Online Appendix: The Electoral Dynamics of Capital Punishment Commutations

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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,[1] 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.

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[1]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.[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 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]
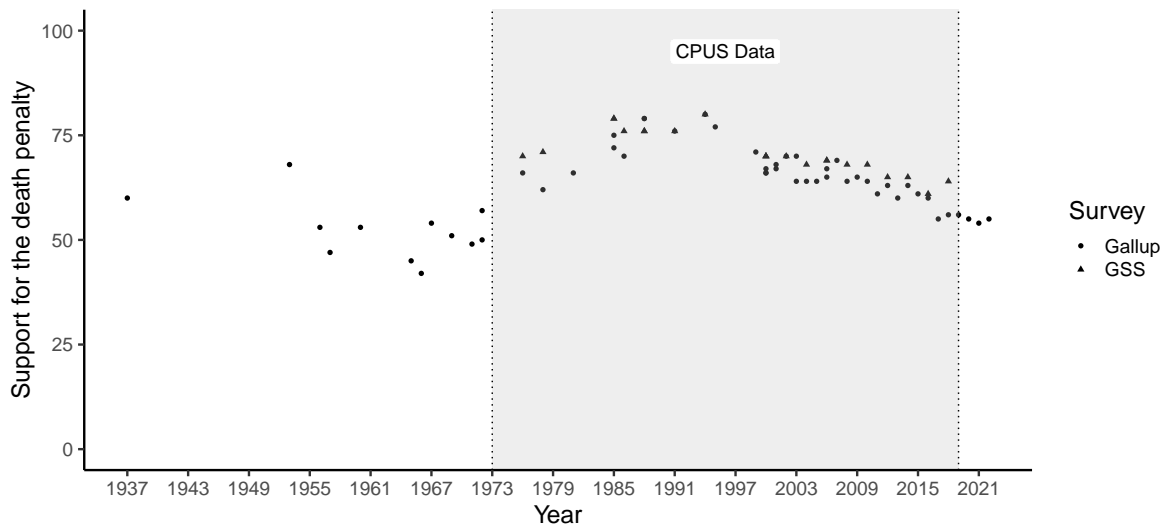

Figure A1: Percentage of respondents in favor of the death penalty, Gallup and GSS.

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[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?"

[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 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.
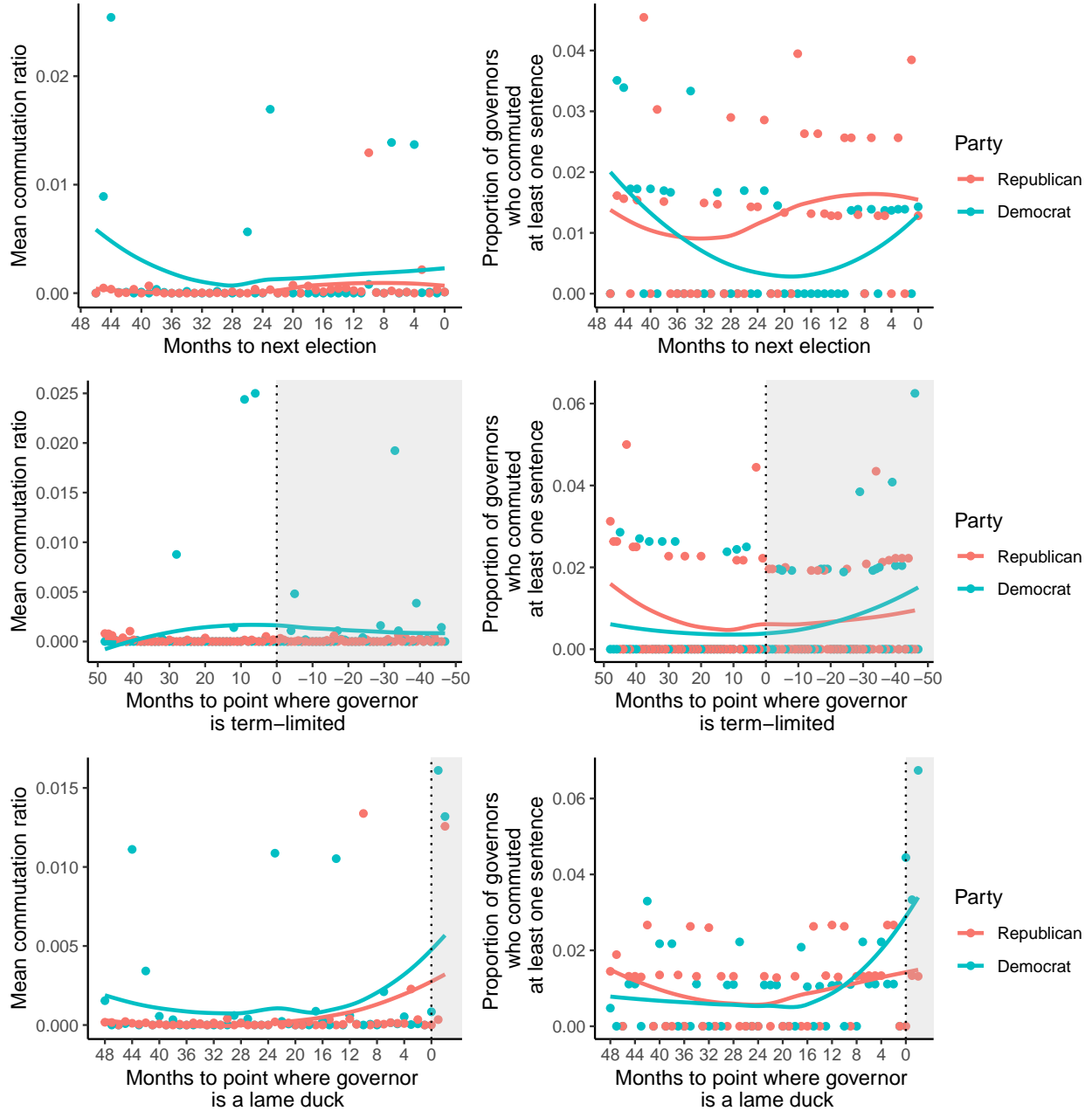

Figure A2: Descriptive results for electoral hypotheses by party.

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.
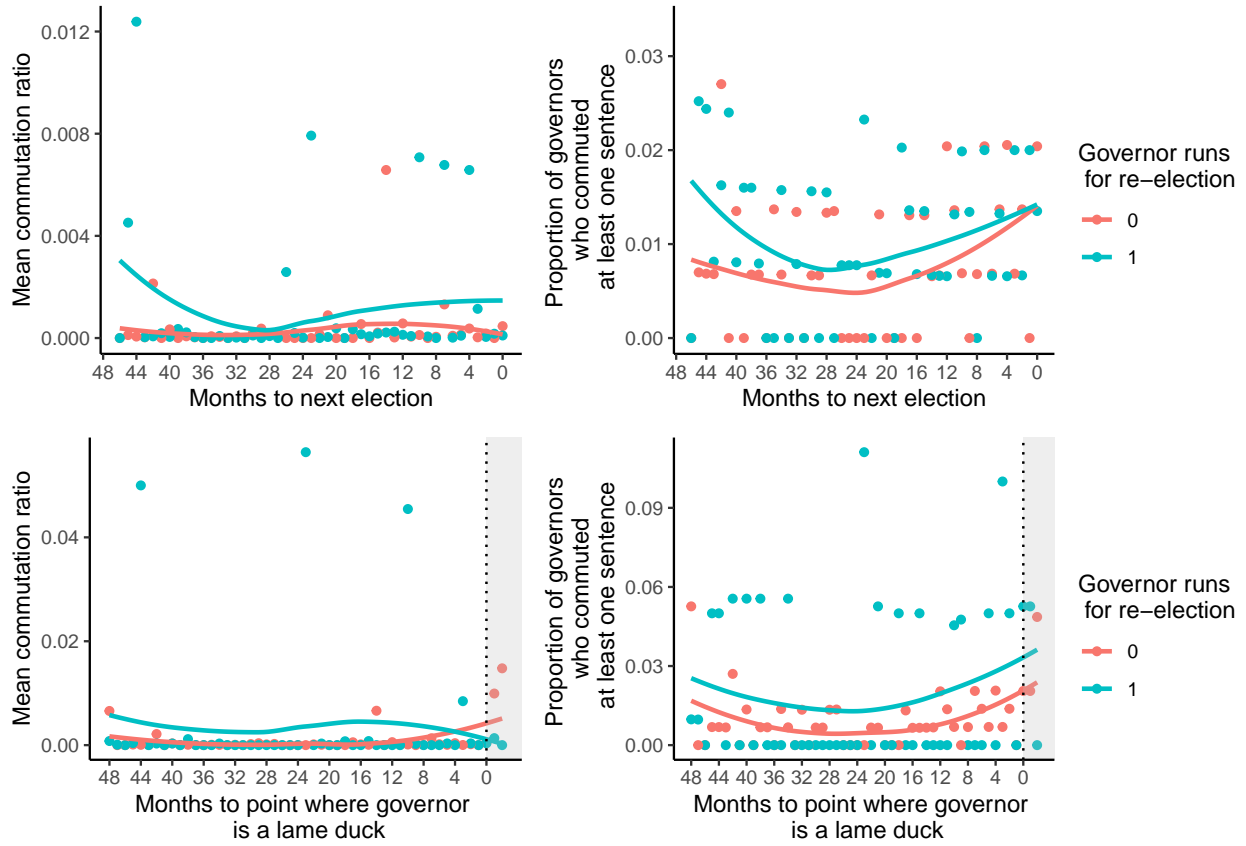

Figure A3: Descriptive results for electoral hypotheses by whether the governor runs for re-election.

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).
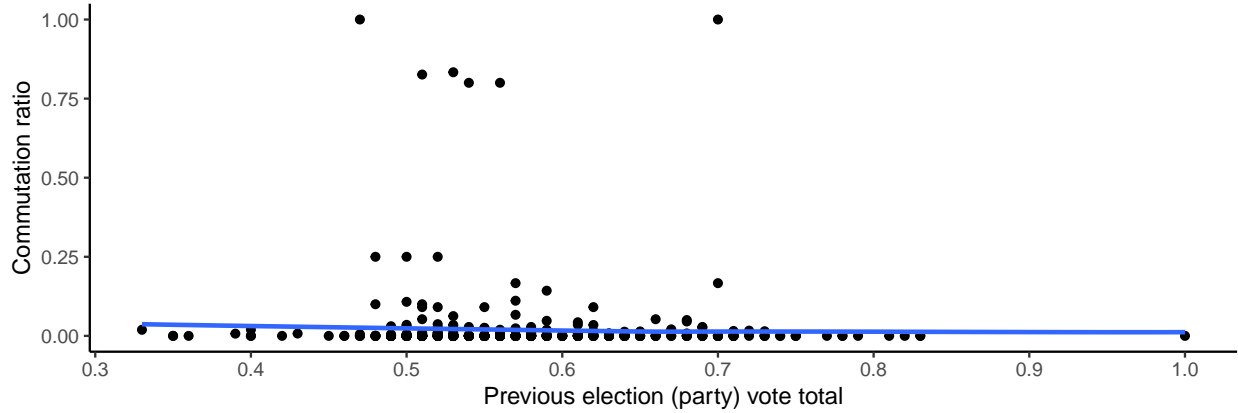

Figure A4: Descriptive results for previous vote share.

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.
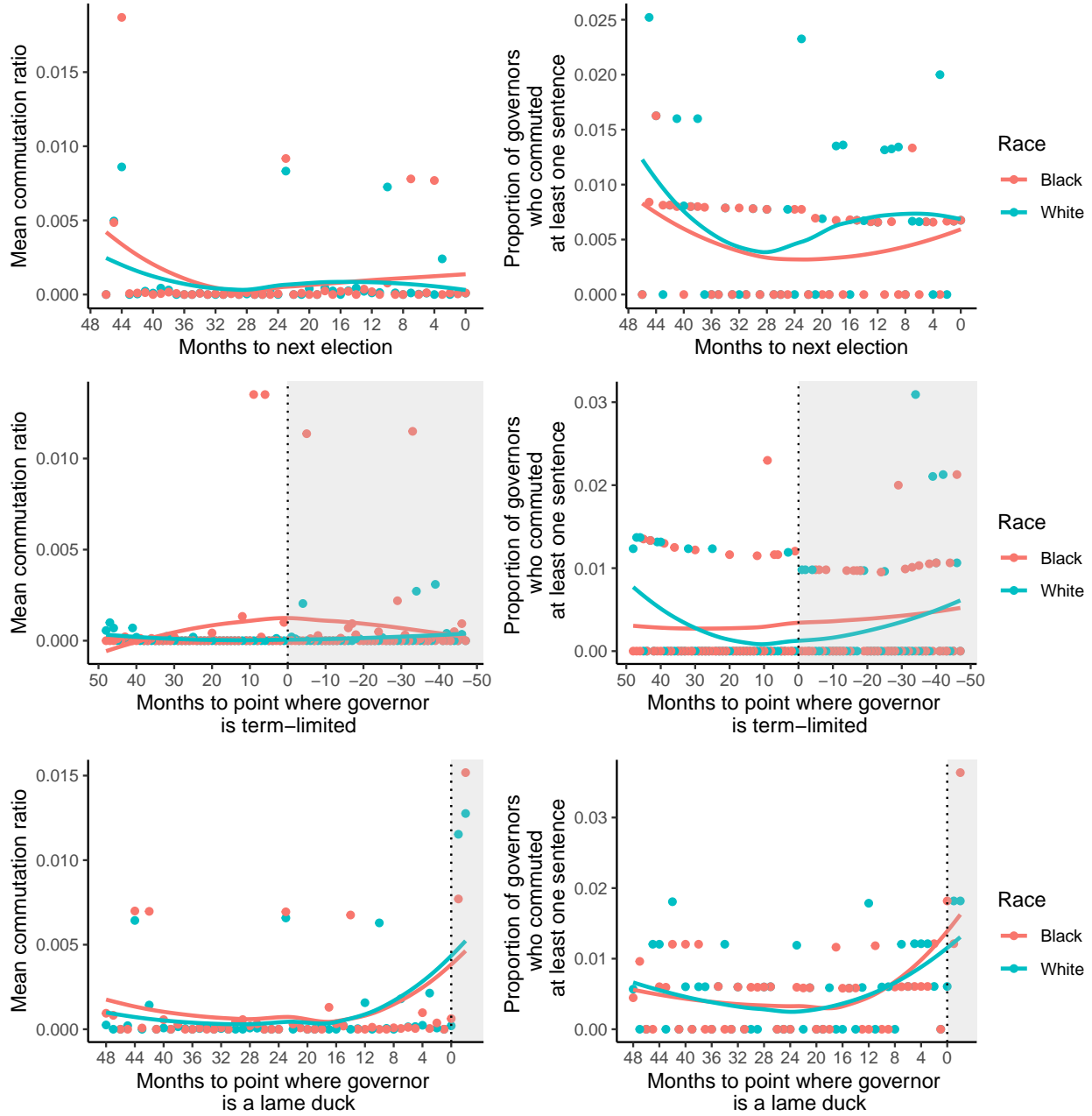

Figure A5: Descriptive results for electoral hypotheses by race, black and white defendants.

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.
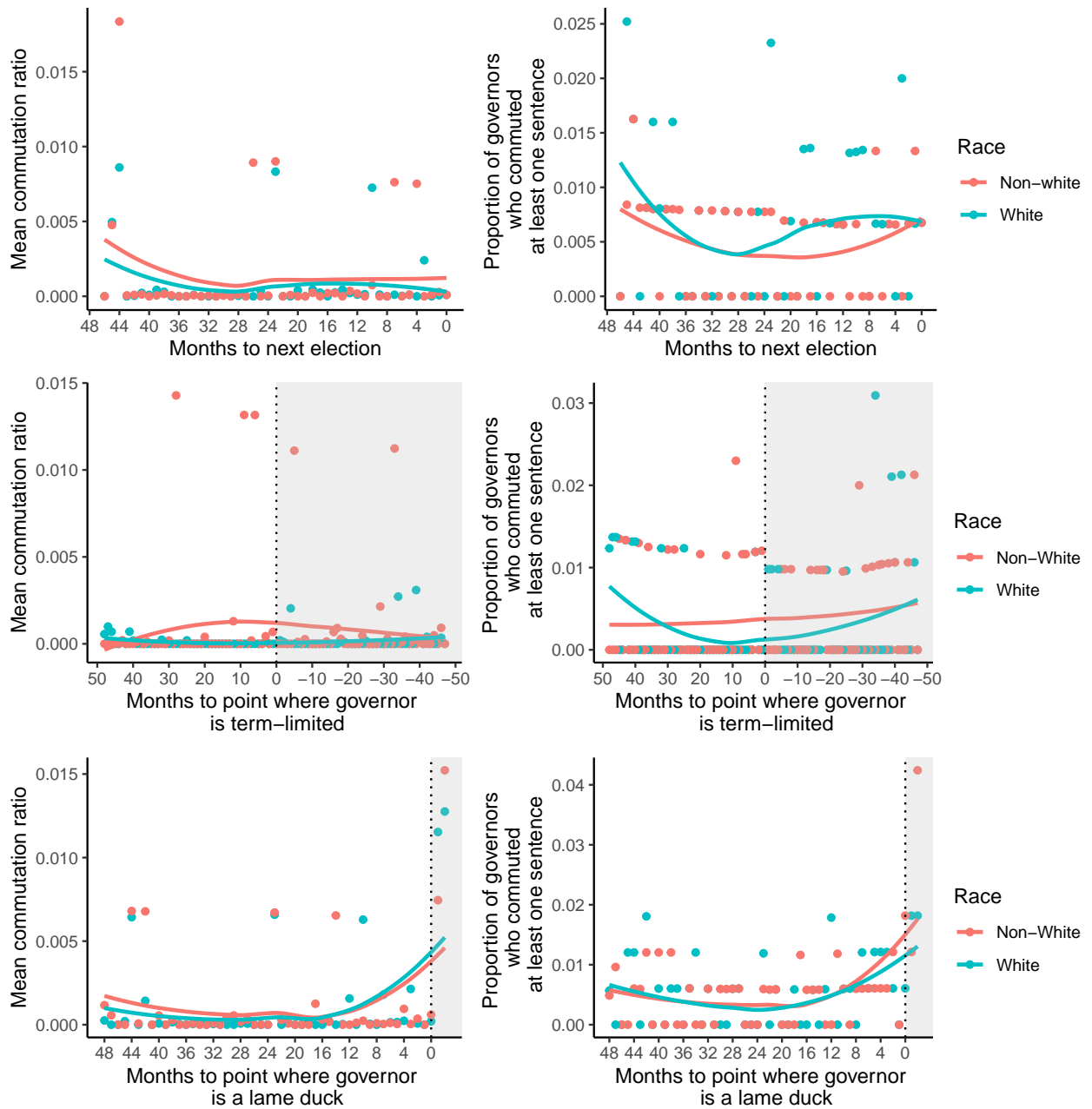

Figure A6: Descriptive results for electoral hypotheses by race, non-white and white defendants.

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 in the main text, 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 in the main text, but excludes the zeroth month when defining the "treatment."
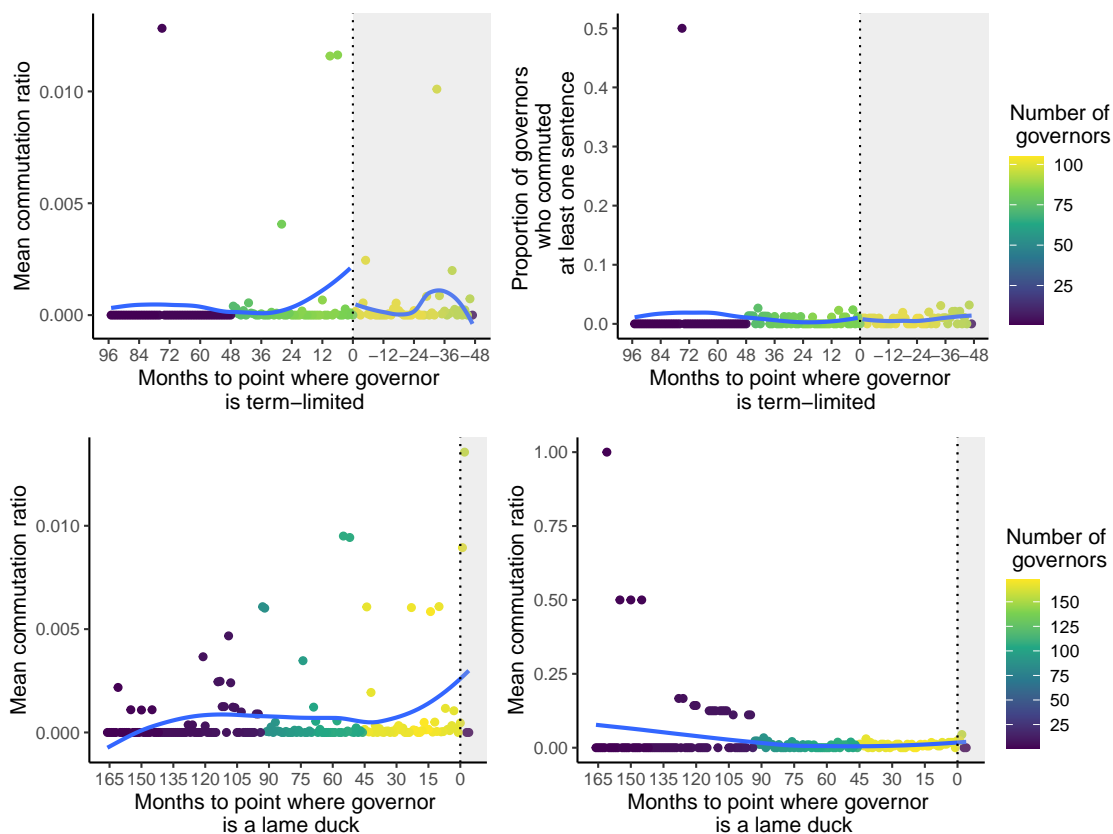

Figure A7: Descriptive results for electoral hypotheses. LOESS curves are weighted by the number of governors that compose the mean in that month.

	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 of the main text. 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.
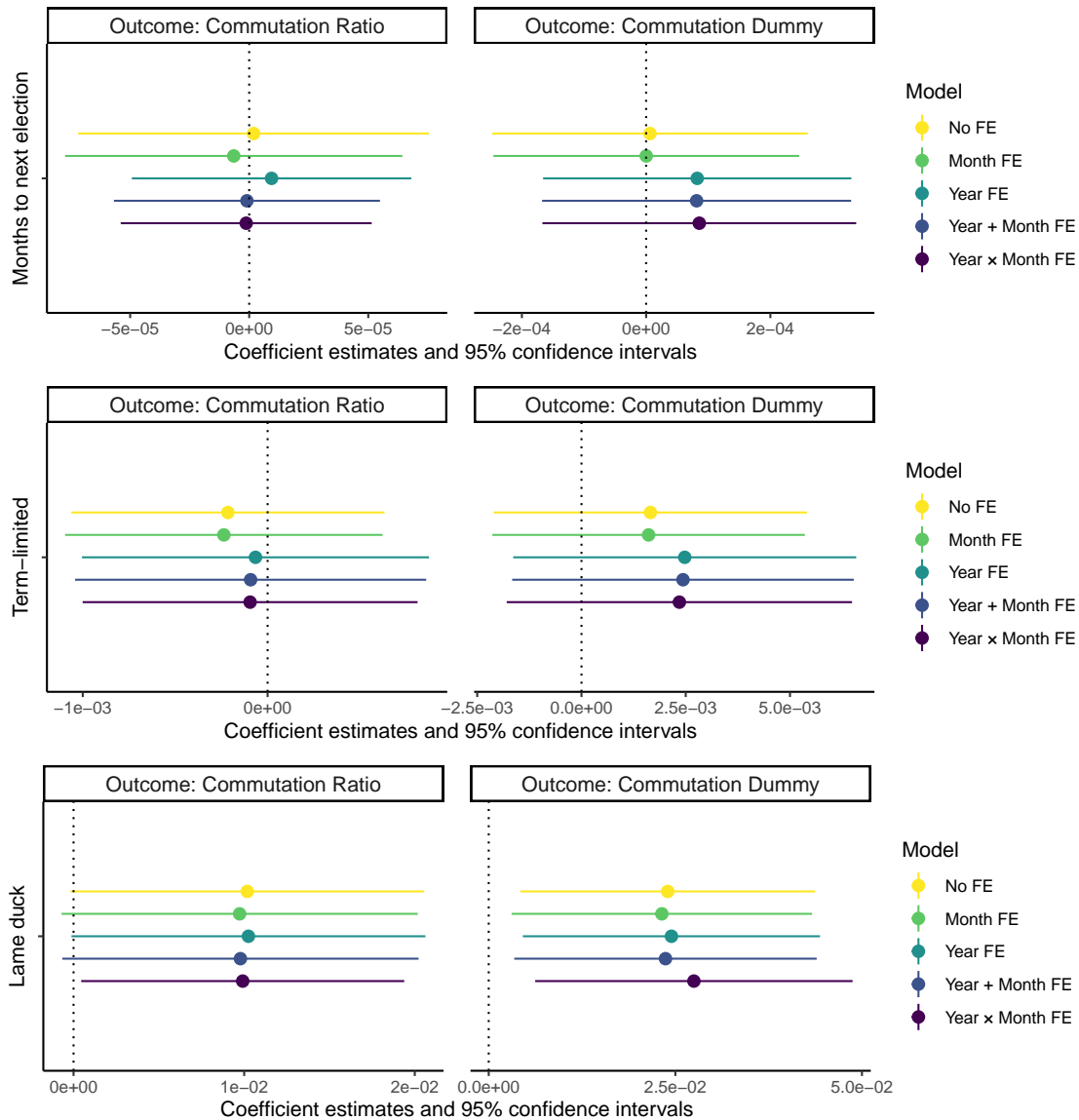

Figure A8: Robustness of regression results to exclusion of governor fixed effects specifications.
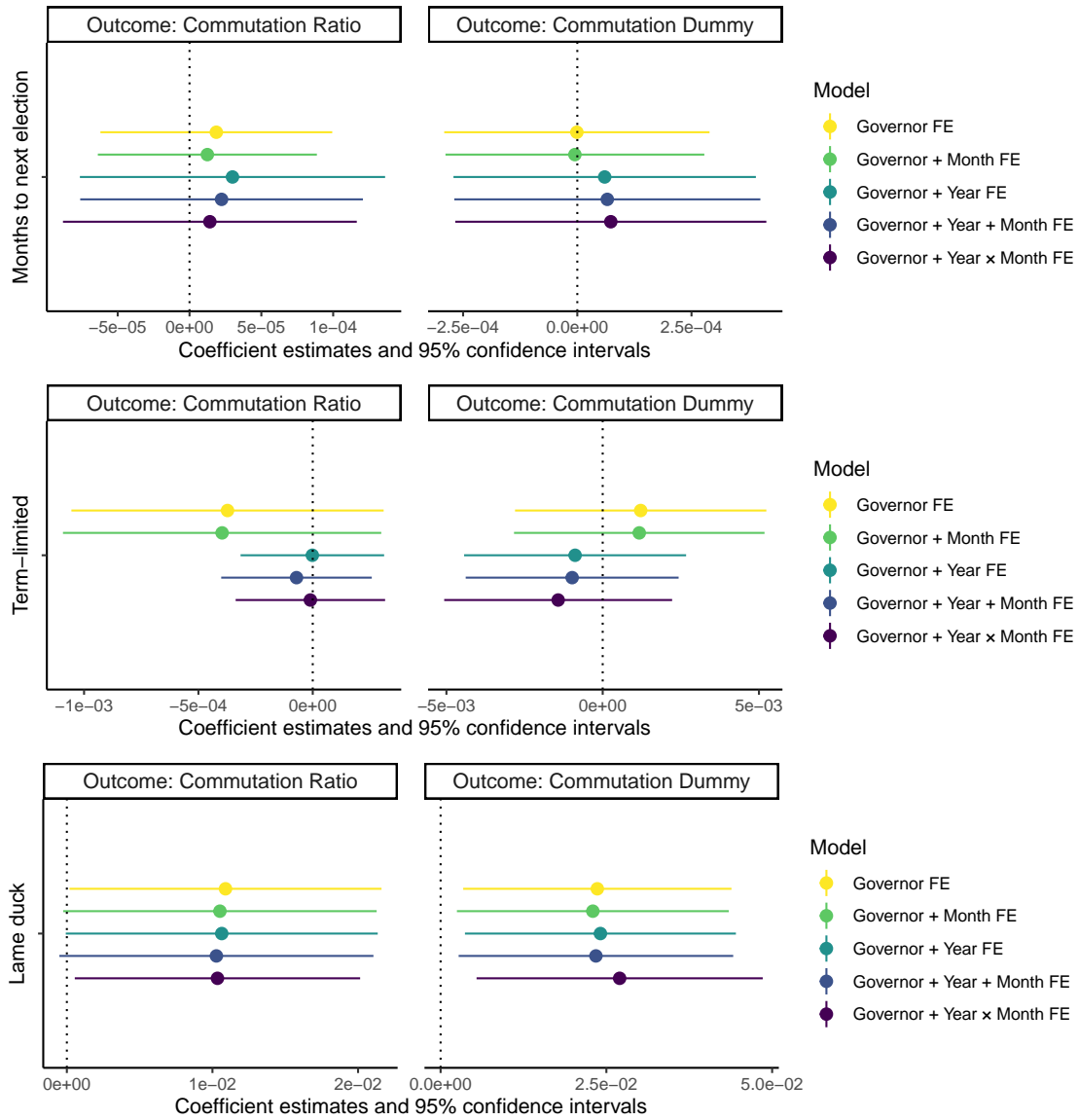
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Figure A9: Robustness of regression results to different governor fixed effects specifications.

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.
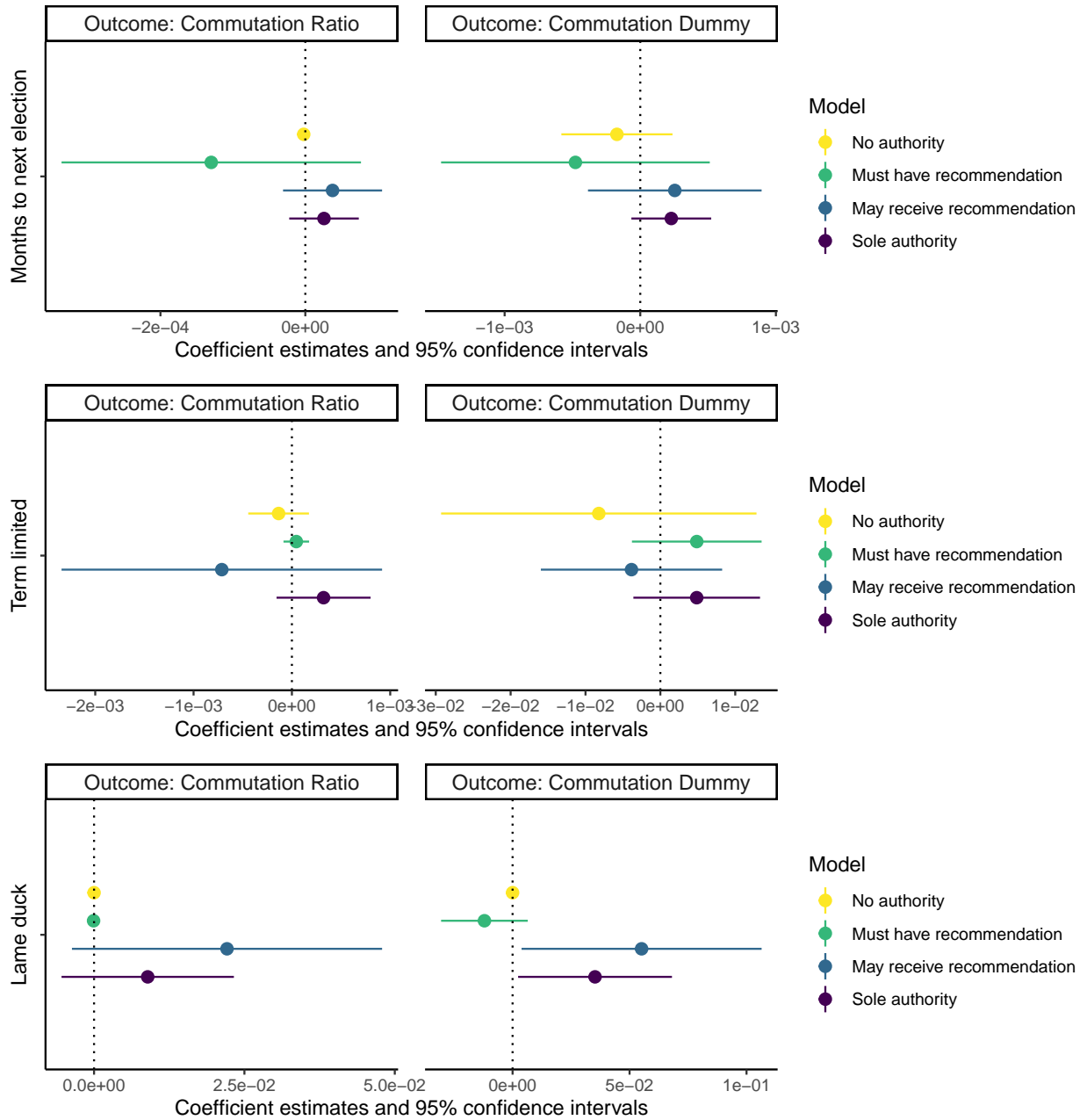

Figure A10: Robustness of regression results to subsetting to different definitions of governor authority.

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.

References

Page, Benjamin I and Robert Y Shapiro. 1992. *The rational public: Fifty years of trends in Americans' policy preferences.* University of Chicago Press.