The Political Dynamics of Capital Punishment Commutations*

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Abstract

We explore political explanations for 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 number of such sentences governors might commute. We do however find that governors are more likely to commute sentences during the "lame duck" period after their successor's election but before their inauguration.

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1 Introduction

Clemency from an executive is often the final option for defendants hoping to escape a pending execution. Several United States governors have authority to commute defendants' death sentences.[1] Yet, considering an aversion of elected officials to appear "soft on crime"—coupled with rising polarization and media scrutiny—governors may be reluctant to commute capital sentences if they fear political consequences from doing so. In practice, this could mean that governors are more likely to commute death sentences when they perceive the costs of doing so as low. By extension, it is possible that not all defendants are equally likely to receive a commutation; those whose sentences can be commuted at a politically opportune time may have a higher probability of escaping capital punishment. It is therefore worthwhile to consider the extent to which political considerations shape governors' commutation decisions.

We examine the role of several such factors in governors' decisions of whether to commute capital sentences. We employ data comprising the entire universe of incarcerated persons under a sentence of death in the U.S. from 1973 to 2019. We pre-registered our regression analyses before obtaining access to these data. 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 and significant association between both of these outcomes and "lame duck" status, indicating that governors are more likely to commute death sentences in their waning days of office—when political costs are most remote. These results suggest that governors do factor political costs into commutation decisions. In total, our results contribute to a growing literature examining how mass preferences shape various outcomes in the criminal justice system (e.g., Enns 2016; Hetey and Eberhardt 2014; Peffley and Hurwitz 2007).

[1] Among the states where the death penalty is legal, procedures for the granting of clemency vary. As of 2022, in nine states—Alabama, Mississippi, South Carolina, California, North Carolina, South Dakota, Kentucky, Oregon, and Wyoming—the governor has sole authority. In others, separate board or advisory groups can issue binding (i.e., the governor cannot proceed without said recommendation) or non-binding recommendations of clemency, and in others still, the board itself determines clemency, not the governor. Table A summarizes these procedures.

2 Governors, The Public, and Clemency

The American criminal justice system has long been exceptionally punitive (Alexander 2010; Enns 2016; Pfaff 2017; Tonry 2011; Weaver 2007; Whitman et al. 2003), and is the only Western nation in which capital punishment is still allowed. One possible explanation for this is that elites drive punitive sentiment among the public, stoking fear of rising crime so that they can subsequently claim credit for addressing the problem. For instance, Ramirez (2013) finds evidence that punitive sentiment among the public follows presidential rhetoric. Other work has similarly found that the salience of crime in the public's mind tracks elite signaling about crime significantly more than actual crime rates (Beckett 1997). In short, this line of work suggests that America's punitive policies may be out of step with the public's true preferences, as elite rhetoric distorts crime severity so that policymakers can subsequently act to solve a problem whose perception they helped to shape.

Other extant research however has found that elites enact punitive policies because such policies are what the public genuinely demands. Despite the fact that people are typically not well-versed on the workings of the criminal justice system (e.g., Pickett et al. 2015; Roberts and Stalans 1997), the mass public in the United States has been uniquely punitive for essentially the country's entire history (Whitman et al. 2003). While this may present somewhat of a puzzle, people do pay more attention to politics when they believe things are not going well (Lyons, Jaeger and Wolak 2013), and in closer proximity to elections (Marshall 2020). If they believe crime is rising via personal experience such as being a victim (Bateson 2012), or by exposure to media reports or personal testimony of others in their community, crime may become a more salient issue in voters' minds. If this is the case, the public may be more likely to express a desire for punitive policies they believe will curb rising crime (Zimring and Johnson 2006). Indeed, Enns (2016) finds that punitive sentiment among the public lags crime rates in a community. With this in mind, punitive policies may reflect rational calculus among elites; simply, politicians may maintain a punitive posture because they are responding to voters' wishes (Enns 2014, 2016).

For our purposes, the *reasons* that a punitive public exists are less importance than the fact that punitive sentiment can be marshaled for political ends. Extensive research has demonstrated that incumbent politicians adapt their behavior in response to electoral incentives (e.g., Downs

1957). Thus, politicians should rationally avoid actions that they believe will be out of step with public sentiment, and might increase their chances of re-election by claiming credit for public action on issues that voters care about (Mayhew 2004).

Thus, if the public holds genuinely punitive policies, we would expect politicians to behave in a punitive fashion so as to avoid appearing out of step with voters. But we should also expect politicians to do so if they themselves shaped mass views of crime. In short, once they have created demand for punitive policies, politicians are obligated to deliver. Punitive policies and electoral rhetoric are therefore appealing ways for politicians to dissuade accusations that they are "soft on crime" (Chambliss 2001; Chevigny 2003; Drago, Galbiati and Sobbrio 2020; Holland 2013). Indeed, some scholars have argued that this cycle explains the generally more punitive direction of criminal policy in the United States since the 1960s (Beckett and Sasson 2003).

We believe this dynamic is likely to apply to salient areas within the broader criminal justice framework, such as capital punishment. 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 how reasonable it is to expect governors to behave as apolitical figures committed to justice—if that was indeed the Court's expectation. Especially if they plan to seek re-election, governors are likely aware that the smallest of executive actions risks provoking public backlash. As with many decisions they must make in office, before they decide whether to commute a sentence, these governors are well-advised to weigh the political costs of doing so.

Available public opinion data suggests that many governors would perceive high costs of commuting death sentences. Figure 1 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.[2] 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

[2]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?"

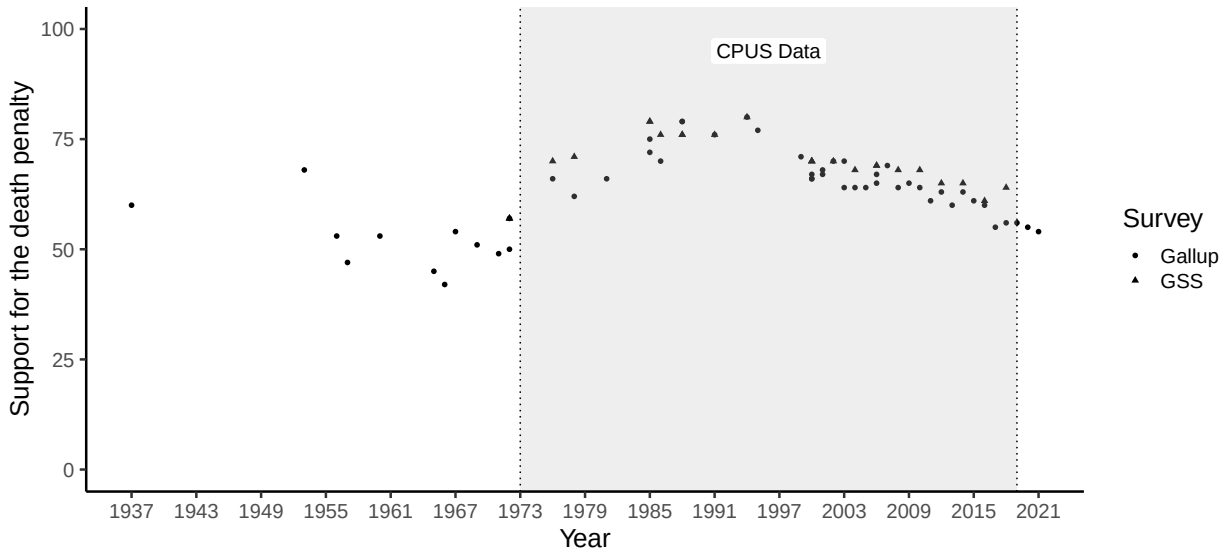

Figure 1: Percentage of respondents in favor of the death penalty, Gallup and GSS.

of murder.[3] 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.[4]

Given public sentiment surrounding the death penalty, governors should expect a commutation to draw media coverage about the defendant, their crime, and mitigating factors (if any) that warrant commutation. Governors are therefore likely to take account of political conditions before deciding to commute sentences, and should be averse to commutation if they see themselves as vulnerable to penalties imposed by a punitive public. We therefore expect governors to be more likely to commute sentences when the political costs of doing so are low.

There is anecdotal evidence suggesting that governors weigh the political pros and cons of commutation decisions. For instance, governors have occasionally granted blanket commutations

[3]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.

[4]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 1. 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.

for all prisoners on death row within their state shortly before their terms expired. Some of these, such as Martin O'Malley of Maryland (in 2014),[5] Dick Celeste of Ohio (1991),[6] or Toney Anaya of New Mexico (1986),[7] were leaving office due to term limits. Some, such as George Ryan of Illinois (2003) made blanket commutations only after deciding not to run for re-election.[8] And others, such as Winthrop Rockefeller of Arkansas (1971), had lost their re-election bids.[9] These governors may have waited until the end of their terms to avoid the perceived political costs of commutation.

In the same vein, electoral considerations have likely affected governors' decisions *not* to commute sentences. One such notable case occurred in 1992, when then-Arkansas Governor Bill Clinton denied a commutation to Rickey Ray Rector, whose suicide attempt upon his arrest for two murders left him severely mentally incapacitated.[10] Rather than commute Rector's sentence, Clinton briefly left the presidential campaign trail and flew to Arkansas to oversee the execution, signaling his support of capital punishment. Clinton's action occurred in the wake of the failed campaign of Michael Dukakis—the former Democratic presidential nominee four years prior—whom Republicans had portrayed as soft on crime (Holian 2004), and provides further evidence that governors keep a keen eye on possible public backlash when weighting whether to commute capital sentences.

The anecdotes above are consistent with our belief that governors are likely to factor political considerations into their decision of whether to commute capital sentences. There is evidence that elites in other areas of the state government have made a similar calculation in either creating or adjudicating policy relating to capital punishment. For instance, Mooney and Lee (2000) find that in re-instituting capital punishment since the 1970s, legislators in states where public opinion was closely divided tracked closely to public sentiment in crafting policy, while elites in states with more one-sided public opinion were more likely to chart their own course. Brace and Hall (1997) also find an interplay between public opinion and elite action among state supreme court justices; specifically, in states where the public is supportive of capital punishment, state supreme court

[5] "On last full day, O'Malley issues orders commuting four death-row sentences," *Washington Post*, 01/20/2015.
[6] "At End of Term, Ohio's Governor Commutes Death Sentences for 8," *New York Times*, 01/11/1991.
[7] "Anaya Spares All Inmates on New Mexico Death Row," *Los Angeles Times*, 11/27/1986.
[8] "Citing Issue of Fairness, Governor Clears Out Death Row in Illinois," *New York Times*, 01/12/2003.
[9] "Arkansas Spares All On Death Row," *New York Times*, 12/30/1970.
[10] On the day of his execution, Rector did not eat the pecan pie that he requested as a dessert for his last meal, presumably to save it for later. "Debate Rages on Executing Inmate in Wheelchair," *New York Times*, 10/30/1992.

justices are more likely to cast votes affirming capital sentences as the date of their re-election nears or if they believe themselves to be in a competitive race.

Previous work has also considered how governors treat the death penalty. Ricknell (2021) examines governors' willingness to veto death-penalty related legislation—regardless of whether it is intended expand or contract capital punishment. Ricknell does not consider the potential for electoral factors to shape governors' veto decisions, but does find a negative association between vetoes and governors serving states with gubernatorial term limits. More closely related to our study, Kubik and Moran (2003) employ a state-year level analysis, and find that defendants are more likely to be executed in election years. This is consistent with our view that governors may be less willing to commute sentences when they believe that it is especially likely that voters will punish them for doing so.

Our project will move beyond Kubik and Moran's initial contribution in important ways. First, we undertake the analysis at the governor-year-month level as opposed to the state-year level. This shift creates theoretical and empirical improvements. Changing focus from states to governors allows us to not only test the effects of election years, but more precisely the months leading up to and after elections.[11] Moreover, we can consider how attributes of the state, the defendant, *and* the governor jointly affect commutation decisions. Empirically, our more fine-grained measures allow for the inclusion of year and month fixed effects—which absorb confounding from broad, national trends in commutation over time, and seasonal trends in commutation practices that are constant across years and improve the credibility of our identification strategy. Second, we extend the period of study by almost 20 years. Third, we ensure the sample constitutes cases where the governors themselves can act directly to commute sentences, as opposed to states where governors must cooperate with other actors, such as commutation boards.

3 Expectations

We test a range of pre-registered hypotheses that examine the link between electoral conditions and governors' likelihood of commuting death sentences in their states. Specifically, we posit that governors' perceived costs of clemency actions are likely to vary across several circum-

[11] In particular, since governor elections are typically in November with the inauguration of the new or incumbent governor in January of the following year looking only at election years may include a portion of the lame duck period, which we argue is theoretically important since this is a period with decreased electoral pressure on the incumbent.

stances related to the timing of elections. If they are running for re-election, governors are likely to perceive political costs of commutations as increasingly acute as the proximity of an upcoming election date nears. Closer to an election, voters are likely to place more scrutiny on the incumbent (Przeworski et al. 1999) and take greater stock of their own situation, whether economic (Duch and Stevenson 2008), security-related (Marshall 2020; Carreras and Visconti 2020), or for other reasons. As noted, the American public has long been punitive (Alexander 2010; Enns 2016; Pfaff 2017; Tonry 2011; Weaver 2007; Whitman et al. 2003). Therefore, 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. As noted above, a number of studies have suggested that state judges (Brace and Hall 1997; Berdejó and Yuchtman 2013), legislators (Mooney and Lee 2000), and governors (Kubik and Moran 2003) take public opinion and/or electoral context into account when making decisions surrounding the death penalty. Accordingly, we anticipate that:

H1. *If the incumbent governor is running for re-election, sentences are less likely to be commuted in months closer to the election.*

These election pressures should not be present among incumbents who are not running for re-election, however. Alt, Bueno de Mesquita and Rose (2011) test classic political economy models of electoral accountability and career concerns by probing variation in U.S. term limits for governors across states and time to disentangle accountability and competence effects in these elections. They find that re-election eligible incumbents have greater incentives to exert effort on behalf of voters. Given this logic and the logic of our theory, then it follows that incumbents who do not run for re-election need not be as concerned with a tough-on-crime public image. 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 when the governor is about to exit office, as the anecdotes in Section 2 indicate. 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—has been analyzed by pundits and studied by scholars of the U.S. Congress (Jenkins and Nokken 2008). However, we hope to provide systematic evidence of its

effects in the context of the choices governors make with respect to death sentence commutation. We hypothesize that:

H3. *Governors are more likely to commute death sentences in months during their "lame-duck" period, after their successor has been elected but before that successor has been inaugurated.*

4 Data

4.1 Capital Punishment Data

We obtained data on capital sentence commutations from "Capital Punishment in the United States, 1973-2019 (ICPSR 37998)" (hereafter CPUS), which was compiled by the United States Department of Justice. These data are compiled at the defendant-year level. They include information about prisoners who are incarcerated under a death sentence in each year of the time series, as well as those whose sentences were vacated or commuted during a given year, and importantly, the month and year when their sentence ended. The data also include socioeconomic indicators about the defendant, information about the defendant's criminal history, and the status of their sentence (including commutation and execution). To protect the privacy of individual defendants, we aggregate the data to the governor year-month level.

4.2 Subsetting Capital Punishment Data

Following our pre-analysis plan, we examine only potential commutations in *state* court. We therefore exclude defendants in the federal system, as it is not possible for governors to commute those sentences. We further 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. For example, the states of Georgia, Nevada, Nebraska, and Utah only allow a board or advisory group to determine clemency. We retain data only from state-years where a governor had the authority to commute capital sentences, summarized in the appendix Table A.[12] Governor-years in states where the death penalty is not functional are set aside.

[12] We also code for governor-months after the imposition of an executive moratorium on the death penalty. These moratoriums suspend further executions, but do not otherwise change any existing convictions or sentences.

4.3 Unit and Outcome Measures

Following our pre-registration plan, the unit of analysis is the governor-year-month. We construct the outcome measure from V31 in the CPUS data ("Reason for Removal from Death Sentence") to avoid mis-classifying defendants whose sentence was overturned in a court action, or whose sentence was declared unconstitutional (3). Among the defendants who were coded as having their sentences commuted (4), we also exclude commutations that were granted for reasons of judicial expediency or as re-sentencing procedures to follow court rulings barring the use of the death penalty in the state as a whole, or for particular defendants. We choose to exclude these cases because they do not reflect agency from the governor conducting the commutation. We also track the month and year in which the sentence was commuted, which is included in the CPUS data.

To merge the CPUS data with the secondary data, we need to map this outcome to the governor-year-month unit of analysis. For the governor-year-month analyses, we first count the total number of defendants whose sentences were commuted in a given state in a given month-year. The primary outcome measure is the proportion of defendants under a death sentence in a state whose sentence was commuted in a given month. 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.

4.4 Political Data

Since our hypotheses require independent variables that are not included in the CPUS file, we merge the CPUS data into a dataset that contains state/governor-level data. The governor data take the format of governor-year-month, so when lengthening the data, an individual governor is represented in multiple rows corresponding to the number of months the governor spent in office. These data include information such as the date of the following election, the date of the beginning of the period where the governor is term-limited, the date of the beginning of the governor's lame duck period, and the governor's partisan affiliation.

5 Empirics

There are several key empirical challenges to identifying the effects of elections on clemency decisions. We tackle the question with a two-way fixed effects approach (Imai and Kim 2019).

First, to separate the effects of differing individual preferences or governor characteristics from election pressures, we rely on governor fixed effects that take into account any variation across governors. The inclusion of these fixed effects 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 stemming from national-level trends in commutation practices over time and seasonal trends in commutation that are constant across years simultaneously. We also present descriptive results to complement the pre-registered model-based results.

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. Normalizing by the total number of defendants ensures that the results are not driven by different governors (or the same governor) having different numbers of possible defendants to commute across time. However, we also present results for an outcome corresponding to 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. Robust standard errors are clustered at the governor level— the level of "treatment" assignment following Abadie et al. (2017).

This approach accounts for any additive unmeasured time-invariant confounders and is the standard for circumstances with generalized treatment regimes in longitudinal data as is our case, even though it makes parametric identification and modeling assumptions. One additional assumption this framework makes that is unlikely to be violated in our case is that past outcomes cannot affect the current treatment. Since the electoral calendars are fixed, and incumbent governors are unlikely to make re-election decisions entirely based on their commutation history, this assumption is tenable.

Given these assumptions, this estimating equation identifies the effect of electoral variables on the timing of capital punishment commutations made by the same governor in the data. The coefficient(s) of interest are β. Its associated variable(s) are different for each hypothesis in the list.

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.

For H1, which states that sentences are more likely to be commuted in the months approaching the nearest election for incumbent governors who are running for reelection, we operationalize the variable associated with β as the number of months between the current month and the nearest election. 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).

H2 states that commutations are more likely after the incumbent governor is ineligible for re-election due to term limits. Thus, 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,[13] and zero otherwise. We expect the coefficients to be positive.

Finally, H3 predicts that commutations will be more likely during the lame-duck period after an election and before an incumbent governor leaves office. In this case, the variable associated with the coefficient of interest will be equal to 1 during the months of the lame duck period and zero otherwise.

6 Results

Before turning to the regression based results, Figure 2 presents descriptive results for each of the three hypotheses described above. Rows in Figure 2 correspond to each hypothesis. Columns reflect two separate outcomes. The left column presents the average proportion of commutations—that is, the number of commutations in a particular month divided by the number of defendants on death row in that same month—across all governors in the subsample for that hypothesis. The right column follows a similar structure but uses as an outcome a dummy for a commutation decision in that particular month, and so describes the proportion of governors who commuted at least one sentence at different points in their term.

Overall, there is little evidence to suggest that governors running for re-election in states where

[13] Note that we cannot identify effects for a single-term governor who does not run for re-election.

the governor has discretion over whether or not to commute sentences time their commutation decisions to fall toward the beginning of their terms and further away from the nearest election. The LOESS curve we fit to the points shows no clear timing in commutation patterns based on the proximity to elections.

The middle panel of Figure 2 tells a similar story for the relationship between term limits and commutation patterns among governors who were ever eventually term-limited. Here, we separately fit a LOESS curve at each side of the 0 mark, which denotes the point where a governor becomes term-limited. For instance, for a governor in a state with a two-term term limit, this would be the month of the governor's re-election. Therefore, negative values along the x-axis describe term-limited governor-months, while positive values describe non-term limited governors. No clear pattern emerges to suggest that term-limited governors are more likely to commute sentences than non-term limited governors.[14]

Finally, the bottom panel of Figure 2 describes commutation patterns from the beginning of any 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. The left portion of this bottom panel provides weakly suggestive evidence that governors commute more sentences during their lame duck period, but the right portion of this bottom panel demonstrates that the proportion of governors who commuted at least one sentence is comparable to the averages in other months during the term.[15]

Table 1 presents naive comparisons by dichotomizing the predictors which correspond to H2 and H3. Aggregating across all governor-months for which a governor is term-limited or a lame

[14]For ease of exposition, in this middle panel, we group together all governor-months with greater than 48 months—four years—to becoming term-limited into the value of 48 months to becoming term-limited. These governors are Bob Miller of Nevada, Kit Bond of Missouri, Mike Huckabee of Arkansas, and Roy Romer of Colorado. The results of the unedited figure are presented in Figure A3.

[15]As with the middle panel, we group governors-months that are greater than four years away from becoming a lame duck all together. Approximately 120 governors compose each of the data points for governors in months that are less than four years. For eight years, this number drops down to approximately 89, and even longer than that, approximately 10 governors and down to compose each of the data points for the outcome variables. Given the nested structure of the "months-to-lame-duck" variable, all these longer-serving governors are not excluded in this figure, we just simply focus on their last four years in office. We also group governors with values of the "months-to-lame-duck" variable that are *less than* -2 together into the value of -2. These correspond to two governors with long lame duck period, neither of whom made any commutations in the later portions of their lame duck period: Rose Mofford of Arizona and David Treen as well as Edwin Washington Edwards of Louisiana. That being said, the interpretation of the results is not substantively different before incorporating these changes, and so we present the unedited figure in Figure A3.

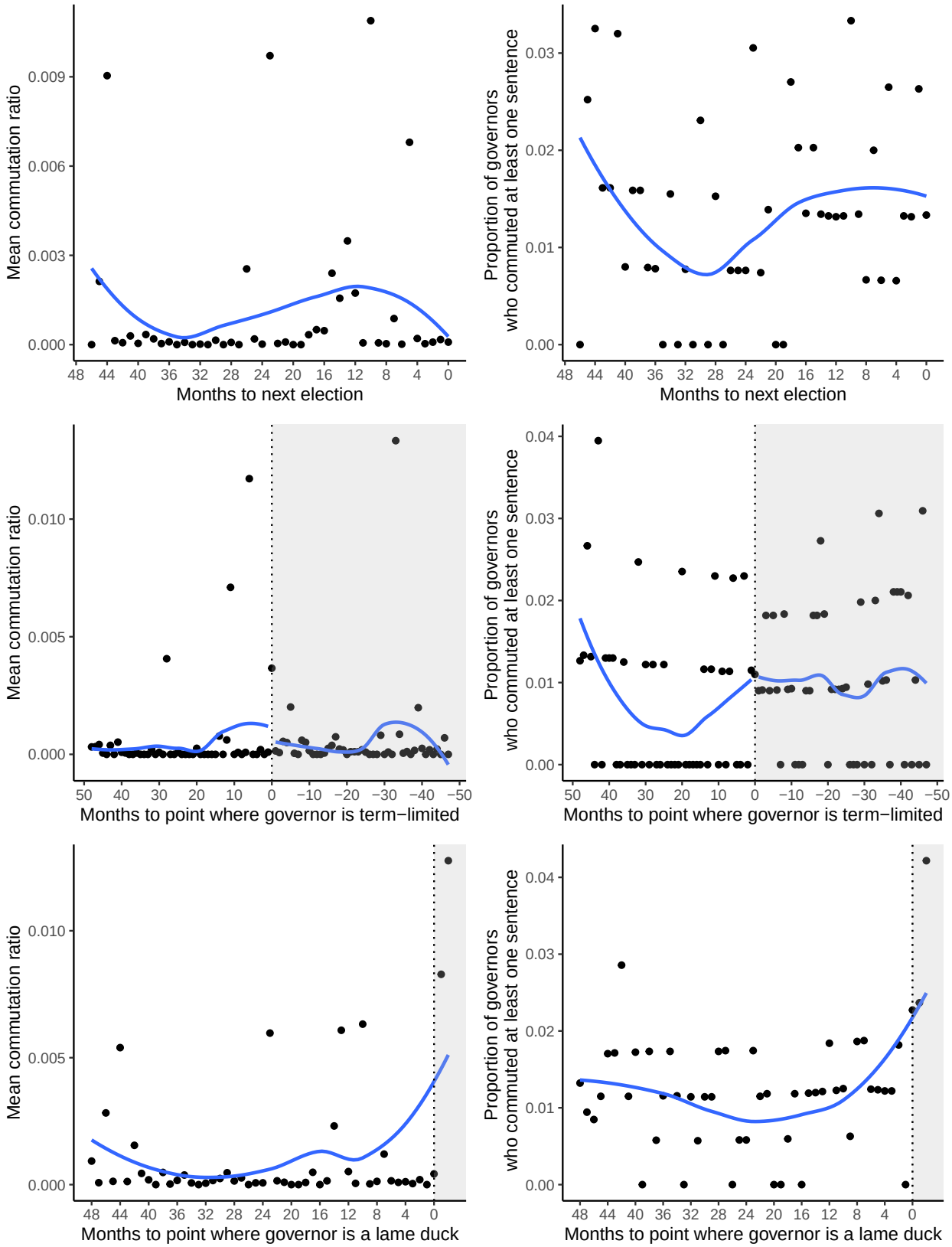

Figure 2: Descriptive results for electoral hypotheses.

	Commutation Ratio	Commutation Dummy	Number of governors
Not term-limited	0.0007	0.0076	100
Term-limited	0.0005	0.0104	132
Not a lame duck	0.0008	0.0121	240
Lame duck	0.0105	0.0328	211

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

duck, inclusive of the "zeroth" month,[16] reveals that term-limited governors are about as likely to commute sentences as non-term-limited governors, and also do not commute a higher proportion of sentences (when they do so) than their non-term limited counterparts.[17] The differences are more pronounced when comparing across lame duck governors and non-lame duck governors. On average, lame duck governors are about three times more likely to commute sentences as non-lame duck governors, and commute a much higher proportion of defendants' sentences when they do decide to commute sentences.

We present pre-specified regression results in Table 2, which include governor fixed effects to make comparisons across the same governor at different points in their term, and year-month fixed effects to absorb national shocks to commutation patterns over time. Columns (1), (3), and (5) present results using the commutations ratio outcome: the number of people whose sentences were commuted in a particular month by a particular governor divided by the number of people under a sentence of death in that same governor-month. Columns (2), (4), and (6) present results using the commutations dummy outcome, which is equal to zero if no sentences were commuted under a particular governor-month and one if at least one sentence was commuted under a particular governor-month, conditional on there at least being one person on death row.

We do not find sufficient evidence to reject the null hypothesis for Hypotheses 1 or 2. The estimated coefficients for the predictors for H1 and H2 are quite small in magnitude and are estimated to be in the "wrong" direction, though they fall well short of the conventional threshold for statistical significance. 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 find evidence of the lame duck period increasing the likelihood of commutations, as

[16] Table A2 presents similar results that are exclusive of the month of the status change.

[17] For conceptual clarity, we exclude term-limited governor months where the governor was also a lame duck.

	Ratio (1)	Dummy (2)	Ratio (3)	Dummy (4)	Ratio (5)	Dummy (6)
Months to election	-0.00005	-0.00011				
	(0.00005)	(0.00024)				
Term-limited			-0.00107	-0.00086		
			(0.00084)	(0.00282)		
Lame duck					0.00839*	0.02256**
					(0.00490)	(0.01091)
"Control" outcome mean	0	0.013	0.001	0.008	0.001	0.012
"Control" outcome std. dev	0.001	0.115	0.019	0.087	0.022	0.109
R^2	0.2	0.2	0.1	0.11	0.08	0.12
Observations	6,370	6,370	9,059	9,059	13,278	13,278
Number of governors	146	146	121	121	214	214

* 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: Main regression results.

well as evidence that governors in the lame duck period commute a larger proportion of sentences, however. In both models, the coefficients for lame duck period are statistically significant in the expected direction. Of the two dependent variables, the lame duck period shows a relatively larger effect for the commutation ratio, corresponding to about one-third of a standard deviation increase based on the standard deviation of the outcome in the whole sample, compared to 20% of a standard deviation when considering the commutation dummy outcome.

It is possible for the overall zero estimates with respect to H1 and H2 to be explained by heterogeneity in treatment effects across different groups. In our pre-registration, we intended to test for heterogeneous effects of electoral pressure across incumbents with different previous margins of victory, incumbents with prior judicial experience and incumbents without prior judicial experience, across party lines, across the race of defendants, and across the race of the defendant's victims. Given our lack of power in the regression results, we do not test for these differential effects in the originally proposed form of regression interactions and only report descriptive patterns across different parties of governors and race of defendants.[18] Thus, we report figures which

[18]We exclude data on the race of defendant's victims because this information is not included in the original ICPSR data and the data-sharing agreement we signed with the Department of Justice precludes us from attempting to identify any of the individuals listed in the data to include additional variables.

are analogous to Figure 2 in the appendix, but disaggregate the points by party of governors and race by defendants in Figures A1 and A2. These results show that the overall lack of patterns in commutation decisions by electoral timing are fairly similar across these two variables, although Democrats appear to have an increased proclivity to commute sentences to Republicans, and the Figure is weakly suggestive of the lame duck results being driven by governors of this party, and more speculatively, among black defendants. This would be consistent with an interpretation that Democrats are more bound by electoral pressures to be "tough-on-crime" than Republicans.

6.1 Robustness checks

The appendix includes a battery of robustness tests that bolster the main results and further probe different possible interpretations. Table A3 separates lame duck governors who became lame ducks after just one term with those who became lame ducks after more than one term. This tests for whether less successful governors (who may potentially have less future career concerns than their more successful counterparts) are driving the lame duck results. We can see that the two types of governors behave relatively similarly in terms of commutation patterns, suggesting the main results are not driven solely by these less successful governors.

We next probe the 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 A4 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 A4 does not condition on their being any defendants on death row. The results are substantively similar.

A related worry 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 further show in Figure A4 results from regressions which were not pre-registered that include different forms of time fixed effects but exclude governor fixed effects that are comparable to the main results: across many of the hypotheses, the coefficient estimates from the models do not attain statistical significance at the $\alpha = 0.05$, level, with the exception of the lame duck variable in the models without any fixed effects and year fixed effects only. We also present Figure A5, 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. This robustness check was not pre-registered.

7 Conclusion

Using data on the entire universe of defendants on death row from 1973-2019, we find limited evidence that governors are responsive to patterns of electoral timing 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. Given the overall inconclusive results, we are left to wonder whether these stem from a lack of power, or true very small effects from proximity to elections and term-limits. The descriptive results point toward the latter, although they are themselves limited by the nature of unconditional comparisons. On the other hand, our theory suggested that governors will be more likely to commute sentences during periods of less intense political pressure. It is also possible that proximity to elections and term-limitations simply correspond to less intense reductions in political pressure relative to the lame duck period. Future research can and should put commutation decisions on the right-hand-side of estimating equations as opposed to what we did in terms of attempting to predict commutation patterns as an outcome. This may provide us 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. Further research should investigate the potential interaction between party and race we suggest in 6, perhaps using a small-N qualitative case study approach.

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A 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, 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 A 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 A6. The main results aggregate all forms of authority except for “sole authority" together. Alaska, Hawaii, 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 since before 1973 and so no defendants from these states are included in our data.

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
Delaware	1973-2016: Must have recommendation
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
Indiana	1973-2019: May receive recommendation
Iowa	
Kansas	1973-2019: May receive recommendation
Kentucky	1973-2019: Sole authority
Louisiana	1973-2019: Must have recommendation
Maryland	1973-2013: May receive recommendation
Massachusetts	
Mississippi	1973-2000: May receive recommendation
	2000-2019: Sole authority
Missouri	1973-2019: May receive recommendation
Montana	1973-2019: May receive recommendation
Nebraska	1973-2019: No authority
Nevada	1973-2019: No authority
New Hampshire	1973-2019: May receive recommendation
New Jersey	1973-2017: Sole authority
New Mexico	1973-2009: Sole authority
New York	1973-2004: Sole authority
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	1973-2019: Sole authority
Wyoming	1973-2019: Sole authority

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

B Descriptive Results by Party

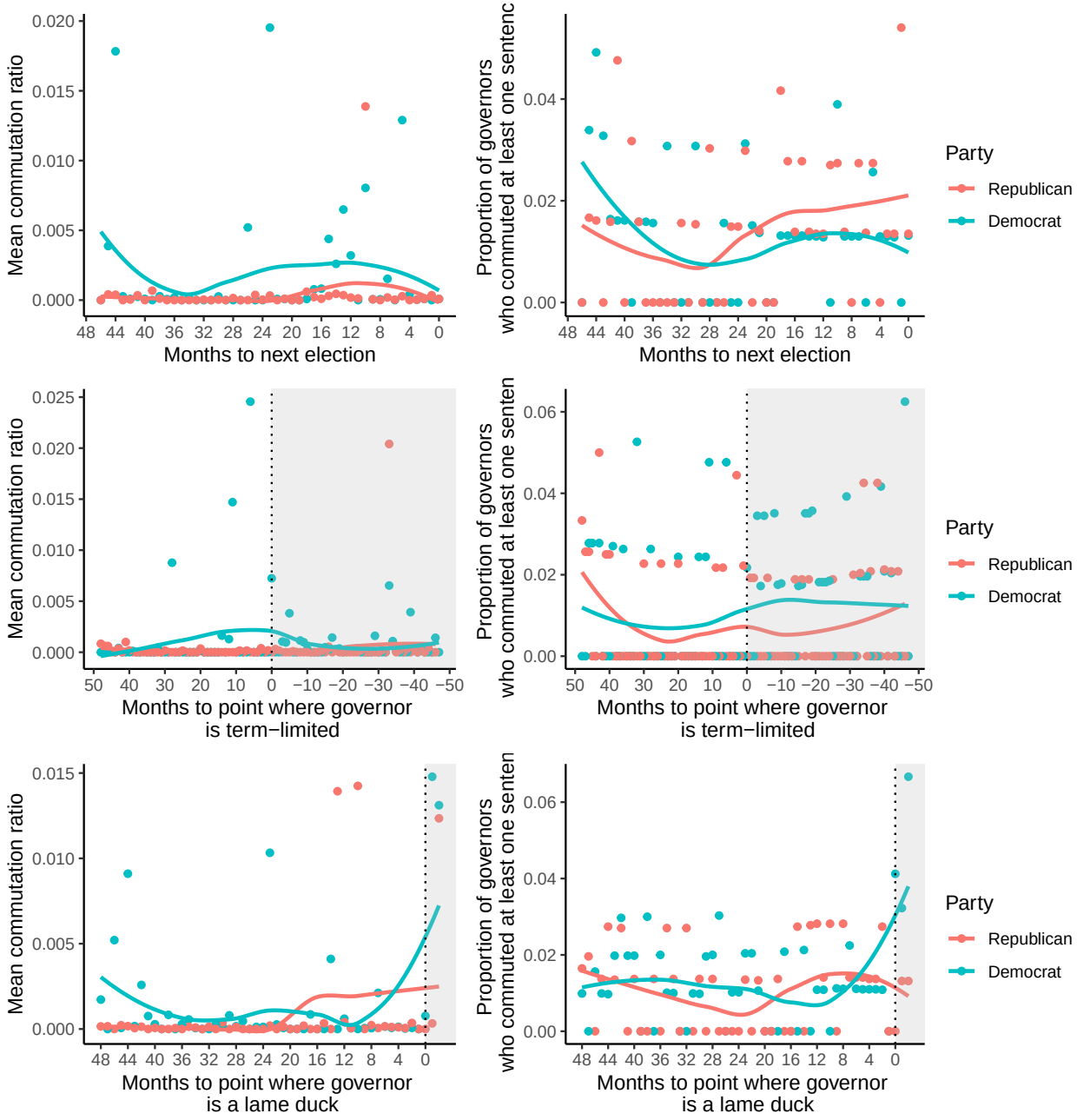

Figure A1: Descriptive results for electoral hypotheses by party.

C Descriptive Results by Race

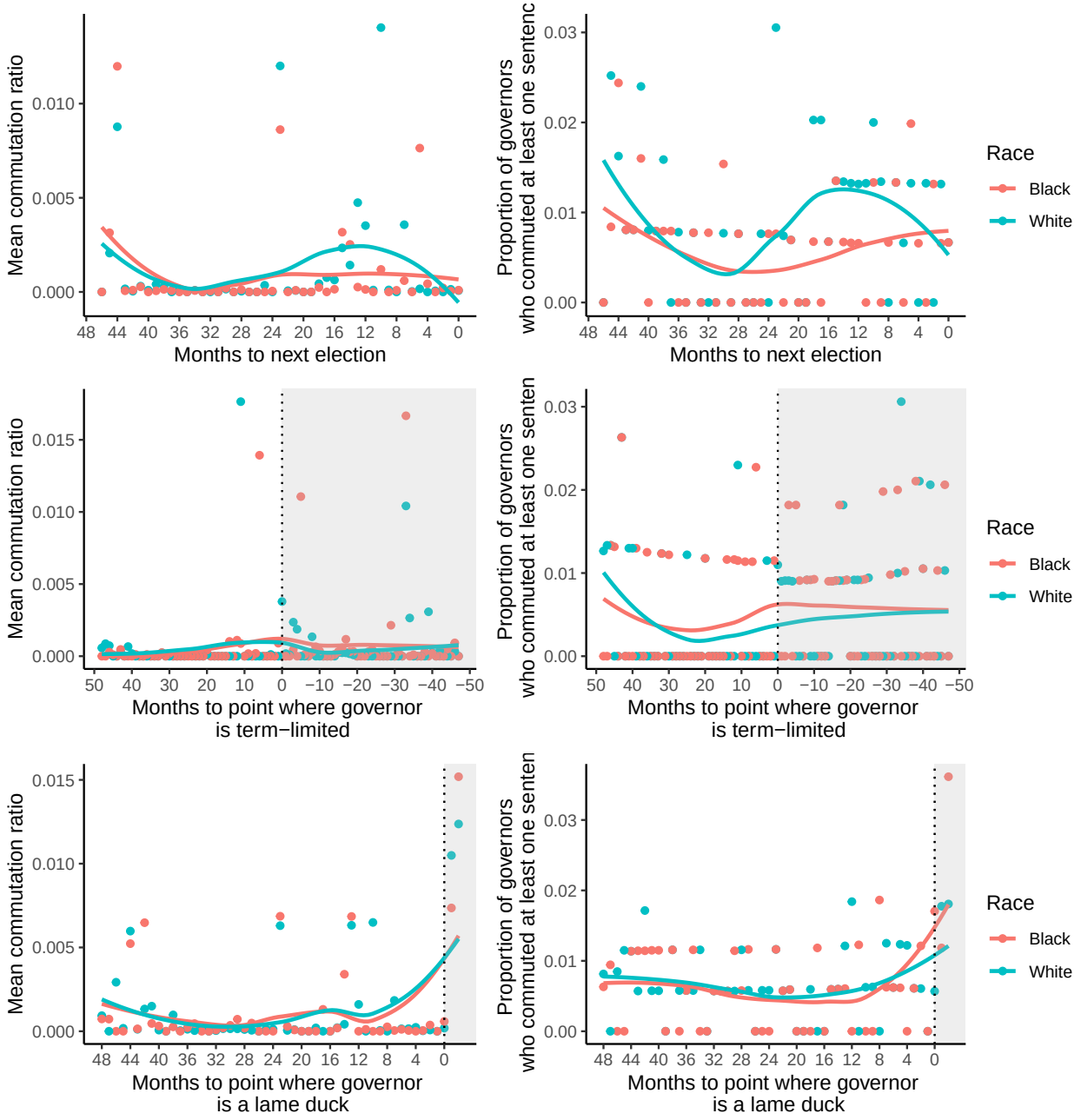

Figure A2: Descriptive results for electoral hypotheses by race.

D Descriptive Results Robustness Checks

Figure A3 presents results for the H2 and H3 panels of Figure 2, but does not aggregate governors together in any way, even if there are very few governors for a particular month. Table A2 presents results from Table 1, but excludes the zeroth month when defining the "treatment."

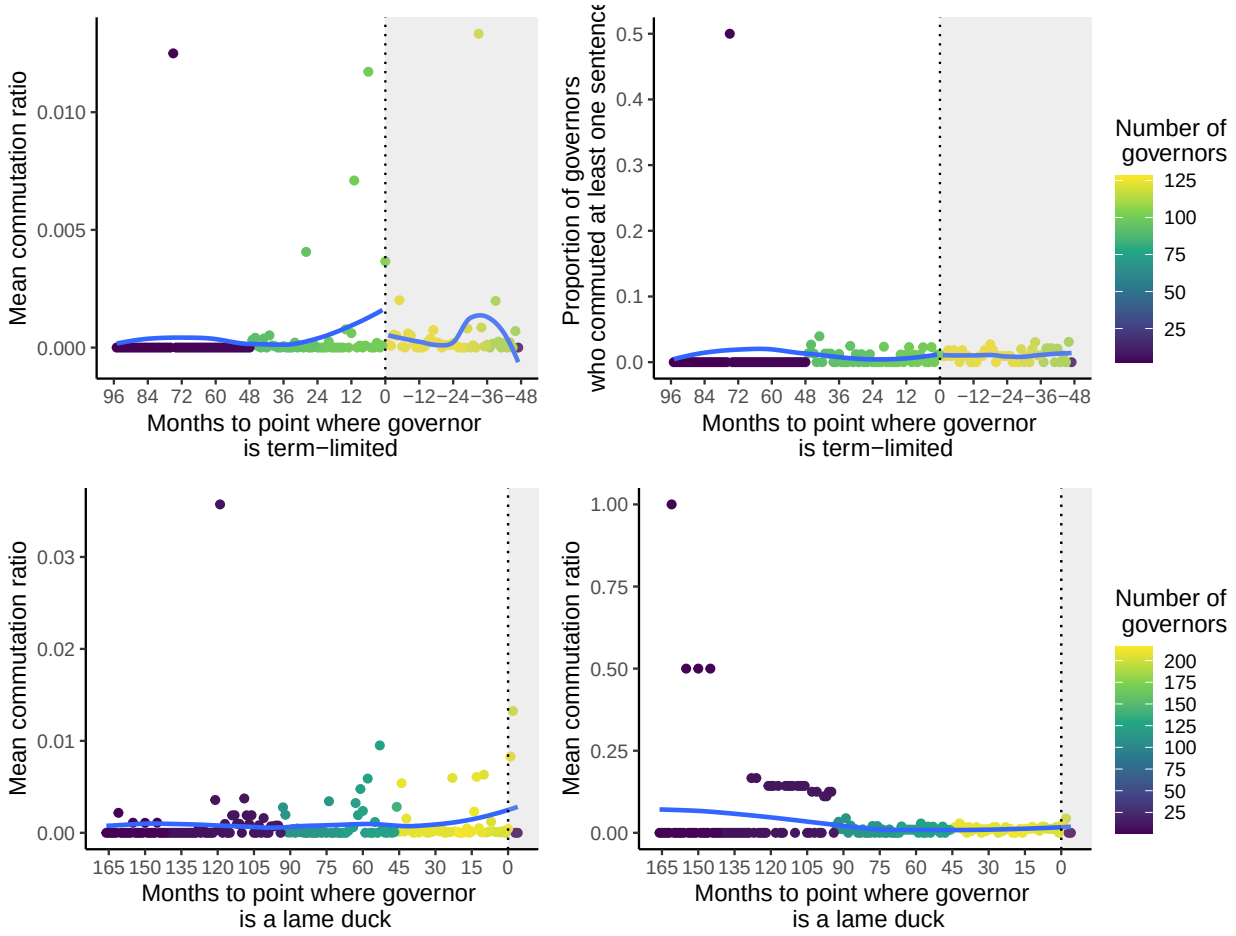

Figure A3: Descriptive results for electoral hypotheses. LOESS curves are weighted by the number of governors that compose the mean in that month.

	Commutation Ratio	Commutation Dummy	Number of governors
Not term-limited	0.0006	0.0076	98
Term-limited	0.0011	0.0114	132
Not a lame duck	0.0008	0.0119	232
Lame duck	0.0070	0.0294	216

Table A2: Naive commutation comparison across term-limited and non-term-limited and lame duck and non-lame duck governors, exclusive of the zeroth month.

E Regression Results Robustness Checks

Figure A4 compare regressions that exclude governor fixed effects across different forms of time fixed effects.

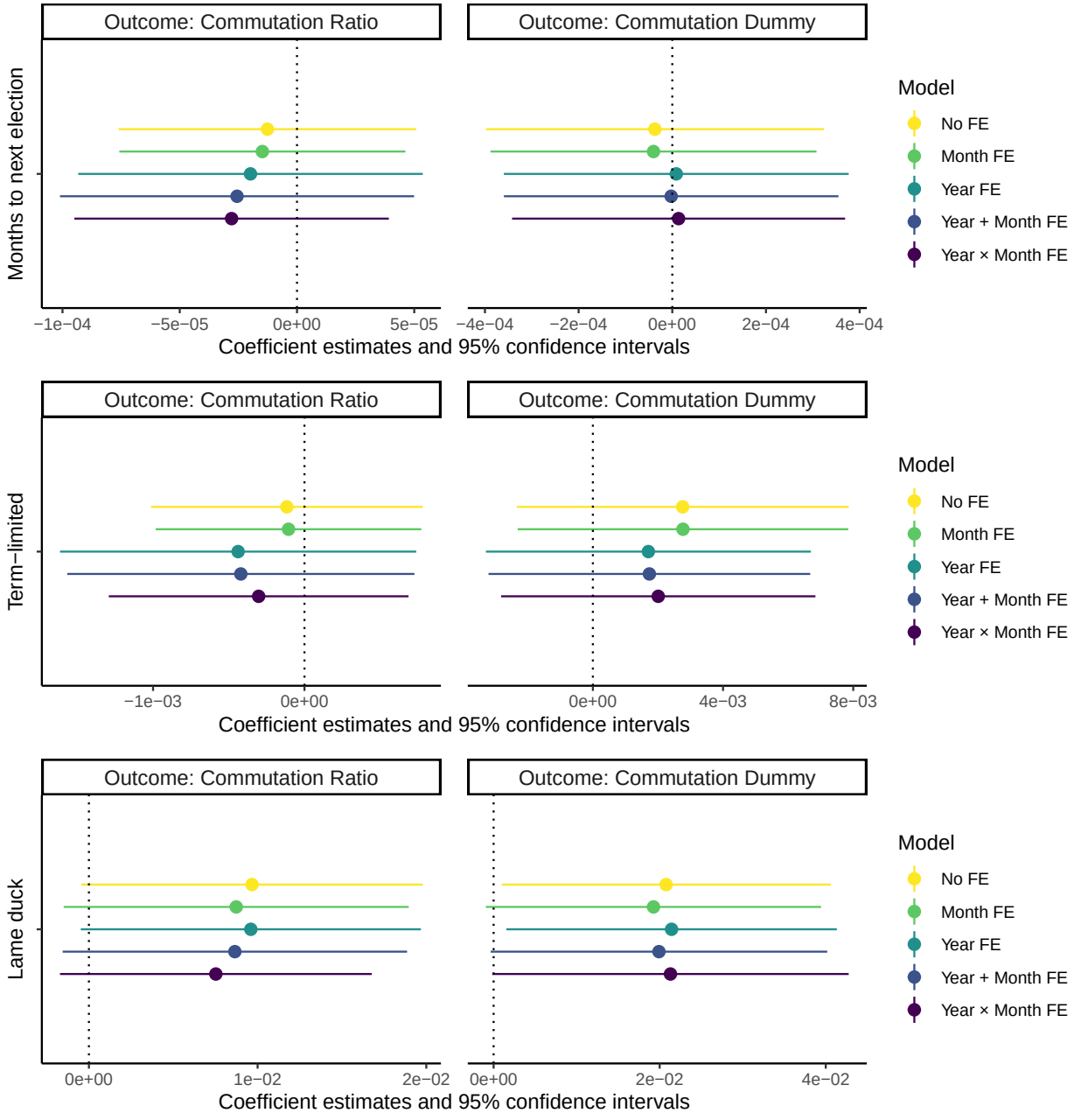

Figure A4: Robustness of regression results to exclusion of governor fixed effects specifications.

Figure A5 compares regressions that include governor fixed effects across different forms of time fixed effects.

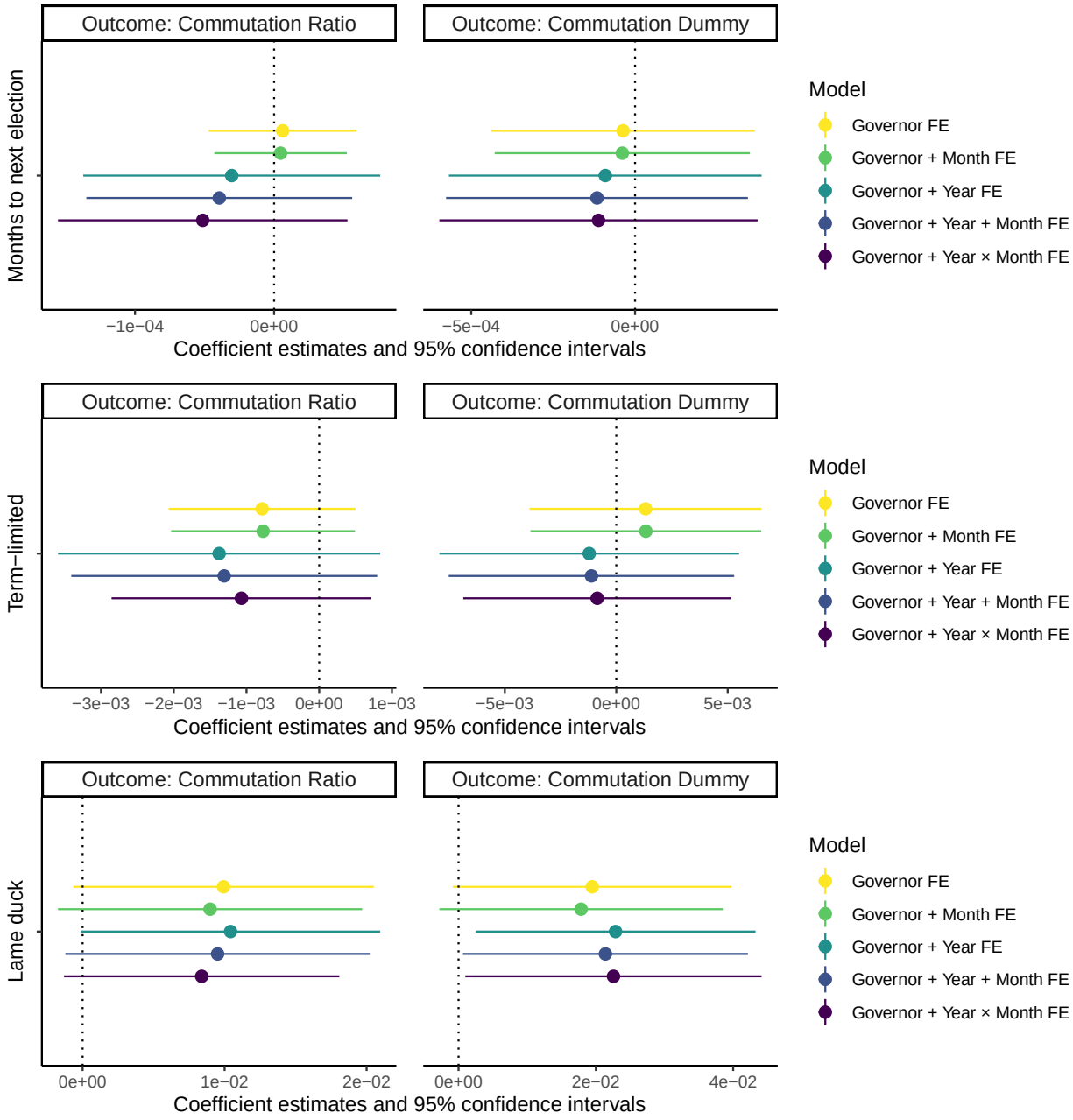

Figure A5: Robustness of regression results to different governor fixed effects specifications.

Figure A6 presents models across subsets of the data with different forms of governor authority over commutation decisions.

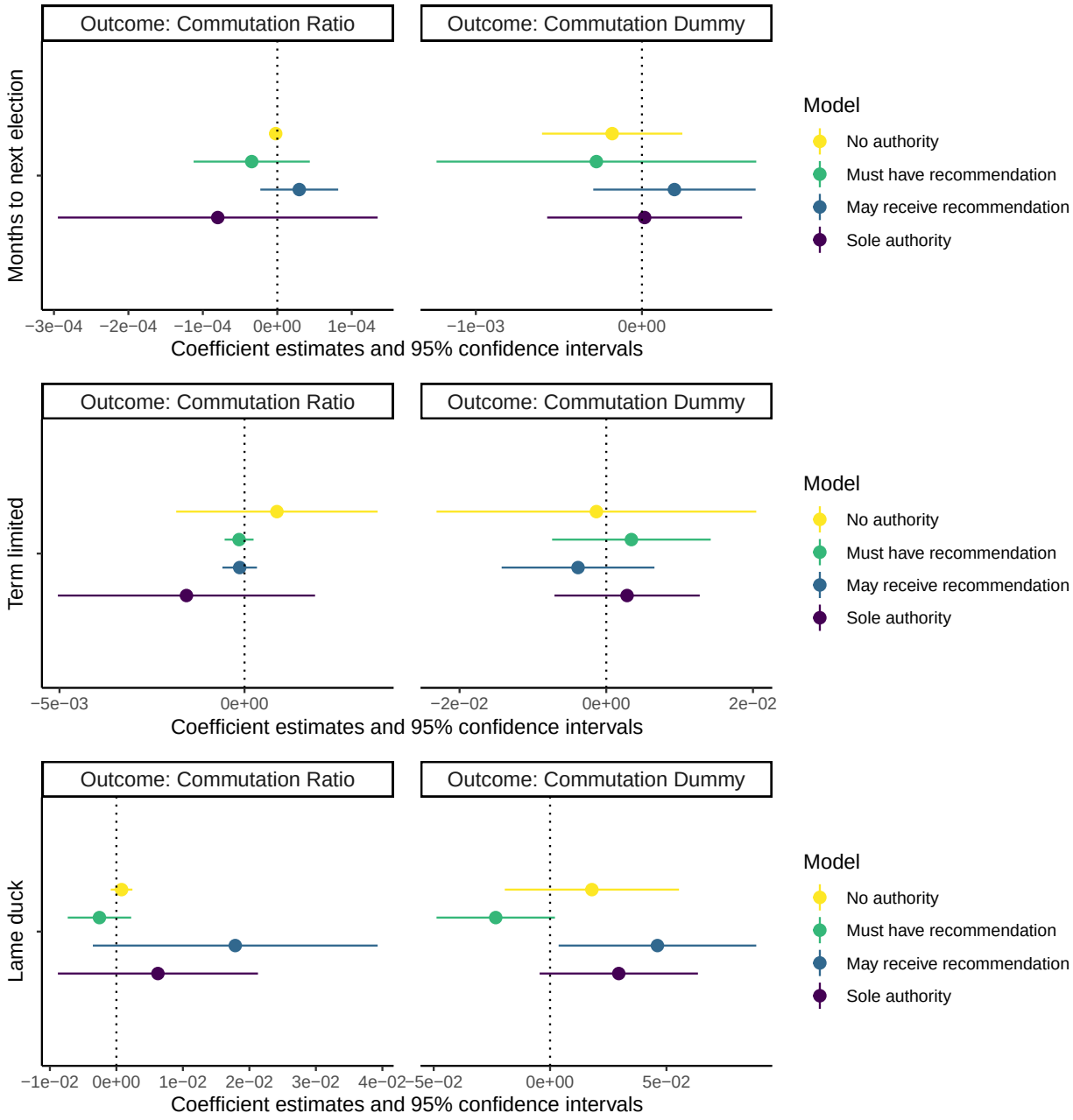

Figure A6: Robustness of regression results to subsetting to different definitions of governor authority.

	Ratio (1)	Dummy (2)	Ratio (3)	Dummy (4)
Lame duck - First Term	0.01031	0.02672		
	(0.00993)	(0.01707)		
Lame duck - After First Term			0.00791	0.01766
			(0.00606)	(0.01435)
"Control" outcome mean	0.001	0.013	0.001	0.011
"Control" outcome std. dev	0.027	0.114	0.021	0.102
R^2	0.19	0.29	0.28	0.19
Observations	3,088	3,088	4,647	4,647
Number of governors	93	93	118	118

* 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 A3: Regression results, disaggregating lame duck.

	Count (1)	Dummy (2)	Count (3)	Dummy (4)	Count (5)	Dummy (6)
Months to election	0.00007	0.00008				
	(0.00043)	(0.00020)				
Term-limited			0.00514	0.00147		
			(0.00451)	(0.00273)		
Lame duck					0.33935	0.01745*
					(0.29716)	(0.00916)
"Control" outcome mean	0.01	0.01	0.007	0.007	0.013	0.01
"Control" outcome std. dev	0.1	0.1	0.092	0.081	0.192	0.099
R^2	0.12	0.18	0.09	0.1	0.05	0.11
Observations	8,159	8,159	10,591	10,591	16,211	16,211
Number of governors	172	172	132	132	251	251

* 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 A4: Regression results when including months with no commutations and controlling for total defendants on death row on the right-hand-side. Outcome is count of commutations and commutation dummy.