Appendix: Arms and Elections: Arms Deals with Autocracies, Defense Contracting and U.S. Presidential Elections

Contents

1 Arms Deals Count Model Check

In this section, I check the hurdle Poisson models of U.S. arms deals. First, I show similar patterns in raw data and general arms deal cycles. Second, I demonstrate that different data and model specifications, Poisson models and a zero-inflated Poisson model give similar inferences to the hurdle Poisson models in the manuscript. Finally, I use posterior predictive checks to show that a hurdle Poisson outcome likelihood fits the observed data best.

1.1 Raw Data

The pattern of increasing arms deals with autocratic allies as presidential elections approach is evident in raw data. Figure 1 plots the number of arms deals per country after dividing state-year observations based on years to a presidential election, four quartiles of polyarchy, and whether a country is U.S. ally. Autocratic U.S. allies average more than one additional deal in election years than in years immediately after an election. More democratic allies receive more arms deals than non-allied states, but deals with these countries do not track the electoral cycle.

1.1.1 General Arms Deal Cycles

In addition to the raw data, arms deal cycles are apparent even without interacting the time to presidential election measure with polyarchy. I plot predicted arms deals as a function of years to a presidential election from a model that does not interact the election timing variable with anything else in Figure 2. Because it averages across all states, the increase of .05 deals across a presidential election cycle is small but distinguishable from zero.

1.2 Dropping Potential Outliers

Another possibility is that the results are due to a few outlier countries, such as Saudi Arabia. If one country is responsible for the overall pattern, then my argument has limited generalizability. To check this, I refit the hurdle poisson model of arms deals six times, dropping Saudi Arabia, the United Arab Emirates, Iran, Brazil and Argentina as potential outlier autocracies who purchased many U.S. arms. As Figure 3 shows, dropping potential outliers does not change the results. The presidential election year increase in deals is slightly smaller when I remove Saudi Arabia, but there is still a clear jump in arms purchases by autocracies.

1.3 Removing Conflict Control

A further possibility is that the deals results are biased by the inclusion of controls that are post-treatment indicators of autocracy. If autocracy causes conflict with the United States, for

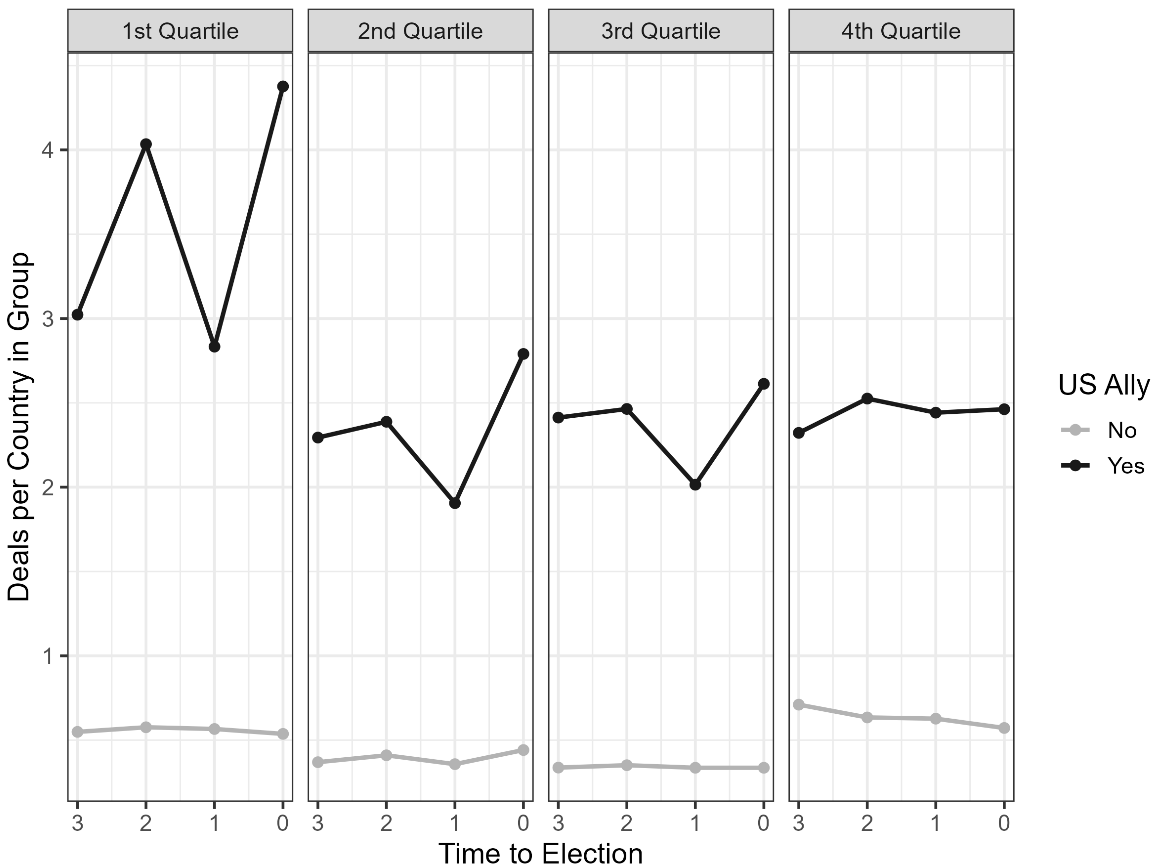

Figure 1. *Average arms deals with the United States per country in each quartile of democracy throughout U.S. presidential election cycles. Colors divide states based on whether they are U.S. allies.*

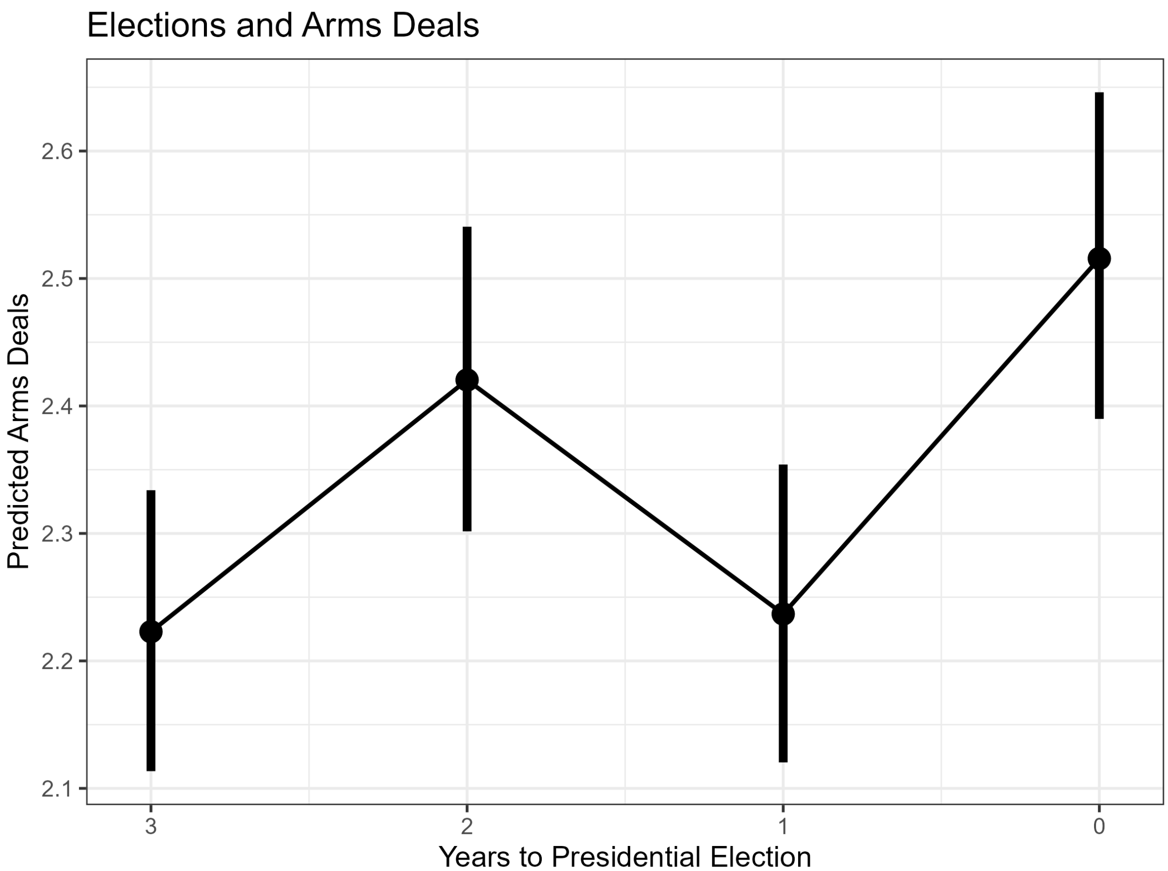

Figure 2. *Predicted arms deals by the United States across the presidential election cycle, all else equal. Estimates from a hurdle Poisson model of arms deals between 1950 and 2014.*

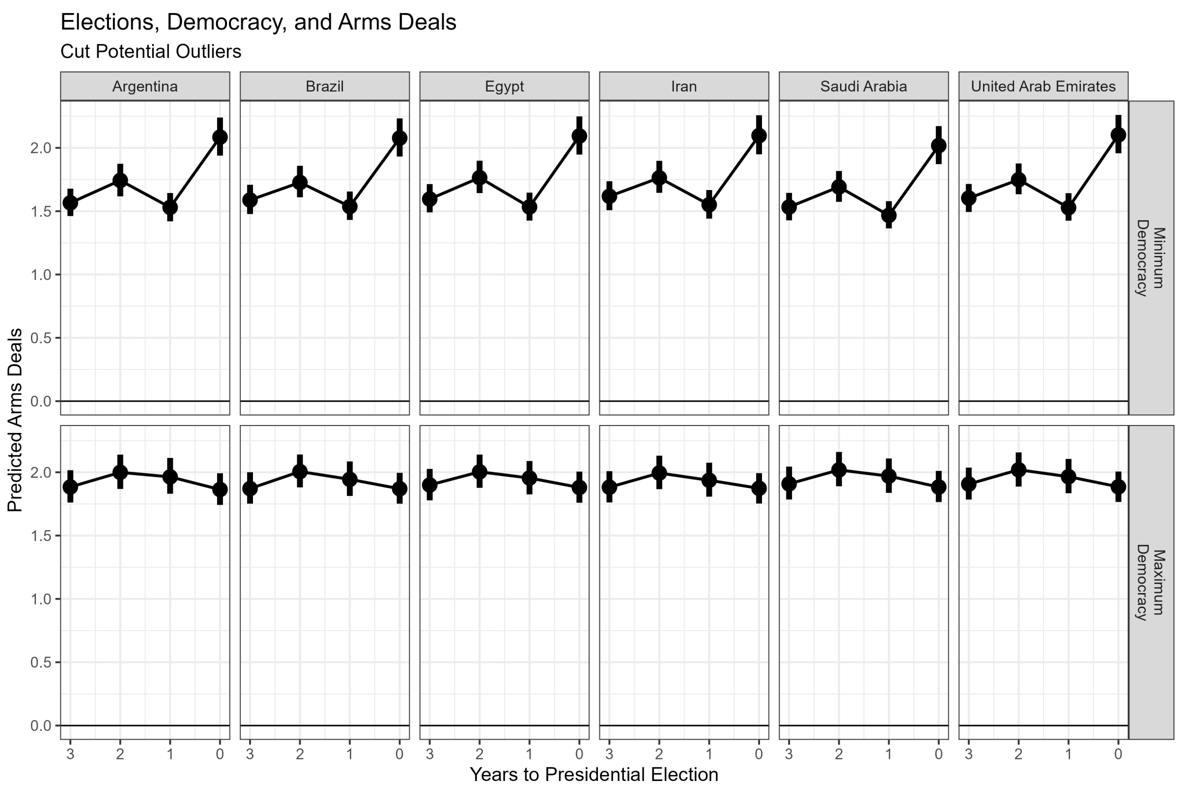

Figure 3. *Predicted arms deals by the United States across the presidential election cycle in six models, each of which drops a potential outlier arms purchaser. Estimates from a hurdle Poisson model of arms deals between 1950 and 2014. Polyarchy fixed to minimum and maximum for predictions.*

instance, then conflict might be a post-treatment variable and introduce bias (Montgomery, Nyhan and Torres, 2018). To check this, I refit the hurdle poisson model and remove the MID involvement variable from the hurdle and outcome equations. As Figure 4 shows, this does not change the results.

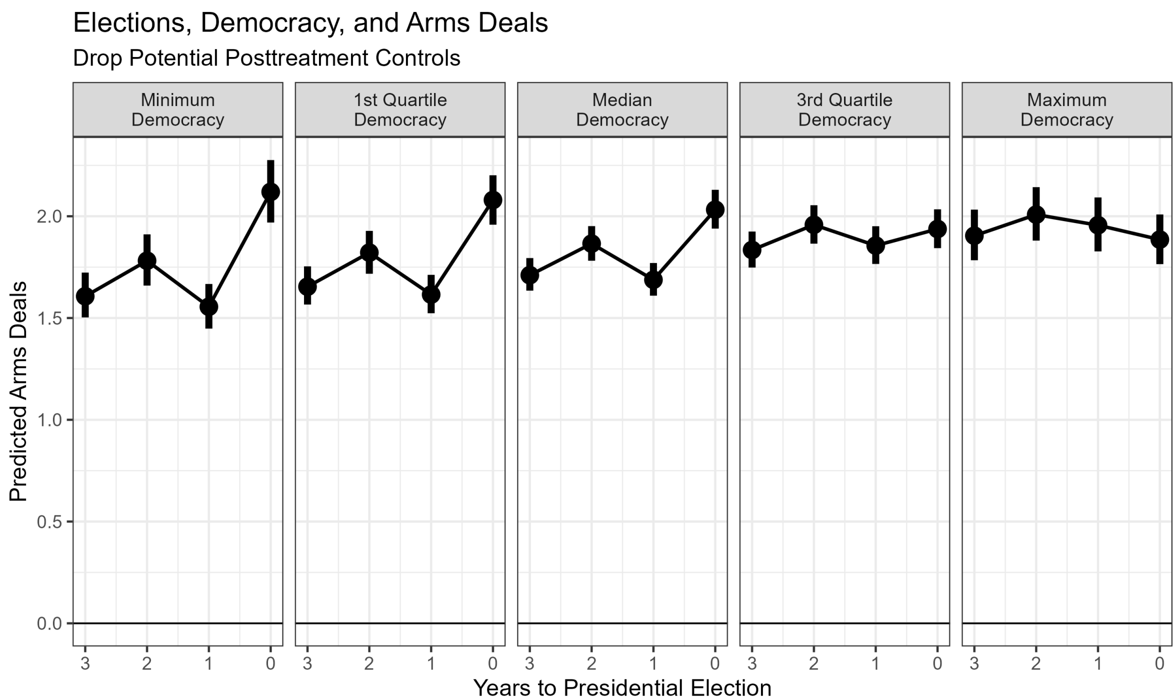

Figure 4. *Predicted arms deals by the United States across the presidential election cycle in a model that drops ongoing militarized disputes as a potential post-treatment variable of democracy. Estimates from a hurdle Poisson model of arms deals between 1950 and 2014.*

1.4 Linear Estimate of Time to Election

In the manuscript, I use dummy variables marking years to a presidential election to capture election proximity. As I show here, using a single indicator of years to an election gives similar inferences. The downside of this measure is that it obfuscates the fall in autocratic deals in the year between Congressional and Presidential elections, as Figure 5 makes clear.

1.5 Alternative Estimators

The same pattern is also apparent if I use three alternative models of arms deals, election timing and autocracy. While the hurdle Poisson is the most theoretically appropriate specification, standard Poisson and zero-inflated Poisson models give similar inferences. The Poisson results hold without controls as well. I plot predicted arms deals across recipient democracy and election timing from each of these models in Figure 6, Figure 7 and Figure 8. All three

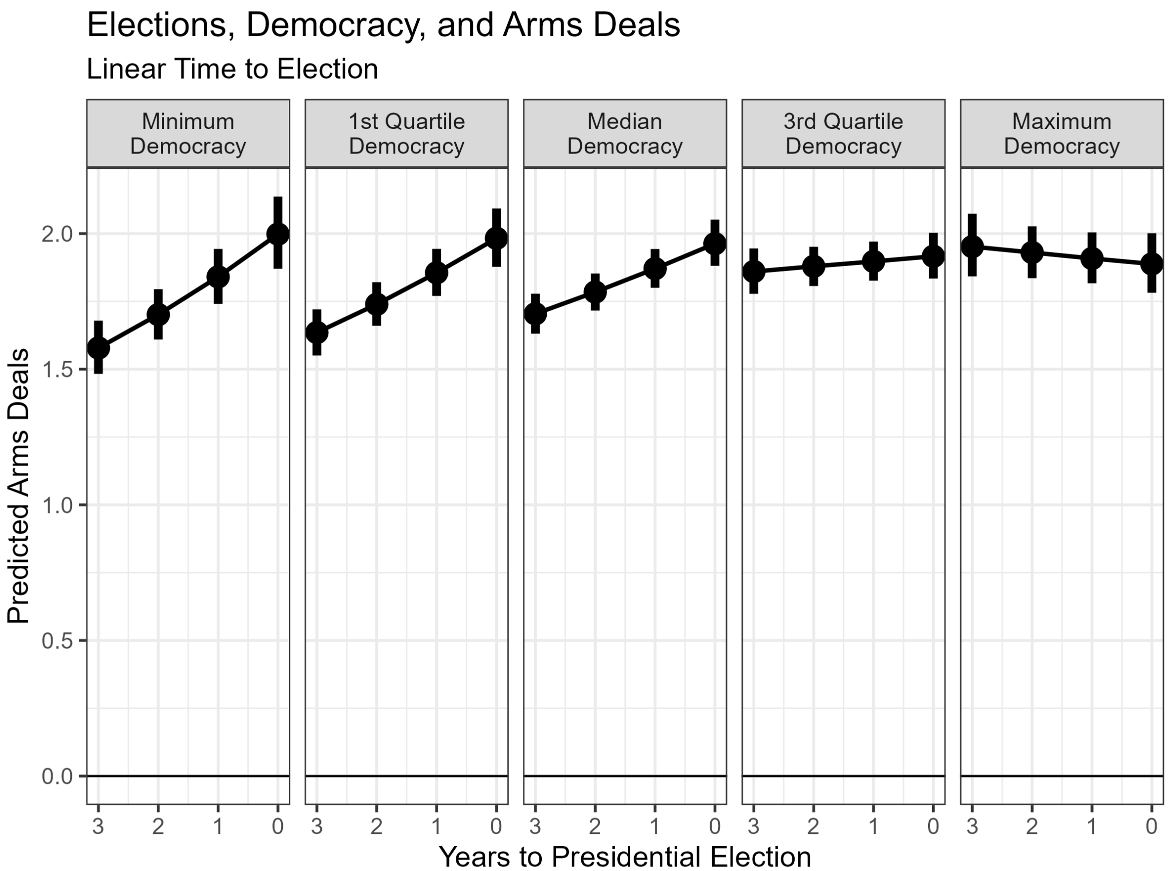

Figure 5. *Predicted arms deals by the United States across the presidential election cycle in six models, each of which drops a potential outlier arms purchaser. Estimates from a hurdle Poisson model of arms deals between 1950 and 2014. Polyarchy fixed to minimum and maximum for predictions.*

estimators suggest increasing arms deals for autocratic allies as elections approach, and little change in deals with democratic allies near presidential elections.

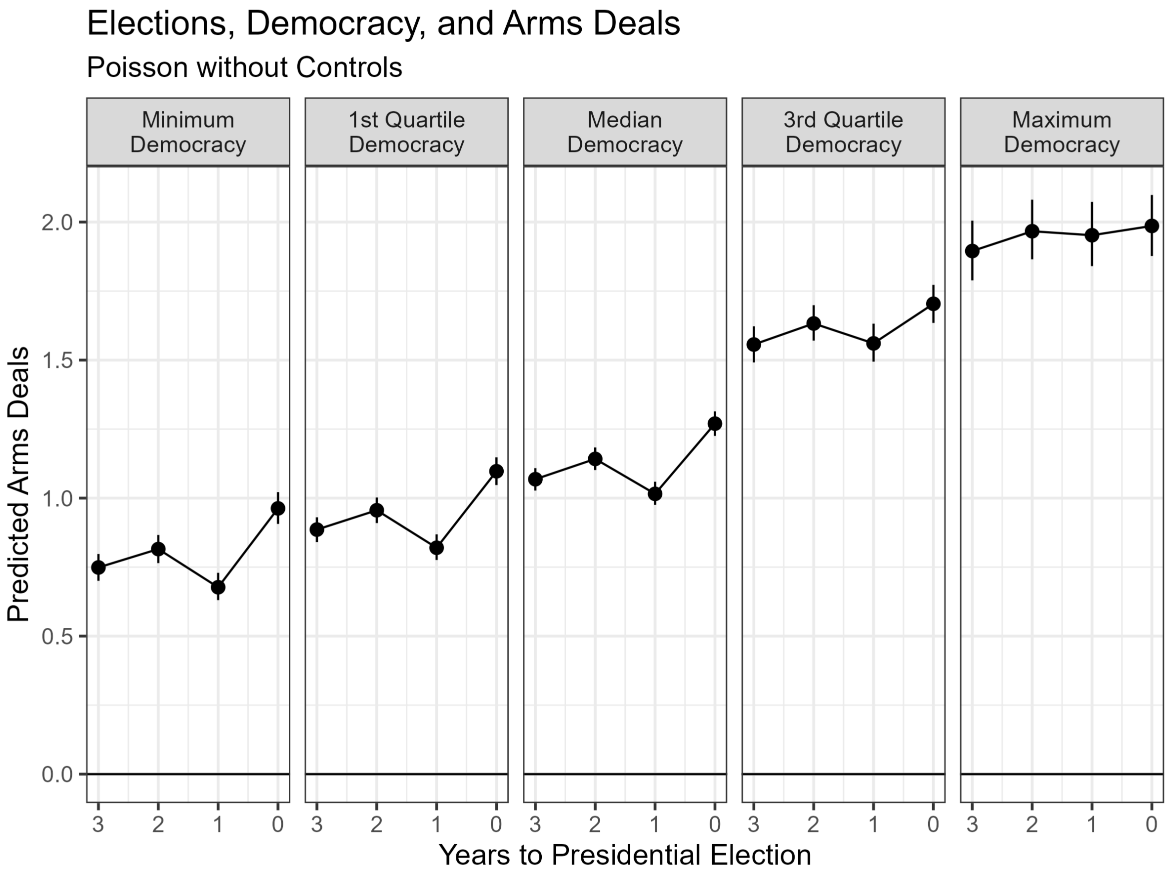

Figure 6. *Predicted arms deals between the United States and other states 1950 to 2014 by presidential election proximity and partner democracy based on a Poisson regression model with only those two variables and no controls. Points mark the estimates and error bars summarize the 90% credible interval.*

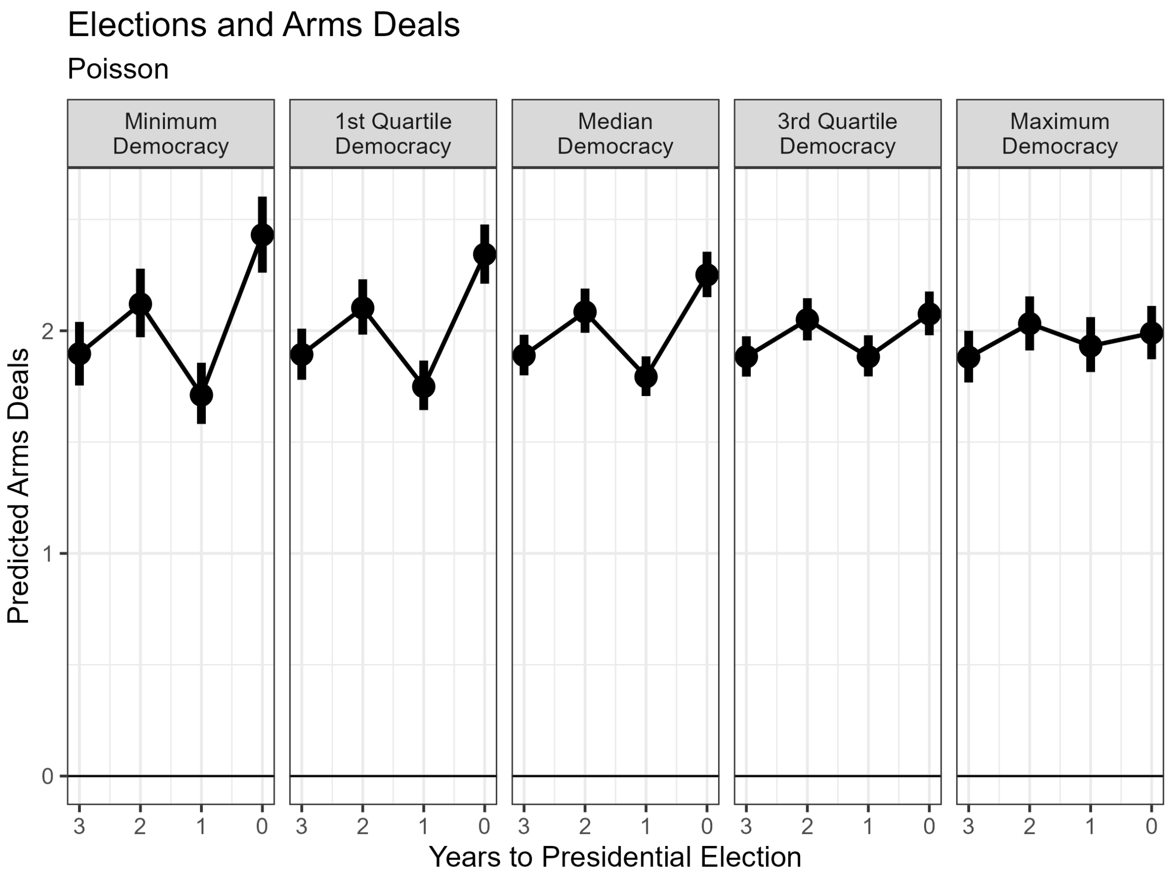

Figure 7. *Predicted arms deals between the United States and other states 1950 to 2014 by presidential election proximity and partner democracy based on a Poisson model. Points mark the estimates and error bars summarize the 90% credible interval.*

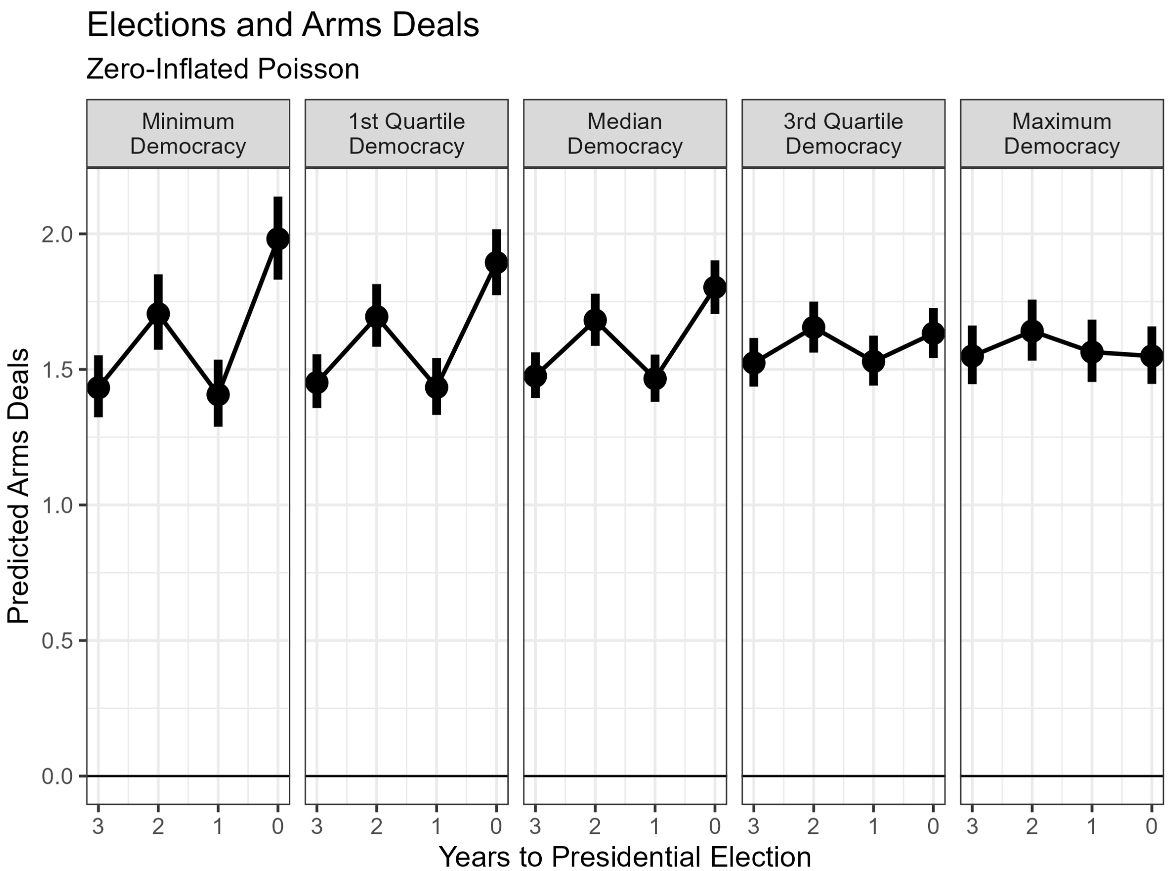

Figure 8. *Predicted arms deals between the United States and other states 1950 to 2014 by presidential election proximity and partner democracy based on a zero-inflated Poisson model. Points mark the estimates and error bars summarize the 90% credible interval.*

1.6 Posterior Predictive Checks

Next, I compare the predictive performance of the hurdle Poisson model to a Poisson and negative binomial specification. While no model perfectly captures the lumpy outcome distribution, the hurdle Poisson is much better at predicting zero deals and the range of observed deals. First, I show the posterior predictive check for the hurdle Poisson in Figure 9.

Figure 9, Figure 10 and Figure 11 are rootograms, which plot expected counts against observed counts. In both figures, the line gives the expected counts based on the model, and the bars mark observed counts. Bars that exceed zero are counts the model under predicts, while bars above zero show underpredicted values. As a result, the hurdle Poisson predicts zero values well, underpredicts some large values, and overpredicts a few small values.

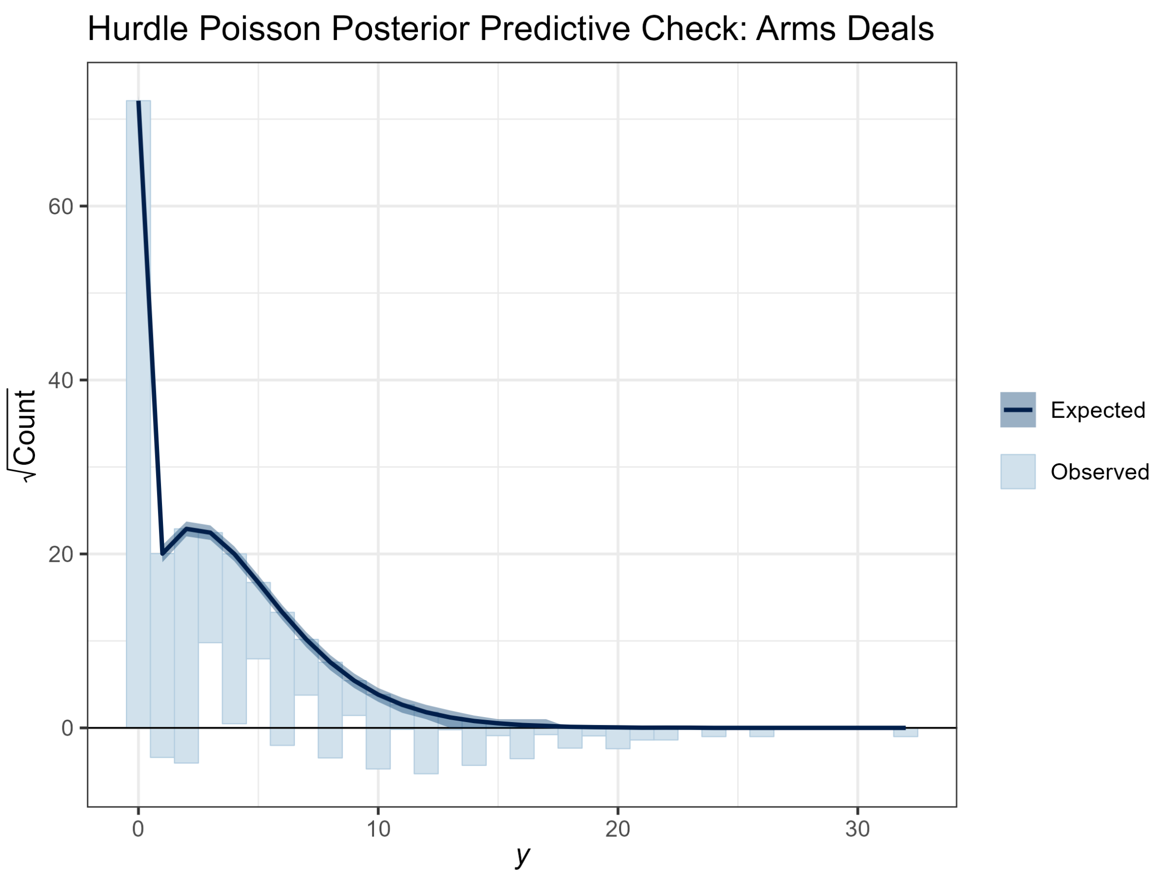

Figure 9. *Posterior predictive check of the hurdle Poisson model of U.S. arms deals. The fitted line gives the expected counts and bars show the observed distribution.*

Relative to the hurdle, a regular Poisson model under-predicts zeros, as Figure 10 demonstrates. This also reduces predictive accuracy for non-zero deals.

While the lumpy distribution of deals complicates predictions with a Poisson likelihood, a negative binomial likelihood fits poorly. As Figure 11 demonstrates, the extra variance in a negative binomial results in underpredicting almost all observed values, as well as predictions

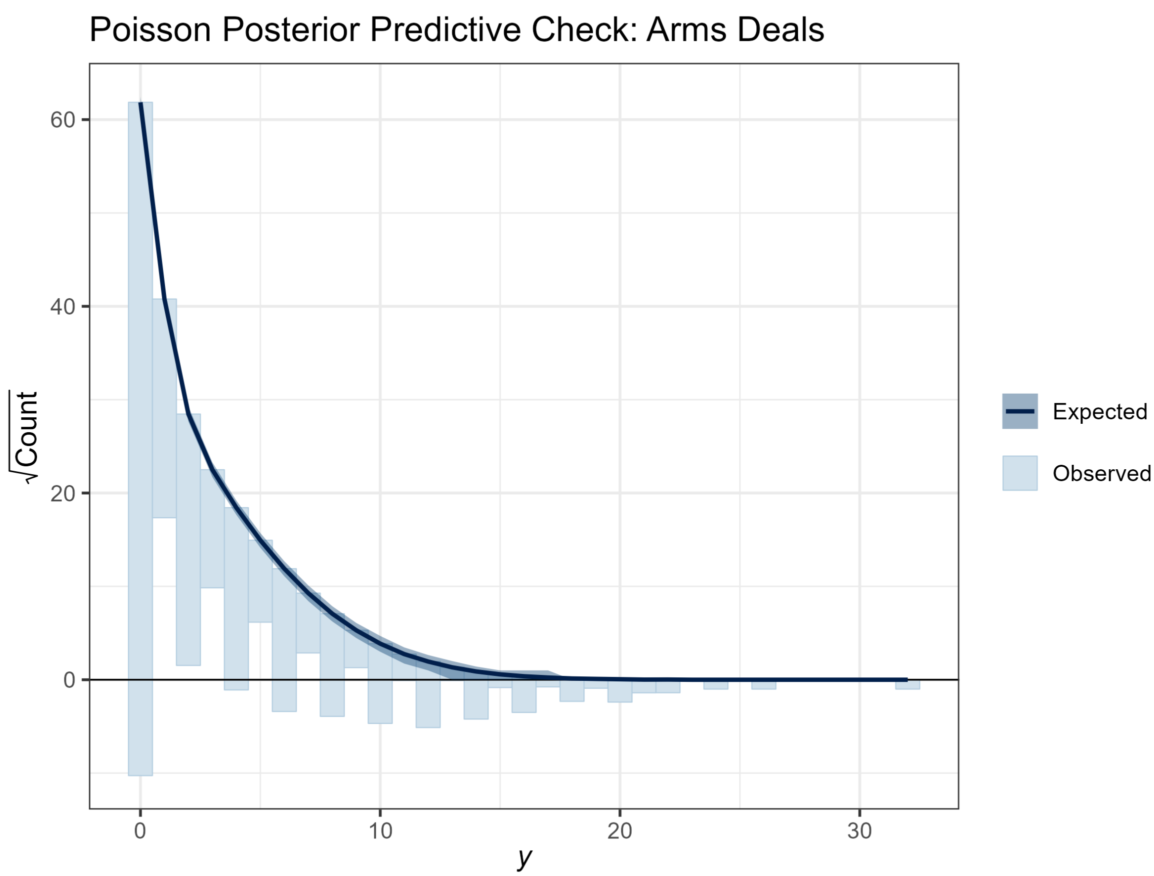

Figure 10. *Posterior predictive check of a Poisson model of U.S. arms deals. The fitted line gives the expected counts and bars show the observed distribution.*

that are far above the range of the observed data. I therefore rely on models with a Poisson likelihood.

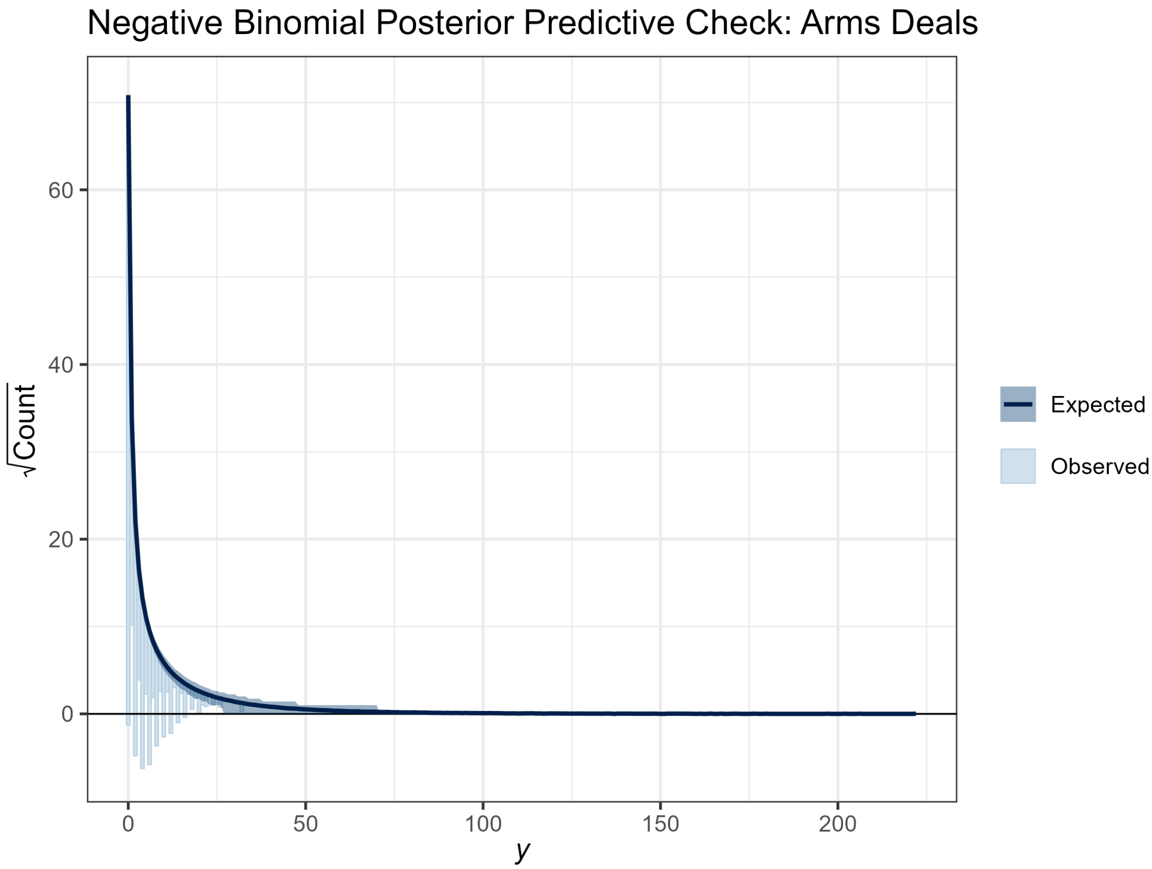

Figure 11. *Posterior predictive check of a negative binomial model of U.S. arms deals. The fitted line gives the expected counts and bars show the observed distribution.*

1.7 Alternative Autocracy Measures

While the polyarchy measure is a more fine-grained treatment of leader accountability to voters and potential opposition influence, as well as granting better temporal coverage, other regime type indicators give similar results. In this section of the appendix, I replace polyarchy with two different measures- the binary democracy indicator of Cheibub, Gandhi and Vreeland (2010) and the autocracy list of Geddes, Wright and Frantz (2014). For the autocracy data, I use a binary indicator of states that are not autocracies- most of these states are not autocracies, but there are a few instances of other regimes as well.

In Figure 12, I plot predicted arms deals across the electoral cycle with the two regime type indicators. Both show the same cycle of rising deals with autocracies during elections, especially presidential election years. At the same time, deals with democracies are high but comparatively stable.

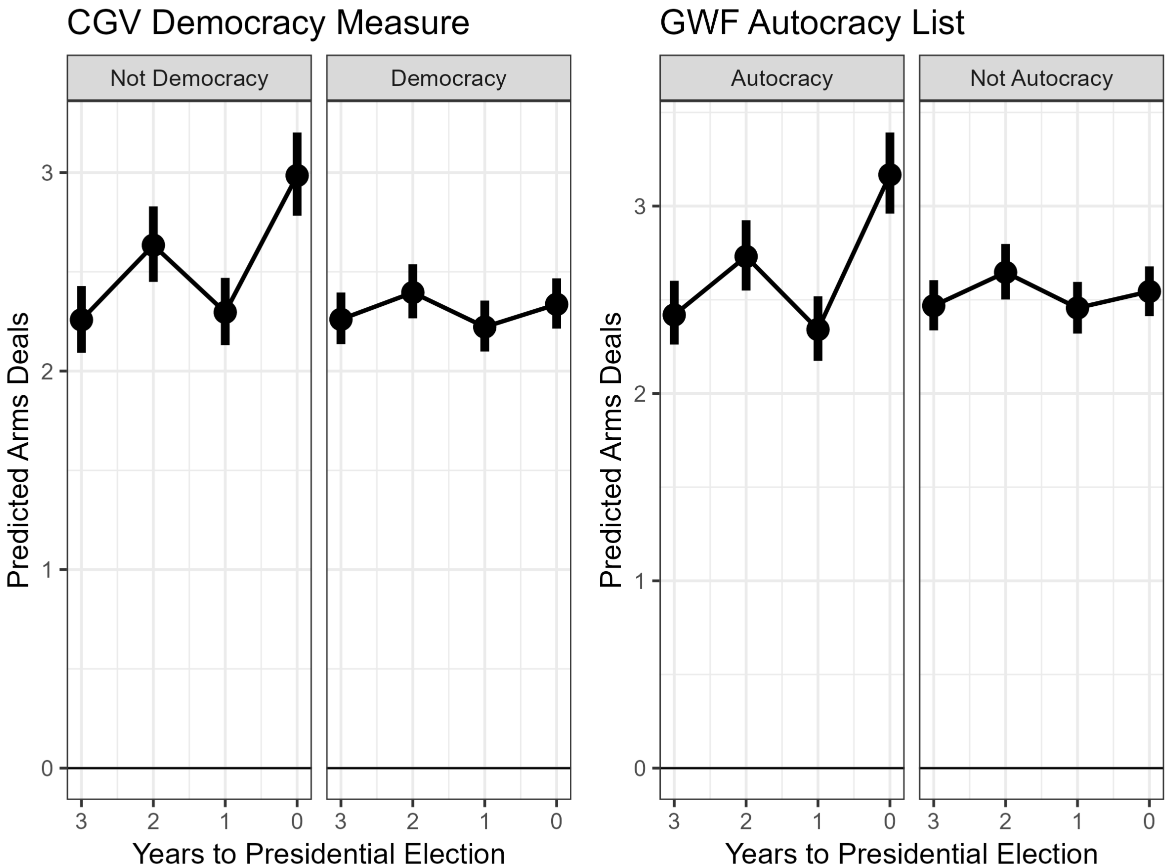

Figure 12. *Impact of election proximity on arms deals with two different regime type measures.*

1.8 Comparing Incumbent and Lame-Duck Presidents

This section of the appendix compares incumbent and lame-duck presidents. Incumbent presidents who are seeking re-election may be more likely to engage in arms sales to autocracies to ensure they personally continue in office. To check this, I added an binary indicator of whether the president was an incumbent or lame duck to the interaction between partner democracy and election proximity.[1] For ease of interpretation and presentation, I use the autocracy list of Geddes, Wright and Frantz (2014) to measure recipient democracy. I plot the results in Figure 13.

While incumbent presidents behave as the theory predicts, with greater arms deals with autocracies in election years and no change in deals with democracies, arms deals under lame

[1] This measure treats Lyndon Johnson as an incumbent until 1967, and a lame duck in 1968, as Johnson announced he would not seek re-election in March 1968.

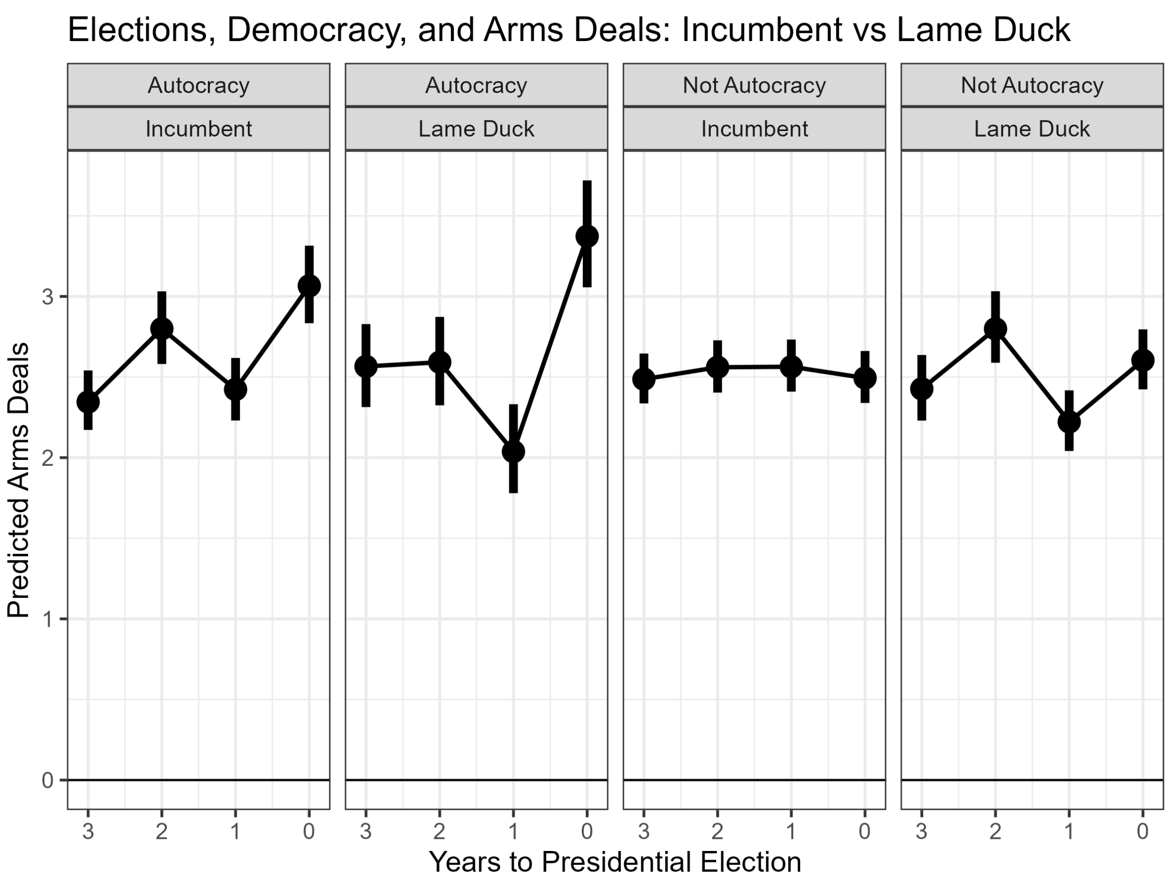

Figure 13. *Impact of election proximity on arms deals divided by whether the incumbent president is standing for re-election or is not running for office themselves.*

ducks are more volatile. Autocratic arms deals under lame duck presidents do not rise in midterm election years, fall in the year before Presidential elections, and rise dramatically in election years. Second term incumbents also make more deals with democracies in midterm elections.

One reason for that lame ducks behave in ways that are inconsistent with the argument may be that incumbent presidents are more common in the data, so there is more noise in the second-term incumbent estimates. Because incumbent presidents including Lyndon Johnson, Gerald Ford, Jimmy Carter and George H.W. Bush failed to secure re-election, incumbent presidents are present in 49 of the 76 years from 1945 to 2020. As a result, roughly one third of all observations in the arms deal data have a lame duck U.S. president. These inferences may more sensitive to idiosyncratic fluctuations. Lame duck leaders may also attempt to sell more arms to autocrats at the end of their term to constrain potential successor and help the candidate from their party.

To further explore the sensitivity of results to president-specific dynamics, I fit models of arms deals that drop each president one by one. Removing each administration shows that inferences about second-term incumbents are more sensitive to specific administrations, while the pattern among first-term incumbents is more stable.

Specifically, dropping the Eisenhower administration attenuates the massive increase in arms deals for lame duck presidents. In his final year in office, Eisenhower struck 24 arms deals with Greece's junta and a further 16 with Peru. Given that his vice-president was running for office, this is somewhat unsurprising. Similarly, removing Truman eliminates the midterm elections democratic deals spike among lame duck presidents, likely due to extensive U.S. arms sales to allies in the early years of the Cold War.

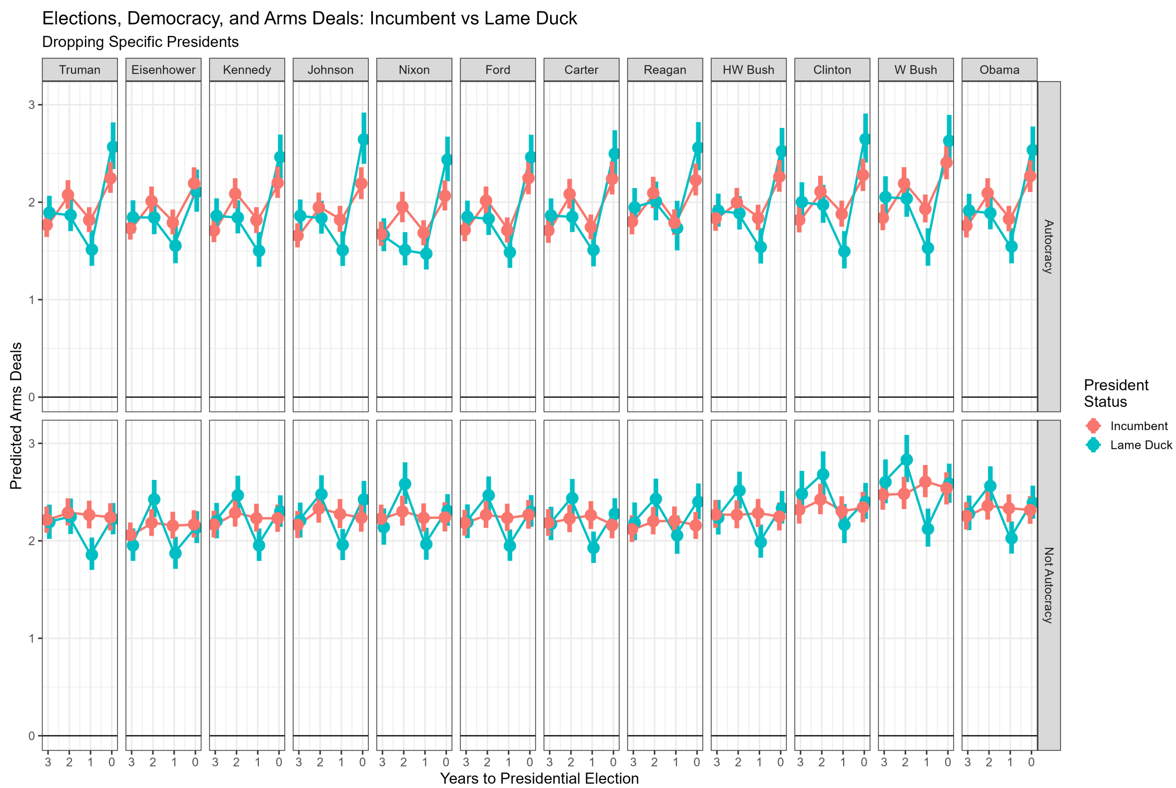

Figure 14. *Impact of election proximity on arms deals divided by whether the incumbent president is standing for re-election or is not running for office themselves, after dropping individual presidential administrations. Facet columns mark which administration was removed from the data, and colors distinguish electoral cycles in arms deals .*

2 Contracts Model Checks

This section checks the second analysis of arms deals and contract awards in the 50 U.S. states. First, I present some raw data. After that, I present additional estimates from the ordered beta regression, including the interaction of deals and swing status, the state varying intercepts and lagged dependent variables. I then show that student-t and hurdle log-normal models of defense contract changes and levels also suggest that arms deals increase contract awards to swing states. Finally, I check whether changing the control variable specification alters the results.

2.1 Raw Contracts: Swing and Other States

Figure 15 shows that median contract awards to swing states rise as elections approach, while median awards to other states rise by much less.

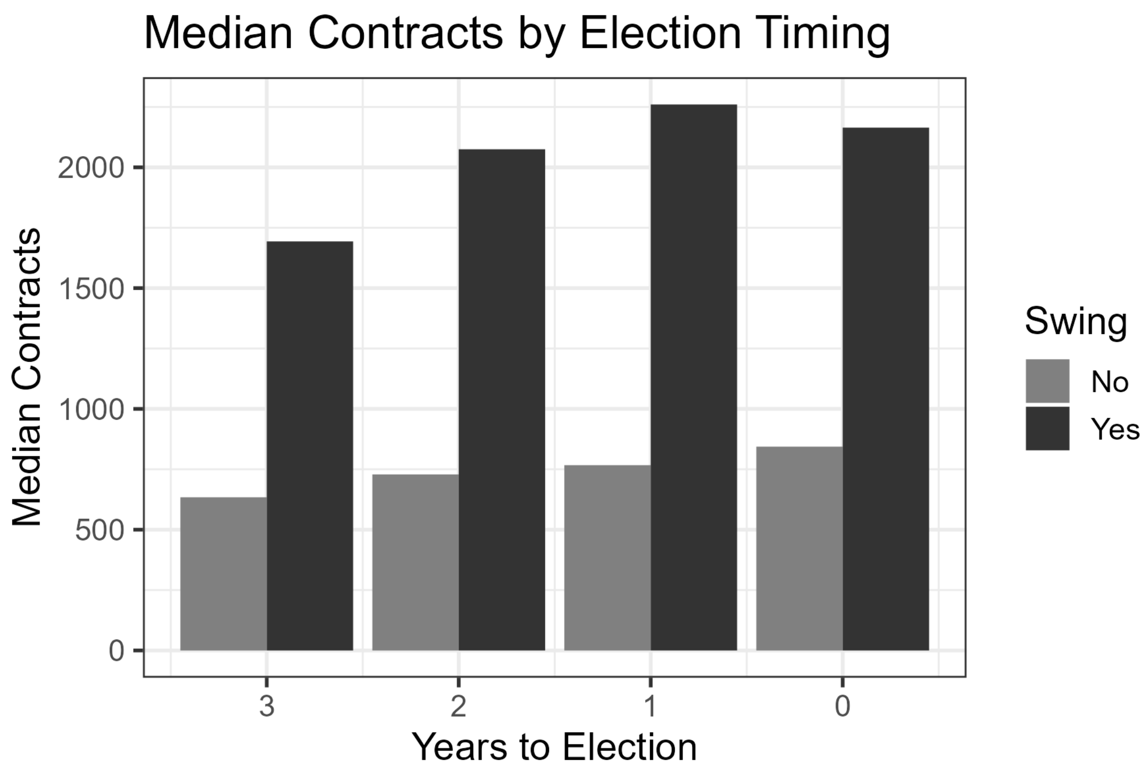

Figure 15. *Median contracts in swing and non-swing states as presidential elections approach. I use the median because averages are sensitive to states with large or minimal arms contract awards.*

2.2 Additional Estimates

Here, I present some key estimates underlying the results in the manuscript. First, Figure 16 presents the interaction coefficient for deals and swing status, the marginal impact of swing state competition as arms deals vary, and predicted defense contracts for hypothetical swing and non-swing states. All these estimates suggest that deals increase defense contracting awards to swing states. In fact, swing states receive fewer contracts than other states in years with low arms deals.

As the top left panel of Figure 16 shows, the impact of increasing arms deals on defense contracting is clearly positive in swing states and uncertain elsewhere. 98% of the posterior mass of the deals and swing state interaction is positive, so the preponderance of evidence supports the deals and contracts hypothesis. Only 34% of the posterior mass in the deals constituent term is positive, however, so there is little evidence that deals increase contract awards outside of swing states.

The top right panel of Figure 16 shows that the marginal impact of swing state status on defense contracts rises as arms deals increase. Deals offset what is otherwise a swing state disadvantage in defense contracts. At the observed minimum of arms deals, a typical swing state receives $300 million less in contract for arms production. The swing state disadvantage at low levels of arms deals occurs because non-swing states like California and Texas have substantial defense industries. When arms deals approach the observed maximum, swing states receive similar contracts to other states.

Finally, predicted defense contracts increase as arms deals increase, but only in swing states, as I show in the bottom panel of Figure 16. Holding all else equal, increasing arms deals leads to greater contracts in swing states. Defense contracts in non-swing states do not respond to increasing arms deals. The gap in defense contracting between swing and other states thus disappears in years with high arms deals.

All three of the quantities in Figure 16 are consistent with one another and with the argument that leaders use arms deals to increase swing state contracts. Increasing arms deals are correlated with greater defense contracts in swing states. As a result, in years with more arms deals, swing states receive similar contracts to other states with larger economies and less electoral competition.

Figure 17 presents state-specific parameter estimates from the ordered beta regression with transformed contracts. There is wide variation in contracting levels and temporal dependence across states. States with higher contracting levels also have more consistent temporal autocorrelation in contracts, while states such as North Dakota receive occasional arms contracts and thus have little temporal dependence.

2.3 Alternative Estimators

This section checks the results in the manuscript by adjusting the outcome measurement and estimation strategy in two ways. First, I do not transform the contracts measure in any way, and fit a log-normal hurdle model, which assumes that the outcome has a zero process and observed values that are approximately normal after a log transformation. This approach

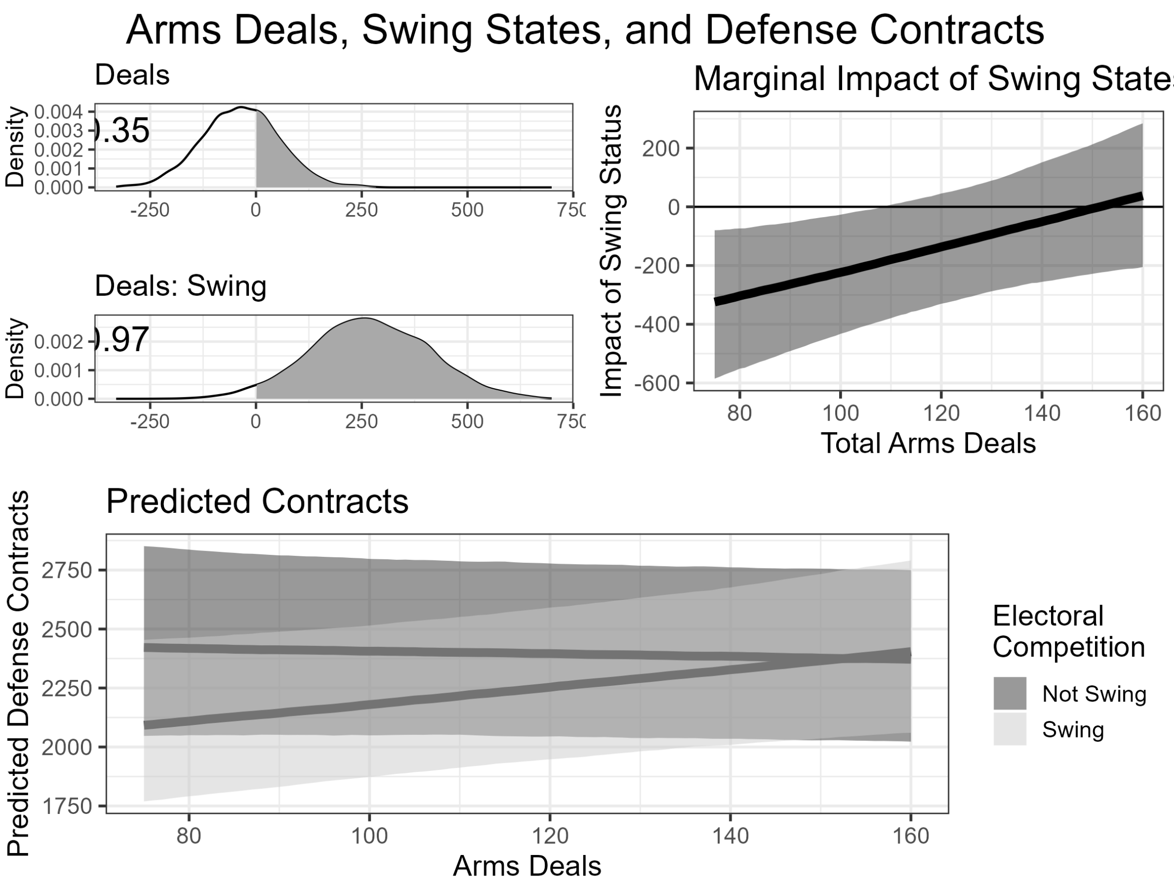

Figure 16. *Interaction coefficients, marginal effects and predicted outcomes from an interaction of swing state status and U.S. arms deals. The outcome is annual defense contracts in the 50 U.S. states from 2001 to 2020, measured in millions of dollars. Lines give the expected value, while the error bars summarize 90% credible intervals. All other variables fixed at the mode or median. Estimates derived from an ordered beta regression with rescaled defense contracts, with estimates transformed back onto the outcome scale.*

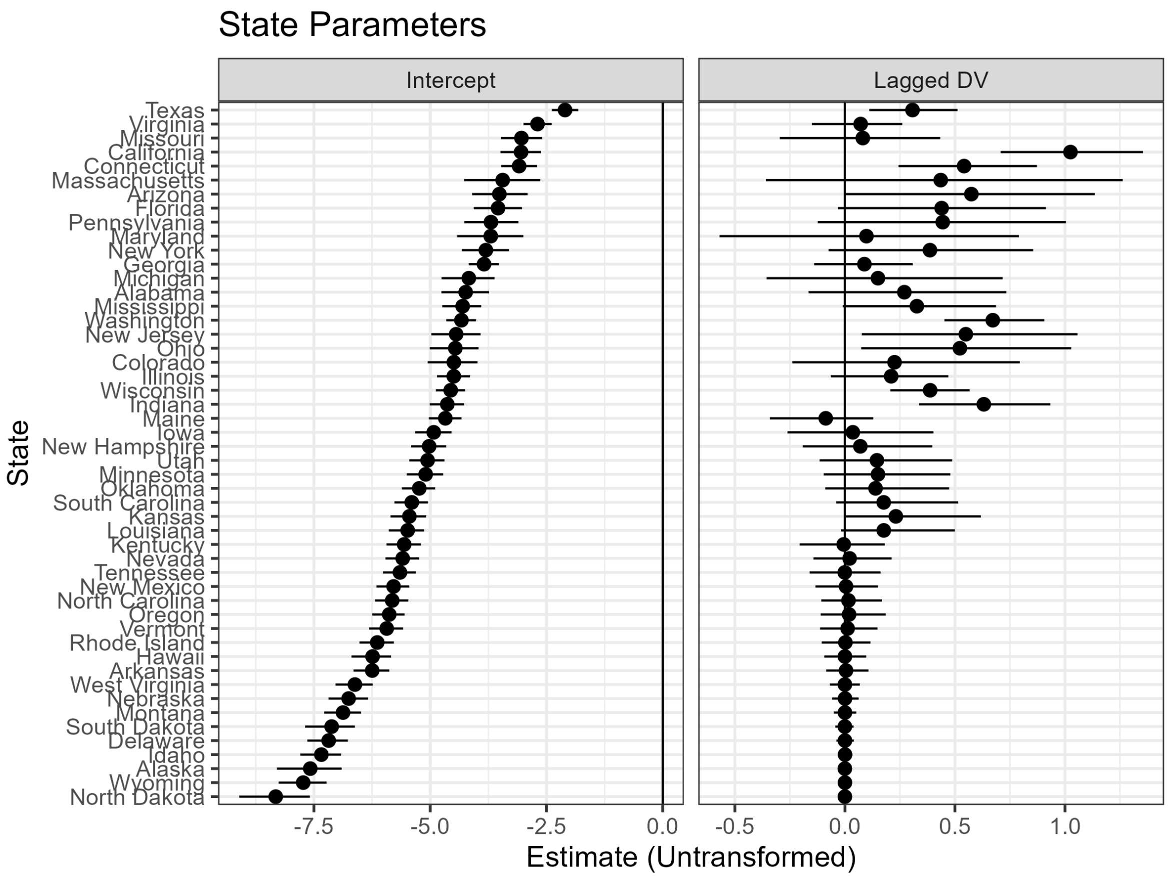

Figure 17. *Estimated state intercept and temporal autocorrelation from ordered beta regression of transformed defense contracts in U.S. states, 2001-2020. Estimates ordered by the magnitude of the varying intercept. Error bars summarize the 90% credible interval.*

does not model state-year observations with zero contracts well, but it fits non-zero contracts tolerably. As the top panel of Figure 18 shows, the interaction between arms deals and swing state status is almost entirely positive. At the same time, the association between deals and the level of contracts outside of swing states is almost entirely negative. This latter estimate is not part of the argument, and may be due to difficulties accounting for zeros in the log-normal hurdle.

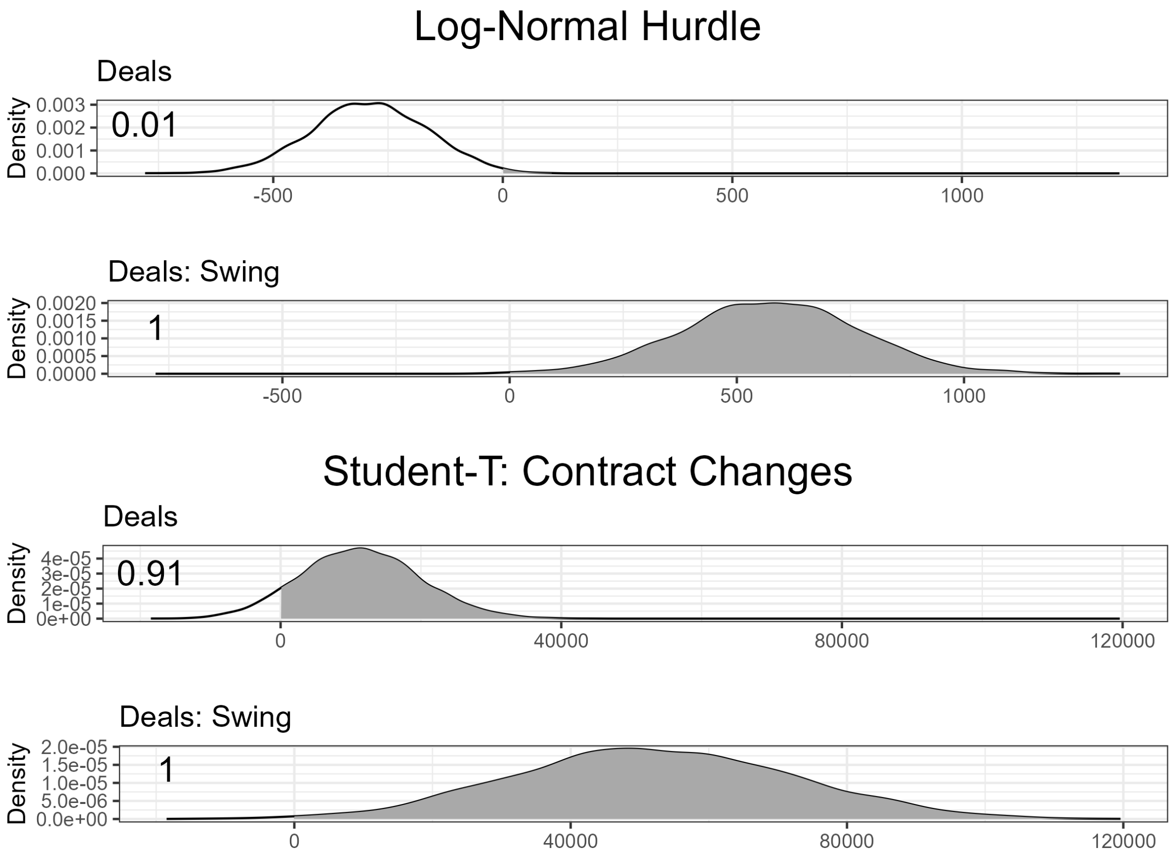

Figure 18. *Shaded area and text give the positive posterior mass of each coefficient.*

A second approach uses the difference in contracts for each state in every year as the outcome. Because this measure is not normally distributed and has fat tails, I use a student-t outcome distribution. The student-t model also omits the state-specific lagged dependent variable, because using changes eliminates some of those dynamics.

Results from the student-t model of contract changes also suggest that arms deals increase contract awards to swing states. 99% of the posterior mass in the interaction coefficient between deals and states is positive. While 92% of the posterior mass in the deals term is positive, which suggests increased deals lead to increased changes in contracts for other states as well, there is a 95% posterior probability that the relationship between arms deals and contracts in swing states is larger. As a result, arms deals increase changes in defense contracts more in swing states than in other states.

2.4 Alternative Control Specifications

This section checks the robustness of the deals and contracts results to alternative control specifications. Specifically, I remove the indicators of state population and wealth to generate a more sparse model specification. Figure 19 shows that inferences about the association between deals and swing state contracts are very similar in this model– the marginal effect of deals remains positive for swing states and close to zero for non-swing states.

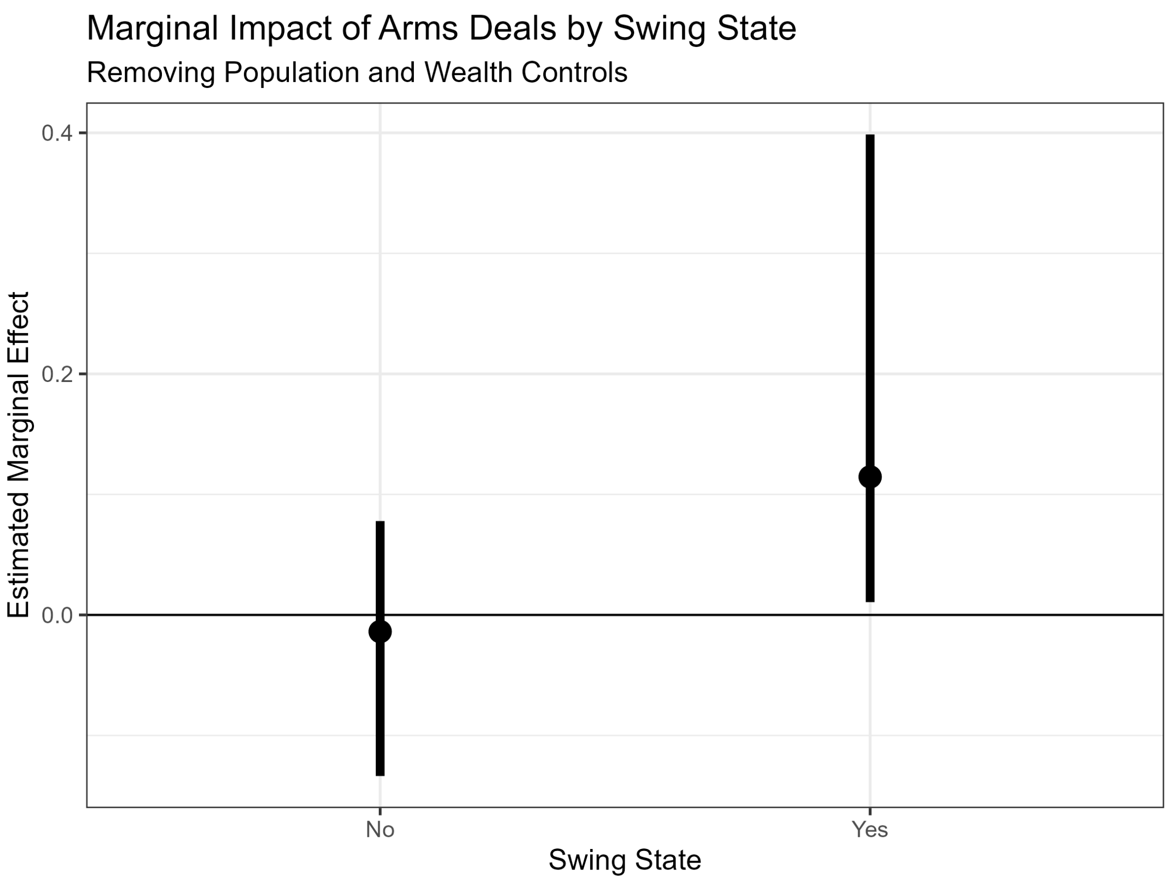

Figure 19. *Marginal effect of increasing arms deals on defense contracts in swing and non-swing states. Estimates in millions of U.S. dollars.*

3 Election Proximity, Swing States, and Arms Deals

Here, I examine how electoral proximity modifies the connection between deals and swing state contracts. The arms deals models show that deals with autocracies increase as presidential elections approach. The defense contracting models show that swing states receive more defense contracts as arms deals rise. If increasing deals go to swing state contracts, then the marginal impact of deals on contracts in swing states should increase around presidential elections.

To check this, I alter the model of defense contracts in the manuscript by interacting the time to election indicator with swing state dummy and arms deals. I then present the marginal effect of deals on defense contracts in Figure 20.

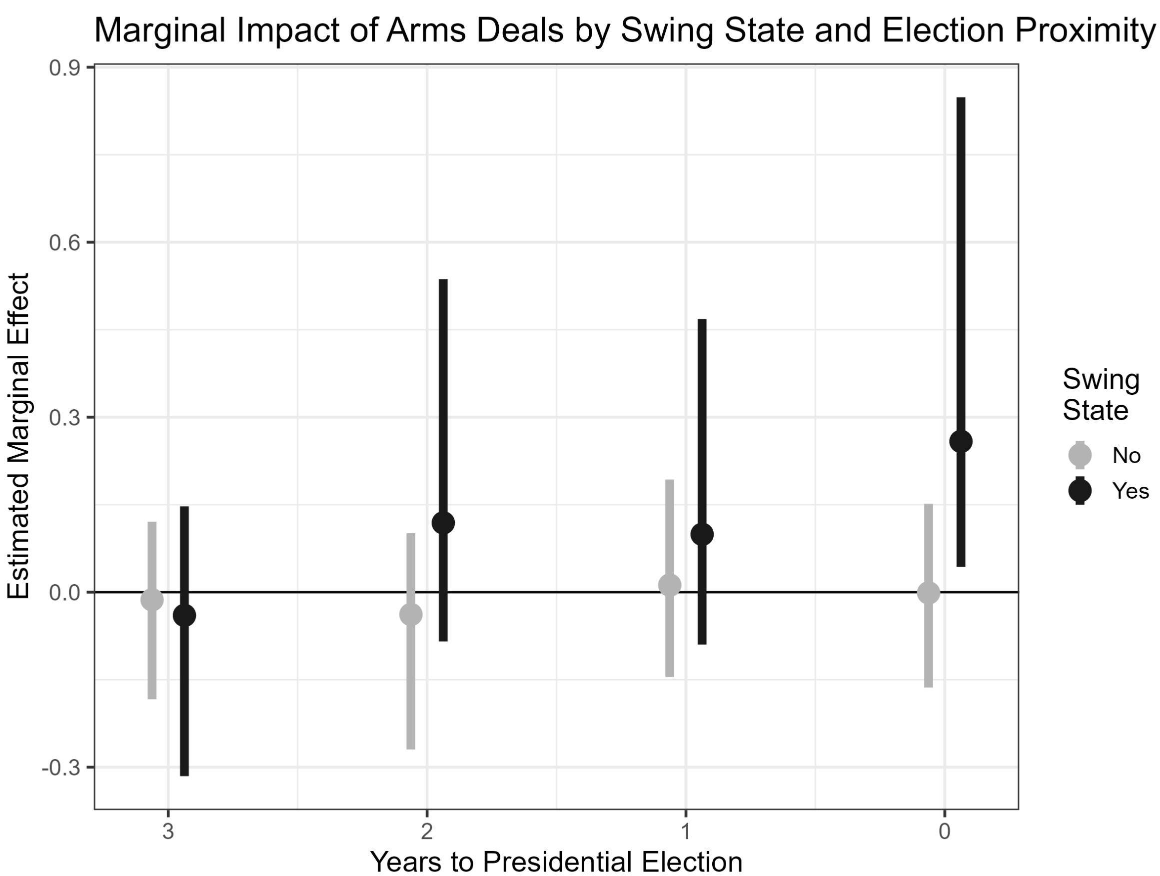

Figure 20. *Marginal effect of arms deals on defense contract awards based on swing state status and presidential election proximity. Estimates in millions of U.S. dollars.*

The marginal impact of arms deals on contracts increases as presidential elections approach, but only in swing states. After an election, deals do not increase contracts in any state. But as a presidential election approaches, the marginal impact of deals on swing state contracts increases and is clearly positive in the year of a presidential election. There is no clear impact of deals on non-swing state contracts at any point in the electoral cycle. This further supports my argument that arms deals with autocracies near elections feed increased swing state contracts.

4 Interaction Robustness

Some models in the manuscript use interactions that assume a linear functional form. Violations of linearity and other issues can generate misleading inferences (Hainmueller, Mummolo and Xu, 2019). Here, I show that more flexible function forms give similar inferences about marginal effects. I do this by using binning estimators to examine the marginal impact of election proximity on arms deals and the marginal impact of swing state status on contracts.

First, I present the marginal effect of election proximity on arms deals in Figure 21. When time to election is 0, the marginal impact of this relative to the year after a presidential election is largest states with a low polyarchy score. This is consistent with the argument, although comparisons at the other two levels are less so.

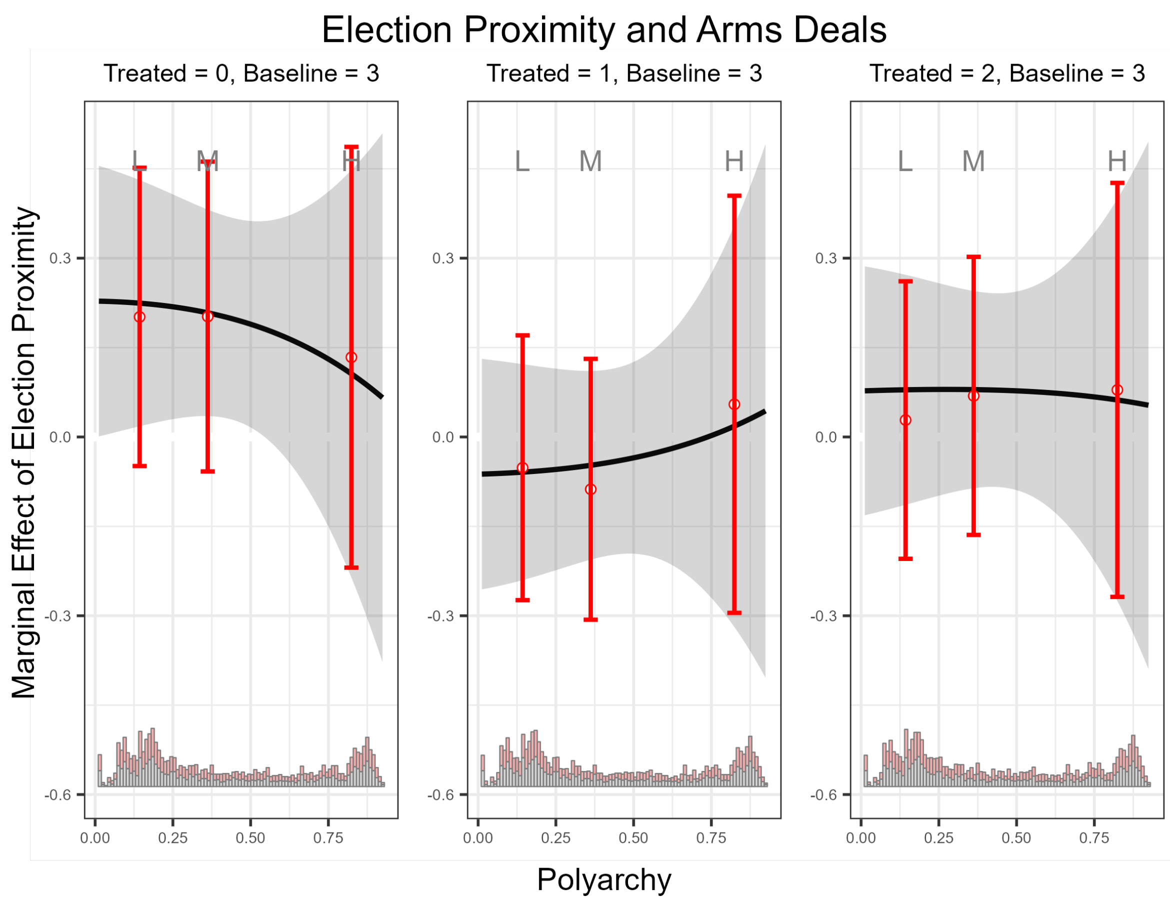

Figure 21. *Marginal effect of election proximity on arms deals across the observed range of polyarchy. Each comparison uses three years to the presidential election as a baseline.*

Second, Figure 22 shows the same pattern in the marginal impact of swing state status as the manuscript. As deals increase, so does the marginal effect of swing state status. Each bin deviates minimally from the linear relationship.

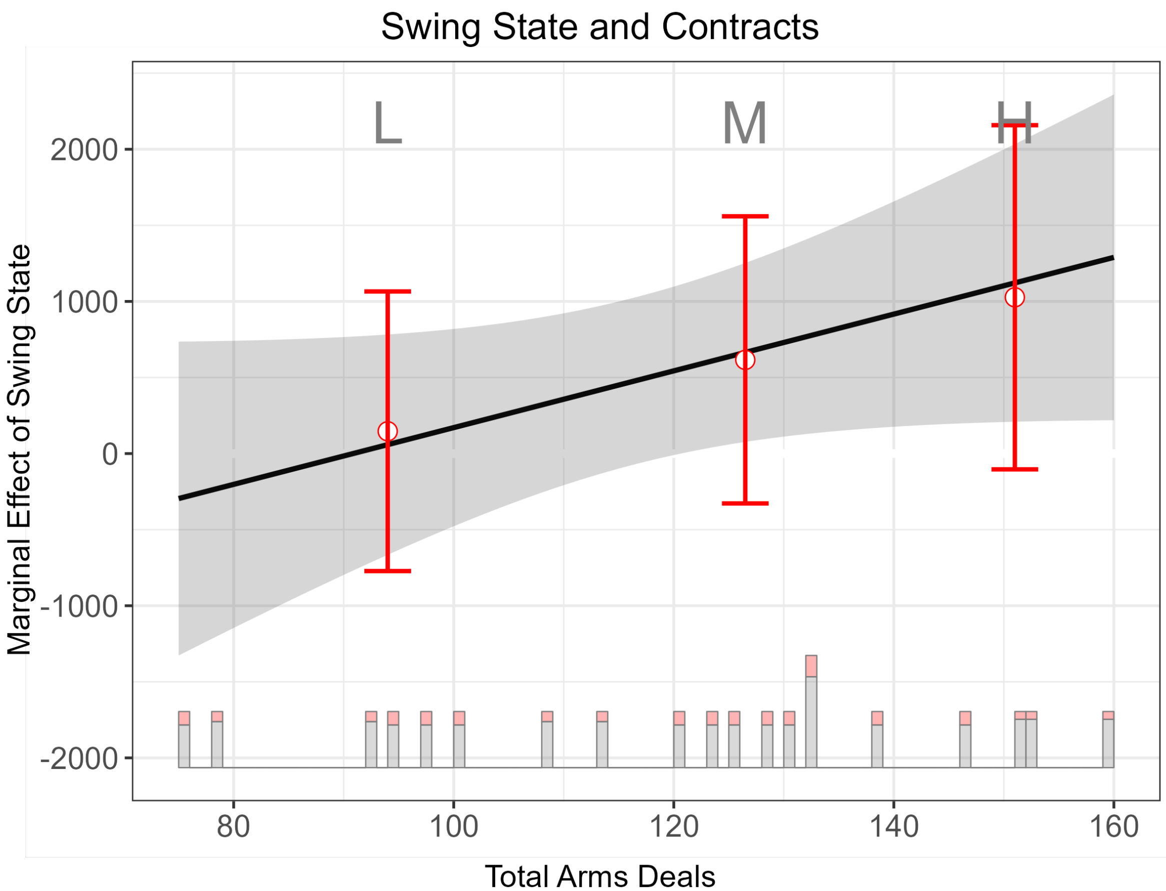

Figure 22. *Marginal effect of swing state status on defense contract awards based on arms deals. Estimates in millions of dollars.*

5 Other Democratic Arms Exporters

In this section, I check whether the argument applies to other democratic major powers that also export arms to many other states. Specifically, I examine whether France, the United Kingdom and Germany sell more arms to autocrats during or near elections. I selected these three countries due to the size of their arms industries, varied electoral systems, and major power status. I find little evidence that these states sell more arms to autocrats around elections.

For all three countries together and each country separately, I fit the same hurdle poisson model as in the manuscript, save for a different measure of election proximity. While French presidential election timing is fixed, Germany and the United Kingdom have variable election timing. Therefore, I interact dummies indicators of the year before and year of an election with the potential arms recipient's polyarchy score. I also use data from the ATOP project (Leeds et al., 2002) to measure whether recipients are allies of the seller, and use that variable in the hurdle equation.

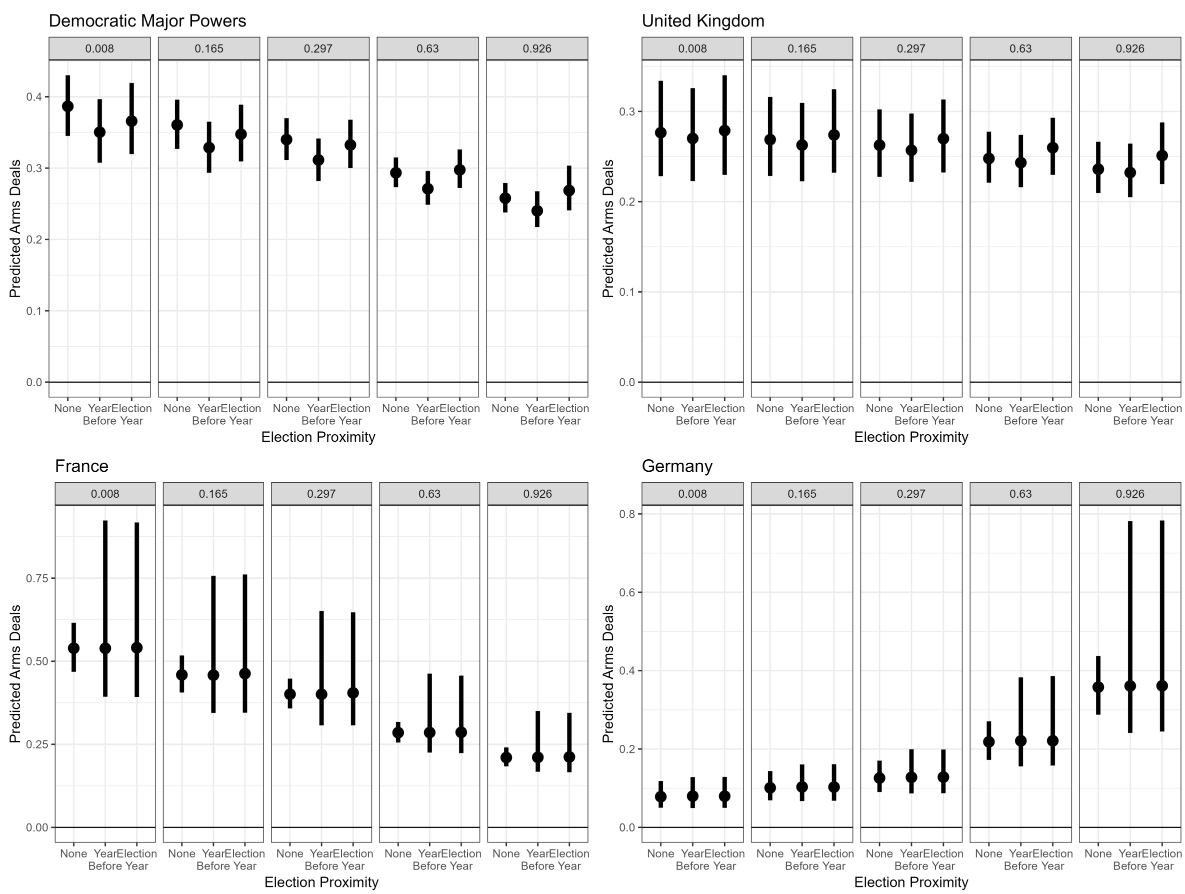

Figure 23. *Predicted arms deals in the year of an election, year before, and all other years in the United Kingdom, France and Germany. Estimates divided by the minimum, 25th precentile, median, 75th percentile and maximum of polyarchy.*

Figure 23 plots the results. Whether I analyze other democratic major powers jointly or individually, there is little evidence that these three states sell more arms to autocrats near elections. France and Britain both sell substantial arms to autocrats, and France is actually more likely to sell arms to autocrats, especially former colonies, but these orders do not track election years. Germany rarely sells arms to autocrats. For France and Germany, the election year indicators are more uncertain because elections are less common.

This suggests that among democracies, the United States is unusual in selling arms to autocratic security protégés around elections. In addition to a wide array of allies and security partners, the Electoral College may explain this difference. Arms sales provide benefits to specific constituencies in every country, but the Electoral College raises the stakes of this process by giving some constituencies exceptional influence. Winning a few thousand more votes in a swing state could turn the election, so U.S. leaders are willing to use every tool at their disposal.

6 Data Details

This section provides additional documentation, of variables, data sources, and coefficient estimates.

6.1 *Swing State List*

Figure 24 lists the set of swing states and the years they are swing states.

State	Start	End
Arizona	2017	2020
Colorado	2009	2012
Florida	2001	2020
Georgia	2013	2020
Iowa	2001	2012
Michigan	2017	2020
Minnesota	2005	2020
Missouri	2005	2012
Nevada	2001	2020
New Hampshire	2001	2020
New Mexico	2001	2008
North Carolina	2005	2020
Ohio	2001	2020
Pennsylvania	2005	2020
Tennessee	2000	2004
Virginia	2001	2020
Wisconsin	2001	2020

Figure 24. *List of swing states and years of swing status.*

6.2 *Variables and Sources*

Table 1 summarizes the variables and sources in the deals models. Some dyadic data from Miller (2022). Table 2 does the same for variables in the contracting models.

6.3 *Coefficient Estimates*

The section presents tables with coefficient estimates. Table 3 summarizes the hurdle Poisson coefficient estimates for the aggregate deals model, while Table 4 summarizes the models of

	Unique (#)	Missing (%)	Mean	SD	Min	Median	Max	Source
Total Arms Deals	27	0	0.9	2.3	0.0	0.0	48.0	(SIPRI, 2021)
Years to Election	4	0	1.5	1.1	0.0	1.0	3.0	Author
US Ally	3	21	0.4	0.5	0.0	0.0	1.0	(Leeds et al., 2002) & Author informal
Partner Polyarchy	914	17	0.4	0.3	0.0	0.3	0.9	(Coppedge, Alvarez and Maldonado, 2008)
Cold War	2	0	0.6	0.5	0.0	1.0	1.0	Author
Global War on Terror	2	0	0.2	0.4	0.0	0.0	1.0	Author
Republican President	2	0	0.6	0.5	0.0	1.0	1.0	Author
Log Petrol Revenue	4742	37	11.4	10.6	0.0	16.5	27.1	(Ross and Mahdavi, 2015)
Log Partner GDP	10481	23	21.6	0.9	17.1	21.7	25.2	(Feenstra, Inklaar and Timmer, 2015)
Ongoing MID	3	30	0.0	0.2	0.0	0.0	1.0	(Palmer et al., 2021)
Log Partner Population	11917	13	15.2	2.1	8.4	15.5	21.1	(Feenstra, Inklaar and Timmer, 2015)
Log Pop. Weighted Distance)	182	14	5.0	1.1	0.5	5.2	7.3	(Fouquin and Hugot, 2016)
Common Language	3	14	0.4	0.5	0.0	0.0	1.0	(Fouquin and Hugot, 2016)

Table 1. *Variables and data sources in arms deals models*

	Unique (#)	Missing (%)	Mean	SD	Min	Median	Max	Source
Contracts	948	5	2761.0	4703.9	0.0	907.3	56290.7	USASpending.gov
Lag Contracts	900	5	2552.6	4314.5	0.0	782.0	34204.8	Author
Arms Deals	20	0	120.1	23.4	75.0	125.0	160.0	(SIPRI, 2021)
Swing State	2	0	0.2	0.4	0.0	0.0	1.0	(Kriner and Reeves, 2015)
Core State	2	0	0.3	0.4	0.0	0.0	1.0	(Kriner and Reeves, 2015)
Global War on Terror	2	0	0.5	0.5	0.0	1.0	1.0	Author
Time to Election	4	0	1.4	1.1	0.0	1.0	3.0	Author
Republican President	2	0	0.6	0.5	0.0	1.0	1.0	Author
Population (Rescaled)	1000	0	0.0	0.5	−1.2	0.1	1.1	(Grossmann, Jordan and McCrain, 2021)
Log GDP (Rescaled)	1050	0	−0.0	0.5	−1.4	0.0	1.1	FRED (St. Louis Federal Reserve)

Table 2. *Variables and data sources in defense contracting models*

arms deals by type.[2]. Table 5 summarizes the three deals models and Table 6 gives ordered-beta coefficient estimates for the sectoral models of defense contracts.

[2] All tables built with modelsummary (Arel-Bundock, 2022)

Table 3. *: Coefficient estimates from Poisson models of US arms deals.*

	Generic Cycle	Regime Cycle (No Controls)	Regime Cycle	Regime and Ally Cycle
Hurdle: Intercept	5		5	5
	(3, 6)		(3, 6)	(3, 7)
Intercept	−0.9	−0.3	−1.0	−0.9
	(−1.5, −0.3)	(−0.4, −0.2)	(−1.6, −0.4)	(−1.5, −0.3)
Presidential Election	0.14	0.3	0.3	0.4
	(0.08, 0.20)	(0.1, 0.4)	(0.2, 0.5)	(0.2, 0.6)
1 Year to Election	0.007	−0.10	−0.05	0.06
	(−0.057, 0.069)	(−0.21, 0.02)	(−0.16, 0.08)	(−0.15, 0.27)
2 Years to Election	0.09	0.09	0.13	0.09
	(0.03, 0.15)	(−0.02, 0.20)	(0.01, 0.25)	(−0.11, 0.29)
US Ally	0.8			0.7
	(0.8, 0.9)			(0.6, 0.9)
Polyarchy	−0.06	1.0	0.4	−0.2
	(−0.14, 0.02)	(0.9, 1.1)	(0.3, 0.6)	(−0.5, 0.1)
Cold War	0.2		0.3	0.2
	(0.1, 0.3)		(0.3, 0.4)	(0.1, 0.3)
Global War on Terror	−0.14		−0.15	−0.14
	(−0.22, −0.06)		(−0.22, −0.07)	(−0.22, −0.07)
Republican President	−0.02		−0.02	−0.02
	(−0.07, 0.02)		(−0.07, 0.02)	(−0.07, 0.03)
Log Petrol Revenue	0.010		0.012	0.009
	(0.007, 0.013)		(0.009, 0.015)	(0.006, 0.012)
Log GDP	−0.034		−0.031	−0.035
	(−0.061, −0.008)		(−0.058, −0.005)	(−0.061, −0.009)
Ongoing MID	−0.3		−0.15	−0.3
	(−0.5, −0.1)		(−0.34, 0.03)	(−0.5, −0.1)
Log Population	0.2		0.2	0.2
	(0.1, 0.2)		(0.1, 0.2)	(0.1, 0.2)
Log Distance	−0.12		−0.07	−0.11
	(−0.15, −0.08)		(−0.10, −0.04)	(−0.14, −0.08)
Common Language	0.044		0.13	0.050
	(0.001, 0.087)		(0.09, 0.18)	(0.006, 0.096)
Hurdle: US Ally	−2		−2	−2
	(−2, −2)		(−2, −2)	(−2, −2)
Hurdle: Polyarchy	0.4		0.4	0.4
	(0.2, 0.6)		(0.2, 0.6)	(0.2, 0.6)
Hurdle: Ongoing MID	0.7		0.7	0.6
	(0.2, 1.1)		(0.2, 1.1)	(0.2, 1.1)
Hurdle: Log GDP	−0.16		−0.16	−0.16
	(−0.23, −0.09)		(−0.23, −0.08)	(−0.24, −0.08)
Presidential Election:Polyarchy		−0.22	−0.4	−0.33
		(−0.41, −0.05)	(−0.6, −0.2)	(−0.76, 0.09)
Polyarchy:1 Year to Election		0.14	0.09	−0.2
		(−0.06, 0.33)	(−0.11, 0.28)	(−0.7, 0.2)
Polyarchy:2 Years to Election		−0.05	−0.08	−0.1
		(−0.23, 0.13)	(−0.27, 0.11)	(−0.6, 0.3)
Presidential Election:US Ally				−0.13
				(−0.34, 0.09)
Polyarchy:US Ally				0.30
				(−0.01, 0.62)
US Ally:1 Year to Election				−0.1
				(−0.3, 0.1)
US Ally:2 Years to Election				0.1
				(−0.1, 0.3)
Presidential Election:Polyarchy:US Ally				−0.002
				(−0.446, 0.442)
Polyarchy:US Ally:1 Year to Election				0.3
				(−0.2, 0.8)
Polyarchy:US Ally:2 Years to Election				−0.01
				(−0.45, 0.45)

Note:
90% Credible Intervals in parentheses.

Table 4. *: Coefficient estimates from hurdle Poisson models of U.S. arms deals by sector.*

	Aircraft	Arms	Electronics	Missile and Space	Ships	Vehicles
Hurdle: Intercept	4.8	7.1	7.7	4.1	6.1	3.9
	(3.0, 6.7)	(4.0, 10.3)	(4.4, 11.0)	(1.0, 7.0)	(3.6, 8.7)	(1.3, 6.4)
Intercept	−0.25	−0.054	0.10	−0.082	−0.34	−0.23
	(−0.99, 0.49)	(−1.017, 0.853)	(−0.83, 1.05)	(−1.011, 0.859)	(−1.26, 0.61)	(−1.16, 0.74)
Presidential Election	0.198	0.20	0.20	0.089	0.15	0.21
	(0.031, 0.357)	(−0.24, 0.62)	(−0.15, 0.54)	(−0.333, 0.526)	(−0.18, 0.48)	(−0.11, 0.50)
Polyarchy	0.142	0.31	0.23	0.61	0.67	0.90
	(−0.067, 0.364)	(−0.11, 0.75)	(−0.19, 0.66)	(0.14, 1.06)	(0.35, 0.98)	(0.56, 1.23)
1 Year to Election	−0.34	−0.23	0.273	−0.11	−0.019	−0.16
	(−0.54, −0.16)	(−0.71, 0.24)	(−0.065, 0.617)	(−0.58, 0.34)	(−0.364, 0.330)	(−0.52, 0.18)
2 Years to Election	−0.0026	−0.512	−0.069	0.34	−0.053	0.15
	(−0.1686, 0.1710)	(−0.995, −0.059)	(−0.425, 0.289)	(−0.11, 0.76)	(−0.371, 0.260)	(−0.16, 0.48)
Cold War	0.40	0.48	0.53	0.220	0.23	0.101
	(0.32, 0.48)	(0.25, 0.70)	(0.25, 0.83)	(−0.031, 0.456)	(0.11, 0.37)	(−0.069, 0.274
Republican President	−0.0718	−0.072	0.215	−0.063	−0.051	−0.0023
	(−0.1384, −0.0018)	(−0.279, 0.141)	(0.037, 0.406)	(−0.285, 0.157)	(−0.175, 0.080)	(−0.1553, 0.145
Log GDP	−0.045	−0.126	−0.0713	−0.054	−0.0489	−0.011
	(−0.080, −0.010)	(−0.206, −0.047)	(−0.1461, 0.0078)	(−0.132, 0.026)	(−0.1014, 0.0016)	(−0.068, 0.047
Ongoing MID	−0.17	−0.33	−0.071	−0.23	−0.0018	−0.14
	(−0.46, 0.11)	(−1.13, 0.38)	(−0.885, 0.630)	(−0.95, 0.45)	(−0.5404, 0.4498)	(−0.65, 0.32)
Log Petrol Revenue	0.0120	−1.4e−02	−0.0157	0.0081	0.0136	0.0111
	(0.0076, 0.0163)	(−2.8e−02, 1.8e−06)	(−0.0280, −0.0029)	(−0.0065, 0.0237)	(0.0052, 0.0225)	(0.0014, 0.0208
Log Population	0.059	0.0967	−0.0086	0.033	0.12	0.0032
	(0.029, 0.089)	(0.0016, 0.1899)	(−0.1111, 0.0896)	(−0.075, 0.140)	(0.06, 0.18)	(−0.0693, 0.079
Log Distance	0.065	0.208	0.25	0.011	−0.157	0.020
	(0.019, 0.111)	(0.067, 0.352)	(0.11, 0.41)	(−0.126, 0.149)	(−0.233, −0.077)	(−0.081, 0.117
Common Language	0.35	0.147	0.103	0.12	−0.029	0.101
	(0.28, 0.42)	(−0.056, 0.354)	(−0.081, 0.292)	(−0.10, 0.33)	(−0.159, 0.100)	(−0.043, 0.244
Presidential Election:Polyarchy	−0.097	0.0051	−0.11	0.12	−0.11	−0.5128
	(−0.370, 0.183)	(−0.5784, 0.5693)	(−0.72, 0.47)	(−0.48, 0.71)	(−0.57, 0.36)	(−1.0204, −0.007
Polyarchy:1 Year to Election	0.51	0.23	−0.37	0.623	−0.10	0.34
	(0.20, 0.82)	(−0.43, 0.89)	(−0.95, 0.22)	(0.028, 1.263)	(−0.58, 0.37)	(−0.19, 0.84)
Polyarchy:2 Years to Election	0.214	0.533	0.042	−0.27	0.28	−0.31
	(−0.079, 0.493)	(−0.079, 1.147)	(−0.544, 0.634)	(−0.90, 0.37)	(−0.15, 0.71)	(−0.83, 0.18)
Hurdle: US Ally	−2.1	−1.4	−2.7	−1.3	−1.7	−1.6
	(−2.2, −1.9)	(−1.6, −1.1)	(−3.0, −2.4)	(−1.6, −1.1)	(−1.9, −1.5)	(−1.8, −1.4)
Hurdle: Ongoing MID	0.64	0.60	1.46	0.22	0.37	0.36
	(0.20, 1.11)	(−0.34, 1.83)	(0.41, 2.99)	(−0.51, 1.12)	(−0.30, 1.12)	(−0.24, 1.06)
Hurdle: Log GDP	−0.141	−0.1364	−0.180	−0.024	−0.1156	−0.051
	(−0.226, −0.057)	(−0.2820, 0.0028)	(−0.326, −0.028)	(−0.159, 0.115)	(−0.2358, −0.0012)	(−0.168, 0.066
Hurdle: Polyarchy	1.05	−0.91	1.4	−0.05	−0.74	1.13
	(0.83, 1.28)	(−1.29, −0.52)	(1.1, 1.7)	(−0.41, 0.30)	(−1.04, −0.45)	(0.82, 1.44)

Note:
90% Credible Intervals in parentheses.

Table 5. *: Coefficient estimates from models of defense contract awards.*

	Rescaled Ordered Beta	Log-Normal Hurdle	Student-T: Contract Changes
Intercept	-5.1	6.2	0.35
	(-5.5, -4.6)	(5.7, 6.8)	(-3.58, 4.15)
Arms Deals	-0.00026	-0.00209	0.081
	(-0.00155, 0.00103)	(-0.00387, -0.00038)	(-0.033, 0.203)
Swing State	-0.289	-0.56	0.087
	(-0.546, -0.033)	(-0.91, -0.21)	(-3.829, 3.976)
Core State	0.0421	0.080	0.0032
	(-0.0073, 0.0890)	(0.014, 0.149)	(-3.9231, 3.9248)
Global War on Terror	0.012	0.26	0.74
	(-0.057, 0.083)	(0.18, 0.35)	(-3.04, 4.49)
Time to Election	0.00093	-0.045	0.076
	(-0.01577, 0.01755)	(-0.067, -0.022)	(-3.475, 3.634)
Republican President	0.028	-0.0083	1.2
	(-0.023, 0.081)	(-0.0768, 0.0582)	(-2.7, 5.1)
Population (Rescaled)	0.098	-0.0097	-0.38
	(-0.012, 0.208)	(-0.1220, 0.1092)	(-4.24, 3.56)
Log GDP	-0.081	0.37	-0.23
	(-0.184, 0.015)	(0.25, 0.49)	(-4.02, 3.51)
Arms Deals:Swing State	1.9e-03	0.0041	0.370
	(-5.9e-05, 3.9e-03)	(0.0014, 0.0068)	(0.097, 0.647)
ϕ	629		
	(566, 697)		
Hurdle: Intercept		-2.9	
		(-3.2, -2.6)	
Hurdle: Log GDP		-0.663	
		(-1.266, -0.063)	
σ		0.39	135
		(0.37, 0.40)	(121, 152)

Note:
90% Credible Intervals in parentheses.

Table 6. *: Coefficient estimates from models of defense contract awards by sector.*

	Aircraft	Arms	Electronics	Missile and Space	Ships	Vehicles
Intercept	−5 (−6, −5)	−5 (−5, −4)	−5 (−6, −5)	−6 (−6, −5)	−5 (−6, −5)	−5 (−5, −5)
Aircraft Deals	−2e−04 (−4e−03, 3e−03)					
Swing State	−0.34 (−0.62, −0.07)	−0.32 (−0.56, −0.07)	−0.10 (−0.24, 0.04)	0.25 (−0.02, 0.52)	0.1 (−0.2, 0.4)	0.09 (−0.18, 0.36)
Core State	−0.02 (−0.07, 0.04)	0.08 (−0.01, 0.18)	0.02 (−0.03, 0.08)	−0.02 (−0.10, 0.05)	0.079 (−0.007, 0.167)	0.3 (0.1, 0.4)
Global War on Terror	0.06 (−0.02, 0.14)	0.10 (−0.02, 0.22)	0.006 (−0.070, 0.081)	0.07 (−0.03, 0.17)	−0.04 (−0.15, 0.07)	0.004 (−0.127, 0.124)
Republican President	−0.02 (−0.10, 0.05)	0.01 (−0.07, 0.10)	−0.02 (−0.06, 0.03)	−0.06 (−0.15, 0.03)	0.05 (−0.03, 0.14)	−0.09 (−0.21, 0.03)
Log GDP	0.09 (−0.03, 0.21)	0.12 (−0.08, 0.30)	0.16 (0.04, 0.28)	0.02 (−0.18, 0.21)	−0.2 (−0.4, −0.1)	−3e−04 (−2e−01, 2e−01)
Population (Rescaled)	−0.004 (−0.149, 0.138)	−0.03 (−0.22, 0.16)	−0.122 (−0.237, −0.004)	−0.01 (−0.21, 0.20)	0.172 (−0.006, 0.352)	−0.09 (−0.26, 0.07)
Time to Election	0.007 (−0.016, 0.029)	0.009 (−0.023, 0.038)	0.003 (−0.014, 0.021)	0.005 (−0.024, 0.035)	−0.002 (−0.034, 0.029)	0.01 (−0.03, 0.05)
Aircraft Deals:Swing State	0.004 (−0.002, 0.009)					
ϕ	401 (360, 445)	185 (166, 206)	663 (597, 735)	270 (240, 302)	170 (151, 189)	101 (89, 115)
Arms Deals		−6e−03 (−1e−02, 8e−04)				
Arms Deals:Swing State		0.011 (−0.002, 0.023)				
Electronics Deals			−5e−04 (−7e−03, 6e−03)			
Electronics Deals:Swing State			0.007 (−0.003, 0.017)			
Missile & Space Deals				0.003 (−0.002, 0.009)		
Missile & Space Deals:Swing State				−0.008 (−0.018, 0.002)		
Ships Deals					0.004 (−0.020, 0.026)	
Ships Deals:Swing State					0.02 (−0.02, 0.06)	
Vehicles Deals						−0.005 (−0.014, 0.004)
Vehicles Deals:Swing State						0.005 (−0.009, 0.018)

Note:
90% Credible Intervals in parentheses.

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