included an extensive geometric analysis of the behavior of the estimation bias that would be introduced in a standard regression procedure from a small margin in seat safety in favor of the Democrats jointly with a Stokes-Iversen error term. The Stokes-Iversen error is as likely to be positive as negative when the control level is at .5. This is the expected value in the model only if seat safety probabilities are identical for the parties. But, with Democrats mildly favored in seat safety, the expected

value of partisan control moves (in the deterministic version) to something larger than .5. This means that the Stokes-Iversen error will, probabilistically, be more frequently negative than positive because the level of Democratic control will be more frequently above .5. Furthermore, this tendency is enhanced as control magnitudes become extreme. The relationships are highly interdependent.

The problem analyzed geometrically in the notes is this: How do these two conditions influence the estimated slope and intercept if the model is fitted to a time series of, say, Democratic partisan control measures? How will the recovery of the essential parameters, r and d, from the regression constraints be influenced by this bias? If one draws some careful pictures of a regression scatter under the joint hypotheses that Stokes-Iversen error operates and that d is slightly greater than r, the geometry gives insight into these issues. I urge the reader to draw some pictures scattering D_{t+1} on D_t, keeping in mind that almost all the data points in the empirical situation of interest—House elections since 1930—lie above .5. You should be able to persuade yourself on the basis of the resulting geometry and some very trivial algebra that: (1) the slope is biased downward, (2) the intercept is biased upward, (3) r is underestimated, and (4) d is correctly estimated only if the downward bias in the slope exactly equals the upward bias in the intercept and otherwise, that is, always, is biased up or down. My own empirical results indidate that the slope bias is roughly twice as large as the intercept bias (Sprague, 1980a: Table 11). These results are all obtained by reasoning from pictures and hence lack rigor even though they are quite persuasive. They were excluded from the original Shambough lecture writeup largely to avoid a long and fruitless debate about statistical issues at the time of presentation, which would divert from a substantive focus. As the article makes clear, there is an alternative and cleaner estimation strategy available.

Serendipity produced the interparty competition curve at the time of writing and tactics of presentation suppressed the error geometries. A planned bit of generalization—only sketched as a possibility in my original notes—furnished new analyses and a second eureka experience. I had the foresight to have coerced Huckfeldt into gathering data from three countries other than the United States in order to see how difficult extension to proportional representation systems would be. The results of these efforts were summarized as

> Principle III. The comparative advantage of one party over another arising from stability or continuity in seat control is extremely sensitive to the multiplicity of parties. As the number of parties grows, the system advantage arising from seat continuity differentials decreases sharply. (Sprague, 1980a: 39–40)

TABLE 7.6
Partisan Seat Shares in the Second House of the Swedish Riksdag, 1952–1964, in Proportions of Major Party Seats

Election	Parties				Total Number of Major Party Seats
	Social Democrats	Center	Liberal	Conservative	
1952	.489	.116	.258	.138	225
1956	.471	.084	.258	.187	225
1958	.491	.142	.168	.199	226
1960	.502	.150	.176	.172	227
1964	.509	.158	.189	.144	222

Note what this implies for the United States. Even with high institutionalization, if the number of parties were increased, the Democratic seat safety advantage would be less consequential. But, of course, seat safety is possible because of our single-member district system, that is, no multimember seats and no proportional representation, and single-member districts are well known to be highly correlated with two-partyism (Duverger, 1963; Rae, 1967; Riker, 1982; Sprague, 1980b). The eureka experience came at the end of the analyses leading to this proposition and involved about thirty minutes total time to the office and returning home with fifteen minutes of computing work at my university. I shall try to reconstruct this event. First I take a description of the analysis justifying Principle III from my original writeup and then point out exactly where and when the delicious eureka event occurred.

A reasonable approximation to the seat safety measure used in the United States can be constructed for Sweden. Within each province a multicandidate proportional representation party list system operates. This allows aggregating the party seat-holding survival rates within each Swedish province. The procedure is analogous to the areal survival measure utilized in the U.S. The Swedish Riksdag was transformed into a unicameral legislature in 1970, and the data studied here are for the second house of the Riksdag prior to that date.

Ignoring minor parties, the virtual stasis of partisan seat shares in the second house immediately prior to the advent of unicameralism is set forth in Table 7.6. The table exhibits very little variation in relative partisan seat shares in the two decades leading up to 1970. In particular, the Social Democrats maintain virtually the same share of all major seats, and the number of major party seats is virtually unchanged. The constancy of the seat share of the Social Democrats is remarkable.

Seat safety numbers for each of the four major parties may be computed as

TABLE 7.7
Sweden. Seat Safety Matrix, S, and Limiting Distribution, P*, for the Four Largest Parties. Second House System Based on Averages for Elections of 1928–1964.

		Social Democrats	Center	Liberals	Conservatives
$S =$	Social Democrats	.967	.011	.011	.011
	Center	.030	.911	.030	.030
	Liberals	.037	.037	.888	.037
	Conservatives	.047	.047	.047	.859
$P^* =$		.526	.195	.155	.123

indicated, and the questions presented are two. First, what is the algebraic generalization of the original model? Second, toward what limiting distribution, if any, does the system move? First the model is set out, and then the second question is addressed.

If the same simplifying assumptions that were used for the U.S. are maintained for Sweden, that is, that aggregation over time and constituencies is reasonable, then single parameters characterizing partisan seat safety may be assembled in a square matrix. The main diagonal can be interpreted as specifying the probability that a seat held in an average province is kept, on the average, after an election. The off-diagonal elements specify the flow from a row state to all the other states; that is, they specify the loss rate of seats to other parties. This requires a simplifying assumption in empirical work for the off-diagonal elements, but that turns out not to be critical. The important facts are the very large entries—entries approaching one—on the main diagonal of this matrix representation of seat safety numbers and loss rate relationships. In analogy with the original model the main diagonal numbers are equivalent to the parameter d and the off-diagonal elements play a role similar to that of $1 - d$, a quantity that does not show up in the original model.

Let the matrix of safe seat probabilities be denoted by S and the vector describing partisan seat shares at an election by P_t. In the case of Sweden the matrix S is four by four and the vector P_t is one by four. The model may be written as the matrix equation

$$P_{t+1} = P_t S$$

This equation is formally identical to a Markov chain, provided the matrix S meets certain conditions of regularity, which will typically be met under the interpretation employed here. The theory of finite Markov chains similar to

that defined here is complete (Kemeny and Snell, 1960) and a few theorems can be exploited for one empirical result. Before completing that exercise an alternative formulation is instructive for the motivation it provides.

It is possible to treat the Swedish situation as made up of only two parties—the Social Democrats and all other competitors. If that is done, the seat safety rate for Social Democrats may be mapped to the parameter d, say d^*, in the original model and the average seat safety for all other parties to the parameter r, say r^*. This produces, for Sweden, an $r^* = .89$ and a $d^* = .97$, which in turn imply a long-run expected advantage for the Social Democrats of .79 *under the counterfactual hypothesis that Sweden is a two-party system.* This argument asserts that, if Sweden were a two-party political system, then the Social Democrats would control four out of five seats in the long-run equilibrium. A cursory inspection of Table 7.6 shows that this is far from the empirical truth, and furthermore, the system looks very much as if it were very close to equilibrium. The two-party model is not consistent with the Swedish observations.

It turns out that the four-party model of the Markov chain formulation produces a very different result—one wholly consistent with the observed lack of movement in partisan seat shares displayed in Table 7.6. The only problem standing between an application of the Markov chain model to these data and a computed result is an assignment of numerical values to the off-diagonal entries for the matrix S. The off-diagonal entries represent losses to other parties, and the simplest assumption is that these losses are distributed to other parties equally probably. With this assumption of the equal probable distribution of the off-diagonal, the matrix S has a certain regularity property, and the general theory of finite Markov chains guarantees that there is a fixed limiting distribution toward which the system moves. Instead of a single equilibrium value the theory guarantees the existence of an equilibrium vector—the long-run values for the entries in P_t, call it P^*. If placed at P^* at time t, the matrix S will simply generate P^* again as the next distribution at time $t + 1$. A routine technology is available for computing this vector or fixed point.

The results of carrying out the appropriate calculation for Sweden are set forth in Table 7.7. The first thing to note is that the limiting distribution P^* is very similar to the observed distributions of Table 7.6. In particular, the value for the Social Democrats is within 2 percent of the 1964 observed value. What is the difference, then, between the two-party and the four-party models? This is the second thing to note: The four-party model recovers a much more believable limiting behavior. These results led to the formulation of Principle III.

Where was eureka? It was in the sudden decision to calculate the generalized model fixed point. I was home on a Sunday starting to write the section

on the comparative generalization. I calculated the limiting value analogous to D^* for Sweden under the two-party simplifying assumption for the Social Democrats and noted the extraordinary discrepancy between the observed stable magnitude of Social Democratic party seat shares and the long-run equilibrium or expected value from the model. The model value was grossly high. Immediately it occurred to me that the complexity of the party structure might be implicated. At school I had an old Markov chain program still implemented on the now obsolete machine on which I had done the original simulations a decade before. It was a matter of minutes to sit down and construct the matrix for the four parties with care and compute the equally probable off-diagonal entries. With very high excitement I went out the front door and walked to my office (about seven minutes away), went to our data lab, searched out an old minitape with the Markov chain program on it, hit start, entered the matrix, and shouted hooray at the almost immediate output, P^*, reproduced in Table 7.7. The discrepancy had disappeared. Institutional party structure apparently was crucial in ameliorating the effects of seat safety or continuity bias. No wonder Republicans have such a hard time gaining control of the House!

Within a half-hour's time I was back at my typewriter at home. I finished the section including the results just shown and finished the paper. It was exciting. It was fun. It had also been about twelve years.

PUBLICATION

I spent an enjoyable week at Iowa City in early April 1980. The lectures went as well as could be expected, and I learned a great deal. The paper on one-party dominance went over particularly well with Gerry Loewenberg, who manages the production end of the *Legislative Studies Quarterly*. At dinner one evening and again the next day he urged me to submit it to that journal for consideration. I did so by giving him two copies to send along to Malcolm Jewell, the editor, at the University of Kentucky. The journal, it might be noted, was not in existence when the model was first publicly presented. Loewenberg and I discussed the excessive length, and he agreed to try his hand at editing—a ruthless hand. In particular all the comparative materials were struck out. In due course I benefited from the comments of an anonymous reviewer, and Jewell agreed to publish it. Without any further hitches the edited version was finally objectified in May 1981.

This process reduced the original ninety pages of manuscript to twenty-seven journal pages. On rereading the article after a year has elapsed, I find no particular changes I would like to make. It is the best I was able to do with the problem and as is sometimes said in political science perhaps it was "close enough for government work."

CONCLUSION

The only moral that seems worth drawing is that self-contained as problem, argument, and model solutions are, the contingent nature of science is nevertheless everpresent. If the interested reader refers to Figure 7.2 in the original article, he or she will observe the next puzzle jumping from the page. How is the lack of stability in partisan dominance in the House prior to 1930 to be explained?

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