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The Incidence of executive vetoes in comparative perspective: Position -taking and uncertainty in U.S. state governments, 1983-1993

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The incidence of executive vetoes in comparative perspective: Position-taking and uncertainty in U.S. state governments, 1983-1993[1]

Abstract: I develop a negative binomial event-count model to investigate the systematic components of executive veto incidence. The paper estimates the model with data from U.S. state governments 1983-93. Four hypotheses drawn from a model in which politicians are motivated not only by policy concerns but confront position-taking incentives as well (presented elsewhere) are tested. I find evidence that (a) there is a significant surge in veto incidence under divided government, but (b) this effect is cancelled by divided assemblies bringing veto incidence back to the level of unified government; that (c) executive veto incidence follows the electoral cycle, higher towards election day; and that (d) the veto incidence rate is related to the override requirement of the session. There is evidence that position-taking is central to veto incidence, although a (hard to estimate) share of the effect is attributable to uncertainty, as Cameron has recently argued.

In this paper I develop a model to investigate the determinants of executive veto incidence. I present the model's logical premises, estimate it with data from U.S. state governments, and interpret the empiric results. The place of the paper in a larger argument is easily seen by reviewing the four steps of a larger research agenda (cf. McCubbins and Thies n.d.).

Research begins with the observation of a puzzle: What is behind the substantial variance of executive veto incidence in systems of separation of power? Table 3.1 portrays the magnitude of this variance with data from North American systems of separation of power. In a bit over one-third (35%) of legislative sessions that took place in state governments of the U.S. between 1983 and 1993, the governor signed every single bill that the assembly sent to his or her desk. In the remainder sessions (65%), the governor vetoed at least one bill. Almost one third of the total sessions (31%) had 10 or more bills vetoed, while a bit over a tenth (13%) had 25 vetoed bills; in 21 sessions (3% of the total) executive vetoes occurred by the hundreds.

[1] I thank Gary Cox, Chris DenHartog, Federico Estévez, Mat McCubbins, and Jeff Weldon for useful comments to previous versions of the paper. Special thanks to Neal Beck.

Table 3.1 Bills vetoed in U.S. state legislative sessions, 1983-93

Number of bills vetoed by the governor during the legislative session	*Frequency*	*Relative frequency*
None	283	.35
1 or more	527	.65
10 or more	254	.31
25 or more	109	.13
100 or more	21	.03
Total number of legislative sessions	810	1

Source: Prepared with data from CSG (various issues).

The second step in my research consists of developing a theory to answer the question of the variance. The model of the legislative process in Magar (n.d.) is a theory of veto incidence: it is an analogy that abstracts away from most of what is going on in the world in order to focus on the determinants of the incidence of vetoes. The position-taking setter game explains vetoes as the outcome of the interplay of politicians' preferences for policy and the desire to advertise the ideals they stand for. Neither preference nor desire are things we can measure, so the theory also posits that the partisan composition of the branches of government is a proxy for preferences, and that approaching elections intensify politicians' desire to advertise the interests they represent.

The third step is to derive predictions from the theory. Because theoretical concepts are typically difficult to measure or observe, theories can seldom be tested directly. Testing requires hypotheses to be derived by relying on auxiliary assumptions (the creation of a second analogy). Magar (n.d.) also took care of this step by deriving four hypotheses that follow deductively from the model's premises. So, for example, the *divided government surge* hypothesis claims that, insofar as veto incidence is driven by conflicting preferences between the branches of government, which are a function of the partisan composition of government, then periods of divided government will be likelier to have a high incidence of vetoes than periods of unified control, other things equal. Three more testable hypotheses were derived from the theory in very much the same fashion.

The fourth step, then, is to design an experiment or a method of observation in order to test the predictions of the theory. Testing the prediction that the likelihood of high veto incidence increases with divided government requires observations to be made. This step involves a third analogy, the design of a method to search for correlation in the empirical data between the factors highlighted in hypotheses and veto incidence. The methodological model that results serves as a tool to process a wealth of information by boiling it down to a small number of informative summary statistics (such as means, regression coefficients, standard errors, and so forth).

In this paper I undertake step four of my research agenda. I make observations to test the following hypotheses from the position-taking setter game of Magar (n.d.):

> H2-The *nil impact* of the override majority on veto incidence;
> H3-The *divided government surge* in veto incidence;
> H4-The *divided assembly slump* in veto incidence; and
> H5-The *electoral oscillation* in veto incidence.

Hypotheses are presented more clearly below; the intuition behind them is simple enough. The agenda setter game is a spatial model that assumes politicians can anticipate what others will do in all steps of the game and take this into consideration in choosing their optimal moves. By this assumption, and given players' preferences for policy, strategic politicians have the capacity to earmark all status quo policies that can be modified (because enough players are left better off by new policy) and all which cannot be modified (because some veto cannot be overridden). When politicians are motivated by policy concerns only (as in the standard setter game, Romer and Rosenthal 1978), any status quo earmarked as non-modifiable results in inaction: there is nothing to win from proposing something that will be killed towards the end of the game. When, however, politicians also wish to advertise their policy ideals to constituents, then inaction may be misinterpreted. A better election strategy consists of proposing policy palatable to voters in order to force an opponent to kill it, clearly explaining why it has not been enacted. The first three hypotheses above point to factors determining the share of status quo policies that are earmarked as non-modifiable: divided government increases the share, split assemblies depress it, while override requirements leave the share untouched. The fourth hypothesis is derived from an assumption that the position-taking incentive gets stronger as the next election approaches. Magar (n.d.) develops the full logic of these hypotheses in a situation where an agenda-setting legislator sends proposals which the executive can only take or leave.

The methodological model I rely for the purpose of hypothesis testing is negative-binomial regression; I describe it at length below. The paper proceeds as follows. Section 1 defines the dependent variable, paying special attention to its limited nature. Analysis of event-count variables such as veto incidence calls for special methodology: Poisson regression. Section 2 develops the primitives of event-count Poisson regression, and argues in favor of making a marginal modification to the model. The modification results in negative-binomial regression, a close relative of Poisson. Section 3 formalizes a negative binomial regression model of veto incidence and estimates it with data from U.S. state governments. Section 4 interprets the empirical results, finding evidence that (a) there is a significant surge in veto incidence when divided government pops in, but (b) this effect is cancelled by divided assemblies bringing veto incidence back to the level of unified government; that (c) executive veto incidence follows the electoral cycle, higher towards election day; and that (d) the veto incidence rate is related to the override requirement of the session. I interpret these findings in light of two interpretations of veto incidence, Cameron's (2000) and my own. Section 5 concludes.

1 Bills, vetoes and event counts

I compiled information on the legislative process in U.S. state governments from the *Book of the States* (CSG various issues). The remarkable wealth of information contained in each biennial compendium of the *Book* (I consulted volumes 24 through 30) includes a synopsis of the constitutional structure of decision-making in each state; the partisan composition of the elected branches of government; the number of bills passed by assemblies in the legislative sessions held during the years reported; and the number of bills vetoed by the governor in each session. I obtained indicators of inter-branch bargaining in most state governments of the U.S. (Nebraska and North Carolina had to be excluded).[2] I chose to observe the legislative process from 1983 to 1993, a period including years before and after the recent depression in U.S. state economies (Gramlich 1991). This resulted in a cross-sectional time-series of legislative sessions in 48 states during a bit over a decade.

The unit of observation is a legislative session; a total of 798 sessions are included in the dataset. So, for example, in 1985 the Alabama state assembly sent 477 bills to the governor's desk for signature in two sessions. A regular session was held from February 5 to May 20, in which 343 bills were passed; a special session was held from August 28 to September 20, with a total of 134 bills. Each session is coded as one observation with aggregate data.

[2] I excluded Nebraska from the analysis because the formally non-partisan nature of the elections impeded me from coding key independent variables. North Carolina was excluded because the governor lacks a power to veto legislation.

The dependent variable in the empirical analysis that follows is the *incidence of executive vetoes* in state legislative sessions. I represent veto incidence in session i by V_i, which is equal to the number of executive vetoes in the session:

$$V_i = \text{number of executive vetoes in session } i. \quad (1)$$

There are two possible limitations to the measure of the dependent variable. One is that the source may conflate vetoes of bills with those of resolutions, treating them as equivalent ("a veto is a veto" summarizes such an approach).[3] This limitation may be unimportant, to the extent that a vetoed resolution is a resolution the governor seems to care for, despite not being law. A second limitation is that the measure may include line-item vetoes along with full vetoes, possibly several in the same bill. I ran alternative specifications of the model below controlling for sessions where the executive possessed a line-item veto to reduce the effect of this possible measurement problem: this variable should capture most of this "artificial" effect on veto incidence. Results only change slightly (see the end of section 4 below).

The point of the empirical model I construct is to estimate the expected number of vetoes in a session, $E(V_i)$. $E(V_i)$ can be broken into two factors of a product, the veto incidence rate of the session (represented by r_i) and the exposure to bills in the session (represented by B_i), as follows:

$$E(V_i) = r_i \times B_i. \quad (2)$$

[3] The *Book of the States* remains silent about the coding procedure of the data under the heading "Measures vetoed by the governor". So far I have been unable to contact someone in the staff who compiles this information to get this information.

The exposure variable B_i is simply the total number of bills that were passed during session *i*. The veto incidence rate is closely related to the probability of a veto: note that in equation (2) we can replace r_i by the probability that a bill is vetoed and still obtain the expected number of vetoes in session *i*. The difference between the two is subtle and has to do with the locus of their determinants: the probability that bill *j* is vetoed depends on features of the *bill j*, whereas the incidence rate of vetoes in session *i* depends on the features of the *session* in question. Estimating the probability of a veto would require information about individual bills; estimating veto incidence rates is done with aggregate data from *legislative sessions*. In this paper I work with aggregate data.

My dependent variable is the same as in Rohde and Simon (1985). Their study estimated the correlation of the number of vetoes and a vector of explanatory factors from each of a large number of legislative sessions in the U.S. federal government. Their estimation method is multiple linear regression analysis. This paper carries an estimate of the same correlation on different data; as pointed, I also use a different estimation method, negative-binomial regression.

The choice of method has to do with the nature of the dependent variable, the values of which are limited to the set of positive integers (0 veto, 1 veto, 2 vetoes, and so forth). The statistical analysis of an "event-count" variable such as veto incidence – one type of *limited dependent variable* – should not be done with standard linear regression (Beck n.d.).

Event counts are non-negative integers representing the number of times a specified event occurs within fixed (but not necessarily equal) periods (Cameron and Trivedi 1998, p. 5; King and Signorino n.d., pp. 3-4). In this paper the event at hand is the executive veto; the periods are legislative sessions. King (1998, pp. 129-31) models the incidence of veto overrides in the U.S.

as an event count; I pull the model one step back in the sequence of action, paying attention to the incidence of executive vetoes.

2 Poisson regression as an analogy for event occurrence

The estimation method I use, negative-binomial regression, is an extension of Poisson regression. In this section I present the essential traits of the latter; the step to negative-binomial regression is straightforward.

The Poisson is known as the statistical distribution of rare events. The Poisson distribution has been used to model, among other phenomena that happen with small probability, the occurrence of fatal horse-kicks in the stables of the Prussian army in the 19th century, the number of telephone connections to a wrong number, and the number of plane crashes.[4] This distribution is characterized by a single "intensity" parameter λ; the mean and the variance of the distribution are equal to this parameter. That is, if V_i is a random variable with distribution

$$V_i \sim Poisson(\lambda_i),^5 \tag{3}$$

then

$$E(V_i) = var(V_i) = \lambda_i. \tag{4}$$

The Poisson distribution is portrayed in Figure 3.1. The figure allows the intensity parameter λ_i to take three values (0.5, 2, and 5) in order illustrate how the shape of the distribution changes with each. As λ_i increases, the mode of the Poisson distribution shifts rightward and the distributions acquires a more bell-shaped profile, though preserving a certain right-skewedness.

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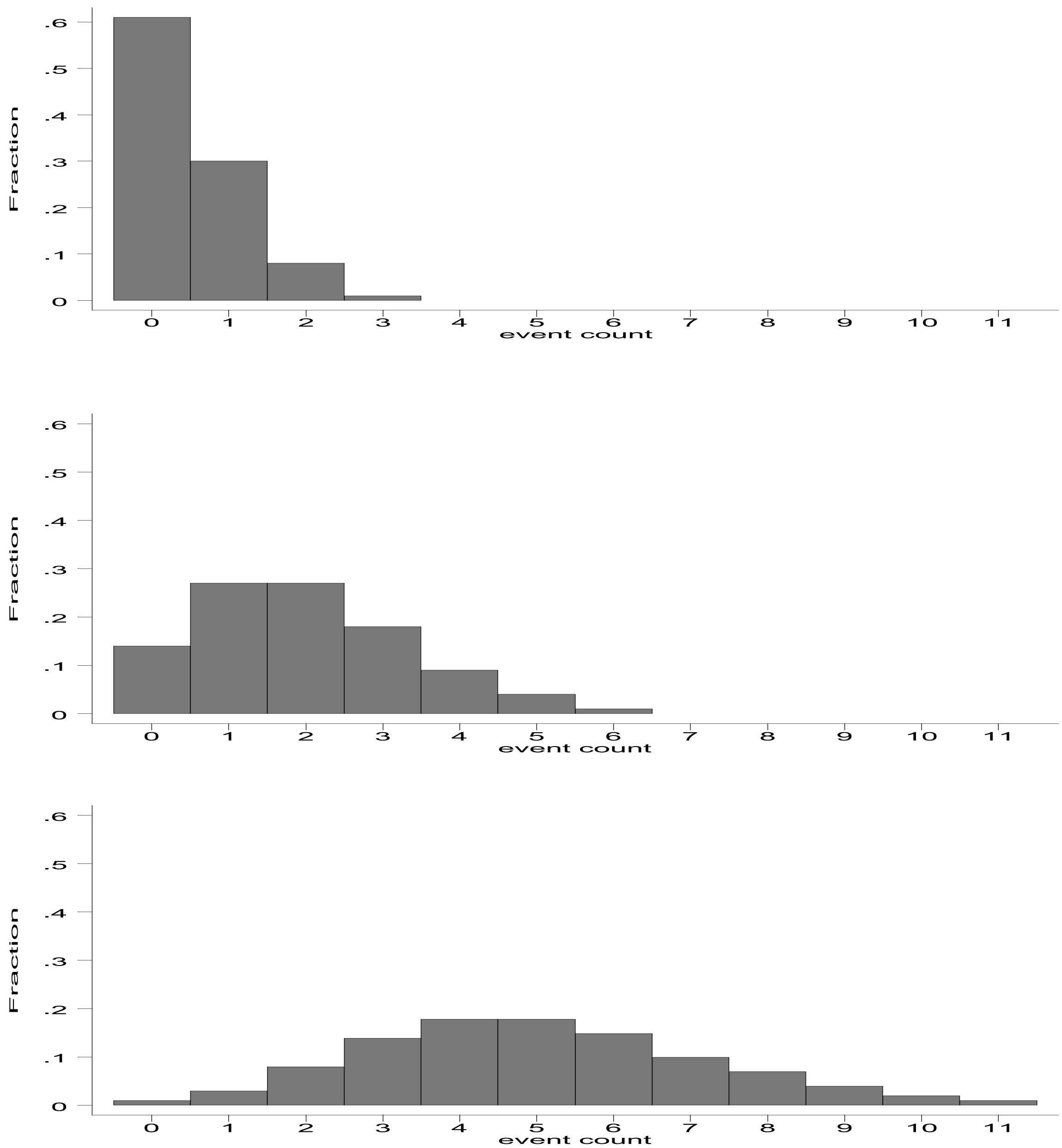
[4] These applications are quoted in Stata (1997, p. 30) and Cameron and Trivedi (1998, pp. 10-15).

[5] The density of the Poisson distribution with parameter λ_i is $f(V_i = v_i) = (exp(-\lambda_i)\lambda_i^{v_i}) \div v_i!$, $v_i = 0, 1, 2, \ldots$, where $exp(\bullet)$ represent the exponential function.

Figure 3.1
Three Poisson distributions: $\lambda = {}^1/_2$ (top histogram), λ =2 (middle), and λ =5 (bottom)



A Poisson regression model of the incidence of vetoes in legislative sessions is built from this distribution. The model begins by assuming that the distribution of the number of vetoes in session i, V_i , follows a Poisson distribution with intensity parameter λ_i, as in equation (3). We seek the determinants of the expected number of vetoes in a session (equal to the average of V_i). By equation (4) we know that the mean of the distribution equals parameter λ_i. The model assumes that the expected number of vetoes in a given session is a function of a vector of regressors X_i and a random component e_i:

$$E(V_i \mid X_i) = \lambda_i = f(X_i, e_i). \tag{5}$$

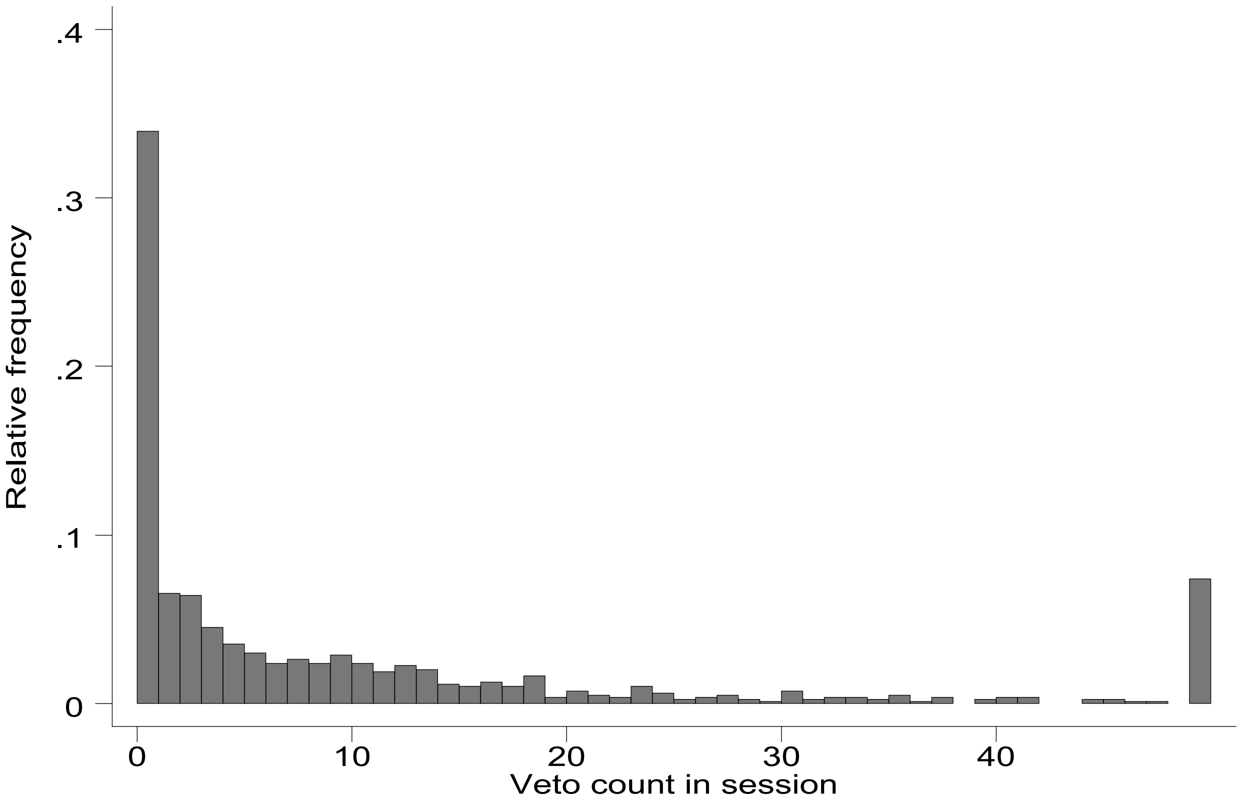
Estimating equation (5) with real data requires that we make some assumptions about the shape of function *f*. There are two considerations in the choice of *f*: the range of function *f* and its form. One desirable property of function *f* is that its range be restricted to positive values, so as to avoid making negative predictions of the expected number of vetoes; the assumptions of Gaussian linear regression place no such restriction on the range of its predictions. Moreover, *f* should be such that the effect of a given independent variable is not linear: it seems harder, a priori, to move from 0 to 1 event than it is to move from 100 to 101 events. Defining *f* as an exponential function (denoted as $exp(\bullet)$) resolves the range and shape issues at once:

$$\lambda_i = exp(\beta X_i + e_i). \tag{6}$$

A final ingredient completes the event-count model that I use to test hypotheses from Magar (n.d.): a substitution of Poisson regression by negative-binomial regression. The difference between the two estimation methods is the assumption each makes about the distribution of V_i. The negative binomial distribution does not assume an equality of the mean and variance of the distribution,[6] so offers increased flexibility over Poisson. Instead, the definition of a negative

[6] The negative binomial distribution is really a Poisson-gamma mixture. In practice, however, negative binomial distribution can be treated as a Poisson with an additional parameter for over-dispersion: Poisson is a special case of the negative binomial (see equation 9 below). A derivation of the negative binomial is presented in Cameron and Trivedi (1998, pp. 70, 100-3).

Figure 3.2
Frequency of vetoed bills in U.S. state legislative sessions



Note: The actual distribution is way more skewed to the right – a long but very "thin" tail extends to the right of the histogram and is not portrayed due to space limitations. Sessions with a count of 50 or more vetoes (59 sessions or 7% of the total) appear stacked in the right-most column, which is "fictitious"; the actual distribution spreads these observations, with increasing sparseness, from 50 to 465 veto counts.

binomial distribution requires one extra parameter accounting for the dispersion of the data around the mean. The distribution can in this fashion approximate data that are "over-dispersed" with respect to a Poisson – meaning that the variance is larger than the mean – or "under-dispersed" – meaning the contrary. A look at the actual distribution of vetoes in U.S. state legislative sessions, portrayed in Figure 3.2, justifies the need to estimate the extra parameter $\hat{\delta}$.

The distribution of vetoes in U.S. state governments has a single mode in zero veto per session and the frequency drops sharply as the number of vetoes per session increases: as seen the distribution evidences an acute right-skewedness (the note in Figure 3.2 explains the presence of the block to the right of the histogram). This mass at the zero-veto category is reminiscent of the distribution of rare events that Poisson stands for, suggesting the distribution is indeed a right choice to model the occurrence of the event at hand. The other feature of the Poisson – the equality of mean and variance – is not approximated well by the empirical distribution. The actual variance of V_i (1,849 vetoes) is way larger than the actual mean of V_i (16 vetoes), a good symptom that the data are "over-dispersed" (see King and Signorino n.d., p. 9).

Negative binomial regression estimates an extra parameter $\hat{\delta}$ to account for the over-dispersion of the data.[7] The variance of the negative binomial distribution is proportional to the mean. That is, if

$$V_i \sim negative\ binomial(\lambda_i, \delta) \tag{7}$$

then

$$E(V_i) = \lambda_i \tag{8}$$

and

$$var(V_i) = \lambda_i(1+\delta\lambda_i).^{8} \tag{9}$$

The coefficient estimates of Poisson regression in the presence of over-dispersed data are consistent, but standard errors are biased towards zero (Hamerle and Ronning 1995, p. 442), invalidating hypothesis testing. Negative binomial regression solves this problem.

[7] Methodologists denote the dispersion parameter by α. I use δ to avoid confusion with the α parameter in Magar (n.d.) (i.e. the weigh of the act-contingent component of politicians' utility functions).

To sum up, Poisson regression is a maximum-likelihood alternative to ordinary least squares that allows to model statistical relations between a limited dependent variable – veto-count data taking only integer values – and a set of regressors. Negative-Binomial regression is an extension that overcomes the Poisson's restrictive mono-parametric nature, estimating an extra parameter to account for over-dispersed data. Event-count methods such as these are gaining popularity in the discipline (see, e.g., Canon 1993; Kastner and Rector 2000; Morris 1999). I turn next to an operational specification of the model, then estimate it with data from U.S. state governments.

3 A negative binomial regression model of veto incidence

At the root of the event-count empirical model is r_i, the incidence rate at which vetoes occur in session i (e.g. $r_i = \frac{20\ vetoes}{1{,}000\ bills\ session} = 0.02$ *vetoes per bill-session*). r_i is directly related to the expected incidence of vetoes: if we estimate the incidence rate to be 0.02 vetoes per bill-session, by equation (2) we can expect 2 vetoes in a session where 100 bills were passed ($0.02 \times 100 = 2$), 5 vetoes during a session in which 250 bills were passed ($0.02 \times 250 = 5$), and so forth.

The five assumptions in Box 3.1 define the event-count model that I estimate with data from legislative sessions in state governments of the U.S. (see Beck n.d., p. 23; Stata 1997, p. 30). The model posits that the expected veto incidence rate in session i can be broken into a deterministic part and a stochastic part (assumption A13). The deterministic part is made of the features of session i (represented by a vector X_i) that determine veto incidence in the theory; each

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[8] Note that if δ were equal to zero the negative binomial would be equivalent to a Poisson. Poisson is a special case of the negative binomial.

Box 3.1
A negative binomial regression model veto incidence

A12-Distribution

The number of vetoes in session i V_i follows the negative binomial distribution:

$$V_i \sim \text{negative binomial}\ (\lambda_i, \delta).$$

A13-Exponential functional form

The incidence rate of session i is an exponential function of the linear combination of a vector of regressors and an error term:

$$r_i = exp\,(\,\beta X_i, e_i\,).$$

A14-The rarity of events

On a very small exposure ε, the probability of finding more than one veto is small compared to ε:

$$\lim_{\varepsilon \to 0} prob\,[V_i > 1] < \varepsilon.$$

A15-Independence

Non-overlapping exposures are mutually independent.

A16-No relevant variable is omitted

The vector of regressors X_i includes variables for all the features of session i that the theory relates to V_i.

feature has a weight in vector β.

By virtue of the negative binomial distribution of V_i (assumption A12), we know by equation (8) that the expected veto incidence is equal to the first parameter of the negative binomial distribution:

$$E\,(\,V_i\,)\ =\ \lambda_i\,. \tag{10}$$

The model seeks the determinants of λ_i. By equation (2) and assumption A13,

$$\lambda_i\ =\ exp\,(\,\beta X_i + e_i\,) \times B_i\,; \tag{11}$$

a basic arithmetic transformation leaves it as

$$\lambda_i\ =\ exp\,(\,ln(B_i\,) + \beta X_i + e_i\,)\,, \tag{12}$$

where B_i is the exposure variable (the number of bills passed in session i).

To estimate the model, the regressors in X_i need to be defined. X_i includes measures of the variables related by Hypotheses 2, 3, 4, and 5 to veto incidence, plus relevant control vari-

Table 3.2
The institutions of veto politics in the constitutions of the U.S. states[a]

$Q = 0$ **(no veto)**	$Q = {}^{1}/_{2} + \varepsilon$	$Q = {}^{3}/_{5}$	$Q = {}^{2}/_{3}$		$Q = {}^{3}/_{4}$
North Carolina[b]	Alabama	Delaware	Alaska (*~rev&app*)	New Hampshire	Alaska (*rev&app*)
	Arkansas	Illinois (*~rev&app*)	Arizona	New Jersey	Illinois (*rev&app*)
	Indiana	Maryland	California	New Mexico	Oklahoma (*rev&app*)
	Kentucky	Nebraska[b]	Colorado	New York	
	Tennessee	Ohio	Connecticut	North Dakota	
	West Virginia (*~rev&app*)	Rhode Island	Florida	Oklahoma (*~rev&app*)	
			Georgia	Oregon	
			Hawaii	Pennsylvania	
			Idaho	South Carolina	
			Iowa	South Dakota	
			Kansas	Texas	
			Louisiana	Utah	
			Maine	Vermont	
			Massachusetts	Virginia	
			Michigan	Washington	
			Minnesota	Wisconsin	
			Mississippi	West Virginia (*rev&app*)	
			Missouri	Wyoming	
			Montana		
			Nevada		

Notes:
(a) Names followed by a parenthesis refer to state governments where different override majorities are required for revenue and appropriations bills (*rev&app*) and for bills other than revenue and appropriations (*~rev&app*).
(b) Excluded from analysis.
Source: CSG (various issues).

ables. Summary statistics of all the variables, with their formal definitions and sources are provided in Appendix 1.

I used the following independent variables in the estimation. $\boldsymbol{Q_i}$ is the share of the assembly required to override an executive veto in the constitution of the state where session *i* was held. Q_i took three values among U.S. state governments: $^{1}/_{2}$; $^{3}/_{5}$; and $^{2}/_{3}$; Table 3.2 provides a summary of the structure of veto politics in the states.[9] The inclusion of Q_i puts Hypothesis 2 to a test. The square of Q_i, is also included in vector X_i, as suggested by Cameron's (2000) explanation of vetoes as mistakes (see Magar n.d.). $\boldsymbol{D_i}$ is a dummy equal to one if the party of the governor did not control both houses of the state assembly, zero otherwise. D_i stands for Divided government, and confronts Hypothesis 3 with evidence. $\boldsymbol{A_i}$ is a dummy variable equal to one whenever the houses of the state assembly were controlled by different parties, zero otherwise. A_i stands for divided Assembly and puts Hypothesis 4 to a test.[10] $\boldsymbol{E_i}$ is the number of days left between the end of session *i* and the legislative election that immediately followed session *i* (if session *i* ended the same day of the subsequent election, $E_i = 0$; if it ended one day before the election, $E_i = -1$; and so forth). E_i approximates Election proximity in the context of aggregate data, and is included to test Hypothesis 5.[11] The square of E_i is also included in the right-hand side of the equation with the intention of capturing the possibility of a declining effect of time.

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[9] North Carolina, the only state where $Q_i = 0$, is excluded from this part of the analysis since explaining the number of vetoes in its legislative sessions is a trivial exercise – zero, no matter what. Including it has no effect in the estimation of coefficients, see fn 18.

[10] There were a few cases where each of the two parties had an equal number of members in one of the chambers, with no third-party members to break the tie. Because one of the parties controlled the other house, I coded these cases as a unified assembly. A breakdown of the partisan composition of state assemblies in the 798 legislative sessions included in the analysis appears in the table below. (Cell entries report the number of sessions with a given combination of characteristics; parentheses report the percentage of the total sessions that number corresponds to.)

House status	Senate status: Dem	Rep	Tie
Dem	482 (60%)	83 (10%)	17 (2%)
Rep	56 (7%)	145 (18%)	6 (1%)
Tie	4 (1%)	5 (1%)	0 (0%)

This breakdown yields the summary descriptive statistics of A_i reported in the appendix.

[11] The aggregate nature of the data in legislative sessions complicates measuring the position-taking incentives. Literally (as claimed in Hypothesis 5) the theory claims that as an election nears a bill is likelier to be vetoed, ceteris paribus. Aggregate data only permit to suggest that a session ending *d* days from the next election should present, ceteris paribus, a higher incidence of vetoes than a session ending $d-1$ days from the next election. In some cases (48 out of 798) the session continued after the election next election (50 days on average, with a standard deviation of 40).

B_i is the number of Bills that the legislative assembly sent to the Executive's desk during session i. As pointed, B_i is the exposure variable in the model; as per (12) it enters the equation in logged form. I do not report the coefficient estimate of $\ln(B_i)$ in the table of results below because it is constrained to take a value of one.[12] The remainder variables in X_i are controls inspired by the literature. ***F_i*** (for Fiscal shock) is a dummy variable equal to one if session i took place in 1991, 1992, or 1993; zero otherwise. This variable controls for the 1991 state and local recession in the U.S. (see Gramlich 1991), a factor that plausibly rendered bargaining more difficult by hardening the budget constraint (cf. McCubbins 1991).[13] ***R_i*** is a dummy equal to one if i was a regular session; zero otherwise. R_i intends to capture a possible source of heterogeneity between Regular and special sessions (thus violating A15).[14] Finally, Alt and Lowry (1994) control for a possible Southern state effect; I do the same by including ***S_i***, a dummy equal to one if the state in which session i took place was part of the old confederacy, zero otherwise. The veto institutions of Southern states approximate those of non-Southern states.[15] Sessions in the

[12] I ran the model without this restriction, estimating a separate coefficient for $ln(B_i)$. It is only possible to reject the hypothesis that this coefficient is not equal to one at the .13 level, suggesting that the restriction is not problematic. Moreover, the remainder coefficients did not show significant changes with this modification.

[13] A fiscal crisis "hardens" government budgetary constraints, reducing the capacity to achieve compromise through deficits. When the budget constraint is "soft", the projects of two opposed sides can be logrolled, resulting in increasing budget deficits. I also noted that in the 48 states, the average number of legislative sessions that started each year in fact increased from 70 in 1983-90 to 89 in 1991-93.

[14] The inclusion of special legislative sessions may be violating this assumption. I include a dummy to control this possible source of heterogeneity. I estimated the statistical model on regular sessions only and found results very similar to the ones reported below; the most striking difference is that the coefficients of variable E are not significant at conventional levels.

[15] Ignoring North Carolina (where $Q=0$, and which is dropped from my sample), the mean Q is .645 in the South, .642 in non-Southern states; the standard deviation around the mean is, respectively .061 and .056. The following table summarizes the breakdown of Q in states included in the analysis:

	$S_i = 0$	$S_i = 1$
$Q = {}^1/_2$	4 (11%)	2 (20%)
$Q = {}^3/_5$	5 (13%)	- (0%)
$Q = {}^2/_3$	29 (76%)	8 (80%)
Total	38 (100%)	10 (100%)

If the table is expressed in terms of legislative sessions (the unit of the present analysis) instead of states the proportions within cells are almost identical.

South had half the incidence of vetoes of non-Southern sessions (8 vs. 17); a proportion of sessions in divided assemblies almost three times smaller (9% vs. 24%); a lower proportion of divided government sessions (39% vs. 62%); and a number of bills passed per session 20% larger (333 vs. 270).

With the addition of a constant to capture some of the effect of omitted variables, the vector of regressors is the following:

$$X_i = (1, Q_i, Q_i^2, D_i, A_i, E_i, E_i^2, F_i, R_i, S_i), \tag{13}$$

where

Q_i = override requirement,
D_i = divided government
A_i = divided assembly
E_i = election proximity
F_i = fiscal shock
R_i = regular session
S_i = South
B_i = bills passed.

With this definition of X_i, the expected incidence of vetoes in session i looks as follows:

$$\lambda_i = exp\,(ln(B_i) + \beta_0 + \beta_1 Q_i + \beta_2 Q_i^2 + \beta_3 D_i + \beta_4 A_i + \beta_5 E_i + \beta_6 E_i^2 + \beta_7 F_i + \beta_8 R_i + \beta_9 S_i + e_i). \tag{14}$$

I expect to obtain the following signs for coefficient estimates. With regards to control variables, post recession sessions should have a higher incidence of vetoes (i.e. β_7 should be positive), Southern states a lower one (β_9 should be negative). I have no expectation attached to incidence in special sessions. Table 3.3 summarizes the signs I expect for the coefficient estimates of the theoretically substantive regressors (dropping the subscripts). Expectations for Q, D, A, and E follow from Hypotheses 2, 3, 4, and 5 respectively. The table also summarizes the

Table 3.3
Expected sign of the coefficients of key variables

Coefficient	*Variable*	*Expected sign of co-efficient*[a]	*Source of expectation*	*Cameron's expectation*[a]
β_1	*Q* override requirement	0	Hyp. 2	?
β_2	Q^2	0	Hyp. 2	–
β_3	*D* divided government	+	Hyp. 3	+
β_4	*A* divided assembly	–	Hyp. 4	–
β_5	*E* election proximity	+	Hyp. 5	–
β_6	E^2	+		?

(a) When the expectation is 0, the corresponding null hypothesis is that the coefficient is different from zero, a two-tailed test; when the expectation is a + (or a –), the null is that the coefficient is smaller or equal (larger or equal) to zero, a one-tailed test.

signs of coefficient estimates one would expect from Cameron's theory of vetoes as mistakes (see Magar n.d.).

Table 3.4 reports the results of estimating equation (14) by maximum-likelihood negative binomial regression. Two criteria evaluate the general fit of the model to the data. One is the p-value for the model's χ^2 statistic, indicating that the null hypothesis that all the coefficient estimates are all equal to zero can be confidently rejected at the .0001 level or better. By the second criterion, there is ample statistical evidence to reject a hypothesis that the Poisson should have been chosen to model the distribution of variable *V* instead of the negative binomial (as per A12). By equation (9), that hypothesis can be rephrased as a claim that parameter $\delta = 0$ (making the negative binomial distribution collapse into a Poisson); since negative binomial regression provides an estimate of $\hat{\delta}$, we are in a position to test a hypothesis that the estimate is nil. A Likelihood-Ratio test, reported in part 2 of the table, permits to conduct the test of the hypothesis that $\delta = 0$. The estimate $\hat{\delta} = .87$ is significantly different from zero at the .0001 level or better. So there is ground to sustain that negative binomial regression is a good choice of method to model this event count.

Table 3.4
A model of the incidence of vetoes in U.S. state governments' legislative sessions

Part 1: Coefficient estimates

	Variable	Coefficient estimate[a] (robust standard error in parentheses)[b]	p-value (two-tailed test unless otherwise indicated)
1	*constant*	-33.144 (7.052)	<.001
Q	*override requirement*	103.243 (24.247)	<.001
Q^2		-87.074 (20.499)	<.001
D	*divided government*	.600 (.096)	<.001 (one-tailed)
A	*divided assembly*	-.609 (.115)	<.001 (one-tailed)
E	*election proximity*	.001 (4×10^{-4})	.004 (one-tailed)
E^2		8×10^{-7} (4×10^{-7})	.030
R	*regular session*	-.071 (.124)	.566
F	*fiscal shock*	.252 (.092)	.003 (one-tailed)
S	*Southern state*	-.477 (.106)	<.001 (one-tailed)
	dispersion parameter (its standard error) $\ln(\delta)$ =	-.141 (.067)	.035
	Pseudo R^2 =	.03	
	Model $\chi^2_{(9)}$ =	108.36	<.0001
	Log Likelihood =	-2069.93	
	Number of observations =	798	

Part 2: Likelihood-Ratio test against Poisson

δ =	.869
$\chi^2_{(1)}$ =	7766.60
p-value (2-tailed) =	<0.0001□

Notes: (a) Negative-Binomial method of estimation. The number of bills passed in the session serves as the exposure variable. For variable definitions, see Appendix 1.
(b) Cf. White 1980.

Tests are also conducted to determine whether or not each coefficient estimate, individually, is statistically discernible from zero. The criterion involves comparing the ratio of the estimate and its standard error, plugging the result against a Wald chi-square distribution (Cameron and Trivedi 1998, p. 47), and then measuring the probability density left in the tail of the distribution beyond the result: this is the p-value. The smaller the p-value of a coefficient estimate, the more confident we can be when rejecting the null hypothesis attached to the coefficient at hand.

I illustrate the statistical evaluation of coefficient estimates with that of variable *R*; it is the same procedure for all the other variables. The coefficient estimate $\hat{\beta}_8$ indicates that, holding other factors constant (including the number of bills passed in the session), regular sessions had an incidence of executive vetoes similar to that of special sessions. Although the estimate for the coefficient is equal to -.071, the data contain no statistical evidence to reject the hypothesis that the coefficient is any different from zero. If an imaginary experiment were held in which drawings of data from legislative sessions were repeated infinitely, and we always chose to reject the hypothesis that this coefficient is zero, we would be wrong (making a type I error)[16] a bit less than 57 out of every 100 times (p-value = .566). The lack of evidence to reject the null hypothesis involving *R* suggests that the assumption that non-overlapping exposures are mutually independent (A15) is not violated. The fiscal shock to local economies in 1991 significantly increased inter-branch conflict, at least with respect to the incidence of executive vetoes. The positive and significant (at the .003 level) coefficient of variable *F* indicates that, all else constant, the number of vetoes went up in sessions initiated on January 1, 1991 or later. The other control variable indicates that the number of vetoes in legislative sessions held in Southern states was below the average in non-Southern states: the coefficient estimate is negative (-.477) and statistically significant (at the .001 level or better).

Coefficient estimates of three out of four variables of substantive interest conform to my expectations summarized in the middle column of Table 3.3. With the exception of the *nil impact* hypothesis (H2), none of hypotheses H3, H4, nor H5 can be rejected with the data at conventional levels (the corresponding null hypothesis for each can be easily derived from the middle column). There is significant statistical evidence to reject (at the .001 level or better) the null that the coefficient of the *D* dummy is nil or negative. All else constant, sessions in which the executive's party did not control both houses of the assembly had a higher incidence of vetoes. Knowledge that the point estimate of *D*'s coefficient is .6 is not more informative at this stage beyond the sign of the estimate. Unlike ordinary least squares estimates, the meaning of negative binomial coefficient estimates needs to be decoded, a task I undertake below.

The data also contain evidence to reject (at similar statistical level as for *D*) the null hypothesis that the coefficient of dummy *A* is zero or positive. Ceteris paribus, we have statistical evidence that sessions in which the same party failed to control both houses of the state assembly had a significantly lower incidence of executive vetoes. I have been incapable of finding evidence among legislative sessions in state governments allowing to reject the *divided government surge* and *divided assembly slump* hypotheses (H3 and H4). The partisan composition of the branches of government plays a significant role in generating observable implications of interbranch conflict; the significance is statistical as well as in the magnitude of the effect, as I point below.

The positive (.001) point estimate of the coefficient for variable *E* indicates that, other factors held constant, the more proximal the next election to the end of a session (i.e. one day less to the polls), the higher the incidence of vetoes in the session. There is evidence to reject the

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[16] Type I errors involve the rejection of a hypothesis that is actually true (Wonnacott and Wonnacott 1990, p. 303).

null associated with Hypothesis 5 ($\beta_5 \leq 0$) at the .004 level of confidence. Moreover, the effect of election proximity was in fact increasing in state sessions, as indicated by the positive (and significant) sign of the coefficient estimate of the square of this variable. The marginal effect of one less day away from the election on veto incidence is positive and statistically significant.

Below I provide a graphical portrait of the effect that nearing elections have on veto incidence. The aggregate nature of the data raises some doubts about the face value of this particular finding; it should be complemented with additional evidence. An example illustrates the potential problem. Imagine a calendar running from January 1 year y to January 1 year y+1; re-label the time-line so that it is measured in negative months (i.e. January 1 year y+1 corresponds to zero, December 1 year y to –1; November 1 to –2; …; January 1 year y to –12). Consider zero to correspond to the next election day, and take three legislative sessions, s, t, and u. Session s runs from –12 to –1; session t runs from –6 to –1; session u runs from –2 to –1. Because all three sessions end the same day, all are coded as having the same electoral proximity (i.e. $E_s = E_t = E_u = -1$). Session s, however, had a long period away from the effect of the election, whereas session u didn't.[17] Research with bill-specific data will be needed to confirm this result, estimating the probability of a veto based on the distance from the bill's consideration to the next election.

On the side of negative findings, data from legislative sessions of U.S. state governments contain no statistical evidence to reject the null attached to the *nil effect of Q* hypothesis (H2). The coefficient estimate of variable Q is large (103). It is also statistically significant (p-value < .001). If we chose to reject the null that this coefficient is not zero, we would be making a type I

[17] I ran the regression measuring election proximity as the (negative) number of days from point x in the session to the next election. The estimate reported and discussed so far takes x to be the end of the session. If x is taken as the day corresponding to the third quartile (of session length) the results are very similar to those reported. Moving x to the middle of the session returns coefficient estimates for E and E^2 very similar in magnitude but less significant statistically (at the .05 and .1 levels, respectively).

error with probability .999: we would be right only 1 out of 1,000 times! In fact, the magnitude of the coefficient estimate suggests that the effect of the veto override requirement on veto incidence is larger than that of any other independent variable.

The rejection of the *nil effect* hypothesis has to be interpreted in light of the discussion of the two complementary determinants of veto incidence (Magar n.d.): position-taking and uncertainty. Aggregate data conflate two types of vetoes into a single measure, complicating the exercise of disentangling them. Yet the inclusion of Q^2 in the regression provides some leverage: since the position-taking logic does not predict an effect of this variable on overall veto incidence, any hint of an effect of this variable can be attributed to Cameronian (2000) uncertainty – which I theoretically associated with the override requirement. In other words, if all the vetoes in sessions were publicity stunts, the coefficient of variables Q and Q^2 would be nil. The non-zero coefficient estimate of Q^2 is evidence that some non-trivial amount of veto incidence was caused by uncertainty. The question remains about what proportion of vetoes belongs to uncertainty and position-taking; estimating these proportions would require, at the very least, non-aggregated data. Below I shall present an attempt to control for the effect of uncertainty in evaluating the effect of position taking (by analyzing sessions with an absolute majority override requirement).

The coefficient estimate of Q^2 conforms to Cameron's expectation: the estimate is negative and large in absolute value (-87); it is also statistically significant (p-value $< .001$). The implication is that the effect on veto incidence increases with Q in a first stage, then decreases in a second stage. The coefficient estimates for Q and Q^2 actually allow a computation of the inflection point at $Q = .59$,[18] the level at which the effect of the override requirement on veto incidence is maximal. As pointed in Magar (n.d.), all the effect of variable Q on the dependent variable can be theoretically attributed to Cameronian uncertainty, since my position-taking model presumes no relation of this variable and vetoes as publicity stunts. The likelihood of a large number of miscalculations about whether or not a coalition to override a veto will form reaches a maximum in sessions held under a three-fifths override-majority requirement; it is smaller for session held at an absolute majority override requirement and at a two-thirds requirement. I provide a graphical representation of this effect in the next section.

The negative finding just reported conforms well with Cameron's theory, not with mine. It should be pointed that there is another prediction on which the two models contradict each other: from the perspective of uncertainty, veto incidence should associate negatively with the electoral calendar (because players somewhat learn about each other's preferences); from the perspective of position-taking, veto incidence should associate positively with election proximity (as per the *electoral oscillation hypothesis* (H5)). The latter hypothesis, as pointed above, could not be rejected at conventional levels. The influence of publicity stunts cancels that of learning, actually adding some more, as evidenced in the positive and significant coefficient estimates of variables E and E^2.

Before turning to interpret coefficient estimates, I report that I also ran a version of the statistical model including fixed state effects. These effects are another possible source of heterogeneity between sessions violating assumption A15. While about half of the state dummies obtained estimates that are statistically significant, estimates for the partisan composition of the branches (variables D and A) in fact grow in magnitude and in statistical significance with the

[18] The inflection point is obtained easily from the coefficient estimates: the first derivative of function $f = 103Q -$

addition of these controls. The fixed-effect model has to exclude variable Q due to its perfect collinearity with a linear combination of the state dummies; its effect is captured by the latter. The estimated coefficient of electoral proximity (variable E) in the fixed-effects model retains the same sign, loses more than half its size, and most importantly loses any sign of statistical significance. The fixed-effects model makes another suggestion about the necessity to seek additional supportive evidence for the *electoral oscillation* (H5) of veto incidence.

4 Interpreting the results

I have not paid attention so far to the estimated *values* of coefficient, only to their sign. There is good reason because, as pointed, the interpretation of negative binomial regression coefficients is different from that of ordinary linear regression. Each coefficient requires some transformation in order to assess the impact that a unit change in the corresponding regressor (IV) has on the regressand (DV). The simplifying assumption of linearity in ordinary regression offers a plain reading of estimates. For example, if we modeled the height differential of father and son as a function of the height differential of grandfather and father (i.e. $\text{centimeters}_{\text{father}} - \text{centimeters}_{\text{son}} = \beta(\text{centimeters}_{\text{grandfather}} - \text{centimeters}_{\text{father}})$) and obtained a $\hat{\beta}$ estimate of -1, the meaning is straightforward: there is a negative one-to-one correlation between the variables, whereby a unit increase of the IV (a father 1 centimeter higher than the grandfather) is associated with a unit decrease in the DV (a son 1 centimeter shorter than the father). As soon as the method of estimation involves a non-linearity assumption (as in negative binomial regression), a coefficient estimate of -1 would not result in the same effect. The impact of the IV on the DV is indeed negative, but the actual effect varies with the value of the IV in question as well as with the value of other IVs (when the model is multivariate).

$174Q^2$, equated to zero, provides the reported result: $f' = 103 - 174Q = 0 \Leftrightarrow Q = .59$.

One way to decode the information conveyed by negative binomial regression involves a comparative statics simulation, using the model's estimated coefficients to obtain the expected veto incidence for different combinations of values for the IVs. The expected number of vetoes per session given the session's characteristics ($\hat{\lambda} \mid X$) is obtained with the product of the vector of coefficient estimates ($\hat{\beta}$) and a matching vector of values for each variable in X:

$$(\hat{\lambda} \mid X) = \exp (\hat{\beta} X). \tag{15}$$

If X' is a vector identical, component by component, to X with the exception of the value for one of the variables, the difference between ($\hat{\lambda} \mid X'$) $-$ ($\hat{\lambda} \mid X$) represents the independent effect on veto incidence attributable to the variable that changed from X to X'. (Note that it is the presence of the exponential function in equation (15), as per equation (14), that renders the interpretation of coefficients tricky.)

Table 3.5 illustrates this exercise in comparative statics for different sets of values for the regressors in the model. Control variables in are held fixed at a regular session ($R = 1$) during the recession ($F = 1$) in a non-Southern state ($S = 0$) throughout the simulation. The exposure (bill output of the session is) set at B = 100 bills, and this way results can be read as percentages. Each cell in the table contains the expected veto incidence (per hundred bills passed in the session) of 12 combinations of values.

Cells in Table 3.5 are analogous to a set of 12 photographic snapshots of the veto incidence (our main subject) and its regressors (the secondary and tertiary characters). Tertiary characters are control variables F, S, and R, left fixed in the exact same value (or location in the analogy). In each snapshot, one and only one of the secondary characters (the partisan configu-

Table 3.5. The expected number of vetoes per 100 bills passed.

		1 year until next election (a) $Q = {}^1/_2$	*1 year until next election* (b) $Q = {}^2/_3$	*1 month until next election* (c) $Q = {}^1/_2$	*1 month until next election* (d) $Q = {}^2/_3$
(i)	*Unified government*	3.44	4.34	4.33	5.46
(ii)	*Divided government with unified assembly*	6.27	7.92	7.89	9.95
(iii)	*Divided government with split assembly*	3.43	4.34	4.32	5.45

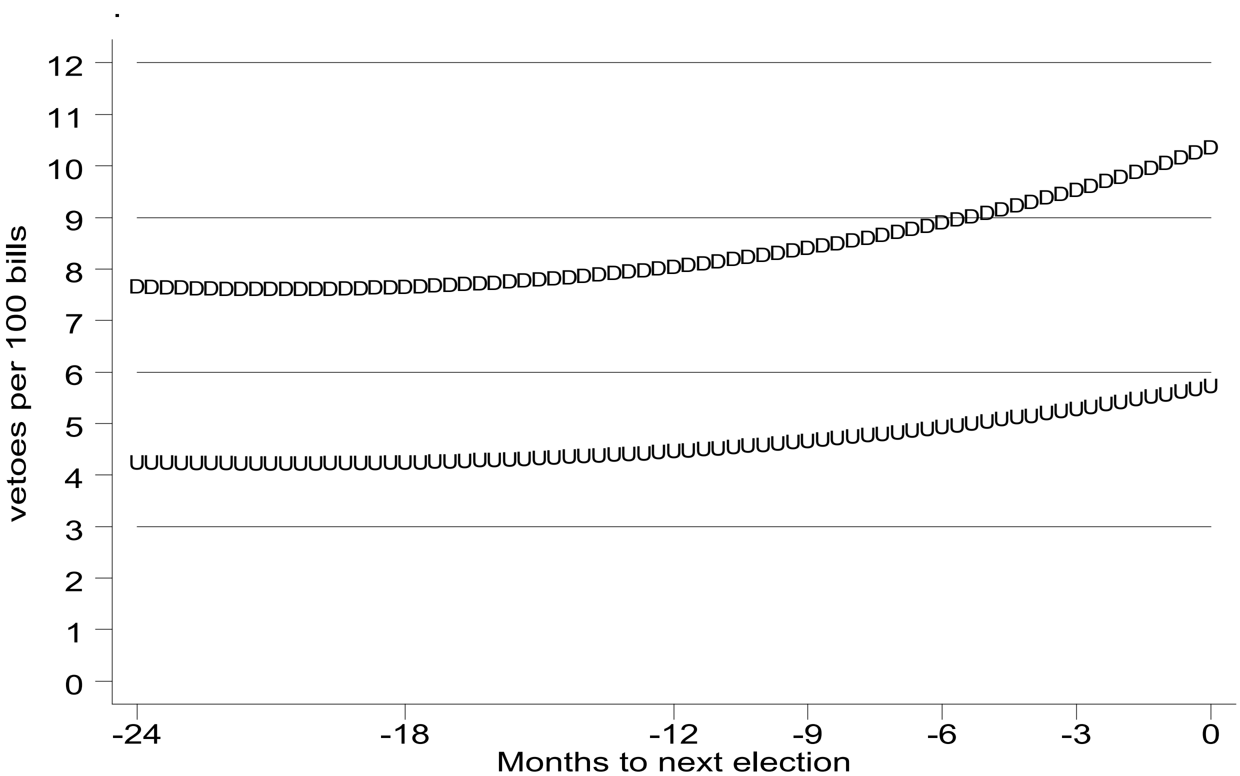
How to read this table: Cell (ai) indicates an estimate of 3.44 vetoes per 100 bills passed in a session in which $Q = {}^1/_2$, in which the executive controls both houses of the assembly, and when the next election is one year ahead; cell (bi) indicates an increase to 4.34 vetoes per 100 bills changing to $Q = {}^2/_3$ while leaving the remainder variables untouched; and so forth. Other variables in the equation are set in the following fashion for all cells: $F = 1$; $S = 0$; and $R = 0$.

ration of the branches, the override requirement, and the electoral proximity) changes "location", thus allowing us to see its independent effect on veto incidence. So, for example, the difference in the value of cell (aii) and the value of cell (ai) informs us that, all other factors held constant, a session held under divided government (with unified control of the assembly by a party other than the executive's) increases the veto incidence rate by 2.83 vetoes for every 100 bills with respect to a session held under unified government (6.27 – 3.44 = 2.83). As pointed, the effect of divided government (or any other regressor, for that matter) is not the same at different values of the other variables. Raising the override requirement renders the effect of divided government more acute: it increases incidence by 7.92 – 4.34 = 3.58 vetoes per 100 bills passed. Similarly, shortening proximity of the next election (from one year to one month) further sharpens the effect of divided government to 9.95 – 5.46 = 5.62 vetoes per 100 bills. This is another look at how compelling the supporting evidence for the *divided government surge* hypothesis (H3) is.

Another interesting result of this snapshot exercise has to do with the relative effects of divided government and divided assemblies: they cancel each other out rather cleanly. The symmetry of coefficient estimates for the two dummy variables (.6 for *D*, –.609 for *A*; their sum practically returns zero) is suggestive of this. Table 3.5 offers a more illustrative portrait of the mirror effect of the two variables by translating it to a more easily interpretable unit, actual veto incidence. The additional vetoes (per each 100 bills passed) brought by divided government are taken away by dividing the control of the assembly as well. Divided assemblies depress the number of bills that can be vetoed by the executive: the second chamber exercises a veto before the executive does. Ceteris paribus, cells (ai) and (aiii), (bi) and (biii), (ci) and (ciii), and (di) and (diii) never manifest a difference of more than .01 vetoes per 100 bills passed. The *divided assembly slump* (H4) served as an antidote for the *divided government surge* (H3) among U.S. state governments.

The effect of variable *E* is also of substantive interest. Differences between cells (ci) and (ai), (di) and (bi), (cii) and (aii), and so forth (six differences in total) estimate the effect on veto incidence of switching from a session ending 12 months from the election to a session ending 1 month before the election. The effect of course varies depending on the values of other regressors but, ceteris paribus, it was roughly 2 vetoes per each hundred bills passed under divided government, roughly 1 veto per 100 bills passed under unified government or divided assemblies. Figure 3.3 offers a more continuous perspective of the same effect, by plotting the expected numbers of vetoes (per 100 bills passed) against variable *E*. The figure consists of two curves, one representing veto incidence in divided government sessions, the other representing veto incidence in unified government sessions (which, as pointed, are the same as divided assembly sessions). The first and second derivatives of both curves are positive, as was indicated

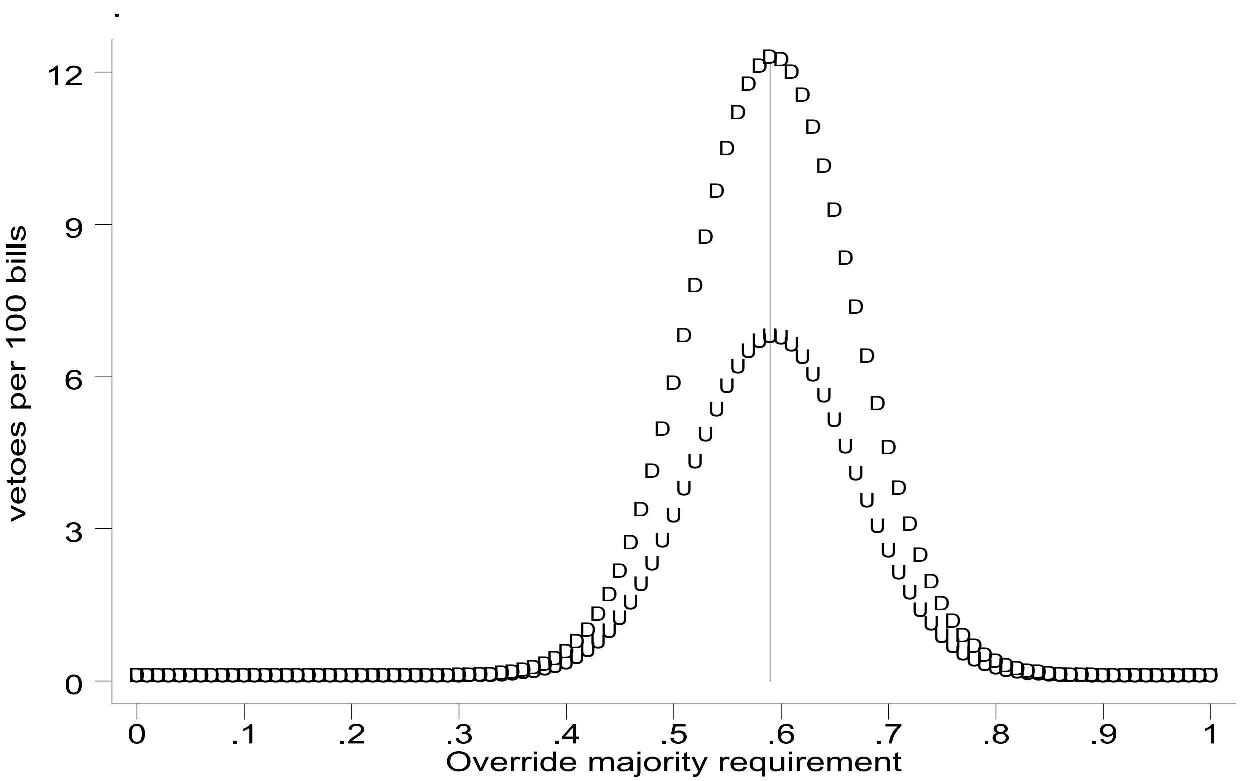
Figure 3.3
Vetoes and the election cycle



Notes: The curves portray the expected number of vetoes per 100 bills passed as a function of the time (months) remaining until the next election. *D*s represent estimates under divided government with a unified assembly, *U*s estimates under unified government. Other variables in the equation are held at the following values for this estimation: $Q = {}^2/_3$; $A = 0$; $R = 1$; $F = 1$; and $S = 0$.

by the coefficient estimates of E and E^2; the divided government curve is approximately twice as far above the origin as the unified government curve is, and has a slightly steeper tail towards the election. As the end of the session falls closer to an election, observable instances of inter-branch conflict increase at an increasing rate. An obliged extension of this project will consist of gathering disaggregated evidence so as to check whether or not individual bills are, ceteris paribus, likelier to be vetoed as the next election approaches. Aggregate evidence is suggestive that this could be the case, but such a claim may fall prey to the ecological fallacy (see King 1997).

Figure 3.4
The effect of uncertainty on executive vetoes



Notes: The curves portray the expected number of vetoes per 100 bills passed as a function of the override requirement *Q*. *D*s represent estimates under divided government with a unified assembly, *U*s estimates under unified government. Other variables in the equation are held at a value of zero, with the exception of *E* which is set to 100 and *R* which is equal to 1.

Another enlightening analysis is that of the impact of variable *Q*. As pointed, I interpret the override majority requirement to be a proxy for the baseline uncertainty that surrounds a given session. The coefficient estimates of *Q* and Q^2 are the largest of the set in absolute value. Figure 3.4 offers a portrait of the substantial effect of uncertainty on executive veto incidence. Holding the remainder variables at fixed values, the effect of increasing the override requirement in U.S. state governments had an inverted parabolic shape reaching a maximum in about $Q = {}^3/_5$. In U.S. state government legislative sessions uncertainty seems to have reached a peak influence under such an override requirement. The tails of the parabola become tangent with the x-axis quite rapidly on each side of the inflection point. By $Q = .4$ or $Q = .8$, the estimated effect has almost vanished.[19] The expected incidence rate is a bit over nil per 100 bills passed when the session is held under a $Q = .4$ institutional setting, regardless of the partisan composition of the branches. Expected incidence skyrockets to something close to 6 vetoes per 100 bills under divided government (3 per 100 under unified government or divided assemblies). At its peak, the independent effect of uncertainty attains 12 vetoes per 100 bills passed under divided government, 6 per 100 under unified party control. This effect is symmetric on the other side of the maximum.

The effect of variable Q (the override requirement) conforms well with vetoes-as-bargaining-ploys; the effect of variable E (electoral proximity) conforms better with vetoes-as-publicity-stunts. How should this contradiction be read? This should be interpreted as evidence that two determinants of veto incidence, Cameronian uncertainty and position-taking, are entangled in the empirical observations being analyzed. Vetoes as mistakes are conflated along with vetoes as publicity stunts. A different research design will be needed to isolate one type from the other. The data I rely on, however, do provide a little leverage to address this issue.

All that can be done in this respect with the evidence at hand is carry a separate analysis of legislative sessions held under an absolute majority override requirement ($Q = .5 + \varepsilon$). A plausible argument can be made that Cameronian uncertainty shrinks to nearly zero in this subset of sessions, the reason being that it becomes nearly unquestionable whether or not a coalition to override an executive veto will form. After all, assuming no abstentions, the very same coalition that formed once in order to pass the bill suffices to override an eventual veto to that same bill. I

[19] Actually, running the model without excluding North Carolina (where $Q = 0$) does not have any noticeable effect on coefficient estimates nor their p-values. This is not true if variable Q^2 is excluded from the equation.

Table 3.6
Estimating the model only on legislative sessions where *Q* equals absolute majority (sessions in Alabama, Arkansas, Indiana, Kentucky, and Tennessee)

Part 1: Coefficient estimates

	Variable	Coefficient estimate[a] (standard error)[b]	p-value (two-tailed test unless otherwise indicated)
1	*constant*	-4.017 (.492)	<.001
D	*divided government*	1.575 (.350)	<.001 (one-tailed)
A	*divided assembly*	.028 (.725)	.485 (one-tailed)
E	*election proximity*	.002 (.002)	.061 (one-tailed)
E^2		2×10^{-6} (1×10^{-6})	.075
R	*regular session*	-.814 (.263)	.002
F	*fiscal shock*	-.503 (.298)	.046 (one-tailed)
	Arkansas	2.037 (.438)	<.001
	Indiana	.410 (.405)	.311
	Kentucky	1.420 (.491)	.004
	Tennessee	-.914 (.370)	.013
	Pseudo R^2 =	.1627	
	Model $\chi^2_{(10)}$ =	60.81	<.0001
	Log Likelihood =	-156.51	
	Number of observations =	71	

Notes:
(a) Negative-Binomial method of estimation. The number of bills passed in the session serves as the exposure variable. For variable definitions see Appendix 1 and text.
(b) The corresponding standard errors are not robust (cf. White 1980), unlike those of Table 3.4.

thus ran a version of the model only on those sessions held in Alabama, Arkansas, Indiana, Kentucky, and Tennessee, five states where vetoes to any bill can be overridden by an absolute majority of the assembly. The model slightly modifies the one whose estimates I reported in Table

3.4: variable Q_i is excluded (because it is a constant in the subset of sessions), and variable S_i I replaced with dummies for the different states whose sessions are included (a fixed-effects model excluding the Alabama dummy). Estimates appear in Table 3.6. The smaller number of sessions (N=71) produces estimates that differ from those of Table 3.4. Yet it is interesting to note that some effect is attributable to election proximity. The effect is twice as large as that estimated for the whole set of sessions (0.002 instead of 0.001), although it is only significant at the .06 level (the other was significant at the .004 level). Yet the finding is evocative: governors who are certain that the assembly can override any veto of theirs still rely on vetoes. Sessions in Arkansas produced the highest veto incidences in the subset, reaching 26, 37, 45, and 67 in sessions ending in 1991, 1985, 1993, and 1987 respectively; Bill Clinton was governor in all but the third (see Appendix 1). It is hard to explain this finding as something other that position-taking exercises.

I also ran an alternative specification of the model controlling for sessions where the executive possessed a line-item veto. A handful of state constitutions do not give the governor a line-item veto (that is, the possibility of vetoing items such as words and sentences of bills, while publishing the remainder into law). Legislative sessions held in Indiana, Maine, Nevada, New Hampshire, North Carolina, Rhode Island, and Vermont, where the executive has a package veto only, might have a different veto incidence than the rest. A higher veto incidence may be artificially created by item vetoes: a single bill, say the budget, may contain hundreds of items that the governor stroke from the original text. The source, unfortunately, does not specify whether a veto was of one type or another.

To control for this possible source of heterogeneity in legislative sessions, and to reduce the effect of this possible measurement problem, I ran the model controlling for the item veto institution. This variable should capture most of the "artificial" effect on veto incidence. Results only change slightly (see Appendix 2).[20] Although the effect of a governor with an item veto is positive and significant, controlling for it causes no major change in the estimates of other coefficients, and has virtually no impact on their statistical significance.

Alternative explanations of the results. Some may argue that the electoral oscillation in veto incidence may be the result of factors not taken into account in the present model. The oscillation may, for example, be the result of budgetary politics. A spurious correlation may result from a possible coincidence of the electoral and budgetary calendars. The budget prompts governors to item-veto many of its components, which I may be mistakenly attributing to position-taking incentives tied to the overlapping election. Also, the finding that election proximity matters can be observationally equivalent with explanations other than mine. For instance, it could be the case that more pork bills go to the executive when an election is near, and executives use the veto not for position-taking, but only because they are fiscally responsible.

I have elements to discredit both critiques. With regards to the spuriousness possibility, the addition of a control for line-item vetoes provides some leverage. The fact that the coefficient of this variable is positive and significant (as should be expected) does not drive the coefficient of the electoral proximity variable to non-significance (see Appendix 2). If the oscillation followed the budgetary calendar only then no variance would be left to be explained by the electoral calendar; this is not the case. The coefficient estimate of variable E_i loses 16% of its impact on the dependent variable when the item-veto is controlled for, but its significance is left virtually untouched. The electoral calendar matters.

[20] Stata 5 (*cite), the statistical software I own and used in the present analysis, does not have a feature to run binomial regression with robust standard errors (to control for possible problems of heteroskedasticity, cf. White 1980). I borrowed Stata 6 to estimate the model in Table 3.4. When I realized variable I_i should be included in the left-hand side I no longer had access to stata 6. This will be a simple problem to overcome in the near future.

The pork argument is rather inoffensive to my own argument because it follows the same logic. My claim is that politicians have incentives to propose popular policy knowing beforehand that some opponent will kill it, and that these incentives accrue as elections approach. Nothing is said in my argument about what exactly such policy will look like; it can represent public goods desired by core constituents which are divisive at the national level (e.g. a liberalization of abortion regulations); it can also represent private goods benefiting a locality at the expense of the nation (e.g. a targetable subsidy). Regardless of whether the bill involves pork or not, this legislation is proposed for the adversary to kill it, thereby explaining to constituents why the public good or pork in question was not enacted despite their representative's activism.

5 Conclusion

In this paper I constructed a model to test four hypotheses derived from the position-taking setter game. The method I relied upon to model executive veto incidence is negative binomial regression. I described the model at length. I then estimated the model on data from legislative session in the governments of U.S. states held between 1983 and 1993.

There is evidence to reject only one out of five hypotheses with substantive theoretical content. Four of the five hypotheses – including the hypothesis that was rejected – were drawn from a position-taking perspective on inter-branch relations. The fifth hypothesis belongs to Cameron's uncertainty perspective on inter-branch bargaining. Table 3.6 summarizes the results of hypothesis tests for this set of variables of theoretical interest. The table also reports the p-value of the coefficient estimate (the criterion for rejection of hypotheses).

Table 3.6
Summary of results from key hypothesis tests

Variable – hypothesis	*Formal hypothesis*	*Result (level of hypothesis test)*

Q	–	nil impact hypothesis	$\beta_1 = 0$	rejected (.999)
Q^2	–	proxy for uncertainty (Cameron)	$\beta_2 < 0$	not rejected (.001)
D	–	divided government surge	$\beta_3 > 0$	not rejected (.001)
A	–	divided assembly slump	$\beta_4 < 0$	not rejected (.001)
E	–	electoral proximity oscillation	$\beta_5 > 0$	not rejected (.004)

The data contain evidence that uncertainty, insofar as Q is good measure of it, plays an important role in the generation of executive vetoes. Since the position-taking theory expects no relation between veto incidence and Q,[21] the independent effect of this variable can be wholly attributed to Cameron's explanation. This effect is substantial, as seen in Figure 3.4. In state government sessions held under divided government, the effect of a change in Q from absolute majority to three-fifths is such that the veto incidence rate jumps from 6% to roughly 12% of bills, then back to 6% when the two-thirds Q is attained. Vetoes-as-position-taking-exercises and vetoes-as-mistakes are conflated in the data, and I not devised a way to disentangle them at this stage of my research.

The uncertainty approach to veto incidence and the position-taking approach share the hypotheses concerning the partisan composition of government. The hypotheses concerning the override majority requirement Q find strong evidence in the data for the uncertainty approach. As far as my understanding of Cameron's model goes, the hypothesis concerning the electoral cycle belongs to the position-taking approach only. I found some evidence in favor of it, but it is

[21] Actually, the theory expects an association between Q and overrides of executive vetoes (i.e. position-taking vetoes or PTVs in Magar n.d.).

weak given the aggregate nature of the data in the paper, not to mention that the finding is not very robust to a change to fixed-effects model.

Appendix 1

Sources and summary statistics of variables in the analysis reported in Table 3.4 appear in Table 3.A1. Table 3.A2 contains summary statistics for the subset of cases reported in Table 3.6.

Table 3.A1.
Description of the variables (excluding Nebraska and North Carolina, n=798 sessions)

Part 1: Continuous variables

Variable Name	Description	Mean	SD	Min.	Max.
V_I	Number of bills vetoed by the governor in session i.[a]	15.49	42.62	0	465
Q_i	Proportion of the state assembly in which session i was held needed to override an executive veto.[a]	.64	.06	.5	.67
E_i	Counting the day of the House election that is closest to the end of session i as day zero, subtract 1 for each day it takes to get to the day session i ended, and obtain in this fashion E_i.[a] In 48 sessions (6%) the next House election preceded the end of the session i.[c] In 99% of sessions, the House election preceded or was concurrent with the Senate election (hence the use of the House instead of the Senate election).[d]	-390.83	252.07	-1,445	-3
B_i	Total number of bills passed by the assembly in session i.[a]	296.84	353.88	1	3,128

Part 2: Discrete variables

Variable Name	Description	Frequency of values 0	1
D_i	Dummy equal to 1 if a party other than the governor's had an absolute majority of seats in at least one chamber in the assembly in session i; equal to zero otherwise.[a]	349	449
A_i	Dummy equal to 1 if different parties held an absolute majority of seats in each chamber of the assembly during session i; equal to 0 otherwise.[e]	638	160
F_i	Dummy equal to 1 if session i started January 1, 1991 or later; equal to 0 otherwise.[a]	545	253
R_i	Dummy equal to 1 if session i was a regular one; equal to 0 otherwise.[a]	317	481
S_i	Dummy equal to 1 if session i took place in Alabama, Arkansas, Florida, Georgia, Louisiana, Mississippi, the Carolinas, Tennessee, Texas, or Virginia; equal to 0 otherwise.[b]	609	189

Sources and notes:
(a) Variables taken from or prepared with information from the *Book of the States* (CSG, Various issues).
(b) From Alt and Lowry (1994).
(c) Twice in Wisconsin the assembly remained in session for nearly two years, with an election in the middle. Of the remainder 46 cases, 28 had a less than 100 but more than 50 days lapse between the ballot and the cloture of the session, while 18 had a 50 or less days lapse.
(d) In 53 sessions (6.6%) the next Senate election occurred after the House one, with the inverse occurring in only 3 sessions (0.4%); in the remainder sessions (92.9%) House and Senate elections were concurrent.
(e) In 32 sessions (4%) parties had exactly half the seats in one of the chambers (never in both). These instances were coded as unified assemblies (see footnote 10).

Table 3.A2:
Descriptive statistics for sessions in states with $Q = ½ + ε$:
Each cell contains the following statistics: Mean (Std. Dev.) [Min,Max]

	Alabama N=16	Arkansas N=16	Indiana N=15	Kentucky N=12	Tennessee N=12
bills vetoed	6.4 (5.5) [0,18]	14.6 (20.1) [0,67]	3.1 (2.9) [0,12]	3.9 (6.9) [0,23]	3.5 (4.8) [0,11]
divided government	.88 (.34) [0,1]	0 (0) [0,0]	.53 (.52) [0,1]	0 (0) [0,0]	.5 (.52) [0,1]
divided assembly	0 (0) [0,0]	0 (0) [0,0]	.27 (.46) [0,1]	0 (0) [0,0]	0 (0) [0,0]
election proximity	-546 (366) [-1198,-39]	-455 (150) [-690,-117]	-448 (147) [-571,-238]	-389 (191) [-692,-203]	-344 (169) [-551,-166]

Sources: see Table 3.A1.

Appendix 2

In order to control for one possible source of heterogeneity in legislative sessions, I ran the model with a dummy variable for sessions where the governor enjoys a line-item veto (all sessions except those held in Indiana, Maine, Nevada, New Hampshire, North Carolina, Rhode Island, and Vermont; 89% of sessions were held under line-item vetoes). Variable I_i equals 1 if session i was held under a constitution granting the governor a line-item veto, 0 otherwise. There is some change in the estimates when variable I_i is included, but none above a 25% increase or decrease in the value of the coefficient estimate (see column f in Table 3.A3), insufficient to shift the sign of any coefficient. Moreover, the statistical significance of coefficient estimates hardly changes at all (column g).

Table 3.A3
Two alternative specifications of the model reported in Table 3.4 (including/excluding variable I_i)

		Equation 1[i]		Equation 2[i]		Difference in coefficient estimates	Relative change in coefficient magnitude	Difference in p-values
		coefficient estimates	p-value[ii]	coefficient estimates	p-value[ii]			
		(a)	(b)	(c)	(d)	(e = c–a)	(f=e×100÷a)	(g = d–b)
1	*constant*	-33	<0.0001	-39	<0.0001	-6.5	19%	0
Q	*override requirement*	103	<0.0001	122	<0.0001	19.7	19%	0
Q^2		-87	<0.0001	-104	<0.0001	-17.2	20%	0
D	*divided government*	0.60	<0.0001[iii]	0.57	<0.0001[iii]	-0.03	-6%	0
A	*divided assembly*	-0.61	<0.0001[iii]	-0.61	<0.0001[iii]	0.004	-1%	0
E	*election proximity*	0.001	0.004[iii]	0.001	0.01[iii]	-0.0002	-16%	0.007
E^2		8.39×10^{-7}	0.03	6.45×10^{-7}	0.08	-1.94×10^{-7}	-23%	0.05
R	*regular session*	-0.071	0.57	-0.062	0.61	0.01	-13%	0.05
F	*fiscal shock*	0.25	0.003[iii]	0.25	0.002[iii]	-0.0002	-0.1%	-0.001
S	*Southern state*	-0.48	<0.0001[iii]	-0.57	<0.0001[iii]	-0.09	19%	0
I	*item veto*			1.07	<0.0001			
	Log Likelihood =	-2069		-2044				
	Model $\chi^2_{(9)}$ or $\chi^2_{(10)}$ =	108	<0.0001	159	<0.0001			
	Pseudo R^2 =	0.03		0.04				
	Number of observations =	798		798				

Notes: (i) Negative binomial method of estimation. The number of bills passed in the session serves as the exposure variable. For variable definitions, see Appendix 1. (ii) The corresponding standard errors are not robust (cf. White 1980), unlike those of Table 3.4. (iii) One-tailed hypothesis test.

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