Veto incidence in comparative perspective: The case of American state governments*

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7 February 2007

Abstract

The paper examines the determinants of executive vetoes in a comparative environment (49 US state governments in the 1979–99 period) providing finer-grained evidence than extant studies about how and why vetoes happen. Results show that divided government and approaching elections have a positive effect on vetoes, even when controlling for the features of the veto, professional assemblies, and the economy. Additionally, parties with enough seats to override vetoes are systematically associated with greater veto incidence. This effect had been overlooked because veto-proof majorities, common in states and in other countries, are rare at the national level in the well-studied case of the US. In divided state governments, between 6 and 10 additional vetoes per session are attributable to approaching elections, between 4 and 7 to veto-proof majorities. Implications for our understanding of the causal mechanism of the veto are discussed.

The executive veto is the power to turn down legislation. One of the central tenets of separation of power, the veto is found today in presidential constitutions worldwide, at both the national and sub-national levels of government. Interest in this institution has spurred a considerable body of research, and much has been learned about its operation and determinants. But knowledge remains remarkably partial, since no one has so far studied the veto systematically outside of the US federal government. This paper makes a first attempt to generalize our understanding of veto incidence by examining American state governments between 1979 and 1999.

A comparative research design offers advantages. Comparative analysis is more powerful than a single-case study at evaluating institutional and partisan effects on observed vetoes. A comparative environment magnifies the variance in the underlying conditions of inter-branch bargaining, and therefore offers richer information about how and why vetoes happen. Analysis reveals that divided government and

*I am grateful to Neal Beck, Gary Cox, Federico Estévez, and two anonymous referees for comments and critiques. Luis Estrada, Juan Antonio Rodríguez Cepeda, and Fernando Rodríguez Doval provided superb research assistance. Mistakes remain the author's responsibility.

approaching elections have a positive effect on veto incidence in states, as in the case of the US federal government. The cross-section time-series also reveals that parties with assembly majorities above override level (a product of party strength and/or veto institutions, as we will see) also have a positive and significant effect on the veto. Findings conform to predictions of a model of vetoes as electoral stunts (Magar n.d.).

1 Two explanations of the veto

A brief review of the theoretical debate is in order. It will help understand the logic behind empirical model specification, the subject of next section, but also to inform the results of the current analysis. The veto has caught the attention of formal theorists in their quest for the micro-foundations of politics. While most share a common understanding that vetoes are only apparent bargaining failures, that they really are signs that negotiation in politics can, and often does adopt forms that no one had modeled, they also have different views of what causes vetoes. At the risk of oversimplifying the debate, we can divide the field into those who propose *uncertainty* and those who propose *position-taking* as the primary cause of vetoes (see Cameron and McCarty 2004 for a review).

Cameron (2000) is representative of the first group. He sees uncertainty about the limits of other politicians' willingness to concede as the main driving force behind veto incidence. When preferences are opaque, assembly members, in their quest for maximal influence on policy, may mistakenly propose something unacceptable to the executive, therefore triggering a veto. The mechanism producing vetoes has two levers. In light of executive opposition to legislation, some vetoes result from not knowing precisely how much needs to be conceded to guarantee a successful override in the assembly. In such circumstances, the executive's dominant strategy is to veto, since there is a non-zero probability that policy will revert to the preferable status quo. Other vetoes result from the assembly having doubts about just how compromising the executive is. In such circumstances the executive can use the veto to build a reputation for toughness, strategically vetoing otherwise acceptable proposals in order to get juicier future concessions.

Groseclose and McCarty (2001) are representative of the second group, favoring position-taking as the primary cause of vetoes. By this line of argument, when executive opposition prevents assembly members from delivering, they can nonetheless mobilize support by making the proposal despite — more precisely, *because* of — the certainty of the veto that will follow. The veto provides an alibi for failing to deliver which, it is assumed, is useful in the next election by showing who stands where on the issue (a blame game). The mechanism of this sort of vetoes is also twofold. Some vetoes are the product of an assembly wishing to blame the executive when no new policy can be agreed between the branches. In such circumstances,

assembly members can propose what they like, let the executive kill it (a veto is sustained), and in fact let the electorate decide the issue in the next election. Since there is nothing in Groseclose and McCarty's model preventing from extending the logic of position-taking to the executive (Magar n.d.), other vetoes are the product of an executive wishing to blame the assembly for policy he or she dislikes, but is veto-proof. In such circumstances, the executive may veto (despite the certain override) to take a verifiable position on the issue.

Which explanation of the veto — uncertainty, position-taking, or a combination of the two — is more accurate remains an open question, one that this paper attempts to start resolving empirically. For this purpose, I argue that the veto-triggering mechanisms of the theories outlined above operate in opposite directions when confronted with two common conditions of the bargaining environment, thereby predicting different effects on veto incidence (Magar n.d. offers a formal treatment of the discussion in the next two paragraphs).

Condition one is a majority party above override level in the assembly. This can be the product of large party contingents, but can also result from smaller veto-override requirements — eg simple majority, as in the case of Tennessee (see Table 1). A party above override in both chambers of the assembly will remove a substantial amount of uncertainty about the outcome of a veto: if the party is cohesive, the same coalition passing a bill in the first place can also render it veto proof. Therefore, when this condition holds we should expect, other things constant, *fewer uncertainty-driven vetoes*. But this condition leaves the mechanics of the position-taking logic unaffected, and its fulfillment should not affect veto incidence: if there is executive opposition, then we should expect *more position-taking vetoes* when this condition is met.

Condition two is election proximity. To the extent that position taking is one of the threads with which electoral campaigns are woven, we ought to expect, other things constant, *an increase in position-taking vetoes* as the next election draws nearer. But since election proximity also means that a two-year legislative period is reaching its end, this condition also implies that the occupants of the branches have been interacting for quite a while. If repeated play brings any learning about what exactly your opponent wants, then this condition is tantamount to "less uncertainty about the executive and what he or she wants" and, hence, it should be associated with *fewer uncertainty vetoes*.

2 Previous research on the veto

The other branch of the literature, empirical models, assumes that observed veto incidence breaks into systematic and stochastic components, thereby adopting the standard form in regression analysis: $V = f(\beta X + e)$, where V is the expected number of vetoes for some unit of observation; X is a set of measures of the alleged

	No veto	*Majority override*	$\frac{3}{5}$ *majority override*		$\frac{2}{3}$ *majority override*		$\frac{3}{4}$ *majority override*
Item veto	$\emptyset$	Alabama *Arkansas* *Kentucky* *Tennessee* West Virginia (rest)[a]	Delaware Illinois (rest)[a] *Maryland* *Nebraska*[b] *Ohio*	Alaska (rest)[a] *Arizona* California *Colorado* Connecticut *Florida* *Georgia* Hawaii *Idaho* *Iowa* Kansas	Louisiana Massachusetts *Michigan* *Minnesota* Mississippi Missouri Montana New Jersey *New Mexico* *New York* *North Dakota* *Oklahoma* (rest)[a]	Oregon *Pennsylvania* South Carolina *South Dakota* *Texas* *Utah* Virginia Washington *Wisconsin* *West Virginia* (rev&app)[a] *Wyoming*	Alaska (rev&app)[a] Illinois (rev&app)[a] Oklahoma (rev&app)[a]
No item veto	North Carolina (until 1998)[c]	Indiana	*Rhode Island* North Carolina (since 1998)	*Maine*	Nevada *New Hampshire*	*Vermont*	(No empirical referents)

Notes: ***a*** Names followed by parentheses refer to constitutions placing different override requirements on revenue and appropriations bills (rev&app) and other bills (rest). Latter requirement used to code these cases in the analysis (see text). ***b*** Dropped from analysis (see footnote 2). ***c*** Sessions in these years excluded from analysis.

Source: CSG (various years).

Table 1: Veto institutions in state governments (italicized states' constitutions allow pocket vetoes)

determinant of vetoes in the chosen unit; β is a vector of parameters to be estimated; e is random error; and $f(\cdot)$ is a functional form relating βX (the systematic component) and e (the stochastic component) to the dependent variable. Empirical work estimating β in the United States divides into work with aggregate data to estimate veto incidence at different units of time; and work with pieces of legislation to estimate the probability that a bill is vetoed. This paper analyzes aggregate data, so this section will review the literature in the first group.

A progressive refinement of data and methods is manifest in eight models, summarized in Table 2. To begin, models vary precisely in the level of data aggregation. In the pioneering study Lee (1975) explained the number of vetoes observed in each Congress with features that remain constant through the biennium. Subsequent studies — including Copeland's (1983), Rohde and Simon's (1985), Hoff's (1991), Wooley's (1991), Shields and Huang's (1995), and McCarty's (n.d.) — refined the unit of observation by relying on yearly aggregates instead. And Shields and Huang (1997) provided even finer-grained evidence in their analysis of monthly aggregates. Studies also vary in the time span of their observations. After Lee chose to observe vetoes from Washington to Nixon (1789–1972), analysts have restricted the observation interval for different reasons. McCarty observed years after Jackson's election in 1829, arguing that the strong electoral (ie, position-taking) component of the veto renders periods before and after politicians began appealing to the mass electorate to fulfill their ambition quite different. Copeland and Hoff, looking for a more controlled test, started after the two-party system stabilized, observing 1860–1980 and 1889–1989, respectively. While Rohde and Simon, Wooley, and the two Shields and Huang studies started after World War Two, when reliable data for key regressors became available. Studies do offer a pretty comprehensive view of the veto in the United States through its constitutional history.

Next, studies vary in the functional form they give to f and the method of estimation. Rohde and Simon, Hoff, and Wooley chose a linear specification, leaving the equation as $V = \beta X + e$. Lee, Copeland, Shields and Huang, and McCarty opted instead for an exponential specification, or $V = \exp(\beta X + e)$, which has the advantage of restricting the range of V (so that the model will not predict negative vetoes) while making the effect of X not linear (it is thereby harder to move from 0 to 1 veto than it is to move from 100 to 101). With regards to method, Shields and Huang (1995) rightly argued that, due to the discrete nature of the dependent variable (0 veto, 1 veto, 2 vetoes, and so forth), β should not be estimated with ordinary least squares (OLS), as previous studies had. Appropriate methods exist to handle such data, the most common being negative binomial regression that both Shields and Huang (1997) and McCarty relied upon.

Substantively, the most interesting variation between models regards the choice of regressors in X, how they are measured, and with what results. While some studies do annul claims made by others, no two studies obtained estimates of opposite

	Regressors included	Lee 1975	Copeland 1983	Rohde& Simon 1985	Hoff 1991	Wooley 1991 major bills	Wooley 1991 minor bills	Shields&Huang 1995	Shields&Huang 1997	McCarty nd
A	ln (*Bills passed*)	+	+	·	+	+	0	+	0	+
B	*Div. government* (*DG*)	0	+	+	0	+	0	0	+	+
	Div. assembly only	·	·	·	·	·	·	·	·	0
	Pivot polarization	·	·	·	·	·	·	·	·	0
	DG × *Elec. year* × *Economy*	·	·	+	·	·	·	·	·	·
	DG × *Pres. for reelection*	·	·	·	·	·	·	·	·	+
C	*Midterm*	0	+	+	·	0	+	+	+	·
	Presidential	·	0	0	·	0	0	0	0	0
	Pres. for reelection	−	·	·	·	·	·	·	·	−
	President's vote share	·	+	·	·	·	·	·	·	+
D	*Presidential approval*	·	·	−	·	−	0	−	0	·
	Economic shock	0	0	·	+	·	·	0	+	0
	War	0	0	−	·	−	0	−	0	0
	Unelected president	·	·	·	+	+	0	0	+	0
	Other	*Democrat* (+) *Was congressman* (−) *Governor* (0) *Overrides* (−) *Time line* (0)	*Democrat* (0) *Cleveland* (+) *FDR* (+) *Was congressman* (0) *Overrides* (+)	·	*Second term* (+) *Year in term* (+)	*Time line* (0)	*Time line* (0)	$Vetoes_{t-1}$ (0)	$Ovrds_{t-1}$ (0) $Ovrds_{t-2}$ (0) $Ovrds_{t-3}$ (+)	·
	Unit of analysis	biennium	year	year	year	year	year	year	month	year
	Years	1789–1972	1860–1980	1945–80	1889–1989	1946–86	1946–86	1947–93	1954–92	1829–1996
	Number of observations	91	121	35	101	41	41	47	283	85
	Functional form	exp(·)	exp(·)	linear	linear	linear	linear	exp(·)	exp(·)	exp(·)
	Method of estimation	OLS	OLS	OLS	OLS	OLS	OLS	Event count	Negative binomial	Negative binomial

Note: Table entries are signs of parameter estimates; 0s indicate parameter failed to achieve confidence at .95-level; dots for regressors not included in analysis.

Table 2: Explaining veto incidence in the US

signs. This should not be surprising, given that all are looking at the same case. The different results are attributable in no small part to variable specification. Four categories of explanatory factors have been included in X.

A. *The president's exposure to legislation.* If no bill is brought to the executive, there can be no veto. This obvious consideration made all, except Rohde and Simon, include the number of bills passed in the time period (logged to account for decreasing effects) in the right side.

B. *Measures of players' preferences.* Since the veto becomes relevant in case of inter-branch conflict over legislation, all models included some measure(s) of *Divided Government (DG)* to capture the effect of the executive's preferences being significantly less in line with those of the assembly majority. Nearly half found no effect of DG on veto incidence (zeroes in Table 2 indicate estimates that did not attain statistical significance at the standard .05 level), which is surprising at first sight. McCarty's results, however, suggest that nil findings are likely to result from a failure to explicitly model the interaction of House and Senate in the definition of this variable. He showed with two dichotomous variables that, whereas instances of unified assembly control by one party against an executive of the other party significantly increase V compared to instances of unified government, instances where assembly control is itself divided do not. Lee's trichotomous measure of DG (0 if unified, 1 if split assembly, 2 if divided government with a unified assembly) treated a categorical variable as if cardinal, and this probably explains his negative finding: the (nil, according to McCarty) effect of moving from $DG = 0$ to $DG = 1$ may well dissolve the (positive) effect of moving from $DG = 1$ to $DG = 2$. The same happens, to some extent, with those who measured DG as the share of seats in both House and Senate held by the party opposing the president: while Rohde and Simon, and Shields and Huang's two studies, found a positive effect for this specification, Hoff and Wooley did not.

C. *The election cycle.* With the exception of Lee, whose biennial data is not suited to detect such effect, all studies found that midterm election years have a positive effect on V. Wooley's result is particularly interesting. In his separate analysis of vetoes of major and minor legislation, he gives evidence that the midterm electoral effect is felt on the latter, but not the former bills. DG and the exposure variable, he found, have the opposite effect, increasing major veto incidence but not minor veto incidence. Important policy appears to navigate the legislative process relatively smoothly when government is unified, but capsizes when government is divided; and assemblies in midterm years seem to produce more trivial policy for the executive to kill — suggesting exercises in position-taking. Presidential election years, however, were never found to affect V per se, although Rohde and Simon found they do so under divided but not unified government. McCarty found that presidents who seek reelection depress V when their party controls Congress, but the effect disappears for opposition-controlled Congresses.

D. *Other controls.* Studies also controlled for features such as economic shocks (some find a positive effect on veto incidence, as one would expect), wars (some find a negative effect, as one would also expect), the president's vote share (positive), and approval (negative).

In sum, empirical scholarship has given us a fairly good understanding of what determines vetoes. The evidence supports that, controlling for factors such as the volume of legislation, bill relevance, and economic conditions constant, the partisan configuration of government and elections have a significant effect on vetoes in the United States.[1] But this impressive array of evidence remains incomplete until it is confirmed by studies of other, comparable cases. I take a step in this direction by analyzing veto incidence in American state governments.

3 A model of veto incidence in state governments

I observe the legislative process in 49 state governments from 1979 to 1999, a period including years before and after the recent depression in state economies (Gramlich 1991).[2] Most of the information was compiled from the *Book of the States* (CSG, volumes 21 through 33). Unlike previous studies with aggregate data, the unit of observation is not a year or a month, but a state's *legislative session*, several of which can occur in a given year or month in a given state. So, for example, in 1979 the Alabama state assembly sent 592 bills to governor Forrest "Fob" James' desk for signature in two sessions. One special session was held from 18 January to 24, in which 10 bills were passed. Then a regular session was held from 17 April to 30 July, with the remainder bills sent to the governor. Aggregate data for these sessions appear in the dataset as observations 1 and 2. In total, 1,365 sessions are included in the analysis.

The dependent variable V is the same as in previous studies, except that here the number of vetoes is observed in a comparative setting (state governments) and in units of aggregation (sessions) that do not last the same, resulting in different number of sessions for each state. Three possible limitations to this measure of veto incidence can be anticipated. First, the source may conflate vetoes of public bills with vetoes of resolutions or private bills, treating all legislation as equivalent. It would be preferable to retain only the first in the analysis. The large number of bills enacted in Alabama's regular session of 1979, in fact, is suggestive of such pooling. This limitation should be unimportant to the extent that such legislation

[1] With the exception of Gilmour (2002), analyses of disaggregated data confirm these findings (Cameron 2000:48; Carson and Marshall 2003; Grier, McDonald, and Tollison 1995). All such studies but Carson and Marshall's have relied on samples of bills in a period. Gilmour's sampling method selected some cases on the dependent variable (203–4) so it is likely that his estimate is biased to some degree, possibly explaining his non-confirming findings.

[2] I excluded Nebraska from the analysis because the formally non-partisan nature of the elections prevented coding key regressors. I also dropped sessions in North Carolina before 1998, the year the governor was finally given a veto power.

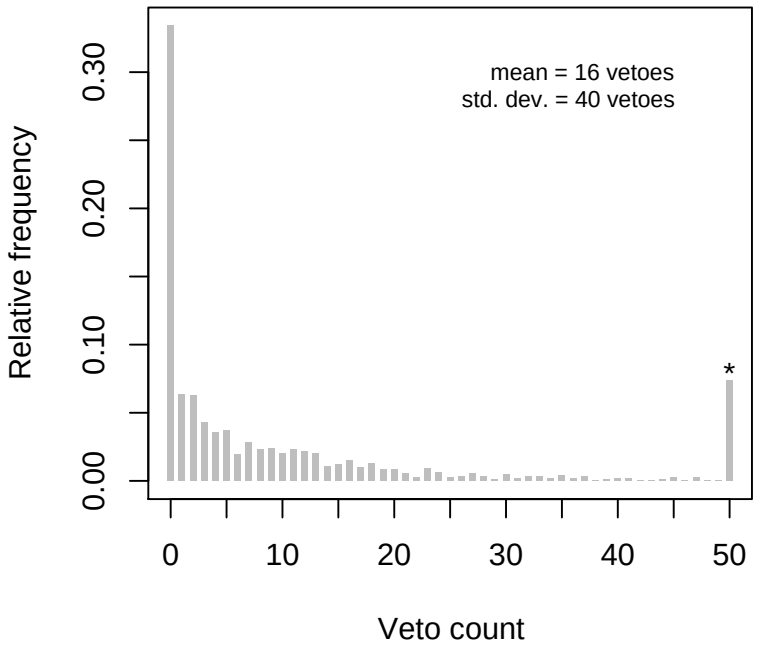

Figure 1: Veto frequency in 1,365 state legislative sessions. The observed distribution has a long but very "thin" tail extending to the right of the histogram and is not portrayed due to space limitations. Sessions with 50 or more vetoes (101 sessions or 7% of the total) appear stacked in the right-most column, marked with an asterisk, which is fictitious. The observed distribution spreads these observations, with increasing sparseness, from 50 to 465 veto counts.

is something the governor cares enough about to deserve a veto, despite not being a public law ("a veto is a veto" summarizes this approach). Second, the measure of V may include line-item vetoes along with full vetoes, possibly several in the same bill. Since most states in the period allowed item vetoes (see Table 1), a control for sessions with this feature is included; it should capture some of the artificial effect on veto incidence. And third, V possibly also conflates regular and pocket vetoes, which Hoff (1991) has argued should be analyzed separately. The problem of pocket vetoes is handled in identical fashion.

Like Shields and Huang (1995), β is estimated with negative-binomial regression. Vetoes in state governments are rare events, as can be seen in the acute right-skewedness of the distribution of vetoes observed in the sessions in the dataset in Figure 1. V has a single mode in zero vetoes per session, and the frequency drops sharply as the number of vetoes per session increases. Vetoes also seem over-dispersed (the observed mean of 16 vetoes is way smaller than its variance of 1,637),

so it is easy to argue that the negative binomial is the appropriate distribution to assume when analyzing this sort of random variable (see Cameron and Trivedi 1998).

All the variables in the equation are defined formally with summary statistics in the appendix. Included in the right side are separate indicators for three breeds of divided government. Following McCarty (n.d.), I distinguish divided assembly sessions by including a dummy variable ***Div. assembly***, equal to 1 when different parties have majority status in each of the chambers of bicameral assemblies; equal to 0 otherwise. And following the discussion in section 1, I also distinguish sessions held under "plain" divided government – by coding a dummy variable ***Plain DG*** equal to 1 when the party in control of the unified assembly had a majority below override level; 0 otherwise — and sessions held under "super" divided government — ***Super DG*** equals 1 when the party in control of the unified assembly had a majority above override level; 0 otherwise. The requirement to override a veto in the U.S. Congress being relatively high (two-thirds of each chamber), a party above override level has been very rare.[3] In state governments, on the contrary, it is quite common to find parties controlling two-thirds or more of both chambers. In addition, override requirements vary considerably from state to state: some, like Alabama, require a simple majority; others, like Alaska, require three-quarters of each chamber for certain legislation (see Table 1). Comparing state governments therefore offers the variance required to examine the effect of institutions and parties on vetoes and, in so doing, test differing predictions of the uncertainty and position-taking explanations of the veto. While we would expect that the coefficient of variable *Plain DG* is positive (compared to unified government, the baseline category), that of *Super DG* depends on the interpretation of what vetoes are about. If position-taking vetoes are predominant in state legislative sessions, we would expect a positive coefficient for *Super DG*; if the uncertainty logic predominates, the coefficient should be nil (indicating no difference in veto incidence with unified government sessions).[4] The omitted category, unified government, serves as the baseline against which to interpret the coefficient estimates of this triad of party profile variables.

As mentioned, state party systems vary considerably from the national. In 461 session in the analysis, or 34% of all the party in control of the unified assembly was above override level. For contrast, only 6% of US Congresses have had this feature since 1829, 3% since 1945. Since many state assembly supermajorities took place under unified government, the figures for *Super DG* are much smaller, but far from insignificant: 8% of all sessions (or 111) are coded as *Super DG*=1. While 55 of

[3]After 1829, parties above override level are found in the 39th Congress (1865–67, during Lincoln's administration), the 43rd (1873–75, Grant's), the 74th and 75th (1935–39, Roosevelt's), and the 89th (1965–67, Johnson's); in all but the first, government was unified.

[4]We might even expect a negative estimate. A significant number of states with supermajorities will have little real party competition, where parties tend to be factious, fractionated, and weaker. We can read Wright and Schaffner (2002) as indicating that one-party/non-partisan chambers tend towards greater dimensionality of the issue space, making it easier for the governor to extract a majority coalition though he/she is in the wrong party (so fewer vetoes). I am grateful to one anonymous referee for pointing this out.

these took place in states with a simple-majority override rule, where any unified assembly is perforce above override level, 8 did so under a three-fifths rule, and as many as 48 under a two-thirds rule.[5] State parties frequently win majorities of remarkable size. All sessions considered, the average share held by the smallest-of-the-two-chambers majority is 63% of seats (with a standard deviation of 12%). This was about the average size of Mexico's PRI up to the 1980s, a party then considered to be hegemonic!

The next variable is ***Election proximity***, over which the two interpretations of the veto hold differing expectations. This variable, which enters the equation logged to capture a possibly non-linear relationship with veto incidence, quantifies the time (in negative days) separating the end of a session from the next House election in the state. So for a session ending on election Tuesday, it equals -1; for a session ending the Monday before, it equals -2; and so forth.[6] As discussed in section 1, the uncertainty explanation of the veto expects a *negative* coefficient for this variable, the position-taking explanation expects a *positive* coefficient. We can anticipate that the aggregate nature of the data complicates measurement of this electoral effect, making it harder to estimate it in state governments. When working with aggregate data, a conjecture can be made that session 1 ending d days from the next election should, all else constant, have more vetoes than session 2 ending $d-1$ days from the next election. If the motivation to veto increases monotonically with *Election proximity* (as my measure assumes), then session 2 has one more day with high motivation than session 1, hence should have more vetoes. But if session 2 were also longer (ie., started earlier) than session 1, the previous effect would be diluted with more low-motivation days at the beginning. This problem is not as severe as it seems, since it works in favor of the null hypothesis that the coefficient is nil: it therefore makes it harder to detect an electoral effect, positive or negative, in state legislative sessions. More worrisome is another potential problem that, if present, would work *against* the null. Imagine now that sessions 1 and 2 both adjourn 30 October (so are coded as having the same *Election proximity*) but session 1 opened 1 October while session 2 opened 1 January; and that both had one bill passed and vetoed on their opening day with no more vetoes in the remainder. Although

[5]The partisan makeup of sessions, classified by the constitutional override rule, is the following.

	1/2-override		3/5-override		2/3-override		All sessions	
PlainDG	—	—	22	(17%)	365	(34%)	387	(28%)
SuperDG	55	(32%)	8	(6%)	48	(5%)	111	(8%)
Div. Assemb.	8	(5%)	48	(38%)	205	(19%)	261	(19%)
Unif. Gov.	107	(63%)	50	(39%)	449	(42%)	606	(45%)
Total	170	(100%)	128	(100%)	1,067	(100%)	1,365	(100%)

Large majorities are not a Southern phenomenon. The average smallest-of-the-two-chambers majority share for sessions in states not belonging to the old Confederacy was 61% (std. dev. 11%); in those belonging to it, 69% (std. dev. 14%).

[6]In some cases (57 out of 1,365, or 4%) the session continued after the next House election (52 days after on average for these cases, with a standard deviation of 37 days). In these cases I coded the variable as -1 (maximal proximity). The logged version of the variable is really $-\ln(-\textit{Election proximity})$ to avoid indeterminacy while retaining the minus sign.

session 2's veto happened away from the election, it is artificially attributed to a small electoral proximity. The only way I could think of controlling for this measurement error was to code *Election proximity* using dates towards the middle of the session instead of its end date: a significant electoral effect is still detected (see footnote 10).

As previous empirical models, ***Bills passed*** is included in the right side of the equation, the total bills sent to the governor in the session, logged. The more bills the governor is exposed to, the more he/she can conceivably veto. A constant and the six regressors described thus far constitute the basic model ("Model 1") that will be estimated.

Other models, with additional variables on the right side to verify the robustness of the estimates, are also specified. Model 2 includes all the variables in Model 1 plus controls for three possible sources of measurement error (the first two were discussed above): ***Item veto***, a dummy equal to 1 if the governor enjoyed the power to veto parts of a bill while keeping the rest for promulgation; ***Pocket veto***, a dummy equal to 1 in states where the assembly could not override vetoes after adjourning; and ***Special session***, a dummy equal to 1 for special sessions; 0 for regular ones. Special sessions will normally follow regular ones, and so would be closer to the election in expectation. The bargaining environment in some special sessions may also be qualitatively different because they are devoted to must-pass legislation that could not be decided in a regular one.

Model 3 then adds two more controls. Dummy ***Professional assembly*** equals 1 in states that Squire (1997) coded as having professionalized assemblies in 1986–88 (the middle of my time-series). Sessions in professionalized assemblies tend to be longer (148 v. 74 days, on average), hence likelier to end closer to the election, and approximate the characteristics of the US Congress better. Some of the vetoes that are captured by my electoral proximity measure may, in fact, be caused by legislators that are better paid and have more staff to do constituent service. *Professional assembly* would absorb this effect. Dummy ***Economy grew*** equals 1 for sessions ending in a prosperous economic year (when logrolls become easier and, in principle, could reduce inter-branch conflict); 0 otherwise, a control present in most previous work.[7]

Model 4 performs a final robustness verification by including one dummy for each state (excluding Wyoming) the session could have taken place in, along with Model 1's variables. This fixed-state-effects model adopts a skeptic's perspective that there is nothing really systematic about veto incidence, and all the action is

[7]Measuring this variable as the rate of growth of the state's gross product made no difference. I also estimated the model with controls for term-limited governors towards the end of the term, for Southern states, and for sessions in states with dual override rule (see Table 1). None had significant estimates nor affected the estimate of other variables. It was not necessary to control for assembly term limits introduced in many states in the first half of the 1990s because the measure became effective after the end of my time series (see Carey 1996 :10).

attributable to state idiosyncrasies. As we will see, the estimates remain mostly unchanged in all four specifications.

Table 3 reports maximum-likelihood coefficient estimates for all models, their standard errors, and the corresponding p-values.[8] To begin, models perform satisfactorily by two standards reported at the bottom of the table. All clear successfully an overall-goodness-of-fit (Wald) test: there is statistical evidence to reject at the .001 level or better the hypothesis that, with the exception of the constant, all parameter estimates in each model are jointly nil. And there is evidence to reject at the .001 level or better (with a Likelihood-Ratio or LR test) the hypothesis that the dispersion parameter α is zero, in which case the negative binomial distribution of V would collapse into a Poisson — a reassurance that the estimation method is well chosen.

Parameter estimation provides very satisfactory results as well. The estimate for the exposure variable *Bills passed* is positive and significant in all models, as we would expect. All else constant, more bills translate into more vetoes, at a decreasing rate (*Bills passed* is logged in the equation). Estimating the model with this parameter constrained to unit, as textbook event-count models do, leaves the remainder estimates nearly identical to those reported. I begin by discussing Model 1's results. Variable *Plain DG* obtains a positive estimate, significant at the .001 level or better. We conclude that, compared to sessions held under unified government (the baseline), those under plain divided government experienced a significant surge in veto incidence, other variables held constant. The estimate for variable *Divided Assembly*, although positive, is indistinguishable from zero at conventional levels (its p-value is .51): veto incidence is not significantly different from that of unified government when control of the assembly is split between the parties. These results conform to received wisdom on veto incidence.

The estimate for variable *Super DG* is an interesting result. Sessions where government was divided and the majority party was above override level experienced, other things constant, a significantly higher veto incidence than the baseline. The coefficient is half the size of *Plain DG*'s, but remains substantial and is significant at the .02 level.[9] This evidence supports position-taking vetoes. Alternatively, to the extent that both uncertainty and position-taking vetoes occur in politics, the latter seem to predominate state legislative sessions, thereby pulling the coefficient to a positive and significant value.

The last estimate in Model 1, the coefficient for *Election proximity*, is also positive and significant at the .01 level. Because *Election proximity* takes negative values, reaching a maximum at -1, the estimated effect is that, other factors in

[8] I estimated the models with Stata (Stata 2003), requesting robust standard errors to control for the possible presence of heteroskedasticity (Huber 1967; White 1980). Estimating the model without this feature produces standard errors that are systematically lower than those reported in Table 3.

[9] A Wald test of the hypothesis *Plain DG*'s and *Super DG*'s coefficients are equal has a p-value of .056. By the .05 convention, we would not reject this hypothesis.

Variable	MODEL 1 parameter estimate[a] (robust std. error in parentheses)[b]		p-value[c]	MODEL 2 parameter estimate[a] (robust std. error in parentheses)[b]		p-value[c]	MODEL 3 parameter estimate[a] (robust std. error in parentheses)[b]		p-value[c]	MODEL 4 parameter estimate[a] (robust std. error in parentheses)[b]		p-value[c]
Constant	−2.79	(.28)	<.001	−3.17	(.49)	<.001	−3.38	(.46)	<.001	−3.53	(.22)	<.001
Plain DG	.61	(.13)	<.001	.54	(.12)	<.001	.53	(.11)	<.001	.75	(.08)	<.001
Super DG	.31	(.13)	.02	.36	(.13)	.01	.53	(.13)	<.001	.95	(.12)	<.001
Divided assembly	.08	(.11)	.51	.05	(.11)	.69	.02	(.11)	.89	.19	(.10)	.05
ln(*Elect. proximity*)	.08	(.03)	.02	.07	(.03)	.03	.03	(.03)	.29	.03	(.02)	.11
ln(*Bills passed*)	1.00	(.03)	<.001	.97	(.06)	<.001	.97	(.05)	<.001	.92	(.03)	<.001
Item veto	—		—	.72	(.15)	<.001	.44	(.15)	.003	—		—
Pocket veto	—		—	−.23	(.09)	.02	−.15	(.08)	.08	—		—
Special session	—		—	−.12	(.25)	.62	−.06	(.23)	.81	—		—
Professional assembly	—		—	—		—	.58	(.09)	<.001	—		—
Economy grew	—		—	—		—	−.11	(.11)	.30	—		—
State fixed effects	—		—	—		—	—		—	(not reported)		
Wald test of nil paramet.:	1,108		<.001	1,084		<.001	1,124		<.001	4,797		<.001
LR test that $\alpha = 0$:	2.0×10^4		<.001	1.9×10^4		<.001	1.5×10^4		<.001	5.5×10^3		<.001
Log likelihood =	−3,723			−3,699			−3,670			−3,347		
N =	1,365			1,365			1,365			1,365		

Notes: (a) Negative binomial method of estimation. (b) Huber 1967; White 1980. (c) Two-tailed tests.

Table 3: Four models of veto incidence in state governments' legislative sessions, 1979–99

Model 1 held constant, a session ending closer to the election had more vetoes than sessions ending previously. Because this variable enters the equation logged, its effect on veto incidence becomes steeper the closer the session ends to election Tuesday, something I illustrate graphically in the next section of the paper. Despite the limitations in the measure discussed above,[10] this final result is also favorable to the position-taking theory of the veto.

These findings are robust to alternative model specifications. Model 2 confirms the suspicion that aggregate data conceal a fair number of item vetoes among total vetoes. Other things constant, there were more vetoes in sessions where the governor had a line-item veto (a disproportionate number of sessions, 1,215 or 89% of all, were held under such provision) than in those where he/she did not. The effect is substantive, both in magnitude (the positive estimate for *Item veto* is larger than *Plain DG*'s) and in statistical significance. Similarly, sessions in which the governor could pocket-veto experienced significantly fewer vetoes than those where the governor could not. This estimate is consistent with a view that assemblies should avoid sending a governor with such power legislation he/she dislikes on the final days of the session, since they would lose the chance to override if vetoed. It is noteworthy that, under this view, the effect of pocket vetoes should work to counter that of *Election proximity*, yet the size of the latter's estimate only suffers marginally from Model 1 to Model 2, and retains significance at conventional levels. Thirdly, special sessions experience fewer vetoes than regular ones, but the estimate is not at all significant. Estimates for Model 2's controls, especially the first two, come as a reminder of the need to replicate this comparative analysis with disaggregated evidence. But we should not lose from sight the fact that, despite the size and importance of the item and pocket veto effects, and the control for special sessions, distinctive partisan and electoral-proximity effects remain in place and significant (*Super DG*'s estimate even gains about 15% in magnitude). This specification brings confidence that Model 1's results were not driven by measurement error.

We learn from Model 3 how critical assembly professionalization is for inter-branch bargaining. Other things constant, sessions involving professional assemblies manifest significantly more vetoes than those that do not, and the effect is even bigger than that of plain divided government. And including this control swells the effect of *Super DG* by a substantial 47% compared to Model 2 (by 71% compared to Model 1) while diluting the effect of *Election proximity* to less than half and pushing this last estimate to statistical insignificance. The dissolution confirms the

[10] I estimated the models measuring *Election proximity* as the (negative log of the) number of days from day d in the session to the next election (plus 1 to avoid indeterminacy). The estimate reported and discussed in the text takes d to be the last day of the session. If d, however, is taken as the day corresponding to three-fourths session's length (so that longer sessions are measured as being less proximal to the election), the results are very similar to those reported in Model 1, although significance for the estimate drops to .07 (one-tailed). Not too surprisingly, moving d to the middle of the session returns an estimate about 30% smaller in magnitude and no longer significant (its one-tailed p-value is .25).

suspicion, raised earlier, that the electoral and professional-assembly effects occur simultaneously, making it harder to disentangle one from the other. Future research will have to replicate the comparative analysis with disaggregated data in order to isolate the electoral effect (present in the US federal government) attributable to position-taking. A prosperous state economy, on the other hand, appears to reduce veto incidence, as expected, but the estimate remains insignificant.

The fixed-effects specification produces interesting results as well. For lack of space, Table 3 does not report the estimates for 48 state dummies included in Model 4. Three changes, when compared to Model 1, deserve comment. First, and most notable, *Super DG*'s estimate triples in magnitude (jumping from .31 to .95) and increases in statistical significance (from the .04-level to below the .001-level). The effect estimated by this model is now higher than the effect of *Plain DG*. Second, compared to Model 1's, *Divided assembly*'s estimate also experiences a substantial increase in magnitude and even attains conventional significance (a .05 p-value). Yet, compared to the effects of the other two breeds of divided government, this effect is much smaller. Third, *Election proximity*'s estimate again loses both more than half its size and conventional significance. Compared to Model 3, the drop in significance is smaller (a p-value of .11), and many might even argue that it is borderline significant.[11]

All things said, the changes manifested in Models 2, 3, and 4 do not fundamentally alter the inferences that could be made from Model 1's estimates. Results are quite robust. Evidence from state governments in the last two decades of the 20th century is mostly supportive of the findings of work using the US federal government. The results reported in this section therefore serve as a first step towards generalizing our understanding of the veto in systems of separation of power. But it has also brought results, concerning the positive effect of majorities above override level on the veto, that had not been documented systematically before, precisely because the underlying conditions are very rarely met in the US Congress.

4 Interpreting the results

How many vetoes can we expect, on the basis of the estimation, for a session with specific attributes? And how precise is this expectation? To answer these substantively interesting questions, begin by setting four imaginary legislative sessions. In session 1 government is unified (so $UG = 1$, which in the equation means that $Plain\ DG = Super\ DG = Divided\ assembly = 0$); in session 2 government is plain divided ($Plain\ DG = 1$); in session 3 it is divided with majorities above override ($Super\ DG = 1$); while in session 4 the assembly is divided ($DA = 1$). In all other

[11] In defense of significance, we could argue that the .11-level of this test remains quite acceptable: if we infinitely repeated draws of the data with random noise, and always chose to reject the null that the coefficient is zero, we would be wrong only 11 out of every 100 times. This is hardly attributable to chance alone.

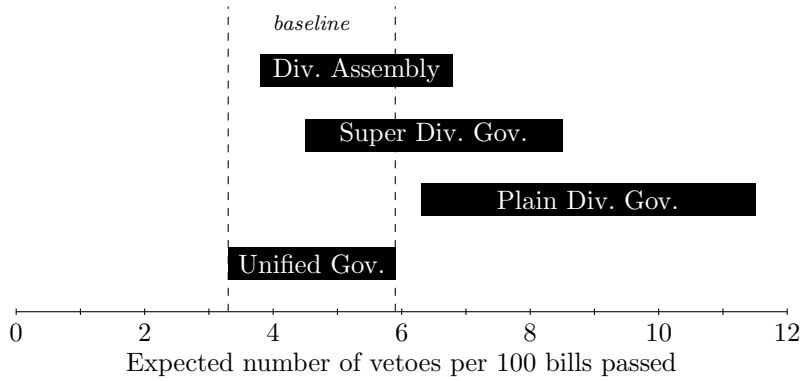

Figure 2: Effects of the partisan makeup of government on expected veto incidence. Horizontal bars cover the 95% CI of the expected veto incidence for sessions held under different partisan configurations. Model 1 was used to produce veto estimates, holding *Election proximity* = −30 and *Billspassed* = 100. Estimates of uncertainty computed with CLARIFY (Tomz, Wittenberg, and King 2001).

respects the four sessions are identical, in order to generate comparative statics predictions on the full range of possible partisan branch makeups. All four sessions are set to end one month ahead of the House election (ie, *Election proximity* = −30) and in all the exposure is fixed at 100 bills (*Bills passed* = 100, which is convenient because expectations will read as percentages). Model 1 is used for this exercise, so these are all the session attributes that need consideration.

Next, predict the expected vetoes for each session using the coefficient estimates reported in Table 3. Expect on average 4.5 vetoes per 100 bills passed in session 1, held under unified government, and more in the other three sessions. Split assembly control between the parties and expect 5.1 per 100 bills on average (session 4). The change is not spectacular when compared with unified government. But expect as many as 8.5 vetoes per 100 bills on average under plain divided government (session 2) and 6.5 per 100 when the opposition has majorities above override in the assembly (session 3). Increases from the baseline are, in the last two cases, substantial (90% more vetoes for session 2; 40% for session 3). And if these imaginary sessions had average legislative productivity in the period (ie, *Bills passed* = 296 instead of 100), the differences would amount, on average, to 12.1 extra vetoes when government is plain divided and 5.6 additional vetoes when government is super divided (above the 13.3 expected for session 1). Partisan effects are large.

Might these differences have arisen by pure chance alone? The algorithms in CLARIFY (Tomz, Wittenberg and King 2001) reveal a negative answer. This procedure creates thousands of replicas of each imaginary session, letting an amount of random noise (proportional to the precision of the estimates) make each of them slightly different from the next. The more concentrated the density around the point prediction, the more confident the prediction; the less the densities for two

counterfactual sessions overlap, the less likely differences arose by chance alone. Figure 2 represents predictions with noise; the confidence interval of unified government, or session 1, extends its limits vertically for reference. The figure shows that divided government does, as a matter of fact, come in different flavors. A governor facing a split assembly vetoes only marginally more than governors with majority control of the assembly. But density estimates, despite being relatively condensed towards the point prediction, overlap so thoroughly with the baseline that it is difficult to distinguish the two veto patterns with any confidence. By sharp contrast, splitting the branches between parties brings clear and distinct increments in veto incidence. Compared with unified government, plain divided government and super divided government sessions experienced noticeable increases in average veto incidence. Differences can be affirmed with more certainty since densities overlap less with the baseline; in the case of plain divided government, a gap is plainly visible. If the pattern for plain divided government conforms well with received wisdom on the veto, that for super divided government is novel, suggesting that assembly supermajorities facing an opposition governor are not associated with less veto incidence by reducing uncertainty. On the contrary, when disputes about legislation arise between the governor and the assembly, giving the majority party enough seats to also be able to override vetoes is accompanied by a more frequent use of the veto. These are, presumably, exercises in position-taking.

[Figure 3 about here]

Figure 3 provides a dynamic picture of the veto-swelling effect of elections—although this should be taken with a grain of salt, given the contagion of the professionalization factor, discussed earlier. It is now the Y-axis that measures expected vetoes per 100 bills passed, comparing a session held under unified government to another held under plain divided government. Circles are point expectations of vetoes per 100 bills for sessions ending as early as 2 years before election Tuesday and as late as 2 weeks before. Vertical bars are 95%-confidence intervals. Three things are noteworthy. First, the figure shows that the accuracy of predictions along the electoral cycle is sufficient (confidence intervals are acceptably narrow) and remarkably stable (confidence intervals have pretty much the same size, only widening slightly for sessions ending very close to the election). Second, a distinctive gap separates predictions for unified government from those for plain divided government, mirroring the previous simulations. Third and most important, an upward-sloping electoral trend is detectable across government partisan makeups. Under plain divided government (the top set of bars) we predict about 7% bills will be vetoed in sessions ending 2 years before the next election, about 7.5% for sessions ending 1 year before election, and about 10% for those ending 2 weeks ahead of the election (all plus or minus 1.5% vetoes). The growth is substantial: for a session with average productivity (296 bills) under plain divided government, we can expect between 6 and 10 additional vetoes on average, attributable to the electoral

cycle from beginning to end. The effect is more modest for a session under unified government (bottom set of bars), but still 2 to 4 extra vetoes are distinguishable from beginning to end of the cycle.

5 Conclusion

There are three lessons of comparative veto politics. (1) Examination of new data confirms the main findings of previous research. Other factors held constant, divided government and nearing elections systematically bring more vetoes in American state governments, as in the United States. (2) A comparative perspective shows that when government is divided, a party commanding enough assembly seats to override the vetoes—a feature rarely observed in the US Congress, but common in state assemblies, as well as in legislatures worldwide – also brings about more vetoes. (3) The veto-swelling effects of the electoral timetable and of veto-proof majorities inform one important debate in the theoretical literature from a fresh empirical perspective: they suggest that, at least in state governments, position-taking is a systematically more important factor driving veto incidence than is uncertainty. A true verification of these lessons calls for a replica of this analysis, by a student of comparative politics, using cross-national evidence. It will most certainly illuminate our understanding of the institution of the veto.

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