POLSCI.733
Maximum likelihood estimation

Term paper

Dag Tanneberg*

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Contents

GitHub: https://github.com/dagtann/mleTermpaper

* dag.tanneberg@wzb.eu

1. Introduction

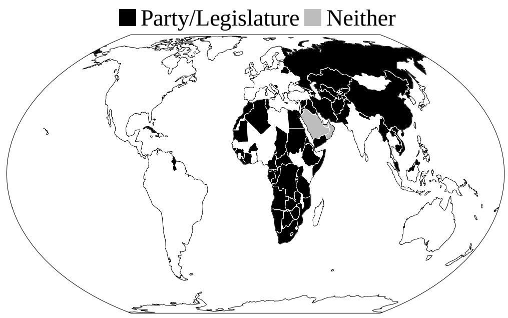

Figure 1: Parties and legislatures in authoritarian regimes, 2004

Research holds that co-optation and political repression are two mainstays of authoritarian rule (Gerschewski, 2013, 21f.). The former is usually defined as "the intentional extension of benefits to potential challengers to the regime in exchange for their loyalty" (Frantz and Kendall-Tylor, 2014, p. 333). Legislatures and political parties are said to simplify such exchanges. After the end of the Cold War those nominally democratic institutions sprung up in almost every authoritarian regime, and by 2004 only Saudi Arabia, Oman, the United Arab Emirates, and Qatar sustained neither political parties nor a parliament. Yet, authoritarian regimes did not forget about political repression. Restrictions on political liberties and violations of physical integrity rights are pervasive in authoritarian politics. However, little is know about how co-optation affects political repression.

This is the point of departure for Erica Frantz' and Andrea Kendall-Taylor's (2014) 'A dictators toolkit: Understanding how co-optation affects repression in autocracies'. Based on extensive quantitative analyses they argue that increasing levels of co-optation lead dictators to reduce restrictions on empowerment rights, but simultaneously they increase physical integrity violations (ibid., p. 332). The authors explain their key finding with the trade-offs involved in political repression. Restrictions on empowerment rights aim at the general public and characterize a diffuse approach to social control. Physical integrity violations in contrast target specific individuals and are attractive when the opposition is known. Nominally democratic institutions offer fora where regime opponents can raise demands, and thus they generate knowledge on the strength of the political opposition. Under the bottom line, institutionalized co-optation generates knowledge on threats to the regime and leads dictators to prefer physical integrity violations over empowerment rights restrictions (ibid., p. 337).

This paper replicates the work of Frantz and Kendall-Taylor. It presents evidence on the violation of a key statistical assumption, it shows the weak predictive power of the original analysis, and it criticizes the use of over-parameterized statistical models. My revision of the original analysis probes the interaction between physical integrity violations and co-optation more directly. As government respect for the integrity of person decreases the credibility of political parties and parliaments diminishes. Hence, their liberating impact on empowerment rights should decrease as physical integrity violations intensify. Section two describes design and data of the original publication, and section three presents the replication results. Section four discusses my modified model, and section five concludes.

2. Design & data

Based on Geddes et al.'s (2014) 'Autocratic regimes' data Frantz and Kendall-Taylor analyze 154 dictatorships over the period from 1981 to 2004. The authors follow the example of J. R. Vreeland (2008) and run ordered logistic regressions (c.f. Fox, 2008; Fox and Weisberg, 2011; Ward and Ahlquist, 8/06/2014) to account for the ordinal nature of their dependent variables. Consequently, the research design probes the effect of co-optation on either type of political repression based on pooled time-series cross-section data. Furthermore, as institutional changes might take time to impact government policies, Frantz and Kendall-Taylor use contemporaneous levels of co-optation (t_0) to predict future levels of political repression ($t + 1$ to $t + 5$). All models include a lagged dependent variable (t_0) to account for serial autocorrelation and standard errors are clustered at the country level to counteract heteroscedasticity (Beck and Katz, 1995). Finally, Frantz and Kendall-Taylor used multiple imputation to avoid inefficiency and biased estimates or inferences (Honaker and King, 2010; Honaker, King, and Blackwell, 2011; King, Honaker, et al., 2001). This section introduces the three key variables involved, Appendix A provides summary information on all variables.

Information on political repression is drawn from two different sources. Empowerment rights restrictions are measured using Freedom House's civil liberties scale. It captures the extent to which citizens enjoy the "freedoms of expression and belief, associational and organizational rights, rule of law, and personal autonomy from the state" (Freedom House, 2010). In contrast to alternative measurements, Frantz and Kendall-Taylor argue, the Freedom House data is not endogenous to the existence of political parties and legislatures, i.e. co-optation. The scale runs from 1 to 7, and higher values denote more restrictions on empowerment rights. Physical integrity violations are measured using the physical integrity index from the CIRI human rights dataset which provides "standards-based measures of government human rights practices" (Cingranelli and Richards, 2010, p. 402). It assesses the extent of torture, political imprisonment, extra-judicial killings, and disappearances on a scale from 0 to 8 whereby higher values denote more government respect for the sanctity of person. Frantz and Kendall-Taylor recode the index such that higher values denote more political repression.

The typology of political repression draws out meaningful differences between authoritarian regimes. This can be seen from Figure 2 which explores their relationship in the unimputed sample. The full range of physical integrity violations is observed, but empowerment rights restrictions do not take their lowest possible value 1. Hence, all authoritarian regimes restrict civil and political liberties, but they do not always disrespect the sanctity of the individual. Moreover, Pearson's r between both repression types is only 0.31, and the LOESS smoother indicates that this already weak relationship disappears in certain regions of the data. More precisely, the smoother stays flat across the most densely populated interval of empowerment rights restrictions (4 to 6) and no inferences whatsoever may be drawn from changes in one type of political repression on the other. Consequently, although authoritarian regimes use both types of political

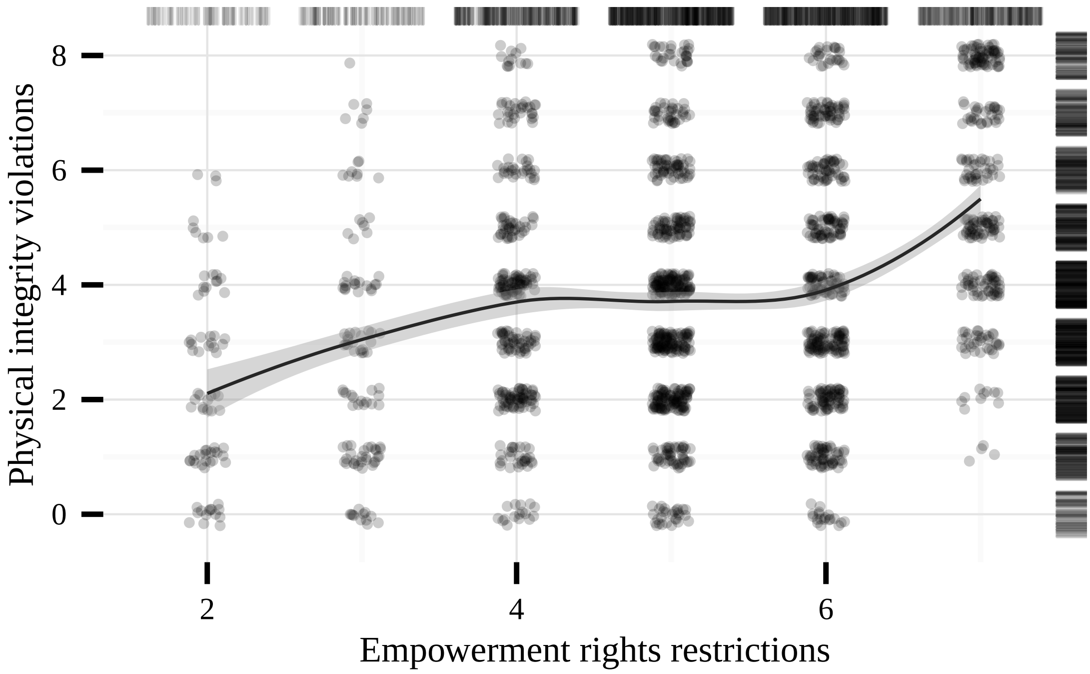

Figure 2: Political repression in authoritarian regimes between 1981 and 2004. Rug plots and LOESS smoother with .95 per cent confidence envelope added.

repression there is empirical reason to believe that they differ to "the extent to which they rely on one type more than the other" (Frantz and Kendall-Tylor, 2014, p. 336).

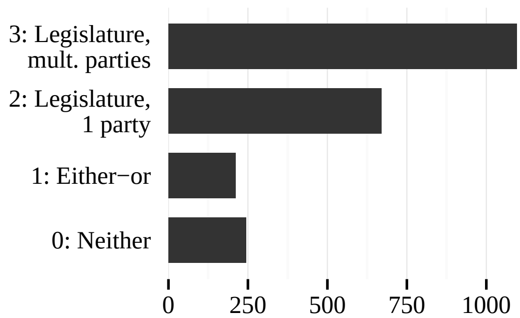

Figure 3: Co-optation, 1981-2004

Frantz and Kendall-Taylor assume that co-optation tips the scales of political repression. They measure this key explanatory variable by the existence of political parties and legislatures. Information on either institution is drawn from the 'Democracy & Dictatorship' data (Cheibub, Gandhi, and J. Vreeland, 2010) that map their de facto existence. Frantz and Kendall-Taylor create an index that takes the value of 3 if there is a multi-party legislature, 2 if there is a single-party legislature, 1 if there is no legislature but at least one political party or, equivalently, if there is a non-partisan legislature, and 0 if neither exists. The authors presume that their index behaves linearly, and they justify their coding scheme with an interest in the "interactive effect" of legislatures and political parties (Frantz and Kendall-Tylor, 2014, p. 338). Figure 3 explores the empirical picture in the unimputed data. The majority of $2,221$ non-missing country-year observations falls into the highest category. Accordingly, more than half of all authoritarian regimes in the data sponsor multi-party legislatures. Single-party regimes come in second, and only a minority of observations ranks lower than 2 on the index. In sum, the crucial empirical distinction is whether authoritarian regimes co-opt via single party or multiple parties.

3. Replication results

At first sight the key findings discussed by Frantz and Kendall-Taylor hold. However, critical evaluation of a key statistical assumption, predictive accuracy, and model parsimony give reason to doubt their conclusions. Following a brief recapitulation of the key results each point is briefly discussed in the remainder of this section.

Figure 5 summarizes all ordered logistic regressions presented in the original article. Differences between the published and the replicated analyses are mostly negligible. With few exceptions coefficients and cluster robust standard errors agree up to two decimal places.[1] As can be seen from the top row in Figure 5 higher levels of co-optation concur with lower levels of empowerment rights restrictions, but they tend to go hand in hand with increases in physical integrity violations. Moreover, in line with the idea of inert government practices the attenuating impact of co-optation on empowerment rights restrictions increases in absolute size when moving from $t+1$ to $t+5$. The same time-dependent dynamic is not observable for physical integrity violations. Finally, the models speak to the staying power of political repression because all lagged responses are positively signed and statistically significant. In short, key findings can be reproduced and a more detailed discussion of the original publication is possible.

Ordered logistic regression rests on the parallel-regressions assumption. It constrains differences between the cumulative distribution functions of any two categories to a constant, and thus all regression coefficients are assumed to be equal across categories (Fox, 2008, p. 476). One way to test the assumption is a χ^2-comparison between the constrained coefficients and their unconstrained alternatives from a multinomial regression. As shown in Table 1 only the $t+1$ and $t+2$ models reject the alternative hypothesis of non-constant coefficients and thus support the choice of statistical method. However, since very conservative Bonferroni adjusted P-values save the null an additional inspection seems justified. To that end $j-1$ separate logistic regressions were fit to the set of binary responses

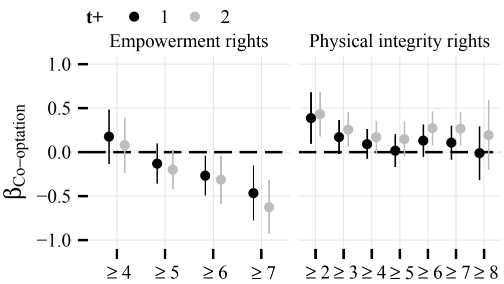

Figure 4: Separate logistic regressions

[1] A fundamental difference concerns the polynomials on tenure duration. The models would not converge in R unless multicollinearity was reduced by using orthogonal polynomials.

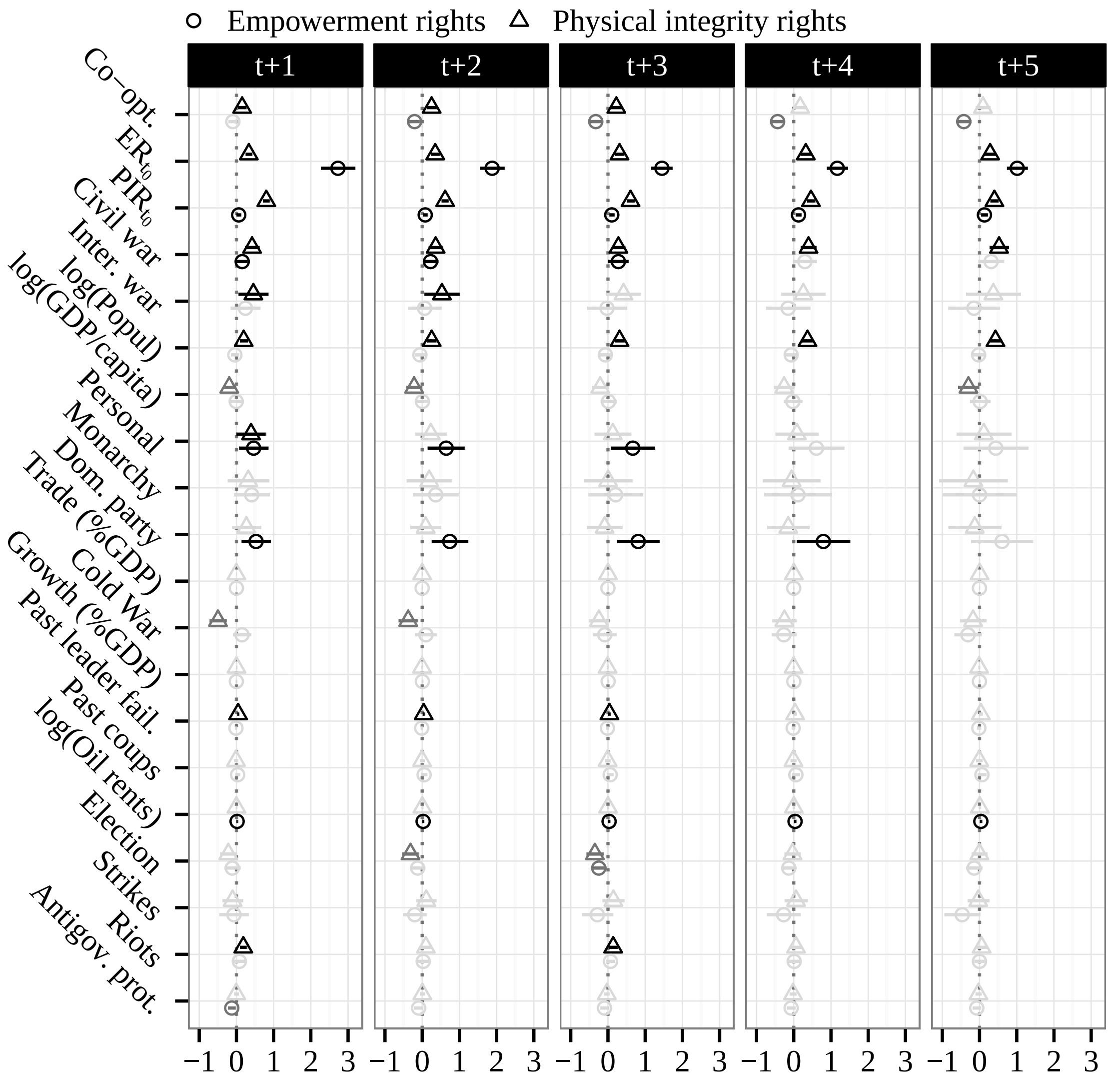

Figure 5: How co-optation affects political repression. Confidence intervals at the .95 level, ● > 0, ● < 0, ● $0 \in$ CI. Cubic polynomials and cut points not shown.

$\mathbb{1}_y(y_i \geq j)$.[2] If the parallel-regressions assumption holds the coefficients should differ little as j increases (c.f. Ward and Ahlquist, 8/06/2014, 107ff. Gelman and Hill, 2006, p. 123). Figure 4 shows the results for the key regressor co-optation with 95 per cent confidence intervals added. While the right-hand panel raises little reason for concern, coefficients in the left-hand panel exhibit a clear trend. As the level of empowerment rights restrictions increases co-optation develops more of a punch. In sum, the majority

Table 1: Parallel-regressions assumption: χ^2-comparisons

		$t+1$	$t+2$	$t+3$	$t+4$	$t+5$
Empowerment rights	Unadj. P-value	1.000	0.499	0.000	0.000	0.000
	Bonf. adj. P-value	1.000	0.833	0.000	0.000	0.000
Physical integrity	Unadj. P-value	0.003	0.002	0.000	0.000	0.000
	Bonf. adj. P-value	0.077	0.052	0.001	0.000	0.000

Note: P-values were averaged over all imputed models.

of models fails the parallel-regressions assumption, but even if it is not rejected further scrutiny yields reason for concern.

How well do Frantz' and Kendall-Taylor's analyses predict political repression in their sample? Since the consequences of failing the parallel-regressions assumption are not well understood predictive accuracy might be more important than compliance with statistical technicalities. Using separation plots Figure 6 probes this possibility for the four models that did not immediately fail the χ^2-comparison (Greenhill, Ward, and Sacks, 2011). Their implications are unsettling. With regard to physical integrity violations the analyses are seemingly unable to discriminate between category members (•) and non-members (•). Furthermore, the line of predicted probabilities stays flat in all but the extreme categories. Turning to empowerment rights restrictions the state of things seems slightly better. Either model, $t+1$ and $t+2$, tends to predict higher probabilities for category members than for non-members. Furthermore, the line of predicted probabilities visibly increases across all levels of empowerment rights restrictions. Notwithstanding, the one-year lead model clearly fits the data best. In sum, decrying statistical significance co-optation offers little leverage on physical integrity violations, and only the one-year lead model for empowerment rights restrictions unquestionably discriminates between members and non-members of every response category.

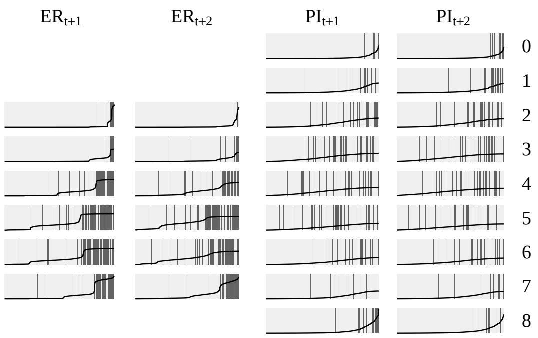

Figure 6: Predictive accuracy

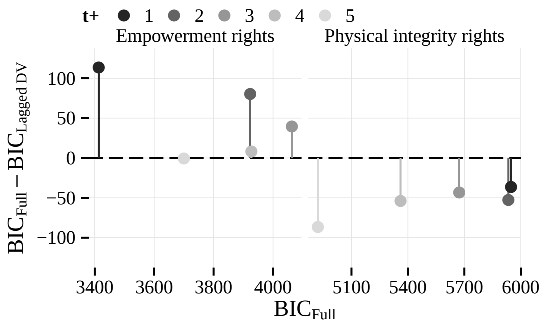

Figure 7: Parsimony

[2] The marginal categories of all responses are sparsely populated and perfect separation occurred. The affected categories were discarded.

However, as shown in Figure 5 co-optation is not even a statistically significant predictor of empowerment rights restrictions at $t + 1$. What variable then accounts for the separation just described? Figure 7 probes this question. It compares BIC values from each full model to stripped down versions that include only the lagged response. If the latter accounts for serial autocorrelation only, then the full set of independent variables adds explanatory power and the BIC should decrease. Consequently, the differences shown on the vertical axis of Figure 7 should be negative.[3] The results are again unsettling. On the one hand the differences in BICs for physical integrity violations are always negative and large in absolute value. Nonetheless, those sizeable improvements in model fit do not add any predictive power at all and are thus inconsequential (c.f. Figure 6).[4] On the other hand, differences in BIC for empowerment rights are always non-negative. While the one-year lead performed best in terms of prediction, it performs worst in terms of parsimony. The corresponding difference in BICs is more than 100 points! Hence, the improvement in fit over the stripped down model does not justify the inclusion of the full set of covariates. More generally, the single most parsimonious predictor of empowerment rights restrictions at $t + 1$, $t + 2$, $t + 3$, and $t + 4$ is the lagged response. In one sentence: Frantz and Kendall-Taylor likely overfit their data.[5]

In sum, Frantz' and Kendall-Taylor's results can be reproduced without noteworthy deviations. However, more nuanced assessements show most models presented in the original publication fail a key statistical assumption of ordered logistic regression. Moreover, it turns out that the core findings go hand in hand with weak predictive accuracy and strong signs of overfitting. It is thus open for debate what we can learn about the interaction of political repression and co-optation from Frantz' and Kendall-Taylor's analyses.

4. Extension

My alternative approach to co-optation and political repression probes their interaction more directly.[6] I assume that the attenuating effect of co-optation on empowerment rights restrictions that Erica Frantz and Andrea Kendall-Taylor discuss is conditional on the level of physical integrity violations. The more dictators engage in torture and mayhem, the less credible become their institutional commitments. Thus, I assume that the extent of physical integrity violations undermines any supporting effect that political parties and legislatures might have on the validity of empowerment rights. To ascertain the validity of this intuition I apply an ordinary least squares regression to the unimputed raw data. As this extension serves as a first approximation to a more general

[3] BIC values were averaged over all imputations.

[4] Predictive accuracy declines with every lead. Results are available from the separation plot scripts on my GitHub.

[5] To remove more than 20 predictors from a statistical model is a somewhat drastic change and inhibits more nuanced assessments. Nonetheless, the results are too unequivocal to be mere artifacts.

[6] For detailed results see Appendix B.

research project I forego multiple imputation and focus on more pressing concerns of variable selection, statistical design, and validation.

Frantz' and Kendall-Taylor's analyses include numerous predictors that are not meaningful control variables. For instance, trade in per cent of GDP per capita neither correlates with political repression nor with co-optation. Thus, as a first design decision I remove all variables from the analysis that have a Pearson's $|r|$ of less than 0.1 with any of the three core variables. Second, I employ co-optation as a categorical predictor to validate the original coding scheme. Third, I replace Beck's and Katz's standard TSCS recipe with the default heteroskedasticity and autocorrelation consistent covariance estimator described by Zeileis (2004, p. 6). Fourth, I focus on the $t+1$ formulation of empowerment rights. Finally, I use 28 of 133 countries in the data as a validation set for the extended model in order to probe its external validity James et al., 2013, 175ff.

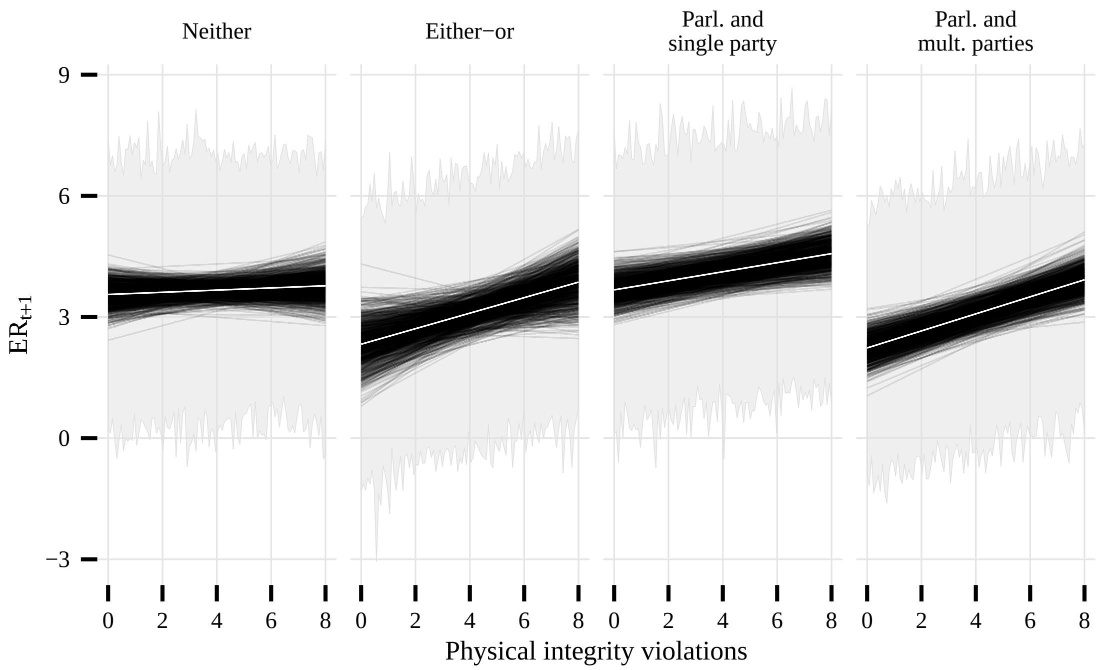

Figure 8: Simulated interaction of co-optation and physical integrity violations

Although most of the basic terms are not statistically significant an analysis of variance and a likelihood ratio test both find the assumed interaction to improve model fit. Using simulations with $1,000$ iterations Figure 8 evaluates it in more detail (King, Tomz, and Wittenberg, 2000). Ribbons in each panel denote stochastic uncertainty (●) and faded lines represent systematic uncertainty (●). White lines average over all estimates of systematic uncertainty and denote a mean effect. Figure 8 shows that empowerment rights restrictions at $t + 1$ increase in physical integrity violations for all but the 'Neither' category of co-optation. Furthermore, the intercepts of the mean effect graphs do *not* systematically decrease when moving from the 'Neither' to the 'Multi-party legislature' category of the co-optation index. Finally, ranges of systematic and stochastic uncertainty overlap between all panels. Taken together these observations on Figure 8 establish three crucial results: 1. The existence of political parties and parliaments does not lend itself to a linear additive index of co-optation; 2. Increasing physical integrity violations concur with increasing restrictions on empowerment rights under almost all institutional settings; 3. This finding seems not to be substantially significant.

Although the preceding analysis dampens optimistic expectations the extended model might still generalize meaningfully to other contexts. After all, simulations are a very tough benchmark for every statistical model. However, when moving to the validation-set my attempted extension fails again. For instance, the root mean square error (RMSE) of the training set is 1.01. It improves considerably over the standard deviation of the dependent variable in the training set (1.24). Nonetheless, the test-set RMSE is 1.49 – an almost 50 per cent increase over the training set. More importantly, the fitted model systematically overestimates within-country variation in the validation sample. This can be seen from the slope plot in Figure 9, which compares within-country standard deviations in empowerment rights restrictions at $t + 1$ to the corresponding within-country RMSE. Special emphasis is given to Saudi Arabia and Georgia which have the largest respectively smallest RMSE. Very few lines exhibit a negative slope or are at least constant as in the case of Georgia. Clearly dominant is the impression of an upward trend in within-country variation, which can be as large as a fivefold increase. This is the case in Saudi Arabia. In short, the assumed interaction does not generalize beyond the training set.

To summarize, despite its statistical significance the assumed interaction of co-optation and physical integrity violations is neither substantially significant nor externally valid. Moreover, it is apparent from the preceding analysis that the existence of political parties and legislatures does not lend itself to a linearly additive index of co-optation. Finally, my simple ordinary least-squares regression overestimates within-country variation in empowerment rights restrictions because it cannot separate lateral from longitudinal variance. In short, the analysis of co-optation and political repression in authoritarian regimes requires better measurements and more sophisticated statistical designs.

5. Summary

Authoritarian regimes maintain power via co-optation and political repression. Contemporary research has long recognized either as a pillar of authoritarian rule, but little is known about their mutual influence. This is the point of departure for Erica Frantz and Andrea Kendall-Taylor who argue that co-optation in the form of political parties and legislatures generates knowledge on the strength of the opposition such that dictators come to prefer targeted physical integrity violations over diffuse empowerment rights restrictions. My replication of their work uncovers the violation of a key statistical

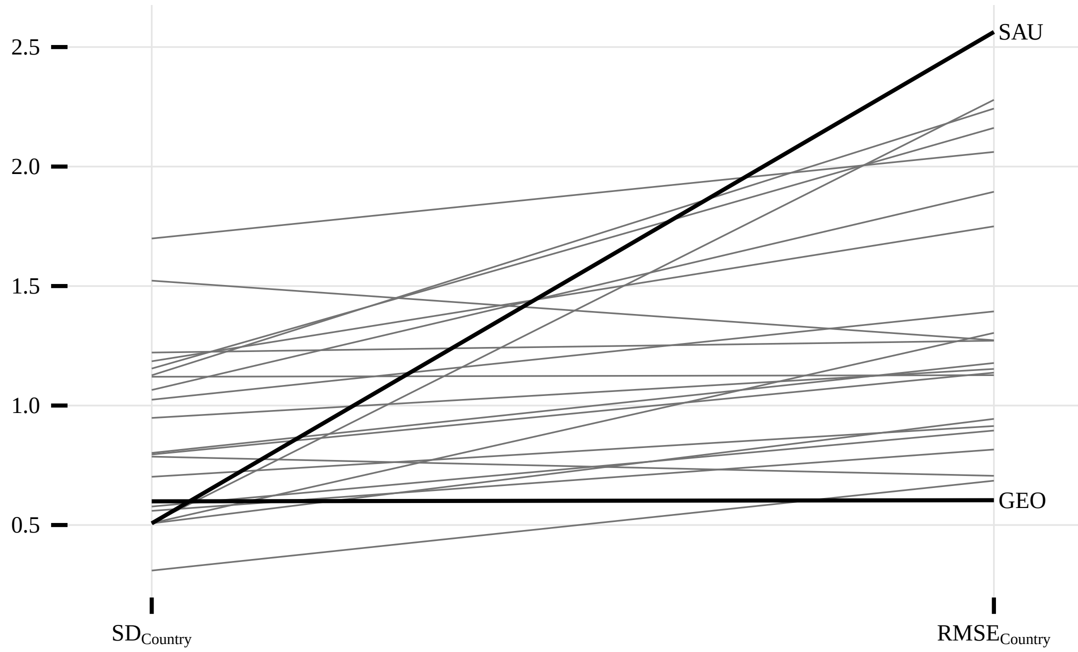

Figure 9: Country-based out-of-sample performance

assumption, it draws out the weak predictive accuracy of their analyses, and it hints overfitting.

My attempted extension of the original contribution emphasizes intuition over statistical complexity. I argue that the liberating effect of co-optation on empowerment rights is conditional on the level of physical integrity violations. As dictators engage increasingly in torture institutionalized co-optation should lose its bite. However, although the assumed interaction effect is statistically significant it lacks substantive significance and does not generalize from the training set. Moreover, my extension shows that the existence of political parties and legislatures does not seamlessly translate into a linear index of co-optation. Future revisions should, first, improve on the measurements involved and, second, employ statistically more sophisticated models such as longitudinal multilevel regression.

A. Summary statistics of controls

To account for alternative explanations of political repression Frantz and Kendall-Taylor include a large set of controls (c.f. Frantz and Kendall-Tylor, 2014, 338f.). Among these are counts of ongoing civil and interstate war as well as domestic political dissent in the form of riots, general strikes, and anti-government demonstrations. Moreover, the authors include counts of past leadership turnovers and attempted coups under the assumption that authoritarian regimes with a history of leadership instability are more willing to repress. Other controls map the socio-economic status and historical context of the regime. For instance, assuming that oil-revenues offer alternative ways of co-optation Frantz and Kendall-Taylor control for oil rents per capita. Moreover, since size and growth of the population have been discussed as potential causes for state repression in the past the authors control for those as well. Moreover, they add indicators on trade and economic well-being as well as regime type. Moreover, to account for its considerable geopolitical repercussions a Cold War dummy is added to the model. Finally, following the advice of Carter and Signorino (2010) cubic splines of leadership duration are added.

Table 2: Sample summary statistics

Statistic	Min	Mean	Max	St. Dev.	N
Co-optation	0	2.179	3	0.998	2,221
Empowerment rights restr.	2	5.180	7	1.292	2,184
Physical integrity violations	0	3.926	8	2.198	2,019
Civil war	0	0.240	5	0.601	2,386
Interstate war	0	0.063	2	0.250	2,386
log(population)	4.215	8.777	14.074	1.712	2,352
log(GDP per capita)	5.139	7.913	10.807	1.058	2,352
Personal regime	0	0.292	1	0.455	1,857
Monarchy	0	0.097	1	0.297	1,857
Dominant party regime	0	0.489	1	0.500	1,857
Trade (% of GDPpc)	0.309	76.026	423.568	45.332	2,024
Cold War	−50.046	1.003	90.470	7.694	2,049
Growth (% of GDPpc)	0	10.827	47	9.513	2,271
Leadership duration	0	4.379	43	6.471	2,386
Past leadership fails	0	2.264	22	3.004	2,386
Past coups	−11.513	−3.867	10.811	8.328	2,250
Oil rents	0	0.090	5	0.442	1,857
Election year	0	0.358	23	1.378	1,857
Strikes	0	0.634	26	2.034	1,857

B. Extended model results

Table 3: Extended model regression results

	ER_{t+1}	
	(1)	(2)
Co-optation: 1. Either-or	-0.65^{**} (0.28)	-1.22^{**} (0.55)
Co-optation: 2. Legislature, 1 party	0.31 (0.22)	0.11 (0.33)
Co-optation: 3. Legislature, mult. parties	-0.74^{***} (0.19)	-1.32^{***} (0.29)
Current PI	0.16^{***} (0.03)	0.03 (0.06)
log(GDP per capita)	0.06 (0.07)	0.06 (0.07)
log(Population)	-0.05 (0.06)	-0.05 (0.06)
Personalist regime	0.18 (0.21)	0.13 (0.22)
Monarchy	0.02 (0.24)	-0.13 (0.22)
Dominant party	-0.002 (0.22)	-0.05 (0.22)
Past leadership failure	-0.18^{*} (0.10)	-0.20^{**} (0.10)
Past coup attempts	0.06^{**} (0.03)	0.07^{**} (0.03)
log(Civil war)	0.29^{*} (0.17)	0.27 (0.17)
log(Interstate war)	0.12 (0.16)	0.18 (0.16)
log(General strike)	-0.05 (0.21)	-0.04 (0.21)
log(Riots)	-0.12 (0.10)	-0.13 (0.10)
log(Antigovernment demonstr.)	-0.18^{*} (0.09)	-0.18^{**} (0.09)
Year-1989	-20.07^{**} (8.17)	-20.00^{**} (8.12)
$(\text{Year-1989})^2$	24.03^{***} (6.50)	23.88^{***} (6.43)
Co-optation 1*Current PI		0.16 (0.11)
Co-optation 2*Current PI		0.09 (0.07)
Co-optation 3*Current PI		0.18^{**} (0.07)
Constant	3.20^{***} (0.84)	3.56^{***} (0.90)
N	1,229	1,229
R^2	0.32	0.33
Adjusted R^2	0.31	0.31
Residual Std. Error	1.03 (df = 1210)	1.02 (df = 1207)
F Statistic	30.98^{***} (df = 18; 1210)	27.77^{***} (df = 21; 1207)

$^{*}p < .1$; $^{**}p < .05$; $^{***}p < .01$

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