On-line Appendix for The veto as electoral stunt

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June 3, 2014

A The formal model

Proof of the stunts game's unique equilibrium is given here. For this purpose, the first node of the game tree in Figure 1 will be referred to as the proposal stage, the second as the veto stage, and the third as the override stage; and the last mode of play, unanalyzed in the text, will be broken into two separate modes parts for analysis here. Four alternative modes of play ensue: the standard ($\pi = 0$), the lexicographic ($0 < \pi < \tau$), the trade-off ($\tau \leq \pi < 1$), and the stunts-only ($\pi = 1$). Assume that whenever $\mathtt{u}_i$ cannot break indifference, player i arbitrarily chooses x_0. Two Definitions, a Lemma, and a Theorem generate the equilibrium and all results in the text.

Definition Let $e_0, v_0 \in [0, 1]$ be the executive and the pivot's respective cutpoints, where

$$e_0 = 2e - x_0 \quad \text{and} \quad v_0 = 2v - x_0. \tag{A.1}$$

Definition Borrowing notation from Cameron (2000), let $\wp_e$ and $\wp_v$ be the executive and the pivot's respective preferred-to sets, where

$$\wp_e = \begin{cases} (x_0, e_0) & if\ x_0 \leq e_0 \\ (e_0, x_0) & if\ x_0 > e_0 \end{cases} \quad \text{and} \quad \wp_v = \begin{cases} (x_0, v_0) & if\ x_0 \leq v_0 \\ (v_0, x_0) & if\ x_0 > v_0. \end{cases} \tag{A.2}$$

Lemma 1 *Player i outcome-prefers policy inside her preferred-to set to the status quo, finds her cutting point outcome-equivalent to the status quo, and outcome-prefers the status quo to policy outside her preferred-to subset. Formally,*

$$\forall\ \omega \in \wp_i, \omega' \notin \wp_i :\ \texttt{Policy}_i(\omega) > \texttt{Policy}_i(x_0) = \texttt{Policy}_i(i_0) > \texttt{Policy}_i(\omega').$$

Proof Consider the executive. Given that, by Eq. 3, $\texttt{Policy}_e$ is single-peaked and symmetric around e; that, by Eq. A.1, e lies at the center of $\wp_e$; and that, by definition, the extremes of $\wp_e$ are outcome-equivalent for the executive, it follows that no point outside $\wp_e$ produces higher outcome-payoff than any point inside $\wp_e$ (all are further away from e, which by Eq. 3 implies a lower outcome-payoff). Because x_0 strictly delimits $\wp_e$, it is closer to e (and by Euclidian utility yields higher outcome-payoff) than any point outside $\wp_e$ except e_0; likewise, any point within the bounds of $\wp_e$ is closer to e (and by Eq. 3 yields higher outcome-payoff) than x_0. Because outcome payoffs are defined in the same way for the legislator and for the pivot, this extends to any player i. ∎

Theorem 1 (The stunts game equilibrium) *Letting x^* be the legislator's optimal proposal, $y^*(x)$ and $z^*(x)$ be the executive and pivot's respective best replies to proposal*

x, and $\epsilon > 0$ an infinitesimal number, the following set of strategies and threshold τ^* define the sub-game perfect Nash equilibrium when $l \leq e$ (the result is symmetric otherwise):

To do: *OJO, la "nueva versión" abajo parece estar mal.*

x v V l e E V v x l e E <– x not in Pv yet accepted V v l x e E V v l E e x

x l v eV E l x v V e E l V v x e E V l v E e x

x l e v EV l x e E v V l E e x v V <– x not in Pv yet accepted E l e V v x

$$
x^* = \begin{cases} l & \textit{if } x_0 < l \textit{ or } \min(e_0, v_0) \leq l \\ & \textit{ or } \{l < x_0 < \min(e_0, v_0) \ \& \ 0 < \pi < \tau\} \\ & \textit{ or } \tau < \pi \leq 1 \\ \min(e_0, v_0) + \epsilon & \textit{if } l < \min(e_0, v_0) \leq x_0 \ \& \ 0 \leq \pi < \tau \\ x_0 & \textit{if } l < x_0 < \min(e_0, v_0) \ \& \ \pi = 0 \end{cases}
$$

$$
y^*(x) = \begin{cases} x \quad (\textit{accept}) & \textit{if } x \in \wp_v \ \& \ \pi = 0 \\ & \textit{or } x \in \wp_e \ \& \ \pi \neq 0 \\ x_0 \quad (\textit{veto}) & \textit{otherwise} \end{cases}
$$

$$
z^*(x) = \begin{cases} x \quad (\textit{override}) & \textit{if } x \in \wp_v \\ x_0 \quad (\textit{sustain}) & \textit{otherwise} \end{cases}
$$

$$
\tau^*(x) = \begin{cases} 0 & \textit{if } l \leq e \leq v \ \& \ \{l < x_0 \leq v \textit{ or } 2v - l \leq x_0 \leq 2e - l\} \\ & \textit{ or } l \leq x_0 \leq e \leq v \\ \frac{2|e - x_0| - \epsilon}{|x_0 - l|} & \textit{if } l \leq e \leq v \ \& \ e \leq x_0 \leq 2e - l \\ \frac{2|v - x_0| - \epsilon}{|x_0 - l|} & \textit{if } l < v < e \ \& \ v \leq x_0 \leq 2v - l \\ 1 & \textit{if } v \leq l \leq e \textit{ or } x_0 \leq l \textit{ or } 2e - l \leq x_0. \end{cases}
$$

Proof Equilibrium is derived by backwards induction. Consider first the case of $\pi = 0$ and only the outcome term of utility needs analysis: $\mathtt{u}_i(\omega \mid a) = \mathtt{Policy}_i(\omega) - \mathtt{Policy}_i(x_0)$. *Override stage.* In light of utility maximization, the pivot's optimal choice follows from Lemma 1: if $x \in \wp_v$ then $\mathtt{Policy}_v(x) > \mathtt{Policy}_v(x_0)$ and she should override the veto to get $\omega = x$; if $x \notin \wp_v$ then $\mathtt{Policy}_v(x) < \mathtt{Policy}_v(x_0)$

and she should sustain the veto to retain x_0. *Veto stage.* The executive's choice also follows from Lemma 1: if $x \in \wp_e$ then $\texttt{Policy}_e(x) > \texttt{Policy}_e(x_0)$ and she should accept the proposal to get $\omega = x$; if $x \notin \wp_e$ then, looking down the game tree with the pivot's eyes—i.e., through $z^*(x)$—she sees two strategic scenarios. When $x \in \wp_v$ the veto is overridden, so actions $a = x_0$ (veto) and $a = x$ (accept) bring about the same outcome $\omega = x$ and the executive is "outcome-indifferent between actions" (or OIA); she arbitrarily vetoes. When $x \notin \wp_v$, the veto is sustained, with preferable outcome x_0; she should again veto.

Proposal stage. With $l \leq e$, three preference profiles are feasible and every $x_0 \in [0, 1]$ in each needs consideration. **(I) Profile** $v \leq l \leq e$. (a) If $x_0 \leq l$ then, by Eq. A.1, $l < e_0$ (because $l \leq e$) and, by Eq. A.2, $l \in \wp_e$. By $y^*(x)$ the executive will accept proposal $x^* = l$ which maximizes $\texttt{PolicyGain}_l$ and the legislator should propose it. (b) If $l < x_0$ then, by Eq. A.1, $v_0 < l$ (because $v \leq l$). So, by Eq. A.2, $l \in \wp_v$. By $y^*(x)$ and $z^*(x)$, the executive vetoes proposal $x* = l$, the pivot overrides, the outcome is $\omega = l$, and she again should propose it. **(II) Profile** $l < v < e$. (a) If $x_0 \leq l$ then, by Eq. A.1, $l < e_0$ (because $l \leq e$) and the case is identical to I.a. (b) With $l < x_0 \leq v$ and Eq. 3, it can be verified that $\texttt{Policy}_l(x_0 + \epsilon) < \texttt{Policy}_l(x_0) < \texttt{Policy}_l(x_0 - \epsilon)$ provided $\epsilon > 0$ is small. Eqs. A.1 and A.2 reveal that proposal $x = x_0 + \epsilon \in \wp_v$ and $y^*(x)$ that the executive would accept it; but it brings $\texttt{PolicyGain}_l < 0$. They also reveal that the desirable proposal $x = x_0 - \epsilon \notin \wp_e \cup \wp_v$; and $y^*(x)$ and $z^*(x)$ reveal that this proposal is vetoed and the veto sustained. So the best the legislator can achieve is to retain x_0 by proposing nothing. (c) With $v < x_0 \leq (2v - l)$ and Eq. A.1 it follows that $l \leq v_0 < v$ and therefore $\texttt{Policy}_l(x_0) = -|x_0 - l| < \texttt{Policy}_l(v_0) = -|v_0 - l|$.

Since $v_0 \notin \wp_e \cup \wp_v$ (because $v < e$) then by $y^*(x)$ and $z^*(x)$ we conclude that $x = v_0$ would trigger a sustained veto. But, provided $\epsilon > 0$ is small, $v_0 + \epsilon \in \wp_v$ and by $z^*(x)$ a veto is overridden with outcome $\omega = v_0 + \epsilon$ and legislator-payoff $\mathtt{u}_l(x = v_0 + \epsilon) = -|v_0 + \epsilon - l| + |x_0 - l| = -\epsilon$ and $\mathtt{PolicyGain}_l(x = v_0 + \epsilon) > 0$. $x^* = v_0 + \epsilon$ should therefore be proposed with the smallest $\epsilon > 0$ possible. (d) If $(2v - l) < x_0$ then, by Eq. A.1 $v_0 < l$ and by Eq. A.2 $l \in \wp_v$. It follows by $z^*(x)$ that any veto is overridden and $x^* = l$ should be proposed. **(III) Profile** $l \leq e \leq v$. (a) If $x_0 \leq l$ then $l \in \wp_e$ and the case is identical to I.a. (b) If $l < x_0 \leq e$ then the case is analytically equivalent to II.b, with the executive in lieu of the pivot, and no proposal should be made. (c) If $e < x_0 \leq (2e - l)$ then the case is like II.c with executive and pivot reversed: the optimal proposal is $x^* = e_0 + \epsilon$. (d) And if $(2e - l) < x_0$ then $l \in \wp_e$ so by $y^*(x)$ the executive accepts $x^* = l$, which should be proposed.

Consider now the case where $\pi = 1$ and $\mathtt{u}_i(\omega \mid a) = \mathtt{Position}_i(a)$. Because $\mathtt{Position}_i(a \in A_i) = -|a - i|$ has the same functional form as $\mathtt{Policy}_i$ and evaluates the same inputs ($A_i = x, x_0$), $\arg\max_{\omega=x,x_0}(\mathtt{Policy}_i(\omega)) = \arg\max_{a=x,x_0}(\mathtt{Position}_i(a))$ and analysis is identical. *Override stage.* By Lemma 1, if $x \in \wp_v$ then $a = x$ (override) is optimal; if $x \notin \wp_v$ then $a = x_0$ (sustain) is optimal. *Veto stage.* If $x \in \wp_e$ then $a = x$ (accept) is optimal; if $x \notin \wp_e$ then $a = x_0$ (veto) is optimal. *Proposal stage.* $x^* = l$ maximizes $\mathtt{Position}_l(a)$, hence this is the optimal proposal (unless $x_0 = l$, in which case, arbitrarily, she proposes nothing).

Consider now the case where $0 < \pi < \tau$. Given the game's structure, player i confronts one of three types of feasible action-outcome pairings: (1) $\omega = a \,\forall\, a \in A_i$; (2) $\omega = x_0 \forall a \in A_i$; and (3) $\omega = x \forall a \in A_i$. (The sequence of play rules out the fourth logical pairing, $\omega = x_0$ if $a = x$ & $\omega = x$ if $a = x_0$.) Note first that when $\omega = a \, \forall \, a \in A_i$, choosing $a = x$ implies $\texttt{Policy}_i(\omega \mid a = x) = \texttt{Policy}_i(x) = \texttt{Position}_i(a = x)$ and choosing $a = x_0$ implies $\texttt{Policy}_i(\omega \mid a = x_0) = \texttt{Policy}_i(x_0) = \texttt{Position}_i(a = x_0)$; and therefore the $\texttt{PolicyGain}$ and $\texttt{Position}$ criteria reinforce the same choice: analysis proceeds identical to $\pi = 0$. Note next that pairings of types (2) and (3) necessarily leave player i OIA—both imply that $\texttt{Policy}_i(\omega \mid a = x) = \texttt{Policy}_i(\omega \mid a = x_0)$—so $\texttt{u}_i$ is determined by $\texttt{Position}_i$ only and therefore players decide as when $\pi = 1$. This simplifies analysis considerably.

Override stage. Unlike other players whose actions can be reversed down the game tree, the pivot's are final, so she may only face type (1) action-outcome pairings. The case of $x = x_0$ is ruled out by treating it as if the legislator had proposed nothing, ending the game. Equilibrium strategy $z^*(x)$ therefore always remains the same as when $\pi = 0$. *Veto stage.* Three cases deserve consideration here. (a) When $x \in \wp_e$ then $\texttt{Policy}_e(x) = \texttt{Position}_e(a = x) > \texttt{Policy}_e(x_0) = \texttt{Position}_e(a = x_0)$; as when $\pi = 0$, she accepts. (b) When $x \notin \wp_e$ & $x \in \wp_v$ $z^*(x)$ indicates an override, leaving the executive in a type (3) pairing: as when $\pi = 1$, she vetoes. And (c) when $x \notin \wp_e$ & $x \notin \wp_v$ $z^*(x)$ indicates a sustained veto, a type (1) pairing: as when $\pi = 0$, she vetoes.

To do: La prueba hasta aquí requiere cambios semánticos menores. Pero del proposal stage hasta el final, requiere menos prosa y más matemática. (11ago2012). Verificar que el cambio en y* en el equilibrio sea consecuente con la prosa (8abr2013).

Proposal stage. Unlike the executive, who becomes OIA or not depending on factors beyond her control (proposal x, x_0, e, and v), the legislator has agency in this matter. In certain game conditions, a certain proposal puts her in OIA (hence behaving as when $\pi = 1$) but others do not (hence behaving as when $\pi = 0$). Whether to make one proposal or the other depends on the actual π—her willingness to concede policy to reach a feasible $\texttt{PolicyGain}$ while sacrificing some $\texttt{Position}$. I consider first the proposal stage when π is infinitesimally small, leaving $\texttt{u}_l(\omega \mid a) = (1 - \pi)\texttt{PolicyGain}_l(\omega) + \pi\texttt{Position}_l(a)$ while also guaranteeing that the outcome term always overshadows the action term; formally

$$\forall\, \texttt{Position}_l(a) : \begin{cases} \texttt{u}_l(\omega \mid a) > 0 & \text{if } \texttt{PolicyGain}_l(\omega) > 0 \\ \texttt{u}_l(\omega \mid a) < 0 & \text{if } \texttt{PolicyGain}_l(\omega) < 0 \\ \texttt{u}_l(\omega \mid a) = \pi\texttt{Position}_l(a) & \text{if } \texttt{PolicyGain}_l(\omega) = 0. \end{cases} \tag{A.3}$$

When Eq. A.3 holds, the legislator is always willing to capture any feasible $\texttt{PolicyGain} > 0$, no matter how small, thus behaving as when $\pi = 0$. He starts behaving as when $\pi = 1$ only when no feasible $\texttt{PolicyGain}_l$ is in sight. From $y^*(x)$ and $z^*(x)$, she knows that this happens whenever $l < x_0 < \min(e, v)$: as when $\pi = 1$, she should propose $x = l$. In all other cases ($v \leq l \leq e$ or $\{(l < v < e$ or $l \leq e \leq v)$ & $(x_0 \leq l$ or $\min(e_0, v_0) \leq x_0)\}$) she should make the same equilibrium proposal as when $\pi = 0$.

Last, τ must be determined: Eq. A.3 holds if, and only if $\pi < \tau$. Therefore $\pi = \tau \longleftrightarrow (1-\pi)|\texttt{PolicyGain}| = \pi|\texttt{Position}|$. By factorizing π it can be established that

$$\tau = \frac{|\texttt{PolicyGain}|}{|\texttt{PolicyGain}| + |\texttt{Position}|.} \tag{A.4}$$

In some conditions π's size is of no importance (any π makes the legislator negotiate as in the standard model, so $\tau = 1$). In others, only when $\pi = 0$ does the legislator behave as in the standard model (so $\tau = 0$). And in others, Eq. A.4 sets the precise threshold for π below which the legislator negotiates as in the standard model, above which she may generate conflict.

Figure 3 summarizes the small-π equilibrium proposals and outcomes uncovered so far in the proof. $\texttt{PolicyGain}_l$ and $\texttt{Position}_l$ do not conflict whenever the game has equilibrium outcome $\omega^* = l$ (ie., when $x_0 \in z_1 \cup z_2 \cup z_3 \cup z_4 \cup z_7 \cup z_8 \cup z_9 \cup z_{12}$). In these cases $\tau = 1$: even a unit π makes the legislator behave as when $\pi = 0$. Neither do they conflict when no proposal with $\texttt{PolicyGain}_l > 0$ is feasible (ie., $x_0 \in z_5 \cup z_{10}$). In these cases $\tau = 0$. They may conflict when the legislator has to make policy concessions to get an outcome (ie., when $x_0 \in z_6 \cup z_{11}$); only $\pi < \tau$ makes the legislator concede. The compromise proposal is $x = \min(e_0, v_0) + \epsilon$. We can compute the legislator's $\texttt{PolicyGain}_l$ and $\texttt{Position}_l$ to establish, with Eq. A.4, that

condition	$\texttt{PolicyGain}_l \geq 0$	$\texttt{Position}_l \leq 0$	τ
$x_0 \in z_6$	$\lvert x_0 - l\rvert - \lvert 2v - x_0 + \epsilon - l\rvert$	$-\lvert 2v - x_0 + \epsilon - l\rvert$	$\frac{2\lvert v - x_0\rvert - \epsilon}{\lvert x_0 - l\rvert}$
	$= 2\lvert v - x_0\rvert - \epsilon$		
$x_0 \in z_{11}$	$\lvert x_0 - l\rvert - \lvert 2e - x_0 + \epsilon - l\rvert$	$-\lvert 2e - x_0 + \epsilon - l\rvert$	$\frac{2\lvert e - x_0\rvert - \epsilon}{\lvert x_0 - l\rvert}$
	$= 2\lvert e - x_0\rvert - \epsilon$		

When $\tau < \pi < 1$ the legislator chooses as when $\pi = 1$. ∎

Corollary 1 *When $\pi = 0$ then $y^*(x^*) \neq x_0 \, \forall \, x^* \in [0, 1]$.*

Proof Set $\pi = 0$ and $l \leq e$, and rely on the reaction functions defined in the Theorem. (1) If $x_0 \leq l$ then $x^* = l$; since $x^* \in \wp_e$ (because $l \leq e$), $y^*(x) = x$ (she accepts). (2) If $\min(e_0, v_0) \leq l < x_0$ then $x^* = l$; since $x^* \in \wp_e \cup \wp_v$ (because $\min(e_0, v_0) \leq l < x_0$), $y^*(x) = x$ (she accepts). (3) If $l < \min(e_0, v_0) \leq x_0$ then $x^* = \min(e_0, v_0) + \epsilon$; since $x^* \in \wp_e \cup \wp_v$ (because $\min(e_0, v_0) \leq x_0$), $y^*(x) = x$ (she accepts). (4) If $l < x_0 < \min(e_0, v_0)$ then $x^* = x_0$, ending the game. By symmetry, all inequalities reverse when $l > e$. In neither of the mutually exclusive and exhaustive cases does the equilibrium path involve a veto. ∎

Corollary 2 (Result 1) *When $l \leq e$ and $0 < \pi < 1$ then $y^*(x^*) = x \longleftrightarrow \{x_0 \leq l$ or $(2e - l < x_0$ & $v < e)$ or $(e < x_0$ & $e \leq v)\}$.*

Corollary 3 (Result 2) *When $l \leq e$ and $0 < \pi < 1$ then $y^*(x^*) = x_0 \longleftrightarrow \{(l < x_0 < (2e - l)$ & $v < e)$ or $(l < x_0 \leq e$ & $e \leq v)\}$.*

Corollary 4 (for Result 3) *When $l \leq e$ and $0 < \pi < 1$ then $y^*(x^*) = x_0$ & $z^*(x^*) = x \longleftrightarrow \{(v \leq x_0 < (2e - l)$ & $l < v < e)$ or $(l < x_0 < (2e - l)$ & $v \leq l)\}$.*

Proof Holding $0 < \pi < 1$ and $l \leq e$, eight cases deserve consideration in light of the Theorem. (1) If $x_0 \leq l$ then $x^* = l \in \wp_e$ so $y^*(x) = x$ (accept). (2) If $2e - l < x_0$ then $x^* = l \in \wp_e$ so $y^*(x) = x$ (accept). (3) If $v \leq l \leq e$ &$l < x_0 \leq (2e - l) \longleftrightarrow v_0 \leq l$ then $x^* = l \in \wp_e$ & $x^* \in \wp_v$ so $y^*(x) = x_0$ (veto) and $z^*(x) = x$ (override). (4) If

$l < v < e$ & $l < x_0 \leq v \longleftrightarrow x_0 \leq v_0$ then $x^* = l \notin \wp_e \cup \wp_v$ so $y^*(x) = x_0$ (veto) and $z^*(x) = x_0$ (sustain). (5) If $l < v < e$ &$v < x_0 \leq (2v - l) \longleftrightarrow l \leq v_0 < v$ then $x^* = v_0 + \epsilon \notin \wp_e$ & $x^* \in \wp_v$ so $y^*(x) = x_0$ (veto) and $z^*(x) = x$ (override). (6) If $l < v < e$ &$(2v - l) < x_0 \leq (2e - l) \longleftrightarrow l \leq v_0 < l$ then $x^* = l \notin \wp_e$ & $x^* \in \wp_v$ so $y^*(x) = x_0$ (veto) and $z^*(x) = x$ (override). (7) If $l \leq e \leq v$ &$l < x_0 \leq e \longleftrightarrow x_0 \leq e_0 < l$ & $x_0 < v_0$ then $x^* = l \notin \wp_e \cup \wp_v$ so $y^*(x) = x_0$ (veto) and $z^*(x) = x_0$ (sustain). (8) If $l \leq e \leq v$ & $e < x_0 \leq (2e - l) \longleftrightarrow l \leq e_0 < x_0$ then $x^* = e_0 + \epsilon \in \wp_e$ so $y^*(x) = x$ (accept).

In cases (1), (2), and (8) $y^*(x) \neq x_0$ (veto); the underlying conditions boil down to those in Corollary 2. In cases (3), (4), (5), (6), and (7) $y^*(x) = x_0$ (veto); the underlying conditions boil down to those in Corollary 3. In cases (4) and (7) $y^*(x) = x_0$ (veto) and $z^*(x) = x_0$ (sustain) while in cases (3), (5), and (6) $y^*(x) = x_0$ (veto) and $z^*(x) = x$ (override); the underlying conditions boil down to those in Corollary 4. Because cases (1–8) are mutually-exclusive and exhaust possible combinations, this proves Corollaries 2, 3, and 4. ∎

B A model of veto incidence in state governments

This methodological appendix develops the empirical models employed in full. Data is described with a defense of the comparative research design proposed. One particular strength is the ability to test empirical implications of the theoretical model. More generally, leaving the federal government to analyze state governments instead furthers the understanding of the executive veto. Data limitations are also elaborated, defending the value of the proposed study. The event count model used for the study of veto incidence is developed at length, estimating alternative equation specifications with alternative methods to demonstrate result robustness. Markov Chain Monte Carlo coefficient estimates are shown to be virtually identical to maximum likelihood versions.

B.1 Data

I observe the legislative process in 49 state governments between 1979 and 1999.[1] The period includes years before and after the the early 1990s recession in state economies (Gramlich 1991). 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 month, but a state's *legislative session*, several of which can occur in a given year. So, for example, in 1979 the Alabama state assembly sent 592 bills to governor Fob James' desk for signature in two sessions. One special session was held from January 18 to 24, in which 10 bills were passed. Then a regular session was held from April 17 to July 30, producing the remainder bills. These sessions appear in the dataset as observations 1 and 2. In total, 1,365 sessions are included in the analysis.

The dependent variable $\texttt{veto.count}_j$—the number of bills vetoed in legislative session j—is the same as in previous aggregate data studies,[2] except that here it is observed in a comparative setting (state governments) and in units (sessions) of

[1] 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.

[2] Magar (2007) reviews U.S. presidential veto studies with aggregate and individual bill data.

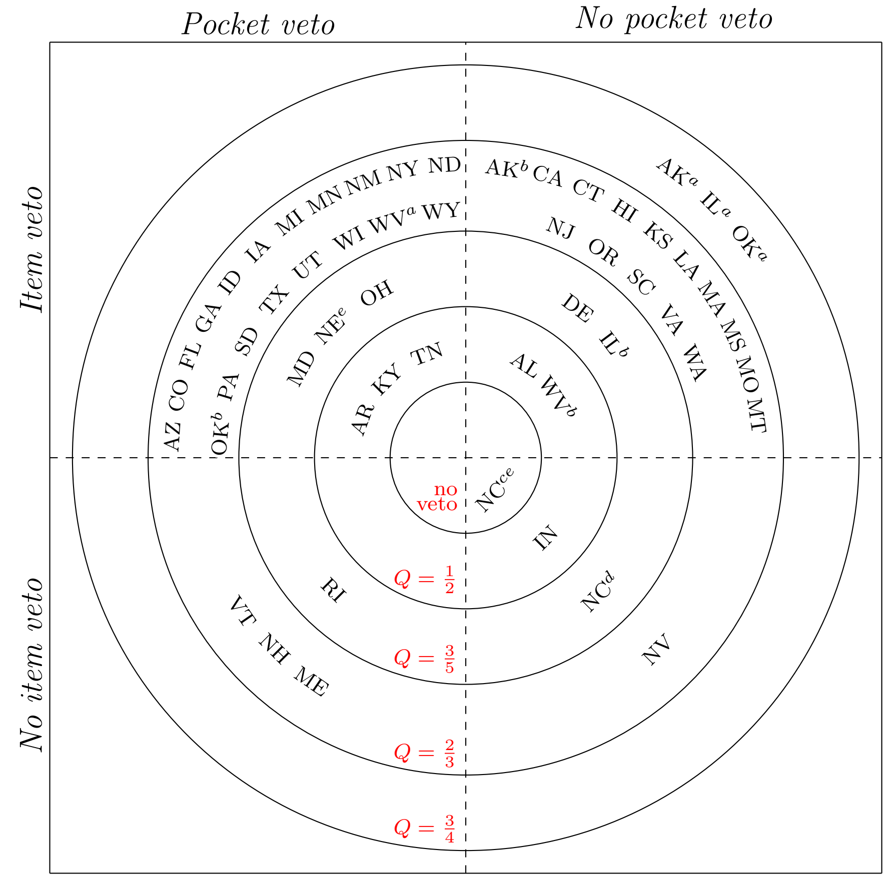

Figure B.1: *Veto institutions in state governments.* Q indicates the vote share needed in the state assembly to override a veto. Super-scripts indicate the following: *a*,*b* are constitutions placing different override requirements on (a) revenue and appropriations bills and (b) other bills (latter used to code these cases); *c* up to 1997; *d* since 1998; *e* dropped from analysis (see footnote 1). Prepared with data from CSG (various years).

unequal length, resulting in different number of observations per state. Average sessions per year in the period ranged from .7 in Nevada to 2.7 in Arizona, with an overall mean of 1.4. Three possible limitations to this measure of veto incidence can be anticipated. First, the source may conflate vetoes of public bills with those of resolutions or private bills. It would be preferable to retain only the first in the analysis. The large number of bills passed in Alabama's 1979 regular session is, in fact, suggestive of such pooling. This limitation should be unimportant to the extent that such legislation 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 $\texttt{veto.count}_j$ 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 Figure B.1), a control for sessions with this feature is included; it should capture some of the artificial effect on veto incidence. And third, $\texttt{veto.count}_j$ possibly also conflates regular and pocket vetoes, which Hoff (1991) has argued should be analyzed separately. The problem of pocket vetoes is handled likewise.

The veto distribution in the period, portrayed in Figure 5, brings to notice the event rarity in state governments. The mode is a session with no veto, peaking at 456 over two decades, one-third of all legislative sessions. The frequency drops abruptly as the veto count rises. Like Shields and Huang (1997), analysis of the limited dependent variable is with negative binomial regression—in data with variance (1,637) two orders of magnitude larger than the mean (16), overdispersion is highly likely (Cameron and Trivedi 1998).

Part 1: Dichotomous variables

Variable name and description	Value frequency 0	Value frequency 1
`Plain.dg` equals 1 for sessions where one party controls a seat majority in both chambers of the state assembly, is below override level in at least one chamber, and does not control the governor's office; 0 otherwise.[a,b]	978	387
`Super.dg` equals 1 for sessions where one party controls a seat majority in both chambers of the state assembly, is at or above override level in both, and it does not control the governor's office; 0 otherwise.[a,b]	1,254	111
`Div.assembly` equals 1 for sessions where the governor's party controls a majority of seats in only one chamber of the state assembly; 0 otherwise.[a,b]	1,104	261
`Super.ug` equals 1 for sessions where one party controls a majority of seats in both chambers of the state assembly, is at or above override level in both, and controls the governor's office; 0 otherwise.[a,b]	1,254	111
`Plain.ug` = 1 − `plain.dg` − `super.dg` − `div.assembly` − `super.ug` (dropped from the equation.)	2,124	606
`Item.veto` equals 1 for sessions where the governor can veto items, retaining the rest of bills for promulgation; 0 otherwise.[a]	150	1,215
`Pocket.veto` equals 1 for sessions where the governor could pocket-veto bills at the end of the session; 0 otherwise.[c]	542	823
`Special.session` equals 1 for special sessions; 0 for regular sessions.[a]	847	518
`Professional.assembly` equals 1 for sessions held in states in Squire's 'professionalized' or 'substantially professionalized' categories; 0 otherwise.[g]	783	582
`Economy.grew` equals 1 for sessions ending in years when the gross state product had a positive rate of growth; 0 otherwise.[d]	356	1,009

Part 2: Non-dichotomous variables

Variable name and description	Mean	SD	Min.	Max.
`Veto.count` is the number of vetoes issued the governor in the session.[a]	15.60	40.45	0	465
`Election.proximity` Counting the day of the House election that immediately followed the session end as –1,[e] subtract 1 for each additional day lapsed until the session end; equals –1 if session ended after the election.[f] The logged version is $-\ln(-$`election.proximity`$)$.[a]	−396.94	257.68	−1,446	−1
`Bills.passed` is the number of bills passed in the session.[a]	295.68	350.14	1	3,128

Sources and notes. (a) CSG (Various issues). (b) Parties in 54 sessions (4%) had exactly half the seats each in one chamber; coded as unified assembly because a majority always existed in the other chamber. (c) State constitutions. (d) SPPQ (2002). (e) When not concurrent (40 sessions or 3% of all), House race always anteceded the Senate's, hence its choice to code this variable. (f) See footnote 4. (g) Equals 1 in NY, MI, CA, MA, PA, OH, AK, IL, CO, MD, HA, WI, FL, NJ, AZ, OK, CT, WA, and IA (Squire 1997:422).

Table B.1: Variable definitions, descriptives, and sources

B.2 The right side

Definitions and summary statistics for regressand and regressors in the models are listed in the Table B.1. Included in the right side of the equation are indicators to differenciate three breeds of divided and two of unified government, based on electoral stunts theory's interaction of the party status of government and the assembly majority's veto override status. Dummies `super.dg` and `plain.dg` control two types of divided government. The first equals 1 when a party other than the governor's controls the unified assembly and is above override level—i.e., capable of overriding vetoes with its votes alone. The other equals 1 when that party is below override level. Dummies `super.ug` and `plain.ug` achieve the same dual controls when the governor's party enjoys majority status in the assembly. And dummy `div.assembly` controls divided assembly sessions, equal to 1 when different parties have majority status in the chambers of bicameral assemblies. The sum of the five dummies being one, `plain.ug` is dropped from the equation to serve as reference category for coefficient interpretation.

Fine-grained government status classification is redundant in studies of the federal government. With a two-thirds veto override requirement and relatively competitive national parties, majorities above override level in the U.S. Congress have been rare. Owing to state party system and state institution variation, they are common subnationally. State parties frequently win majorities of remarkable size. The average seat share held by the smallest-of-the-two-chambers majority in the period is 63 percent (with a standard deviation of 12). At 66, the 1979–1996 mean size of hegemonic

PRI's contingent in Mexico's lower chamber was very close. As seen in Table 3, parties frequently command two-thirds majorities in both chambers of the state assembly. In more than one thousand sessions held under the same override rule of Congress, 24 percent had parties above override level. For contrast, just 3 percent of U.S. Congresses have had this feature since 1945, none since 1968. Large majorities are not a Southern phenomenon: the average smallest-of-the-two-chambers majority seat share for sessions in states not belonging to the old Confederacy was 61 percent (std. dev. 11), versus 69 percent (std. dev. 14) for those in it. State institutional differences, seen in Figure B.1, further swell observations of above-override-level parties to 34 percent of sessions in the period—there are states, like Alabama, where veto override is by majority rule and the party in control of the assembly is always above override level.

Comparative veto politics therefore offers variance to examine the effect of institutions and parties on the veto and test differing predictions of uncertainty and position-taking. While the coefficient of variable `plain.dg` should always be positive (more vetoes than in plain unified government ceteris paribus), that of `super.dg` depends on the veto theory. If position-taking vetoes are predominant in state legislative sessions, a positive coefficient for `super.dg` is expected; if the uncertainty logic predominates, the coefficient should be nil (indicating no difference in veto incidence with unified government sessions).[3]

[3] A negative estimate might even be expected. A significant number of states with supermajorities will have little real party competition, where parties tend to be factious, fractionated, and weaker. Wright and Schaffner (2002) can be read 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.

The veto theories also hold opposed expectations regarding the next variable, `election.proximity`. This regressor, 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 state House election. So for a session ending on election Tuesday, it equals -1; for a session ending the Monday before, it equals -2; and so forth.[4] The uncertainty veto theory 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 (i.e., 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 Oc-

[4] 60 sessions (4.4 percent) continued beyond the next House election, ending 52 days afterwards on average with a standard deviation of 37 days. Twice in Wisconsin the assembly remained in session for nearly two years with an election somewhere in the middle. 38 cases had a less than 100 but more than 50 days lapse between the ballot and the cloture of the session, while 20 had a 50 or less days lapse. The variable is coded as -1 (maximal proximity) for these cases. The logged version is $-\ln($`election.proximity`$)$ to avoid indeterminacy while retaining the minus sign.

tober (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 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 4).

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 regressors 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 has two extra controls. Dummy `professional.assembly` equals 1 in states that Squire (1997) coded as having professionalized assemblies in 1986–88 (the middle of the 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 U.S. 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` should absorb this effect. Dummy `economy.grew` equals 1 for sessions ending in a prosperous economic year (when budget constraints are softer and logrolls become easier, reducing inter-branch conflict in principle), a control present in most previous work.[5]

Model 4 performs a final robustness verification by including state fixed effects (one dummy for each state excluding Wyoming) along with Model 1's regressors. This specification adopts a skeptic's perspective that there is nothing really systematic about veto incidence, and all the action is attributable to state idiosyncrasies. As will be seen, the estimates remain mostly unchanged in all four specifications.

Veto determinants will estimated with both Maximum Likelihood and with Markov Chain Monte Carlo methods.[6] ML has the appeal of convention, supporting classic

[5]Measuring this variable as the rate of growth of the state's gross product made no difference. The model was also estimated 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 Figure B.1). None had significant coefficients nor affected the estimate of other variables'. Controlling for term limits introduced in many states in the first half of the 1990s was unnecessary because they became effective after the end of the time series (see Carey 1996:10).

[6]R (R Dev. Core Team 2011) and jags (Plummer 2003) used for estimation and post-estimation analysis, relying on packages MASS (Venables and Ripley 2002), lmtest (Zeileis and Hothorn 2002), and R2jags (Su and Yajima 2012).

	MODEL 1			MODEL 2			MODEL 3			MODEL 4		
	$\hat{\beta}$	std. error[a]	p-value[b]	$\hat{\beta}$	std. error[a]	p-value[b]	$\hat{\beta}$	std. error[a]	p-value[b]	$\hat{\beta}$	std. error[a]	p-value[b]
Constant	−2.83	(.14)	<.001	−3.20	(.24)	<.001	−3.41	(.24)	<.001	−3.52	(.21)	<.001
`plain.dg`	.64	(.08)	<.001	.63	(.08)	<.001	.58	(.08)	<.001	.74	(.08)	<.001
`super.dg`	.34	(.11)	.003	.46	(.12)	<.001	.58	(.12)	<.001	.94	(.16)	<.001
`super.ug`	.05	(.08)	.530	.16	(.08)	.056	.08	(.09)	.330	−.02	(.11)	.870
`div.assembly`	.11	(.09)	.220	.14	(.09)	.110	.06	(.09)	.480	.18	(.10)	.059
ln(`election.proximity`)	.08	(.02)	<.001	.07	(.02)	.001	.03	(.02)	.130	.03	(.02)	.160
ln(`bills.passed`)	1.00	(.01)	<.001	.96	(.03)	<.001	.96	(.03)	<.001	.92	(.01)	<.001
`item.veto`	—		—	.73	(.09)	<.001	.45	(.09)	<.001	—		—
`pocket.veto`	—		—	−.25	(.06)	<.001	−.15	(.06)	.006	—		—
`special.session`	—		—	−.16	(.14)	.250	−.08	(.15)	.590	—		—
`professional.assembly`	—		—	—		—	.58	(.06)	<.001	—		—
`economy.grew`	—		—	—		—	−.10	(.06)	.094	—		—
State fixed effects	—		—	—		—	—		—		(not reported)	
LR test of nil coefficients:		1,446	<.001		1,496	<.001		1,553	<.001		2,199	<.001
LR test that $\alpha = 0$		19,600	<.001		18,660	<.001		14,969	<.001		5,535	<.001
Log likelihood		−3,724			−3,699			−3,670			−3,347	
N		1,365			1,365			1,365			1,365	

Notes: (a) Robust standard errors (Huber 1967; White 1980). (b) Two-tailed tests.

Table B.2: Four models of veto incidence in state governments' legislative sessions, 1979–99. Maximum-likelihood negative binomial method of estimation.

hypothesis tests. And MCMC has power to gauge the conjugate effect of regression coefficients by simulation, predict quantities of interest with measures of precision, and mitigate methodological complications associated with panel data (see Gelman and Hill 2007). Both approaches yield similar results. Table B.2 reports ML results for all models. Coefficient estimates, their standard errors, and the corresponding p-values.[7]

Models perform satisfactorily by three standards. All clear successfully an overall-goodness-of-fit likelihood ratio test (reported at the bottom of the table). There is statistical evidence to reject at the .001 level or better the hypothesis that, the constant excepted, coefficient estimates in each model are jointly nil. There is also evidence to reject at the .001 level or better the hypothesis that the dispersion parameter α is zero, in which case the negative binomial distribution of `veto.count` would collapse into a Poisson—a reassurance that the estimation method is appropriate. And the coefficient estimate for the exposure variable `bills.passed` is positive and significant in all models, as was expected. All else constant, more bills translate into more vetoes, at a decreasing rate (`bills.passed` is logged in the equation). Estimating models with this parameter constrained to unit, as textbook event-count models do, leaves the remainder estimates nearly identical to those reported.

Coefficient estimates are is also very satisfactory. Model 1 results are discussed first. Variable `plain.dg` gets a positive estimate, significant at the .001 level or better. So compared to baseline sessions held under plain unified government, those

[7] Robust standard errors were computed 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.

under plain divided government experienced a significant surge in veto incidence, other regressors in the model held constant. And although positive, the estimate for `div.assembly` is statistically indistinguishable from zero at conventional levels (its p-value is .22): veto incidence is not significantly different from that of unified government when control of the assembly is split between the parties. Both results conform to received wisdom on the veto.

The result for `super.dg` is noteworthy. 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 .003 level. This evidence supports position-taking vetoes. To the extent that both uncertainty and position-taking vetoes occur in politics, the latter seem to predominate systematically in state legislative sessions, pulling the coefficient to a positive and significant value. The same cannot be said of `super.ug` in the basic model. The coefficient is seven times smaller than `super.dg`'s, but the .53 p-value makes it indistinguishable from zero.

Like other Model 1 variable coefficients, `election.proximity`'s is positive. It is also significant at the .001 level or better. Because `election.proximity` takes negative values, reaching a maximum at -1, the estimated effect is that, other factors constant, a session ending closer to the election had more vetoes than sessions ending previously. Because it enters the equation logged, the effect on veto incidence becomes steeper the closer the session ends to election Tuesday, something that the next section of the paper illustrates graphically. Despite the limitations in the measure discussed above,[8] this final result is also favorable to the position-taking theory of the veto.

The findings are robust to alternative model specifications. Model 2 confirms the suspicion that aggregate data conceal a fair number of item vetoes among full vetoes. Other things constant, there were more vetoes in sessions where the governor had line-item veto power (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 had pocket veto experienced significantly fewer vetoes than otherwise. This estimate is consistent with a view that assemblies should avoid sending legislation that a governor with such power dislikes on the final days of the session. They lose the chance to override if vetoed. It is noteworthy that, under such view, the pocket veto effect works counter `election.proximity`'s, yet the latter's estimate size only suffers marginally from Model 1 to Model 2, and retains significance at conventional levels. And special sessions experience fewer vetoes than regular ones, but the estimate is insignificant. 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 must not lose from sight that, despite the size and importance of the item and pocket veto effects, distinctive partisan and electoral-proximity effects remain in place and

[8] RE DO: 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).

above-override-level indicators gain in size and significance. `Super.dg` and `super.ug`'s coefficients gain one-third and three times in size, respectively, compared to Model 1, and the latter achieves a .056 p-value—barely missing conventional significance. Controls yield improved coefficient estimates while bringing confidence that Model 1's results were not driven by measurement error.

Model 3 reveals how critical legislative professionalization is for inter-branch bargaining. Other things constant, sessions in assemblies with compensation, staff size, and days in session closer to U.S. Congress levels manifest significantly more vetoes than those further behind, and the effect is as large as `plain.dg`'s. And including this control swells `super.dg`'s effect by another quarter compared to Model 2 (by 70 percent compared to Model 1) while diluting the size and significance of `super.ug` and `election.proximity` to about half and pushing this last estimate to statistical insignificance. The dissolution confirms the suspicion, raised earlier, that the electoral and professional-assembly effects occur simultaneously, making it harder to disentangle one from the other.[9] Another call for comparative analysis replicas with disaggregated data. A prosperous state economy, on the other hand, shrinks veto incidence, as expected, but the estimate remains insignificant.

The fixed-effects specification is also informative. For lack of space, Table B.2 does not report estimates for 48 state dummies in Model 4. Three changes, when compared to Model 1, deserve comment. Most notable, `super.dg`'s estimate triples in magnitude (jumping from .34 to .94) with more statistical significance. The effect estimated by

[9]A specification was estimated interacting the professionalism and government status dummies. The only government status category experiencing a near significant hike (at the .11-level) in professional v. unprofessional assemblies is super unified government. Pooling all `super.ug` = 1 sessions together blurs this effect.

this model is now higher than `plain.dg`'s. Compared to Model 1's, `div.assembly`'s estimate also experiences a substantial increase in magnitude and is nearly significant at conventional level (a .059 p-value). The effect remains small compared to other two breeds of divided government. And `super.ug` and *ElectionProximity*'s estimates again lose size and significance (the latter is even negative, though indistiguishable from zero). Compared to Model 3, the drop in significance is bigger.

All things said, changes manifested by Models 2, 3, and 4 are small and do not fundamentally affect 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 on the U.S. 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 Congress.

Table B.3 reports inferences for the MCMC estimaton of model 2. The model performs well by three standards. Sampled sequences for the model's parameters have reached a steady stage after hundreds of thousands of iterations of the algorythm, as indicated by all $\hat{R} \approx 1$ in the Table. Sampling only every 50th element of the final 10 thousand iterations removed most autocorrelation in the sampled values—an effective sample size of 270 for `plain.dg`'s coefficient is farthest from 600, yet large. And with a 2.5-percentile of the posterior sample at 1.05, it is safe to conclude that parameter σ_ϵ is not zero, indicative of overdispersion in the count data. Inferences drawn from

	Posterior sample								
Coefficient	mean	std.dev	2.5%	25%	50%	75%	97.5%	$\hat{R}$	n.eff
Constant	-4.00	0.36	-4.72	-4.25	-3.99	-3.75	-3.32	1.00	600
ln(`bills.passed`)	.97	0.05	0.88	0.94	0.97	1.00	1.07	1.00	600
`plain.dg`	.77	0.11	0.56	0.70	0.77	0.85	0.99	1.06	270
`super.dg`	.59	0.15	0.31	0.49	0.59	0.70	0.86	1.00	600
`super.ug`	.20	0.12	-0.02	0.12	0.20	0.28	0.43	1.00	600
`div.assembly`	.25	0.13	0.01	0.16	0.25	0.33	0.49	1.00	600
ln(`election.proximity`)	.07	0.03	0.02	0.05	0.07	0.08	0.12	1.00	510
`item.veto`	.74	0.12	0.52	0.67	0.74	0.82	0.98	1.01	600
`pocket.veto`	-.18	0.08	-0.34	-0.23	-0.18	-0.13	-0.03	1.00	600
`special.session`	-.23	0.20	-0.60	-0.36	-0.23	-0.10	0.16	1.00	600
σ_ϵ	1.10	0.03	1.05	1.08	1.10	1.12	1.16	1.00	600
Deviance	4892.21	45.94	4806.09	4860.46	4890.30	4920.46	4987.25	1.00	600

Table B.3: Inferences for model 2 estimated with MCMC. Three chains were updated 350 thousand times each, keeping every 50th instance of the final 10 thousand as posterior sample.

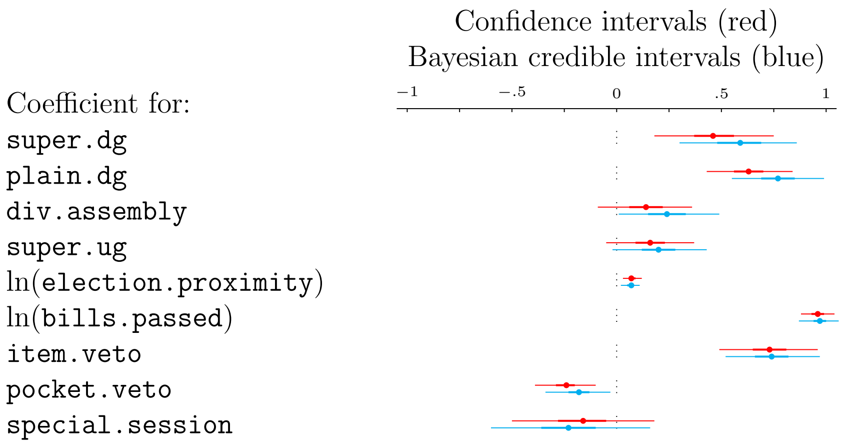

Figure B.2: Comparing coefficient estimates and precision for alternative estimation methods of model 2. Charts report the point prediction, the 50%, and the 95% confidence intervals of ML model in red; and the median, 50%, and 95% and intervals of MCMC model's posterior sample in blue.

the MCMC estimation, as Figure B.2 reveals, are remarkably close to those made with the ML estimation. The largest difference involves `plain.dg`'s 50%-confidence interval not overlapping with the MCMC posterior interquartile range. The more standard 95% versions, however, overlap to such extent that any difference is highly likely to be the product of chance alone. It is safe to rely on the MCMC results to interpret regression results, as the main text does.

To do: CONCLUDING PARAGRAPH: Hypothesis testing follows directly from Model 2—parsimonious, it has controls for likely sources of measurement error in aggregate data. Simulations done with Model 6's posteriors. All done in the text.

C Code for MCMC estimation of model 2

```
### PREPARE DATA FOR BUGS RUN
vetoed <- d$vetoed
X <- as.matrix(cbind (d$constant, d$lnb, d$dgua, d$dgsua, d$dgda, d$lnelprx1, d$sug,
                      d$item, d$pocket, d$special))                          ## MODEL 2
vars <- c("const", "lnb", "dgua", "dgsua", "dgda", "lnelprx1", "sug",
```

```
          "item", "pocket", "special")                                        ## MODEL 2
colnames(X) <- vars
K <- ncol(X); N <- nrow(X)

############### OVERDISPERSED POISSON REGRESSION (cf Gelman+Hill p. 325, 382) #################
cat("
model {
  for (n in 1:N){                                         # loop over observations
      vetoed[n] ~ dpois(mu.v[n]);                         # stochastic component
      log(mu.v[n]) <- inprod(beta[],X[n,]) + epsilon[n]; # PREPARE X IN R
      epsilon[n] ~ dnorm(0, tau.epsilon);
                }
  ## Non-informative priors
  for (k in 1:K){
      beta[k] ~ dnorm(0, .00001);
                }
  tau.epsilon <- pow(sigma.epsilon, -2); # sigma.eps measures amount of overdispersion (if 0 then classical poisson)
  sigma.epsilon ~ dunif(0,100);
}
", file="overdispPoisson.txt")

## OVERDISPERSED POISSON MODEL PREP
veto.data <- list ("vetoed", "X", "N", "K")
veto.inits <- function (){
    list (
    beta=rnorm(K),
    sigma.epsilon=runif(1)
    )
    }
veto.parameters <- c("beta", "sigma.epsilon")
mod <- "overdispPoisson.txt"

start.time <- proc.time()
results <- jags.parallel (veto.data, veto.inits, veto.parameters,
                mod, n.chains=3,
                n.iter=350000, n.thin=50, n.burnin=340000)
time.elapsed <- round(((proc.time()-start.time)[3])/60,2); rm(start.time)
cat("\tTime elapsed in estimation:",time.elapsed,"minutes","\n")

results$BUGSoutput$summary
traceplot(results)
```

PASTE OUTPUT LOG HERE ML

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