The Electoral Dynamics of Capital Punishment Commutations*

Julian E. Gerez† Michael G. Miller‡

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Abstract

We explore electoral explanations for United States' 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.

*This study was approved by the The Barnard College Institutional Review Board (2021-1120-028E, on file with authors). The pre-analysis plan corresponding to this study, which was written prior to the authors requesting access to the capital punishment data from ICPSR, can be accessed here: https://osf.io/5y4wp/. Earlier drafts of this paper were presented at the 2022 Meeting of the Midwest Political Science Association, and at the 2022 State Politics and Policy Conference. We thank participants in those panels for helpful suggestions. We also thank Michael Auslen, Elena Barham, John Marshall, Oscar Pocasangre, and Zara Riaz for helpful comments, and Nicki Camberg for research assistance.

†Ph.D. Candidate, Department of Political Science, Columbia University, 420 W. 118th St., New York, NY 10027. Email: julian.g@columbia.edu.

‡Associate Professor, Department of Political Science, Barnard College, Columbia University, 3009 Broadway, New York, NY 10027. Email: mgmiller@barnard.edu.

1 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 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 U.S. governors have authority to commute capital sentences in their states.[1] Given public 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 U.S. 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, in-

[1] Table A1 summarizes procedures for granting clemency in the states.

dicating 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 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.

2 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 et al. 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:

H1. *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 H1 are not present. Hence:

H2. *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:

H3. *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.*

3 Data

We obtained data on capital sentence commutations from "Capital Punishment in the United States, 1973-2019 (ICPSR 37998)" (hereafter CPUS), compiled by the U.S. 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 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 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.

4 Empirics

We present two sets of results to test H1-H3. We begin by presenting descriptive statistics which aggregate together governors for particular months with reference to the key predictor of interest. For example, for H1, we group by months leading up to the following election for governors who run for reelection. 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 H2, and we group by months to the point where the governor is a lame duck for H3.

[2]Additionally, tests of H1 include only governors who are running for re-election, while tests of H2 exclude the lame duck period to test only the term-limited period.

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:

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

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 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 appendix 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 H1, 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 H2, we can only compare governors who at one point were

[3] This 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.

[4] We only include governors who run for re-election in tests of H1.

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 zero otherwise. We expect the coefficients to be positive. For H3, the variable associated with the coefficient of interest will be equal to 1 during the months of the lame duck period and zero otherwise. We expect the coefficients to be positive here as well.

5 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 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 0 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 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]

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

[6] In 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 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.

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

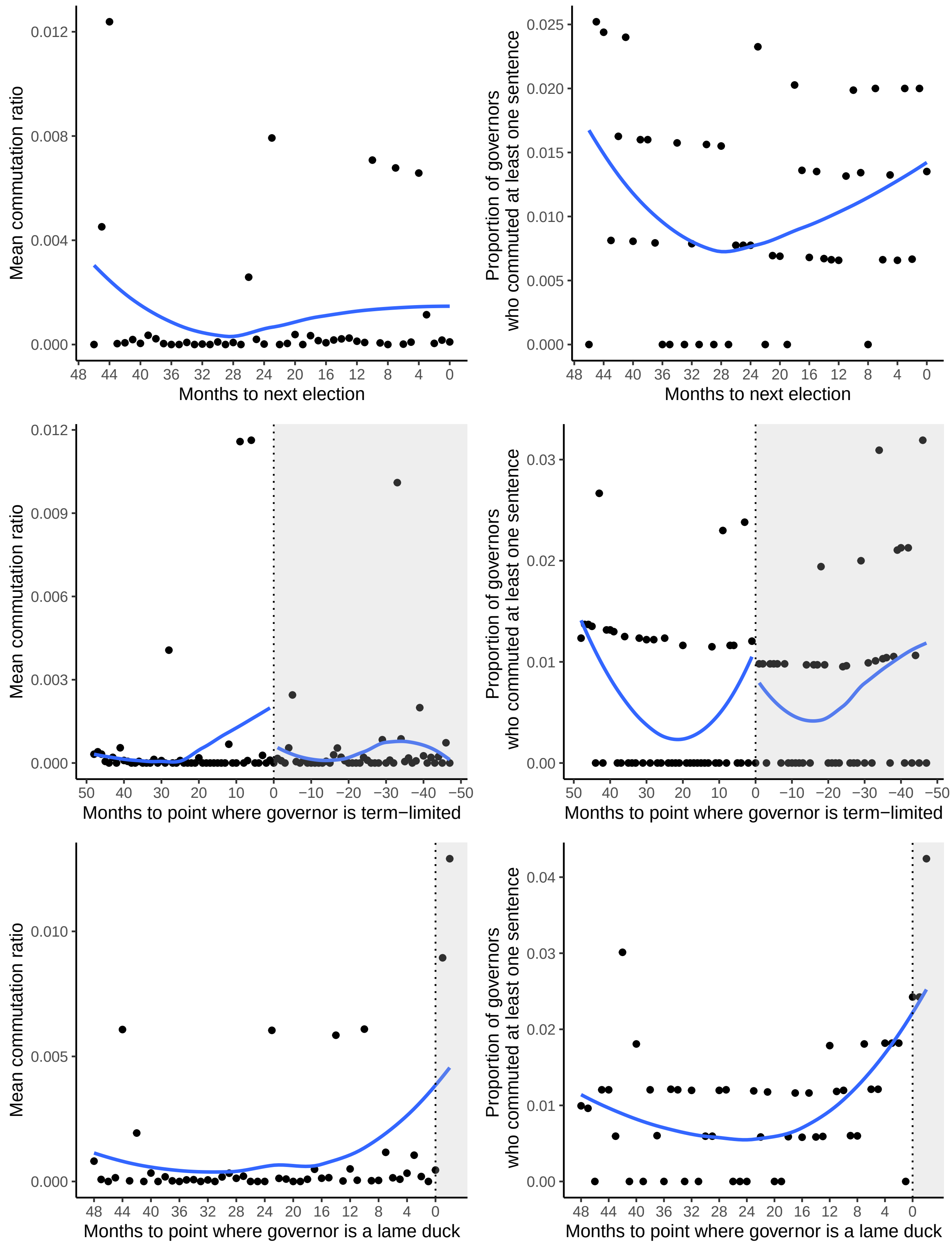

Figure 1: Descriptive results for electoral hypotheses.

	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
Number of governors	89	117	204	166

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

Table 1 presents naive comparisons by dichotomizing the predictors which correspond to H2 and H3, 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 influence 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 H1 and H2 are quite small in magnitude. Moreover, the coefficients for H2 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:

the later portions of their lame duck period.

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

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

[10]However, 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.

	Ratio	Dummy	Ratio	Dummy	Ratio	Dummy
Months to election	0.00001	0.00007				
	0.00005	(0.00017)				
Term-limited			-0.00001	-0.00142		
			(0.00015)	(0.00170)		
Lame duck					0.01034**	0.02700**
					(0.00495)	(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
Observations	6,331	6,331	8,637	8,637	13,170	13,170
Number of governors	149	149	118	118	209	209

* p < 0.1, ** p < 0.05, *** p < 0.01

Notes: 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.

Table 2: Regression results.

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 H3: Governors are more likely to commute sentences when they are in their lame duck period.

The appendix includes a battery of robustness tests that bolster the main results (See Figures A7-A10 and 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 H1 and H2. Moreover, the results for H3 may vary due to other political factors. In the appendix, we therefore report a number of alternative analyses. These include descriptive patterns and regression results across different parties of governors (Figure A2), whether the governor runs for governor again in the future (Figure A3 and Table A3), the governor's previous vote share (Figure A4 and Table A4), and defendant race (Figures A5-A6 and Tables A5-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 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 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 Figure A3 and 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 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.

6 Conclusion

The U.S. Supreme Court has emphasized that executive clemency actions are not a simple act of mercy (*Gregg v. Georgia*, 428 U.S. 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 U.S. 390, 414 (1993)). Yet, it is unclear

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

[12] We collected data on 83 presidential candidates since 1968 who were formerly governors. After conducting the filtering steps described in Section 3 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.

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 1973-2019, 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 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 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.

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Online Appendix: The Electoral Dynamics of Capital Punishment Commutations

Table of Contents

A1 Governor authority over clemency decisions

We distinguish between states where the governor has the power to commute capital sentences at the time the defendant was on death row, and states where the governor does not have this power. There are four broad forms of governor authority over clemency decisions (though each may differ slightly in the details of administration) ranging from complete governor authority to no governor authority: 1) the governor has sole authority, 2) the governor may receive a non-binding recommendation of clemency from a board or advisory group,[13] 3) the governor must have a recommendation of clemency from a board or advisory group, and 4) a board or advisory group determines clemency decisions. Sometimes, the governor can appoint members to said board, though we do not distinguish states along this dimension. Table A1 summarizes these institutions and how they have changed over time in our sample. Although these should be interpreted with caution given the lack of power, we present results disaggregated by these different forms of authority in Figure A10. The main results aggregate all forms of authority except for "no authority" together. Alaska, Hawaii, Iowa, Maine, Michigan, Minnesota, North Dakota, Rhode Island, Vermont, West Virginia, and Wisconsin are excluded from the table since these states abolished or effectively abolished the death penalty before or during 1973 and so no defendants from these states are included in our data.

[13]In some circumstances, this recommendation *must* be sought out. Nevertheless, if the recommendation received is non-binding, we classify it in this second group.

State	Governor authority over clemency decisions
Alabama	1973-2019: May receive recommendation
Arizona	1973-2019: Must have recommendation
Arkansas	1973-2019: May receive recommendation
California	1973-2019: Sole authority
Colorado	1973-2019: Sole authority
Connecticut	1973-2015: No authority
	2015-: Capital punishment abolished
Delaware	1973-2016: Must have recommendation
	2016-: Capital punishment abolished
Florida	1973-2019: Must have recommendation
Georgia	1973-2019: No authority
Idaho	1973-2000: No authority
	2000-2019: Must have recommendation
Illinois	1973-2011: May receive recommendation
	2011-: Capital punishment abolished
Indiana	1973-1986: May receive recommendation
	1986-2019: May receive recommendation
Kansas	1973-2019: May receive recommendation
Kentucky	1973-2019: Sole authority
Louisiana	1973-2019: Must have recommendation
Maryland	1973-2013: May receive recommendation
	2013-: Capital punishment abolished
Massachusetts	1973-1984: Must receive recommendation
	1984-: Capital punishment abolished
Mississippi	1973-2000: May receive recommendation
	2000-2019: Sole authority
Missouri	1973-2019: May receive recommendation
Montana	1973-1986: Must receive recommendation
	1986-2019: May receive recommendation
Nebraska	1973-2019: No authority
Nevada	1973-2019: No authority
New Hampshire	1973-2019: May receive recommendation
New Jersey	1973-1986: May receive recommendation
	1986-2017: Sole authority
	2017-: Capital punishment abolished
New Mexico	1973-2009: Sole authority
	2009-: Capital punishment abolished
New York	1973-2004: Sole authority
	2004-: Capital punishment abolished
North Carolina	1973-2019: Sole authority
Ohio	1973-2019: May receive recommendation
Oklahoma	1973-2019: Must have recommendation
Oregon	1973-2019: Sole authority
Pennsylvania	1973-2019: Must have recommendation
South Carolina	1973-2000: May receive recommendation
	2000-2019: Sole authority
South Dakota	1973-2019: Sole authority
South Carolina	1973-2000: May receive recommendation
	2000-2019: Sole authority
Texas	1973-2019: Must have recommendation
Utah	1973-2019: No authority
Virginia	1973-2019: Sole authority
Washington	1973-2018: Sole authority
	2018-: Capital punishment abolished
Wyoming	1973-2019: Sole authority

Table A1: Summary of governor authority over clemency decisions across states and time.

A2 Public opinion of capital punishment

Available public opinion data suggests that many governors would perceive high costs of commuting death sentences. Figure A1 summarizes national-level public opinion toward the death penalty over time from Gallup and the General Social Survey, and highlights the years of our study.[14] The trend is similar between the two sources. There is a clear increase in public support for the death penalty from the 1960s to 1990s, a trend that Page and Shapiro (1992) attribute to rising violent crime rates during this period. Indeed, by 1976—near the start of our period of study—66% of U.S. respondents said they were in favor of the death penalty for a person convicted of murder.[15] By 1994, the proportion of the public in favor of the death penalty had increased to 80% before beginning a period of steady decline. As recently as late 2021 however, a majority of U.S. respondents still favored capital punishment.[16]

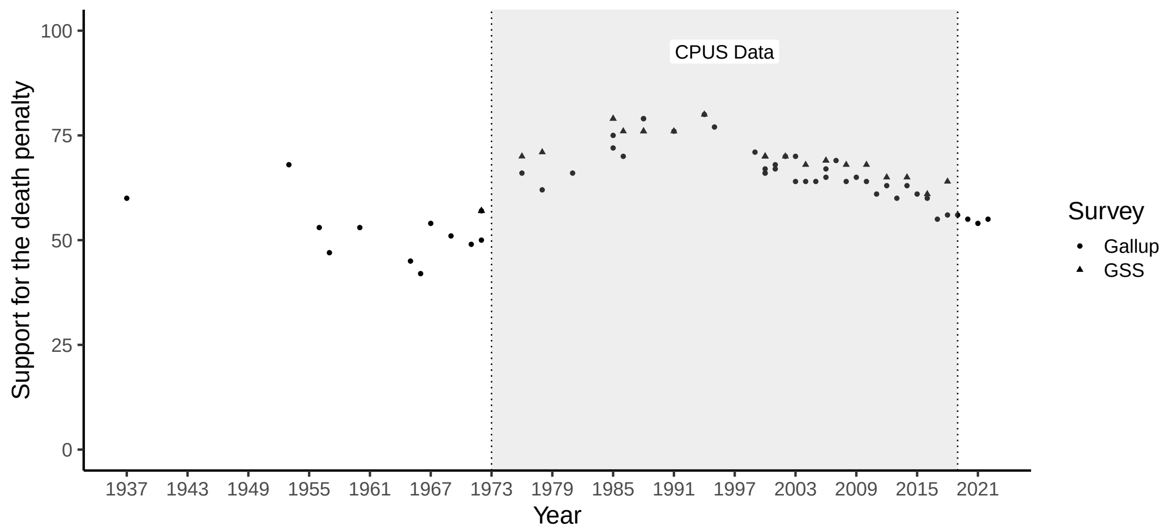

Figure A1: Percentage of respondents in favor of the death penalty, Gallup and GSS.

[14]Gallup's survey asks "Are you in favor of the death penalty for a person convicted of murder?" while the GSS asks "Do you favor or oppose the death penalty for persons convicted of murder?"

[15]This was the first time Gallup asked this question after the beginning of our panel. Prior to the start of our sample, from the late 1950s to the late 1960s, favorability toward the death penalty was much lower—in 1966, 42% of U.S. respondents in favor of the death penalty. Before the 1950s, however, the proportion of respondents in favor of the death penalty was much larger.

[16]State-level public opinion toward the death penalty has not been surveyed with enough regularity to provide an overarching summary similar to that in Figure A1. That said, in order for a potential commutation to be part of the data we are using, capital punishment must at some point have been—or must still be—legal in the states included in our sample. Since the legality of the death penalty is likely endogenous to bottom-up and top-down pressures to support capital punishment, it follows that commutation is likely to be even *costlier* for governors within our sample of states and years than relative to the U.S. in general.

A3 Results by party

Figure A2 presents the main descriptive figures disaggregated by governor's party. Table A2 reports the main regression results interacted by governor's party.

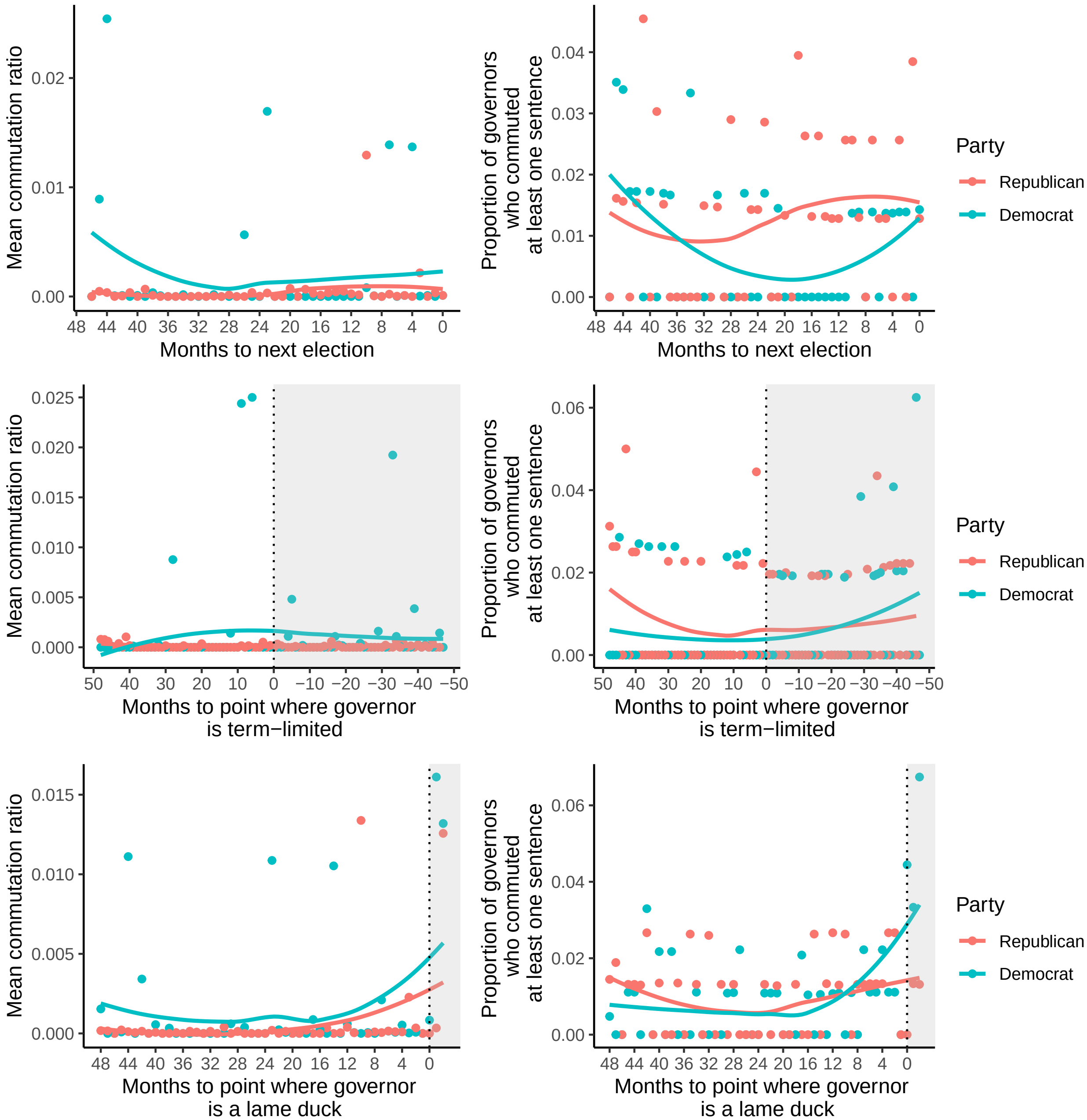

Figure A2: Descriptive results for electoral hypotheses by party.

Table A2: Regression results, party interaction.

	Ratio	Dummy	Ratio	Dummy	Ratio	Dummy
Republican governor	0.00062	0.00808	-0.00009	0.00304	-0.00092	0.00060
	(0.00184)	(0.00829)	(0.00076)	(0.00332)	(0.00062)	(0.00311)
Months to election	0.00004	0.00021				
	(0.00006)	(0.00016)				
Months to election × Republican governor	-0.00009	-0.00031				
	(0.00009)	(0.00032)				
Term-limited			-0.00033	0.00452		
			(0.00071)	(0.00302)		
Term-limited × Republican governor			0.00016	-0.00641		
			(0.00076)	(0.00386)		
Lame duck					0.01353*	0.04378**
					(0.00769)	(0.01675)
Lame duck × Republican governor					-0.00784	-0.03682*
					(0.00986)	(0.01905)
"Control" outcome mean	0.00009	0.014	0.001	0.004	0.001	0.007
"Control" outcome std. dev.	0.001	0.12	0.033	0.065	0.029	0.084
R^2	0.13	0.12	0.15	0.07	0.06	0.06
Observations	6,331	6,331	8,637	8,637	13,170	13,170
Number of governors	149	149	118	118	209	209

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: The specification in each column includes year-month and state 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.

A4 Results by re-election

Figure A3 and Table A3 report results disaggregated by whether the governor runs for re-election or not. Note that H2 cannot be evaluated in a similar manner since by definition term-limited governors cannot run for re-election.

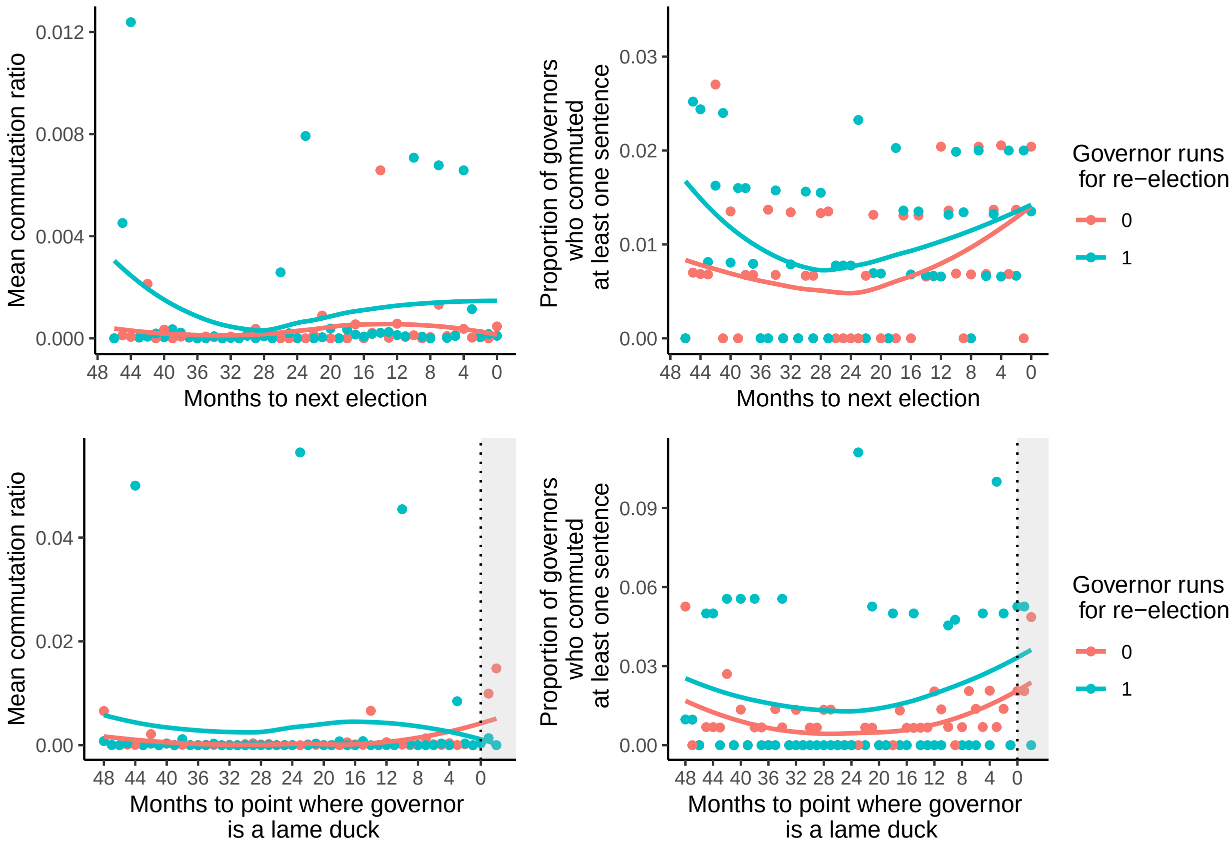

Figure A3: Descriptive results for electoral hypotheses by whether the governor runs for re-election.

Table A3: Regression results, runs for re-election interaction.

	Ratio	Dummy	Ratio	Dummy
Governor runs for re-election	0.00047	0.00064	0.00055	0.00192
	(0.00093)	(0.00524)	(0.00039)	(0.00203)
Months to election	0.00001	-0.00008		
	(0.00002)	(0.00012)		
Months to election × Governor runs for re-election	0.000002	-0.00001		
	(0.00004)	(0.00021)		
Lame duck			0.01217**	0.02947**
			(0.00578)	(0.01154)
Lame duck × Governor runs for re-election			-0.01405*	-0.01756
			(0.00709)	(0.03095)
"Control" outcome mean	0.0005	0.02	0.0003	0.008
"Control" outcome std. dev.	0.004	0.142	0.013	0.088
R^2	0.11	0.1	0.09	0.1
Observations	13,165	13,165	13,170	13,170
Number of governors	219	219	209	209

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: 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.

A5 Results by previous vote share

Figure A4 plots the bivariate relationship between previous party vote share for each governor in the data against commutation behavior. Table A4 interacts the main regression specification with margin of victory (as measured by the two-way vote share of the governor's party in the previous election).

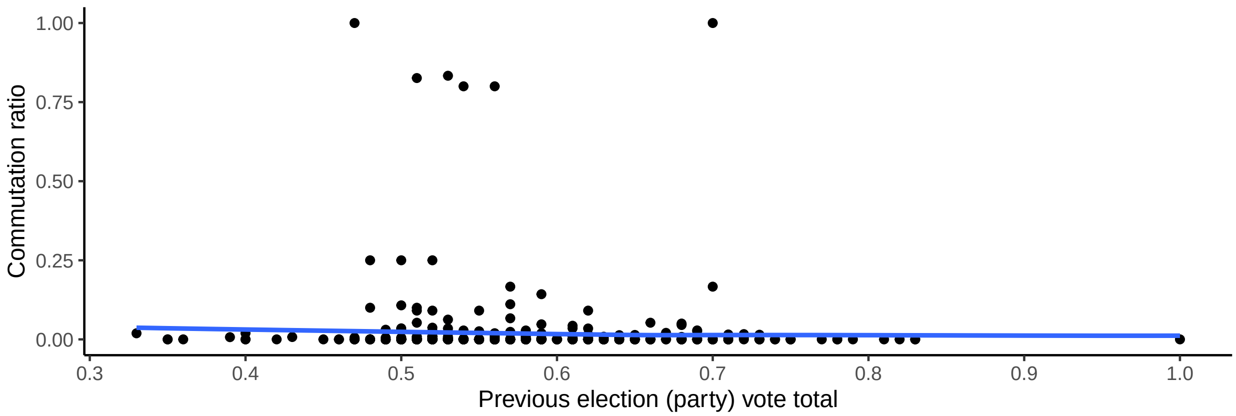

Figure A4: Descriptive results for previous vote share.

Table A4: Regression results, previous vote share interaction.

	Ratio	Dummy	Ratio	Dummy	Ratio	Dummy
Previous vote share	-0.01918	0.04292	-0.00298	-0.01887	-0.00084	0.00089
	(0.03152)	(0.10421)	(0.00421)	(0.03033)	(0.00281)	(0.01995)
Months to election	0.00016	0.00146				
	(0.00033)	(0.00183)				
Months to election × previous vote share	-0.00027	-0.00253				
	(0.00057)	(0.00319)				
Term-limited			-0.00629	-0.02284		
			(0.00637)	(0.01823)		
Term-limited × previous vote share			0.01064	0.03728		
			(0.01065)	(0.03228)		
Lame duck					0.04543	0.02947
					(0.02934)	(0.05644)
Lame duck × previous vote share					-0.06029	-0.00397
					(0.04413)	(0.09419)
"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
Observations	6,331	6,331	8,637	8,637	13,146	13,146
Number of governors	149	149	118	118	208	208

* p < 0.1, ** p < 0.05, *** p < 0.01

Notes: 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 not including margin of victory are equal to zero.

A6 Results by race

Figure A5 presents the main descriptive figures disaggregated by defendant race. Table A5 reports the main regression results interacted by defendant race.

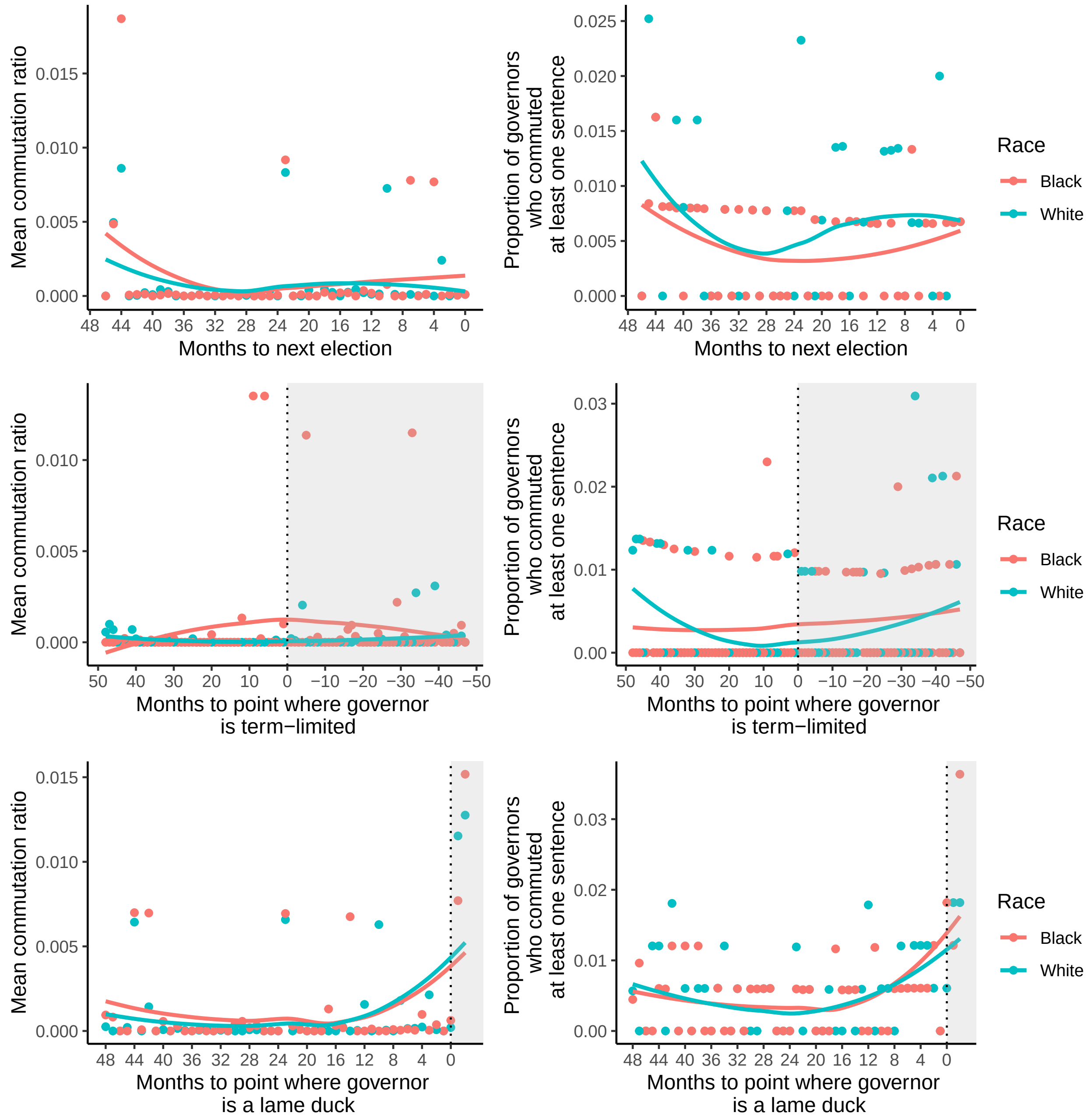

Figure A5: Descriptive results for electoral hypotheses by race, black and white defendants.

Table A5: Regression results, race interaction (black is the reference category).

	Ratio	Dummy	Ratio	Dummy	Ratio	Dummy
White defendant	0.00105	0.00272	-0.00017	-0.00075	-0.00017	0.00062
	(0.00073)	(0.00270)	(0.00022)	(0.00120)	(0.00022)	(0.00092)
Months to election	0.00004	0.00007				
	(0.00007)	(0.00010)				
Months to election × White defendant	-0.00004	-0.00003				
	(0.00004)	(0.00010)				
Term-limited			0.00025	-0.00067		
			(0.00040)	(0.00117)		
Term-limited × White defendant			-0.00017	0.00010		
			(0.00042)	(0.00168)		
Lame duck					0.01079**	0.02125**
					(0.00544)	(0.00851)
Lame duck × White defendant					0.00069	-0.00668
					(0.00209)	(0.00740)
"Control" outcome mean	0.0001	0.007	0.001	0.003	0.001	0.005
"Control" outcome std. dev.	0.001	0.082	0.024	0.055	0.026	0.067
R^2	0.14	0.09	0.14	0.04	0.07	0.06
Observations	11,548	12,662	15,690	17,274	23,836	26,340
Number of governors	149	149	117	118	208	209

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: 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.

Figure A6 and Table A6 are analogous to Figure A5 and Table A5, but the reference category is all nonwhite defendants as opposed to Black defendants. This analysis was not pre-registered.

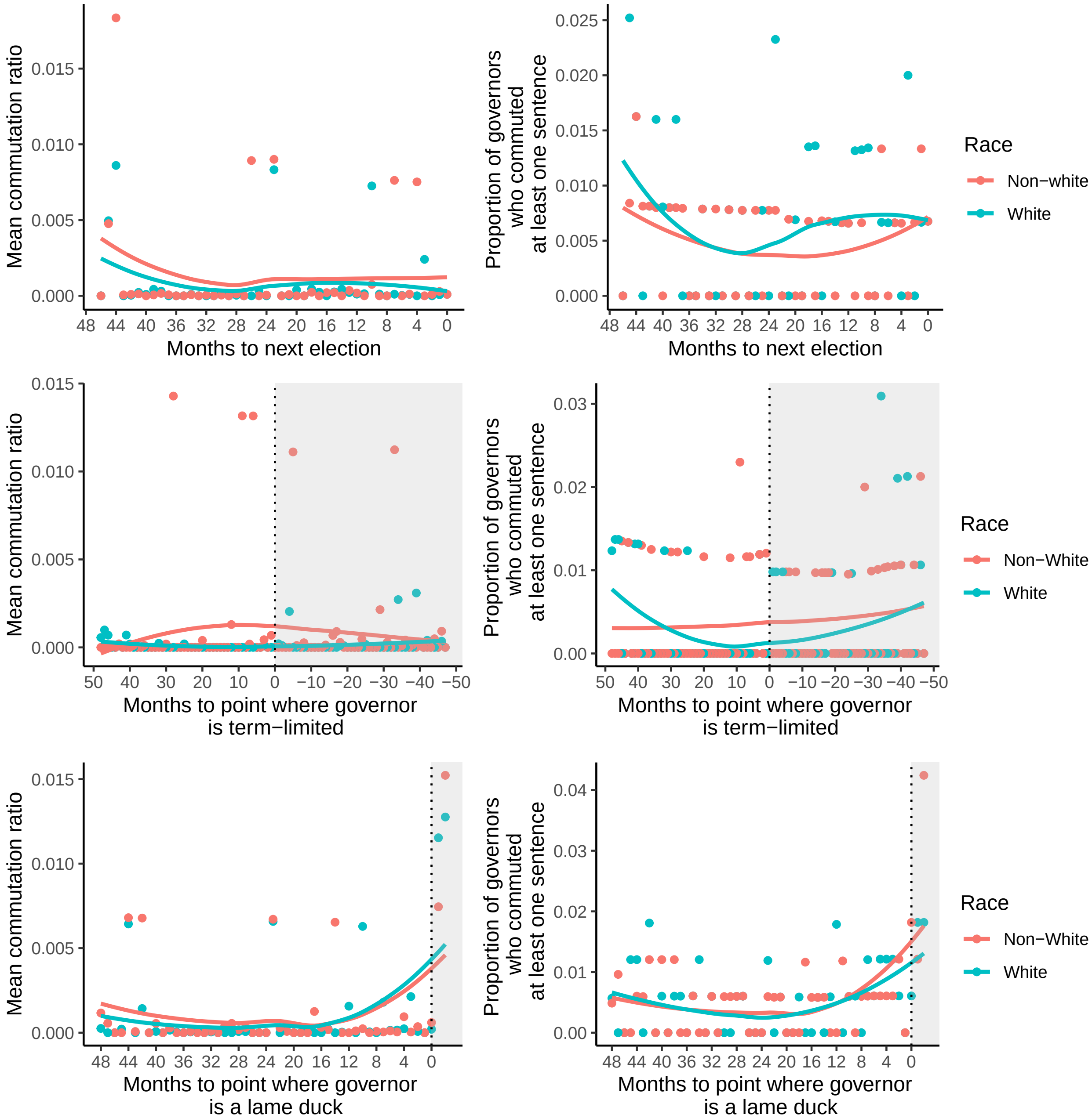

Figure A6: Descriptive results for electoral hypotheses by race, non-white and white defendants.

Table A6: Regression results, race interaction (non-white is the reference category).

	Ratio	Dummy	Ratio	Dummy	Ratio	Dummy
White defendant	0.0007	0.00209	-0.00054	-0.00124	-0.00032	0.00039
	(0.00076)	(0.00282)	(0.00043)	(0.00120)	(0.00026)	(0.00093)
Months to election	0.000004	0.00005				
	(0.00007)	(0.00010)				
Months to election × White defendant	-0.00004	-0.00002				
	(0.00003)	(0.00010)				
Term-limited			0.00009	-0.00090		
			(0.00043)	(0.00119)		
Term-limited × White defendant			0.00011	0.00038		
			(0.00050)	(0.00169)		
Lame duck					0.01062**	0.02376***
					(0.00527)	(0.00893)
Lame duck × White defendant					0.00090	-0.00948
					(0.00213)	(0.00802)
"Control" outcome mean	0.0001	0.007	0.001	0.003	0.001	0.005
"Control" outcome std. dev.	0.001	0.082	0.029	0.059	0.027	0.069
R^2	0.13	0.09	0.12	0.04	0.07	0.06
Observations	11,660	12,662	15,888	17,274	24,161	26,340
Number of governors	149	149	118	118	209	209

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: 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.

A7 Descriptive results robustness checks

Figure A7 presents results for the H2 and H3 panels of Figure 1, but does not aggregate governors together in any way, even if there are very few governors for a particular month. Table A7 presents results from Table 1, but excludes the zeroth month when defining the "treatment."

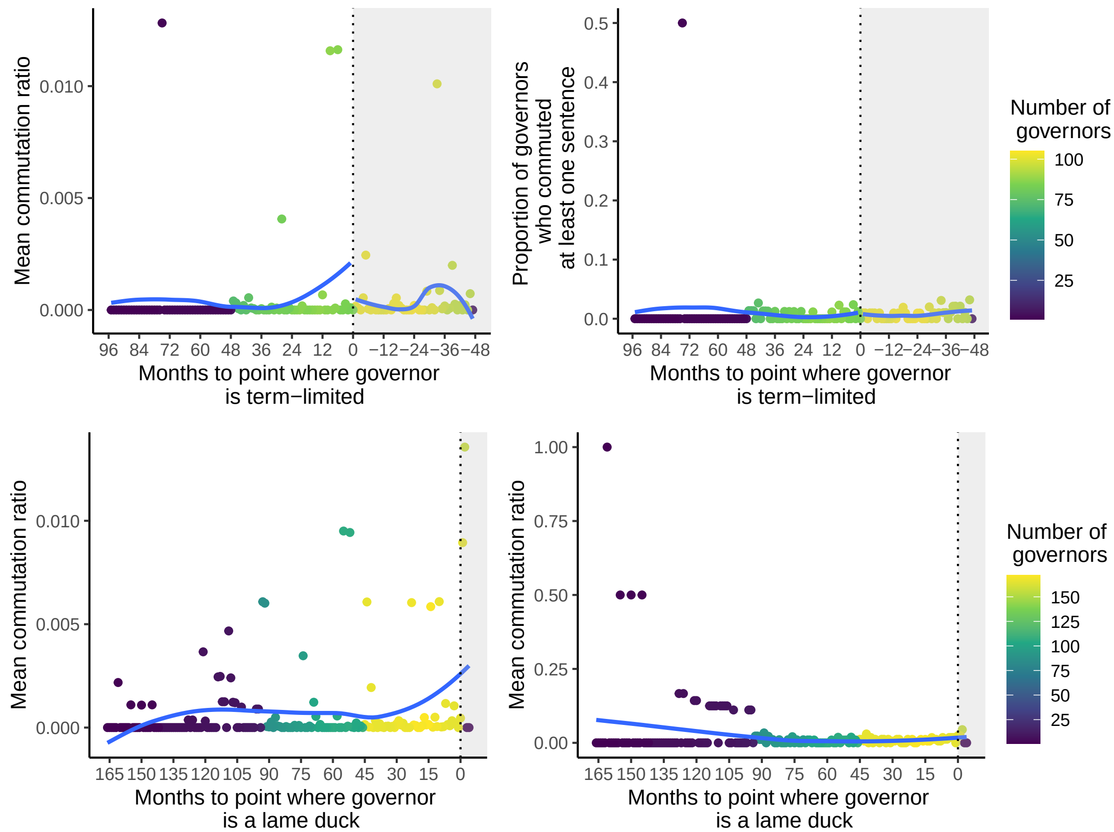

Figure A7: Descriptive results for electoral hypotheses. LOESS curves are weighted by the number of governors that compose the mean in that month.

	Not term-limited	Term-limited	Not a lame duck	Lame duck
Commutation ratio mean	0.0007	0.0004	0.0007	0.0074
Commutation dummy mean	0.0058	0.0072	0.0092	0.0303
Total months with commutations	23	34	116	15
Total months with no commutations	3,917	4,663	12,559	480
Number of governors	88	117	203	166

Table A7: Naive commutation comparison across term-limited and non-term-limited and lame duck and non-lame duck governors, exclusive of the zeroth month.

A8 Regression results robustness checks

Table A8 presents an alternative not pre-registered conceptualization of the main predictor variable for H1, months to election. Instead of using months to the next election, we define the predictor here as a dummy variable that takes on a value of 1 if the governor is within 12 months of their next election and zero otherwise. This makes the coefficient here more comparable to the coefficients that correspond to H2 and H3 in Table 2. The interpretation of the results does not change.

Table A8: Regression results, year to next election.

	Ratio	Dummy
12 months to election dummy	0.00006	0.00421
	(0.00111)	(0.00376)
"Control" outcome mean	0.001	0.01
"Control" outcome std. dev.	0.024	0.099
R^2	0.17	0.16
Observations	6,331	6,331
Number of governors	149	149

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: 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.

One concern about our results might be that our equivocal results are driven by the inclusion of many fixed effects in the baseline specification which includes governor and year × months fixed effects—lowering the number of degrees of freedom of the model. We show in Figure A8 results from regressions that include different forms of time fixed effects but exclude governor fixed effects that are comparable to the main results. We also present Figure A9, which shows that the results are substantively similar when including governor and year and month fixed effects separately, governor and year fixed effects only, governor and month fixed effects only, and only governor fixed effects. These robustness checks were not pre-registered.

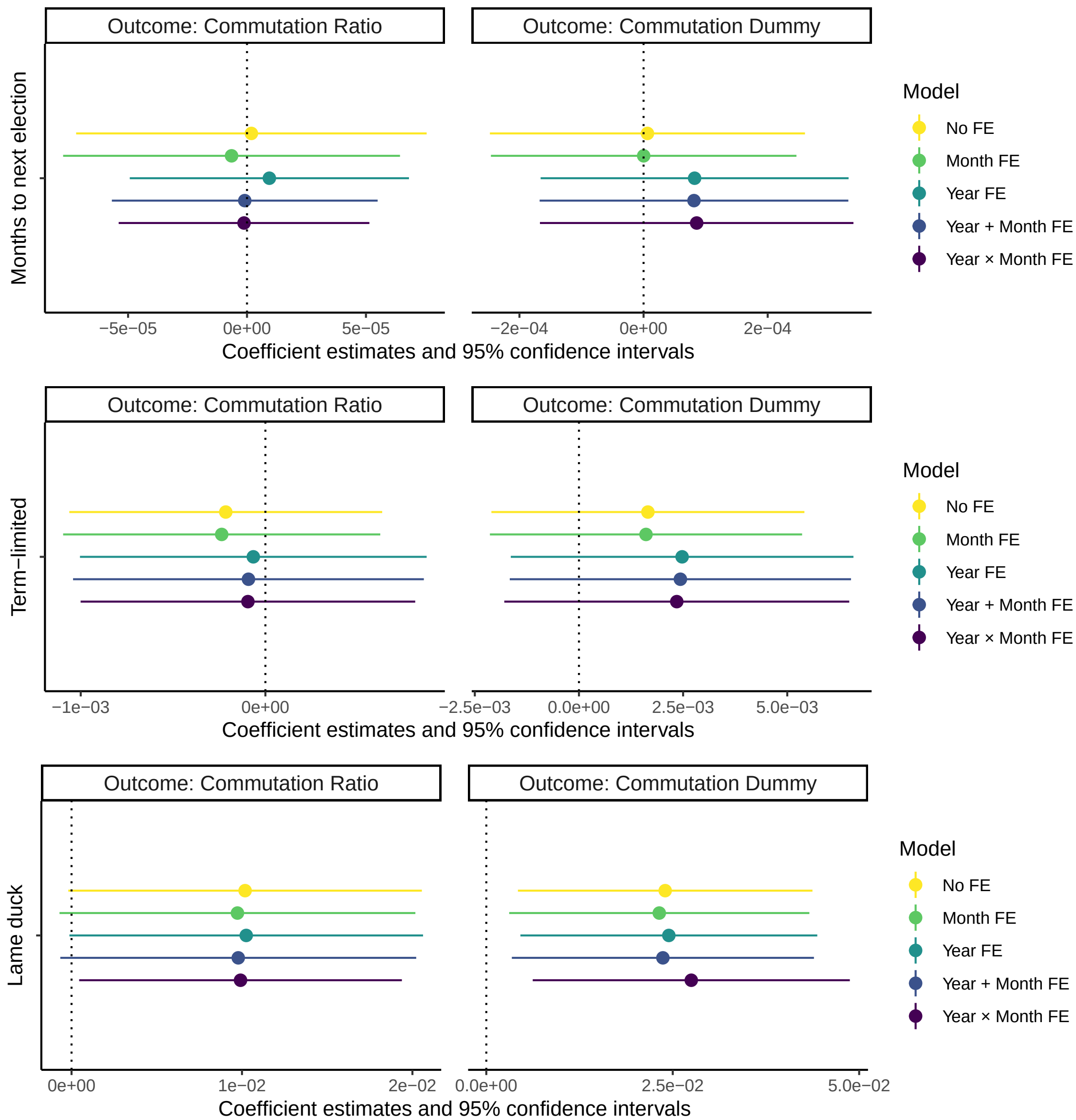

Figure A8: Robustness of regression results to exclusion of governor fixed effects specifications.

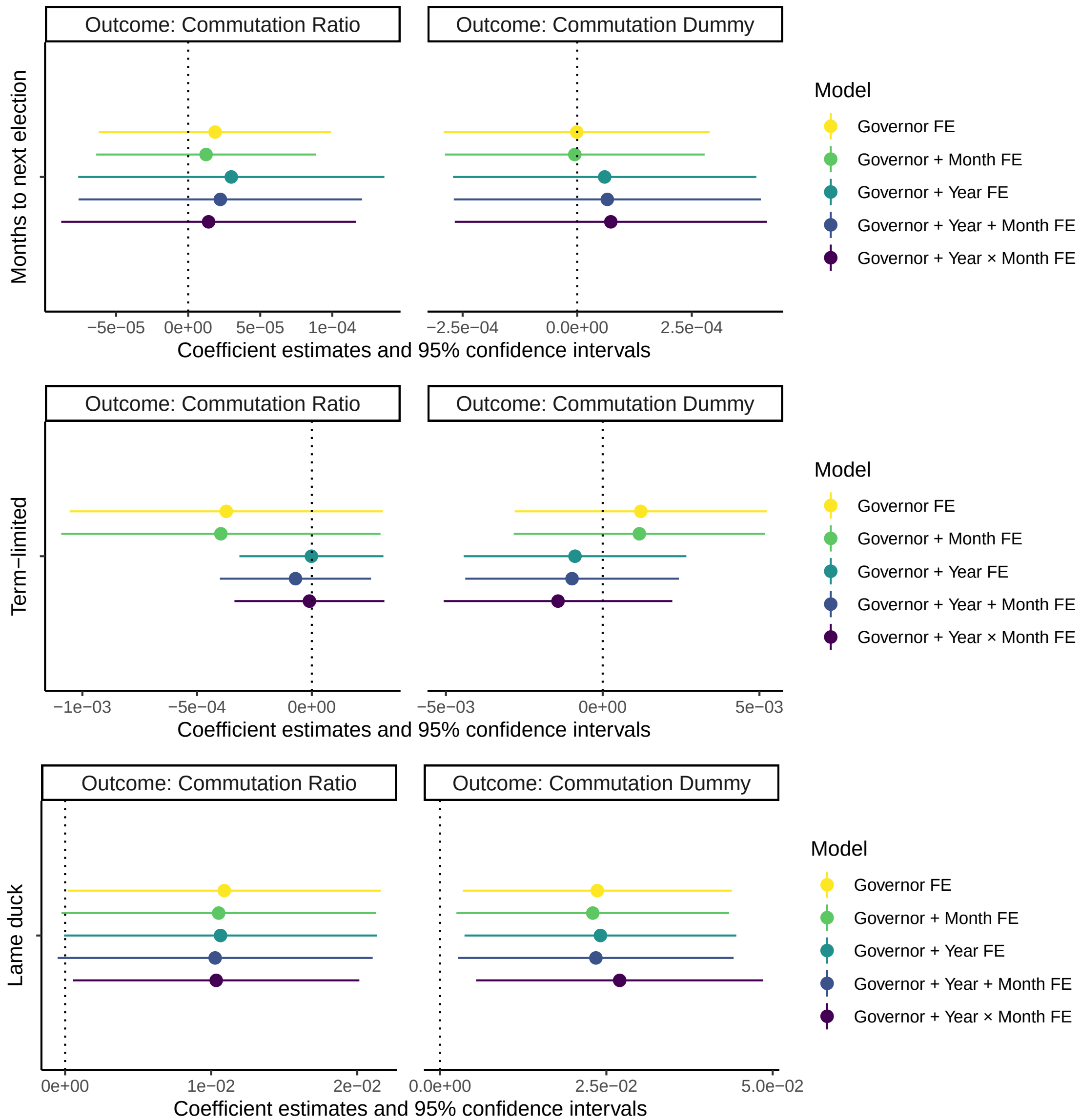

Figure A9: Robustness of regression results to different governor fixed effects specifications.

Figure A10 presents models across subsets of the data with different forms of governor authority over commutation decisions. Although power is limited, reassuringly, the results for the lame duck period appear to be primarily driven by the cases where governors have more authority. Moreover, the coefficients for the placebo “no authority" governors are close to zero or estimated imprecisely.

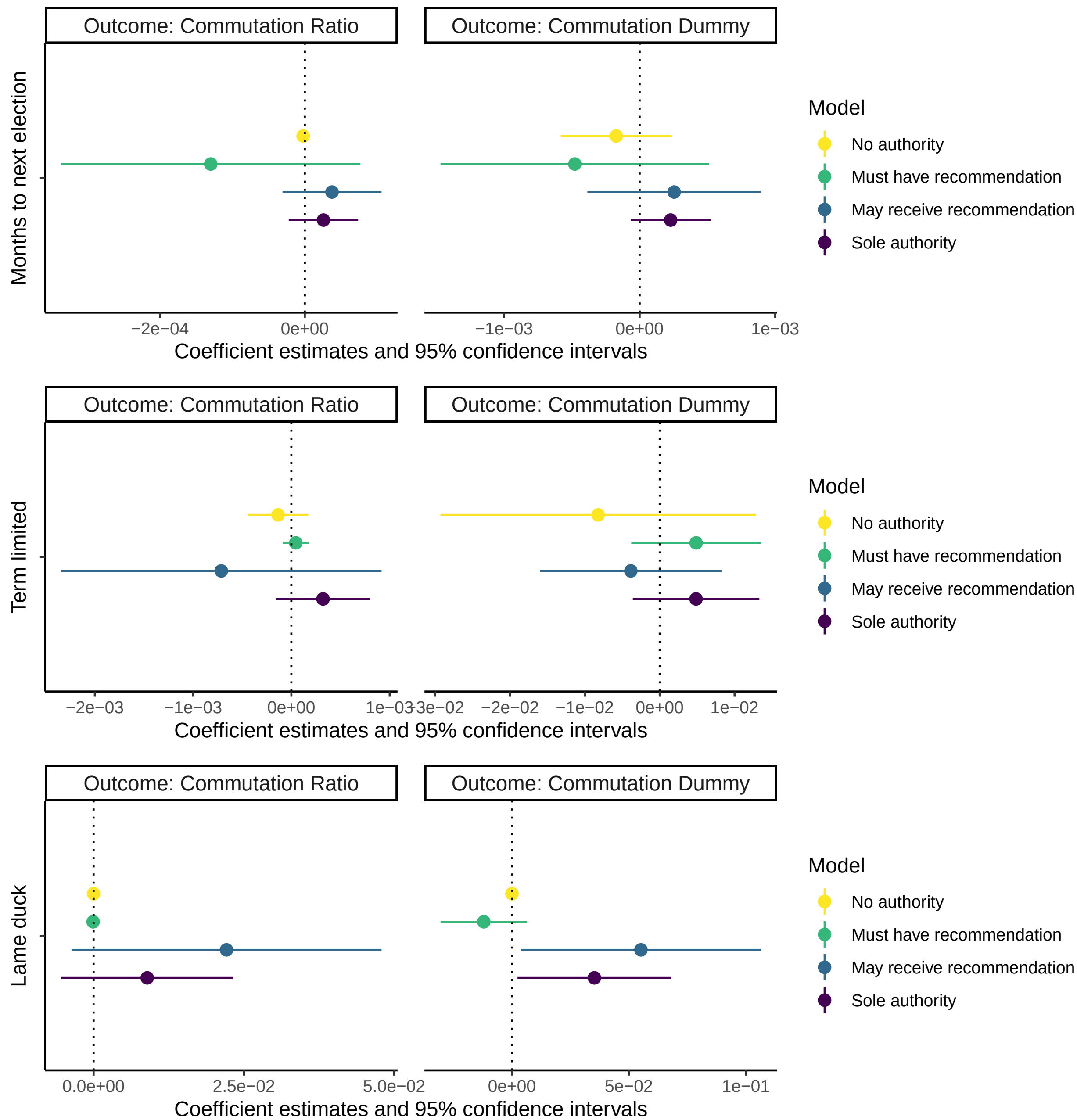

Figure A10: Robustness of regression results to subsetting to different definitions of governor authority.

Another possible concern with the models that include governor fixed effects is that the results could be biased if all defendants on death sentences are commuted, or all defendants exit death row for another reason. Consider the following extreme example for illustrative purposes: A governor with a strong predilection for commuting sentences decides to commute sentences of all defendants on death row toward the beginning of their term irrespective of electoral pressures. If they commute all sentences, then their outcome variables will be missing in the sample for governor-months after their mass commutation, assuming no further defendants receive death sentences in their state. It is plausible that this governor *would* commute further sentences in months closer to elections too, but we cannot see this manifestation of these outcomes. Table A9 reports results where instead of dividing by the total number of defendants on death row for the outcome, we simply predict the count of commutations, so governor-months with zero defendants on death row are not treated as missing. Similarly, the commutation dummy outcome in Table A9 does not condition on their being any defendants on death row. The results are similar.

Table A9: Regression results.

	Count	Dummy	Count	Dummy	Count	Dummy
Months to election	0.00012	-0.000004				
	(0.00026)	(0.00014)				
Term-limited			-0.00062	-0.00083		
			(0.00191)	(0.00159)		
Lame duck					0.30306	0.02013**
					(0.26386)	(0.00805)
"Control" outcome mean	0.009	0.009	0.005	0.005	0.009	0.007
"Control" outcome std. dev.	0.097	0.097	0.073	0.068	0.155	0.083
R^2	0.1	0.14	0.07	0.07	0.05	0.09
Observations	8,766	8,766	10,863	10,863	17,934	17,934
Number of governors	185	185	132	132	250	250

* p < 0.1, ** p < 0.05, *** p < 0.01

Notes: 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.