



SHORT ARTICLE

The Electoral Dynamics of Capital Punishment Commutations

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Abstract

We explore electoral explanations for U.S. governors' willingness to commute death sentences in their state. Across descriptive tests and pre-registered regression specifications, we find little evidence that election timing or term limits affect either the probability of commuting death sentences or the proportion of such sentences governors might commute. However, we do find evidence that governors are more likely to commute sentences – and commute sentences for a higher proportion of defendants – during the “lame duck” period after their successor's election but before their inauguration.

Keywords: governors; executive elections; capital punishment; criminal justice; commutations

Introduction

The United States is the only Western nation that applies the death penalty regularly, which perhaps is unsurprising given the uniquely punitive character of the American criminal justice system (Enns 2016; Weaver 2007). Available public opinion research suggests that as of late 2021, a majority of Americans still approved of the death penalty (see [Supplementary Figure A1](#)). Thus, consistent with recent work suggesting that the continuation of punitive policies is largely driven by mass opinion (Enns 2014; 2016), politicians may maintain a harsh position on capital punishment because they fear straying too far from voters' wishes.

Governors' positions on capital punishment may become manifest in several ways, from the signing or vetoing of legislative bills (Ricknell 2021) to the issuing of executive orders. However, given that commutations are often unilateral and always irreversible, they are a unique form of gubernatorial power worth further study. Several American governors have authority to commute capital sentences in their states.¹ Given public

¹[Supplementary Table A1](#) summarizes procedures for granting clemency in the states.

sentiment surrounding the death penalty, governors may be reluctant to commute capital sentences if doing so risks them being branded as “soft on crime” and invites political consequences. Thus, we should expect political costs to factor into governors’ decisions of whether to commute death sentences. In particular, we note that governors may not be equally likely to grant commutations at every point in their term, and may be especially likely to commute sentences at a politically opportune time. It is therefore worthwhile to consider the extent to which electoral considerations shape governors’ commutation decisions. In this note, we test three pre-registered hypotheses that examine the link between electoral conditions and governors’ likelihood of commuting death sentences in their states. We employ data comprising the entire universe of incarcerated persons under a sentence of death in the United States from 1973 to 2019, linking each to their respective governors. Using fixed effects models which account for unobserved variation across governors and over time, we test how political pressure stemming from elections affects commutation patterns. Across these models and descriptive results, we find little evidence that election timing or term limits affect either the probability of commuting death sentences or the number of such sentences governors might commute. We do, however, find a positive association between both of these outcomes and “lame duck” status, indicating that governors are more likely to commute death sentences – and commute higher proportions of sentences – in their waning days of office when electoral costs are most remote. These results suggest that governors do factor political costs into commutation decisions.

Our results contribute to the literature in at least two key areas. First, previous work has found suggestive evidence that governors weigh political factors in their approach to clemency generally (Gunderson 2022) and that elites in all three branches of state government consider political factors when adjudicating matters related to capital punishment (Brace and Hall 1997; Kubik and Moran 2003; Mooney and Lee 2000; Ricknell 2021). This note therefore contributes to growing research not only on how governors approach commutation decisions, but how elites in state government discharge their duties when it comes to capital punishment. By extension, we offer new evidence for whether political considerations influence capital sentence commutation, which has implications for assessing whether the death penalty is equitably administered.

This note also contributes to a growing literature examining the interplay between mass preferences, elite behavior, and various outcomes in the criminal justice system. Previous work has examined how public opinion affects not only the dynamics of capital punishment (e.g., Peffley and Hurwitz 2007) but also factors such as sentence length (Doherty *et al.* 2022) and mass incarceration (Enns 2016). However, other research has also found that politicians have sought to shape public opinion about crime so that they can capitalize by taking punitive positions (Beckett 1997). In examining the considerations that governors make when deciding whether to commute a capital sentence, this note therefore sheds further light on how elites might take mass preferences into account when acting within the criminal justice system.

Expectations

We posit that governors’ perceived costs of clemency actions are likely to vary across several circumstances related to the timing of elections, following extensive research that incumbent politicians adapt their behavior in response to electoral incentives

(e.g., Downs 1957; Przeworski, Stokes, and Manin 1999). First, governors are likely to feel that opponents could exploit a high-profile commutation of a capital sentence to cast aspersions on their ability to fight crime. If they are running for re-election, governors are likely to perceive the political costs of commutations as increasingly acute as the proximity of an upcoming election date nears. Accordingly, we anticipate that:

Hypothesis 1: If the incumbent governor is running for re-election, they are less likely to commute sentences in months closer to the election.

On the other hand, it follows that incumbents who *cannot* run for re-election need not be as concerned with a tough-on-crime public image because the election pressures driving Hypothesis 1 are not present. Hence:

Hypothesis 2: Relative to months a governor is not term-limited, commutations are more likely in months when a governor is term-limited.

Finally, election pressures are all but eliminated during the “lame duck” period – the time between the election of the subsequent governor and the inauguration of the subsequent governor while the current governor is still in power. We therefore hypothesize that:

Hypothesis 3: Governors are more likely to commute death sentences in the months that comprise their “lame duck” period, after their successor has been elected but before that successor has been inaugurated.

Data

We obtained data on capital sentence commutations from “Capital Punishment in the United States, 1973–2019 (ICPSR 37998)” (hereafter CPUS), compiled by the US Department of Justice at the defendant-year level. Access to these data is restricted; we pre-registered our analysis before obtaining them. The CPUS data include information about prisoners who are incarcerated under a death sentence in each year, and the month and year when their sentence ended due to death, commutation, or the removal of a sentence by a State Supreme Court or Appellate Court. The CPUS data correspond to 8,030 defendants, of which 348 received commutations.

We merged the CPUS data with original data containing information about the relevant governor during the period of a defendant’s incarceration, including the dates of the following election, the beginning of the period where the governor is term-limited, and the beginning of the governor’s lame duck period. These dates subsequently drive the coding of our independent variables.

To protect the privacy of individual defendants, we aggregate the data to the governor year-month level, so an individual governor is represented in multiple rows corresponding to the number of months the governor served. Following our pre-analysis plan, we examine only potential commutations in *state* cases, and retain data only from state-years where a governor had the authority to commute capital sentences. The first outcome measure is the proportion of defendants under a death sentence in a state whose sentences were commuted in a given month. Normalizing by the total number of defendants ensures that the results are not driven by governors having different numbers of possible defendants in their states across time. We also construct a binary outcome, taking the value of 1 if a governor commuted at least one

sentence in that month and 0 otherwise – conditional on there being at least one defendant whose sentence could be commuted in that month.

The CPUS data allow us to avoid misclassifying defendants whose sentence was overturned in a court action and to exclude defendants with mandated re-sentencing. Moreover, we exclude state-years where an independent advisory board has sole authority over commutation decisions, as described in [Supplementary Table A1](#). We exclude all these cases because they do not reflect agency from the governor conducting the commutation.²

After filtering the data, 438 governor-terms that correspond to 266 governors remain. Of these 438 governor-terms, 362 governor-terms (corresponding to 224 unique governors) oversaw at least one defendant on death row whose sentence could be commuted. Commutation itself is relatively rare: Only 74 governor-terms commuted at least one sentence. The mean proportion of defendants whose sentences were commuted per governor-term is approximately 2%, while the median is 0.

Empirics

We present two sets of results to test Hypotheses 1–3. We begin by presenting descriptive statistics that aggregate together governors for particular months with reference to the key predictor of interest. For example, for Hypothesis 1, we group by months leading up to the following election for governors who run for re-election. Within each month, we calculate the mean value of all the governors' commuting behavior in that particular month. Analogously, we group by months to the point where the governor is term-limited for Hypothesis 2, and we group by months to the point where the governor is a lame duck for Hypothesis 3.

In addition to the descriptive figures, we employ a two-way fixed effects approach (Imai and Kim 2019) to address several key empirical challenges. First, we rely on governor fixed effects to separate the effects of differing individual preferences or governor characteristics from election pressures; this allows us to identify variation in commuting practices among the same governor at different points of the election cycle. Second, we include year-month fixed effects, which guard against confounding from national-level trends in commutation practices over time and seasonal trends in commutation that are constant across years simultaneously. Our most general specification can be written as follows:

$$commutations_{iym} = \beta politics + \alpha_i + \gamma_{ym} + \epsilon_{iym}, \quad (1)$$

where $commutations_{iym}$ is the number of commutations made by governor i in year y and month m divided by the total number of defendants on death row in governor i 's state in year y and month m . As noted, we also present results for an outcome corresponding to the presence of at least one commutation in the governor-year-month unit. Governor and year-month fixed effects are indicated by α_i and γ_{ym} , respectively. Following Abadie *et al.* (2017), we cluster robust standard errors at the governor level – the level of “treatment” assignment. Our main regression estimates use OLS because of the incidental parameter problem for nonlinear models with

² Additionally, tests of Hypothesis 1 include only governors who are running for re-election, while tests of Hypothesis 2 exclude the lame duck period to test only the term-limited period.

many fixed effects (Neyman and Scott 1948), since fixed effects are necessary for identification.

This approach accounts for any additive unmeasured time-invariant confounders and is the standard for generalized treatment regimes in longitudinal data, even though it makes parametric identification and modeling assumptions.³ The [Supplementary Material](#) presents a variety of robustness checks for the regression specifications.

The coefficient of interest in each model is β . Its associated variable(s) are different for each hypothesis in the list. For Hypothesis 1, we operationalize the variable associated with β as the number of months between the current month and the nearest election.⁴ We expect the associated coefficients to be positive: The greater the distance to the next election for the incumbent, the greater the likelihood of commutation(s). For Hypothesis 2, we can only compare governors who at one point were eligible for re-election and at another were not, for example, the first term of a governor and the second term of that same governor when the state has a two-term limit. In this case, the variable associated with β will be equal to 1 for the months following the point at which the governor is ineligible to run for re-election,⁵ and 0 otherwise. We expect the coefficients to be positive. For Hypothesis 3, the variable associated with the coefficient of interest will be equal to 1 during the months of the lame duck period and 0 otherwise. We expect the coefficients to be positive here as well.

Results

[Figure 1](#) presents descriptive results, with rows corresponding to each hypothesis described above. Each column reflects one of the two separate outcomes. The left column presents the mean proportion of commutations aggregated across all governors for the particular month to the relevant event for that hypothesis. The right column represents the binary outcome, and so describes the proportion of governors who commuted at least one sentence in a given month.

In the top panel of [Figure 1](#), the LOESS (locally estimated scatterplot smoothing) curve provides little evidence that governors running for re-election commute capital sentences when the next election is more distant. In the middle panel, we separately fit a LOESS curve at each side of the zero mark, which denotes the point where a governor becomes term-limited. Negative values along the x -axis describe term-limited governor-months, while positive values describe non-term-limited governors. Here again, no clear pattern emerges to suggest that term-limited governors are more likely to commute sentences than non-term-limited governors.⁶

Finally, the bottom panel describes commutation patterns from the beginning of a governor's term to the beginning of their lame duck period. Given the limited number

³This framework also assumes that past outcomes cannot affect the current treatment. Since electoral calendars are fixed, and incumbent governors are unlikely to make re-election decisions solely and entirely based on their commutation history, this assumption is tenable.

⁴We only include governors who run for re-election in tests of Hypothesis 1.

⁵We cannot identify effects for a single-term governor who does not run for re-election.

⁶In this middle panel, we re-code all governor-months (for four total governors) in excess of 48 months to becoming term-limited, truncating to 48. See [Supplementary Figure A7](#) for an unedited figure. Unreported results that present these plots where governors are aggregated by quarter instead of by month show similar patterns.

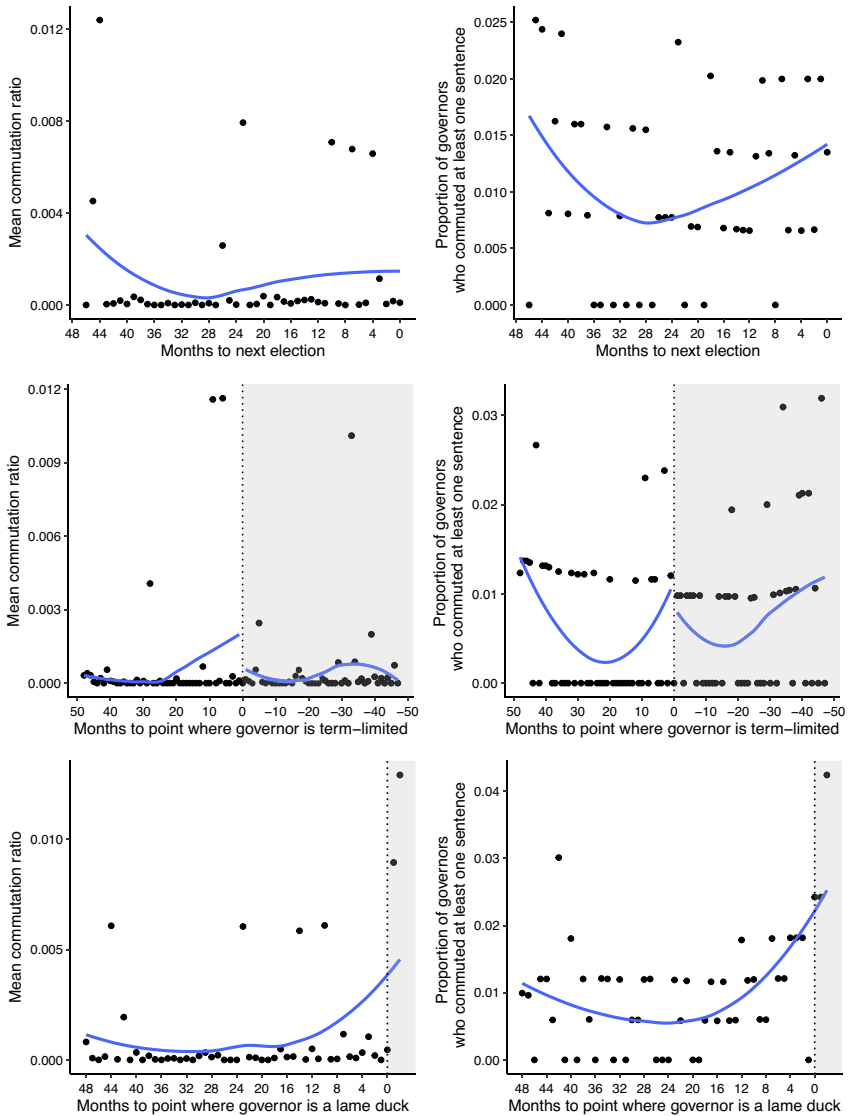


Figure 1. Descriptive results for electoral hypotheses.

of months that lame duck periods cover, we do not fit a LOESS curve to each side of the zero mark, but rather a single LOESS curve to the overall data. This panel provides suggestive evidence that governors are more likely to commute sentences – and commute a higher proportion of sentences during their lame duck period.⁷

⁷Again, for exposition, we truncate all governor-months in excess of 48 months to becoming a lame duck to 48 (see [Supplementary Figure A7](#) for untruncated results). Similarly, we recode values of the “months-to-

Table 1. Naive commutation comparison across term-limited and non-term-limited and lame duck and non-lame duck governors

	Not term-limited	Term-limited	Not a lame duck	Lame duck
Commutation ratio mean	0.0007	0.0004	0.0007	0.0109
Commutation dummy mean	0.0057	0.0074	0.0093	0.0333
Total months with commutations	23	34	120	11
Total months with no commutations	4,001	4,579	12,720	319
No. of governors	89	117	204	166

Table 1 presents naive comparisons by dichotomizing the predictors which correspond to Hypotheses 2 and 3, aggregating across all governor-months – inclusive of the “zeroth” month.⁸ Table 1 reveals that commutations are about as likely in the months where governors are term-limited as in the months where governors are not term-limited, and the proportion of sentences commuted during each of these periods are similar.⁹ The differences are more pronounced when comparing across lame duck governor months and non-lame duck governor months, however. On average, months where the governor is a lame duck are about 3.5 times more likely to see at least one sentence commuted as non-lame duck governor months, and a much higher proportion of defendants’ sentences are also commuted in these months. In total, descriptive evidence suggests that commutations are more likely in the lame duck period, but that neither electoral proximity nor term-limited status influences commutation decisions.

We present regression results in Table 2. Columns 1, 3, and 5 present results using the commutations ratio outcome. Columns 2, 4, and 6 present results using the binary commutations outcome. The estimated coefficients for the predictors for both Hypotheses 1 and 2 are quite small in magnitude. Moreover, the coefficients for Hypothesis 2 are also estimated to be in the “wrong” direction, though it is worth noting that both sets of coefficients are estimated imprecisely.¹⁰ As such, we cannot conclude that either increased distance to an election or term limits increase the likelihood of commutations (or the proportion of commuted sentences).

We do, however, find evidence of the lame duck period affecting sentencing behavior. In columns 5 and 6, the coefficient is in the expected direction and is statistically significant.

There is a higher likelihood of commutations in the lame duck period, and higher proportions of sentences are commuted during this period. Each coefficient corresponds to about 0.4 and 0.3 of a standard deviation increase based on the standard deviation of the outcome in the whole sample. We therefore find support for Hypothesis 3: Governors are more likely to commute sentences when they are in their lame duck period.

lame-duck” variable that are *less than* -2 to -2 . These correspond to two governors with long lame duck periods, neither of whom made any commutations in the later portions of their lame duck period.

⁸Supplementary Table A7 presents similar results that are exclusive of the month of the status change.

⁹We exclude term-limited governor months where the governor was also a lame duck.

¹⁰However, note that the coefficient for months to an election cannot be directly compared to the coefficients for the term-limited and lame-duck variables, given that the former is not a binary variable.

Table 2. Regression results

	Ratio	Dummy	Ratio	Dummy	Ratio	Dummy
Months to election	0.00001 0.00005	0.00007 (0.00017)				
Term-limited			−0.00001 (0.00015)	−0.00142 (0.00170)		
Lame duck					0.01034** (0.00495)	0.02700** (0.01090)
“Control” outcome mean	0.0001	0.014	0.001	0.006	0.001	0.009
“Control” outcome std. dev.	0.001	0.116	0.023	0.075	0.023	0.096
R^2	0.17	0.16	0.18	0.08	0.09	0.1
No. of obs.	6,331	6,331	8,637	8,637	13,170	13,170
No. of governors	149	149	118	118	209	209

Note. The specification in each column includes year-month and governor fixed effects. Standard errors clustered by governor are in parentheses. “Control” outcome mean” and “Control” outcome std. dev.” refer to the mean and standard deviation, respectively, of the outcome variable when the predictor variables are equal to zero.

* $p < 0.1$.

** $p < 0.05$.

*** $p < 0.01$.

The [Supplementary Material](#) includes a battery of robustness tests that bolster the main results (see [Supplementary Figures A7–A10](#) and [Supplementary Tables A7–A9](#)), but it is still possible that heterogeneous treatment effects across governors with different attributes explain the overall zero estimates with respect to Hypotheses 1 and 2. Moreover, the results for Hypothesis 3 may vary due to other political factors. In the [Supplementary Material](#), we therefore report a number of alternative analyses. These include descriptive patterns and regression results across different parties of governors ([Supplementary Figure A2](#)), whether the governor runs for governor again in the future ([Supplementary Figure A3](#) and [Supplementary Table A3](#)), the governor’s previous vote share ([Supplementary Figure A4](#) and [Supplementary Table A4](#)), and defendant race ([Supplementary Figures A5 and A6](#) and [Supplementary Tables A5 and A6](#)). We also considered whether effects differ for governors who ultimately ran for president.

While not all of these analyses were pre-registered, they do uncover some nuance in our results. In [Supplementary Figure A2](#), we disaggregate effects by party, which reveals little in the way of commutation patterns by electoral timing or term limits. That said, Republican governors running for re-election are more likely to commute sentences closer to an election than Democratic governors, and Democratic governors are more likely to commute sentences than Republican governors during the lame duck period. This last point is supported by the regression results in [Supplementary Table A2](#), which suggest that the lame duck effect is driven by Democratic governors.¹¹ This may suggest that Democrats are more conscious of electoral pressures to be “tough-on-crime” than Republicans.

Further supplemental analyses do little to undercut our conclusions with respect to our three hypotheses, however. In non-pre-registered analysis in [Supplementary Figure A3](#) and [Supplementary Table A3](#), we find further suggestive evidence that the lame duck results are driven by governors who do not run for any election again in the future, which supports the explanation that the lame duck effect is a function of an

¹¹Note that these models contain state fixed effects instead of governor fixed effects.

acute reduction in political pressure. Similarly, while small samples limit our ability to draw definitive conclusions, governors who run for president after completing their term – and who may still be conscious of political costs regardless of political conditions as governor – appear to commute at a similar rate as non-presidential candidates: 0.216 compared to 0.203.¹² That said, future presidential candidates do appear to commute a lower proportion of sentences on average – 0.568 versus 0.954 – suggesting that they may take a longer range view of political costs. Caution is warranted in making too much of these differences, given the small number of future presidential candidates. Nonetheless, our supplementary analyses do not suggest that unobserved variables are affecting our analysis in a way that runs contrary to the conclusions we report above.

Conclusion

The US Supreme Court has emphasized that executive clemency actions are not a simple act of mercy (*Gregg v. Georgia*, 428 US 153 (1976); see, e.g., Acker and Lanier 2000), but are to be used as a “fail safe” for the criminal justice system so that, for instance, governors can weigh new or mitigating factors in a case (*Herrera v. Collins*, 506 US 390, 414 (1993)). Yet, it is unclear how reasonable it is to expect governors to behave as apolitical figures committed to justice – if that was indeed the Court’s expectation. Using data on the entire universe of defendants on death row from 1973 to 2019, we find limited support for our pre-registered hypotheses that governors are responsive to either electoral timing or term limits when it comes to commutation decisions. If there is any period in their terms where governors are more likely to commute sentences, it appears to be during their lame duck period.

We point out that in contrast with other forms of executive action, commutations are unique because they cannot be rescinded. As opposed to, say, an executive order related to criminal justice reform that can be easily overturned with the election of a subsequent governor, governors may use a set of commutations in the waning days of their terms to take concrete action that they know cannot be reversed. Commutations are therefore an important aspect of gubernatorial power.

Our results align with previous work (Gunderson 2022), suggesting that politics may influence governors’ clemency decisions. There is some normative comfort in that only the most acute remediation of political costs (i.e., the period in which their political careers are likely ending) affects governors’ propensity to commute sentences. This is further supported by heterogeneous effects that suggest these lame duck effects are driven by governors who never again run for re-election, and are thus likely at the end of their political careers. Thus, the null results for proximity to elections and term limitations may simply be a result of less intense reductions in political pressure from these relative to the lame duck period. However, in tandem

¹²We collected data on 83 presidential candidates since 1968 who were formerly governors. After conducting the filtering steps described in the “Data” section and further selecting only presidential candidates who were governors within last 5 years of their terms, only 25 unique governors remain, corresponding to 37 governor terms. Given the small number of governors, we do not create figures which are analogous to the main figures of the research note, test for heterogeneity, or estimate a regression using presidential candidacy as a predictor. However, we do report naive comparisons of commutation behavior between active presidential candidates and those who are not running for higher office.

with prior research, our analysis is a reminder that governors are still political actors, and as such we should not expect their behaviors to be wholly apolitical.

We do not believe ours should be the final word on the politics of commutation. Future research could shift commutation decisions to the right-hand side of estimating equations. This may provide evidence, for example, that the public is indeed not responsive to commutation patterns and so governors may have no reason to time their commutation decisions strategically in the first place. Further work should also further investigate potential heterogeneous effects, perhaps using a small-N qualitative case study or process tracing approach. Indeed, we hope that this research note provides a launch pad for more studies of this nature, which are particularly necessary given the rarity of commutation events. Doing so will shed further light on the political influences of a decision with literal life-and-death implications.

Supplementary material. The supplementary material for this article can be found at <http://doi.org/10.1017/spq.2023.21>.

Data availability statement. Replication materials are available on SPPQ Dataverse at <https://doi.org/10.15139/S3/MWW6LZ> (Gerez and Miller 2023).

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