

# Online Appendix: Are coups good for democracy?

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**Abstract:** This document includes details on the specifications that produce the plots in the main text, along with numerical summaries of the main results. It also provides results from alternative specifications and robustness checks.

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## Appendix A: Coups and autocratic regime collapse: transitions to democracy and adverse regime change

This section discusses the statistical tests used to assess which regime transitions follow coups in dictatorships.<sup>1</sup> Some of the tests discussed are reproduced from the manuscript for the reader’s convenience. Several additional tests are reported as robustness checks.

To conduct our analysis, we begin with the latest coup data from Powell and Thyne (2011),<sup>2</sup> and merge it with an updated version of Geddes et al. (2014) (henceforth GWF). Thus, we obtain a sample of all autocratic regimes that experienced at least one coup in the period 1950-2015. After structuring our data at the country-year level, we then construct our dependent variable: an indicator of whether the autocratic regime collapses in the current year or the subsequent two years. Before proceeding, though, we clarify what is meant by “regime”.

An autocratic regime – or dictatorship – is a set of formal and/or informal rules for choosing leaders and policies (Geddes et al., 2014). The rule central to distinguishing one autocratic regime from another is the one that identifies the group from which leaders can be chosen and determines who influences personnel choice and policy—one autocratic regime replaces another when the group of elites who hold power changes. Autocratic regime collapse, in turn, occurs when the set of formal and informal rules for choosing leaders and policies changes. Crucially, this conceptualization of autocratic regimes affect the analysis of coup outcomes.

Coups in autocratic regimes may, in some cases, lead to regime collapse: when the coup leaders oust the current elites from power and establish a new ruling group. However, in many cases coups in autocracies only replace the incumbent leader with another member of the ruling elite. In other words, when a successful coup occurs in an autocracy, one of two outcomes emerges: the ruling group retains its power (*No change*), or loses its power (*Regime change*). The latter outcome, in turn, can bring to power one of two sets of elites: a new autocratic regime (*Autocratic transition*), or a democratic government (*Democratization*).

### Descriptive patterns

The top panel of Figure A-1 shows the frequency distribution of these potential outcomes.<sup>3</sup> Prior to 1990, the most common outcome is *Autocratic transition*, the next most common is *No change*, and *Democratization* the least common. In the post-Cold War period, coups are more destabilizing, as 90% are followed by regime collapse within two years. While *Democratization* becomes more likely in this period, *Autocratic transition* is still the modal outcome. Therefore, the increasing

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<sup>1</sup>We note that our study is not the first to address this question. See Miller (2011), Marinov and Goemans (2014), Thyne and Powell (2014), Beger (2015), and Noyes (2015) for additional analyses.

<sup>2</sup>This was downloaded from <http://www.jonathanpowell.com/coup-detat-dataset.html> on May 18, 2015.

<sup>3</sup>We only include coups that target an incumbent autocratic regime after January 1 of its first year in power. We consider regime collapse in the year of an observed coup event or the subsequent two years.

incidence of democratization should not obscure the more likely scenario of coups replacing one set of autocratic elites with another.

Most studies of coups focus on successful events. But what if coups affect the regime’s probability of collapse because they signal factionalism within the ruling elite? If that is the case, *Attempted coups*, irrespective of whether they achieve their goal, is the appropriate metric for our analysis. For this reason, the bottom panel of Figure A-1 redraws the distributions from the top panel, while grouping together successful and failed coups.

In both periods, *No change* is now the most likely outcome. Yet, this is partly owed to the fact that *No change* now includes not just successful coups that reshuffle the regime’s leadership without uprooting the regime itself, but also failed coup attempts, which are very frequent.<sup>4</sup> More importantly, the pattern observed in the top panel of the figure is roughly preserved: during the Cold War *Autocratic transition* is substantially more likely (38%) than *Democratic transition* (10%), but these two outcomes are almost equally likely in the past 25 years (27% percent and 31%, respectively).

## Empirical tests

In the main manuscript, we assess whether this descriptive pattern holds in regression models that account for confounders. Our focal independent variable is an indicator of whether a successful coup occurred in the current year or the prior two years. When coding coups in the current/observation year, we take care to exclude coups that occur in the same calendar year as the regime collapse event, but at a later calendar date.<sup>5</sup> This means we include coup events that, in some cases, *are the same political event* as the regime collapse event coded by GWF.<sup>6</sup>

We employ a linear probability model with regime-case and year fixed effects, and controls for regime duration (cubic polynomial) and leader time in power (logged). We estimate a separate equation for each of the three following outcomes: *All regime transitions*, *Democratic transition*, and *Autocratic transition*. Crucially, in the equations for the latter two, we drop observations where the other outcome of interest equals 1 to ensure we compare the probability of, for example,

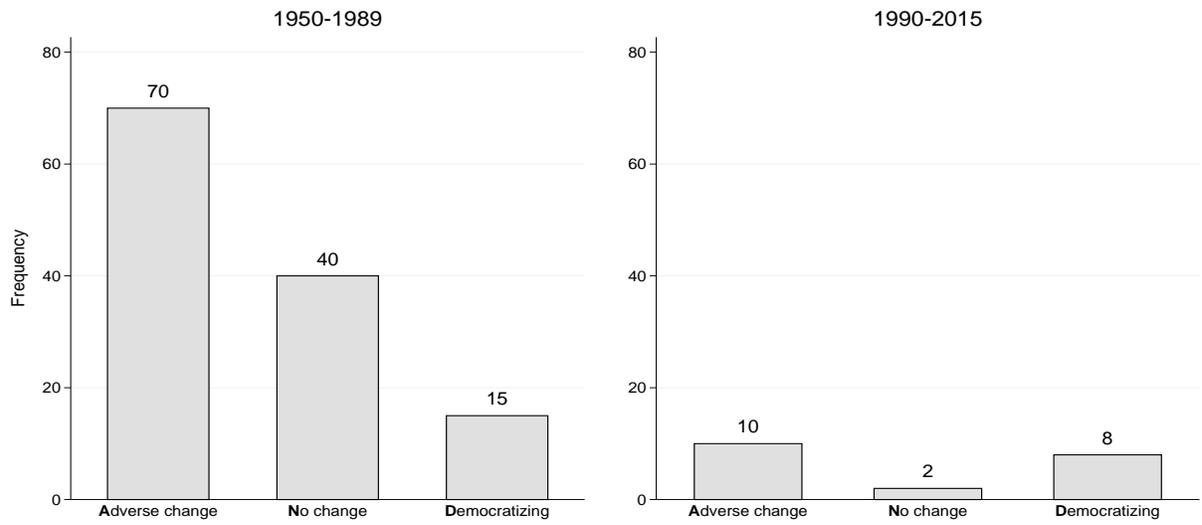
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<sup>4</sup>To put the recent failed coup attempt in Burundi (5/2015) in context, it falls into the *No change* category in the bottom-right plot of Figure A-1, since the Nkurunziza regime remained in power (as of this writing). This is not unusual; indeed, it is the most common outcome of a coup attempt in dictatorships, even in the post-Cold War period.

<sup>5</sup>In coding regime change coups, we include coup events coded by Powell and Thyne (2011) only if they occur up to the day after the regime collapse date coded in GWF.

<sup>6</sup>Using the Powell-Thyne data involves omitting two coup events that resulted in regime collapse according to GWF: Panama 1982 and Georgia 1992. The January 1992 regime failure in Georgia was a violent overthrow of the civilian government by a faction of the militia forces that had been incorporated into the national army (Zurcher, 2007: 126-27). The March 1982 coup in Panama, in turn, ousted the commander-in-chief of the National Guard. The civilian president chosen by Torrijos and the rest of the civilian government were forced to retire a few months later, completing a transition from the Torrijos regime, in which both officers in the National Guard and a broad alliance of civilians influenced policy, to a regime based on one faction of the National Guard and a much narrower group of civilians (Kempe, 1990: 114-24). As such, we follow GWF in coding these two events as coups, and classify them as regime collapse coups.

(a) Successful coups



(b) Attempted coups

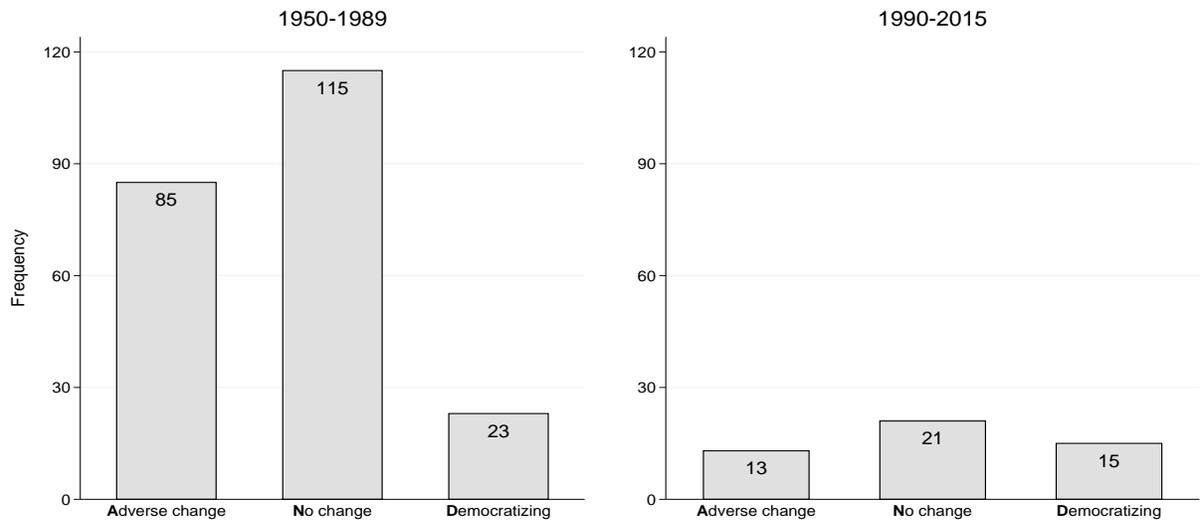


Figure A-1: Simple selection tabulations

democratization to the probability of no regime transition.<sup>7</sup> Also, note that using regime-case fixed effects isolates the variation across time within each autocratic regimes, thereby nullifying the leverage of cross-regime differences; for example, regimes’ (unobserved time-invariant) instability, and their (unobserved time-invariant) propensity to experience a coup.

The model estimated is:

$$Y_{it} = \alpha_0 + \beta_1(CW \times Coup)_{it} + \beta_2(postCW \times Coup)_{it} + \beta_3LeaderTime_{it} + \xi_i + \tau_t + \zeta_d + \varepsilon_{i,t} \quad (1)$$

where  $\beta_1$  is the coefficient for coups during the Cold War (*CW*) period and  $\beta_2$  is the coefficient for coups during the post-Cold War (*postCW*) period, while  $\xi_i$  are regime-case fixed effects,  $\tau_t$  are year fixed effects, and  $\zeta_i$  are regime duration polynomials.<sup>8</sup> Standard errors are clustered at the regime-case level.<sup>9</sup> We estimate a linear model so as not to drop autocratic regimes, such as China and Saudi Arabia, that do not experience transition during the sample period (1950-2015).<sup>10</sup>

The results presented in the main manuscript (Figure 1) are reported in Table A-1. The first three columns examine *Successful coups*, while the latter three look at all *Coup attempts*, successful and failed. Column 1 shows a positive correlation between coups and regime collapse in both time periods (*CW*, *postCW*). However, Columns 2 and 3 betray the source of this correlation: the association between coups and *Autocratic transition*, not democratization. A substantively identical pattern holds in the regressions for *Coup attempts* (Columns 4-6).

## Alternative specifications

We also fit a non-linear model that includes the regime case- and year-means for all explanatory variables as proxies for regime- and year-fixed effects (?: 487). This approach preserves the full sample (unlike a conditional logit), appropriately models the binary nature of the dependent variable (unlike the linear model), yet conditions the parameters of interest on regime-effects and year-effects. In doing so, the interpretation of the coefficients is similar to that from a fixed effects model.

We report the results in Table A-2. They are similar to those from the linear model: there is no positive correlation between coups and *Democratic transition* (Columns 1-2), but a strong

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<sup>7</sup>This is akin to how a multinomial logit works. If we have two outcomes of interest, *Democratic transition* (D) and *Autocratic transition* (A), then a multinomial logit estimates both  $Pr(D)$  vs.  $Pr(NoCollapse)$  and  $Pr(A)$  vs.  $Pr(NoCollapse)$ . An alternative is to estimate:  $Pr(D)$  vs.  $[Pr(A) + Pr(NoCollapse)]$  and  $Pr(A)$  vs.  $[Pr(D) + Pr(NoCollapse)]$ . However, this would yield a lower estimate for the coefficient of coups in the *Democratic transition* equation, and a larger estimate in the *Autocratic transition* equation.

<sup>8</sup>Separate parameters for the Cold War and post-Cold War periods are dropped because of the year fixed effects. Also, we should note that the linear model and the particular parameterization of the interaction between the Cold War indicator and the coup variables are presentational short-cuts. In the replication code, we test separate models for the Cold War and post-Cold War periods. Naturally, the results are substantively identical.

<sup>9</sup>Non-robust, unclustered errors are slightly smaller.

<sup>10</sup>In the sample used, 61% of regimes do not democratize and 58% do not experience an autocratic transition. See Wright et al. (2015) for a discussion of estimating unit fixed effects in data on autocratic regime collapse.

Table A-1: *Linear probability models of coups and regime transition*

	Successful coups (1-3)			Coup attempts (4-6)		
	All regime transitions	Democratic transition	Autocratic transition	All regime transitions	Democratic transition	Autocratic transition
	(1)	(2)	(3)	(4)	(5)	(6)
Pre-1990 coup	0.185** (0.03)	0.005 (0.02)	0.192** (0.03)	0.128** (0.02)	0.008 (0.01)	0.128** (0.02)
Post-1989 coup	0.292** (0.09)	0.082 (0.07)	0.271** (0.08)	0.159** (0.05)	0.057 (0.04)	0.126** (0.04)
Leader time (log)	0.010 (0.01)	-0.006 (0.00)	0.017** (0.00)	0.005 (0.01)	-0.007 (0.00)	0.012** (0.00)
Regime duration	0.011** (0.00)	0.003** (0.00)	0.008** (0.00)	0.011** (0.00)	0.003** (0.00)	0.008** (0.00)
(Regime duration) <sup>2</sup>	-0.000** (0.00)	-0.000** (0.00)	-0.000** (0.00)	-0.000** (0.00)	-0.000** (0.00)	-0.000** (0.00)
(Regime duration) <sup>3</sup>	0.000** (0.00)	0.000** (0.00)	0.000** (0.00)	0.000** (0.00)	0.000** (0.00)	0.000** (0.00)
Intercept	0.049 (0.07)	0.000 (0.04)	0.048 (0.04)	0.052 (0.07)	0.000 (0.04)	0.051 (0.04)
Regime-case fixed effects	✓	✓	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓	✓	✓
Log-likelihood	1420	2895	2863	1367	2896	2751
R <sup>2</sup>	0.302	0.284	0.319	0.286	0.284	0.285
N	4748	4629	4637	4748	4629	4637
Regimes	285	277	270	285	277	270
Years	1950-2010	1950-2010	1950-2010	1950-2010	1950-2010	1950-2010

Regime-case clustered standard errors in parentheses; p<0.1+ p<0.05\* p<0.01\*\*.

positive association between coups and *Autocratic transition* (Columns 3-4), in both time periods, and irrespective of which coup variable is used (*Coup success* or *Coup attempt*).

A second set of regressions fits a linear model (as in Table A-1), but substitutes regime-case fixed effects with country-fixed effects. This controls for (unobserved time-invariant) cross-*country* heterogeneity, but not similar cross-*regime* heterogeneity. The results are reported in Figure A-2. They show that the associations between recent successful coups or coup attempts and *Autocratic transition* holds. However, we also note a significant positive correlations between recent coups/attempts and *Democratic transition* in the post-1989 period. This suggests that the null result for democratizing coups in the post-Cold War period (in the main text, and in Tables A-1 and A-2) hinges on within-regime variation and not within-country heterogeneity.

The differences between regime-case and country fixed effects are not trivial. First, 58 % of countries in the sample used go through more than one authoritarian regime. Thus, using regime-case fixed effects means controlling for more (unobserved time-variant) within-country variation than country-fixed effects. Second, and more importantly, regime-case effects allow different authoritarian regimes within the same country to have different baseline probabilities of collapse. For example, in Guinea, the Lansana Conté regime lasted 24 years, whereas the ensuing Dadis Camara regime lasted one year. If we want to allow for different baseline probabilities of collapse for each regime, a regime-case effects model is the appropriate specification.

Nevertheless, even if we believe that country fixed effects are more appropriate, this need not obscure the strong correlations reported between coups and autocratic transitions. Even though there is a specification (country fixed effects) and a sample (post-1989) that support the hypothesis that coups are democratizing, most of our analysis thus far suggests that coups are more likely to be followed by an autocratic transition.

## Accounting for coups concurrent with regime collapse events

Up to this point, we have explored what happens *after* a coup event. However, our findings rely on data where, in some cases, the event coded as a recent coup by Powell-Thyne is the *same* event as the regime collapse event coded by GWF. That is, information from the same historical event is contained in variables on both sides of the regression.<sup>11</sup>

To address this issue, we use three approaches to excluding information from Powell-Thyne coup events that constitute the same historical event as GWF regime collapse events. All of the ensuing models are non-linear, with regime-case and year means as proxies for fixed effects (similar to Table

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<sup>11</sup>For example, Geddes et al. (2014: Codebook p. 56) note that the 1962 democratic transition in the Dominican Republic (the collapse of the Trujillo regime) began on January 16, when a '[c]oup ousted Balaguer, Trujillo's designated successor, and initiated a transitional government to oversee competitive elections (Wiarda, 1975: 263). [The] provisional government oversaw fair and competitive elections in December 1962, won by the opposition.' Thus, the coup attempt coded by Powell-Thyne and the regime collapse date coded by GWF are the same. Note: Powell-Thyne code the 1962 coup event as a failed coup.

Table A-2: *Non-linear models of coups and regime transition*

	<u>Democratic transition</u>		<u>Autocratic transition</u>	
	(1)	(2)	(3)	(4)
Pre-1990 coup success	0.988 (0.62)		3.443** (0.53)	
Post-1989 coup success	1.185 (0.94)		4.463** (0.71)	
Pre-1990 coup attempt		0.681 (0.47)		2.918** (0.48)
Post-1989 coup attempt		0.535 (0.67)		2.668** (0.64)
Leader time (log)	-0.303 (0.20)	-0.375* (0.19)	0.874* (0.43)	0.617 (0.39)
Regime duration	2.081** (0.48)	2.064** (0.48)	0.983** (0.23)	1.047** (0.25)
(Regime duration) <sup>2</sup>	-0.043** (0.01)	-0.042** (0.01)	-0.013** (0.00)	-0.014** (0.00)
(Regime duration) <sup>3</sup>	0.000** (0.00)	0.000** (0.00)	0.000** (0.00)	0.000** (0.00)
Intercept	1.668 (3.01)	1.389 (2.79)	2.164 (2.86)	1.327 (2.68)
Regime-case means	✓	✓	✓	✓
Year means	✓	✓	✓	✓
Log-likelihood	-282	-284	-238	-255
N	4629	4629	4637	4637
Regimes	277	277	270	270
Years	1950-2010	1950-2010	1950-2010	1950-2010

Regime-case clustered standard errors in parentheses; p<0.1+ p<0.05\* p<0.01\*\*.

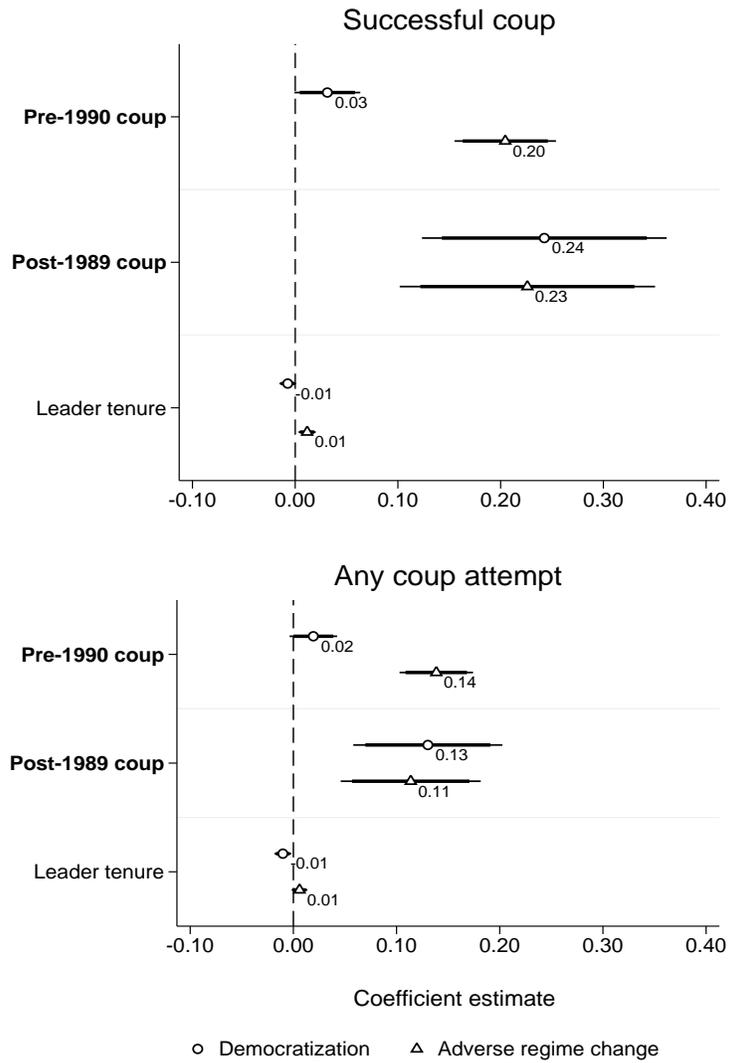


Figure A-2: Coups and autocratic regime transitions, country effects models

A-2).<sup>12</sup> First, we rerun the models by recoding all *recent coups* (our focal explanatory variable) that coincide with regime collapse events (our outcome variable) as zero.<sup>13</sup> This approach retains the full sample, as well as regime collapse events that follow coups but are not concurrent with them; it simply uses a more conservatively coded coup variable. Results from using this variable are reported in Table A-3, Columns 1-2. A second approach (Columns 3-4) is to drop all observations containing regime collapse events that coincide with coups. This is similar to “right-censoring” regimes—to borrow language from survival analysis—that end with a coup event. A final approach (Columns 5-6) is to drop all observations from regimes whose collapse coincides with a coup. This converts the models into a test of whether recent coups destabilize autocratic regimes *that did not end with a coup*.

These tests reveal a similar null finding for ‘democratizing coups’: though the estimate for post-1989 coups is positive in the democratic transition models, it is not statistically different from zero. The tests for autocratic transitions, in turn, show a large positive coefficient, but it is only significant for post-1989 coups. However, we should note that this correlation hinges on two cases: Rwanda 1994<sup>14</sup>, and the forced departure of Mubarak in Egypt in 2011. This finding should therefore be interpreted with appropriate caution.

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<sup>12</sup>To obtain convergence in the maximum likelihood optimization, we exclude include the regime- and year-means for the duration polynomials.

<sup>13</sup>We identify these by flagging all Powell-Thyne coups that occur within one day of the GWF regime collapse date.

<sup>14</sup>Powell-Thyne code the April plane crash that killed Habyarimana as a coup.

Table A-3: *Alternative coup and regime collapse variables/samples*

	Recoded coups		Drop same event observations		Drop regimes with coup collapses	
	Democratic transition	Autocratic transition	Democratic transition	Autocratic transitions	Democratic transition	Autocratic transition
	(1)	(2)	(3)	(4)	(5)	(6)
Pre-1990 coup	-0.624 (0.62)	-0.010 (0.46)	-0.403 (0.60)	0.716 (0.53)	-0.435 (0.60)	0.720 (0.51)
Post-1989 coup	0.399 (1.03)	4.368** (1.43)	0.559 (1.06)	4.707** (1.73)	0.499 (1.05)	4.822** (1.75)
Leader time (log)	0.247 (0.25)	1.566** (0.40)	0.216 (0.26)	1.242* (0.52)	0.252 (0.27)	1.324** (0.51)
Regime duration	0.134** (0.04)	0.080+ (0.04)	0.140** (0.04)	0.105* (0.05)	0.121** (0.04)	0.072 (0.05)
(Regime duration) <sup>2</sup>	-0.003** (0.00)	-0.003* (0.00)	-0.003** (0.00)	-0.003* (0.00)	-0.003* (0.00)	-0.003+ (0.00)
(Regime duration) <sup>3</sup>	0.000* (0.00)	0.000** (0.00)	0.000* (0.00)	0.000** (0.00)	0.000+ (0.00)	0.000* (0.00)
Intercept	0.355 (1.23)	-0.376 (1.03)	-1.704+ (0.89)	-1.141 (1.07)	-1.142 (1.04)	1.184 (1.04)
Regime-case means	✓	✓	✓	✓	✓	✓
Year means	✓	✓	✓	✓	✓	✓
Log-likelihood	-410	-424	-379	-244	-372	-230
N	4629	4637	4620	4580	3916	3876
Regimes	277	270	276	268	213	205
Years	1950-2010	1950-2010	1950-2010	1950-2010	1950-2010	1950-2010

Regime-case clustered standard errors in parentheses; p<0.1+ p<0.05\* p<0.01\*\*

## Appendix B: Verification and extension of Thyne and Powell (2014)

This section provides the details for the replication and extension of the primary statistical tests in Thyne and Powell (2014).<sup>15</sup>

We begin with the replication data from Thyne and Powell (2014), which structure the analysis at the country-year level. The dependent variable is a binary indicator of whether the country attains a score of 6 or more on the Polity IV index (-10 being least democratic, and 10 most democratic), while attaining a score of less than 6 in the prior year. The main explanatory variable is an indicator of whether a successful coup took place in the observation year or either of the two prior years.

The left panel of Figure B-1 reports the results. (Numerical results appear in Table B-1.) The first set of estimates is the verification of the main results reported in Thyne and Powell (2014) (Table 1, Column 1). The coefficient for *Recent coup* is positive and statistically significant at the 1% level (Table B-1, Column 1). Next, we add a binary indicator for *Military regime* from GWF.<sup>16</sup> The *Recent coup* coefficient now drops in size, and is not significant at conventional levels (Table B-1, Column 2). The final set of estimates in the left panel of Figure B-1 replicates the baseline model from Powell-Thyne, but restricts the sample to the post-Cold War period (1989-2008). Again, the coefficient for *Recent coup*, while still positive, is not statistically different from zero (Table B-1, Column 3).<sup>17</sup>

The right panel of Figure B-1 extends the analysis by directly accounting for regime-case heterogeneity. To do this, we estimate a linear probability model. The first set of estimates, again, comes from the baseline specification of Thyne and Powell (2014), but using the sample of autocracies coded by GWF. The coefficient on *Recent coup* is positive, and significant at the 5% level (Table B-1, Column 4). Next, we control for *Military regime*; the estimate for *Recent coup* drops by half, and is not statistically different from zero (Table B-1, Column 5). The last set of estimates includes regime-case fixed effects, thereby controlling for all constant differences between autocratic regimes (including geography, colonial history, autocratic regime type, how the regime seized power, and all other country- and regime-specific variables). The estimate for *Recent coup* turns negative, and is not statistically significant (Table B-1, Column 6).

In short, the ‘democratic coup’ finding is not robust: controlling for military regimes, restricting the sample to the post-Cold War period, or controlling for time-invariant regime-specific factors nullifies the finding. This suggests that we re-examine the larger political context in which coups in autocracies take place.

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<sup>15</sup>The file `PTverification.do` in our replication materials contains the code used in this section.

<sup>16</sup>GWF do not include small population countries in their sample. Thus, to match the Powell-Thyne sample, we code this variable as 1 for periods of military rule in Fiji, and as 0 for all years for the following small-population countries: Bahrain, Bhutan, Djibouti, Guyana, Kosovo, and Papua New Guinea.

<sup>17</sup>In the replication files we report a similar set of specifications for *any* recent coup attempt, not just successful ones. The substantive results are similar to those in the left panel of Figure B-1, except that the coup coefficient in the model controlling for *Military* is statistically significant at the 3% level.

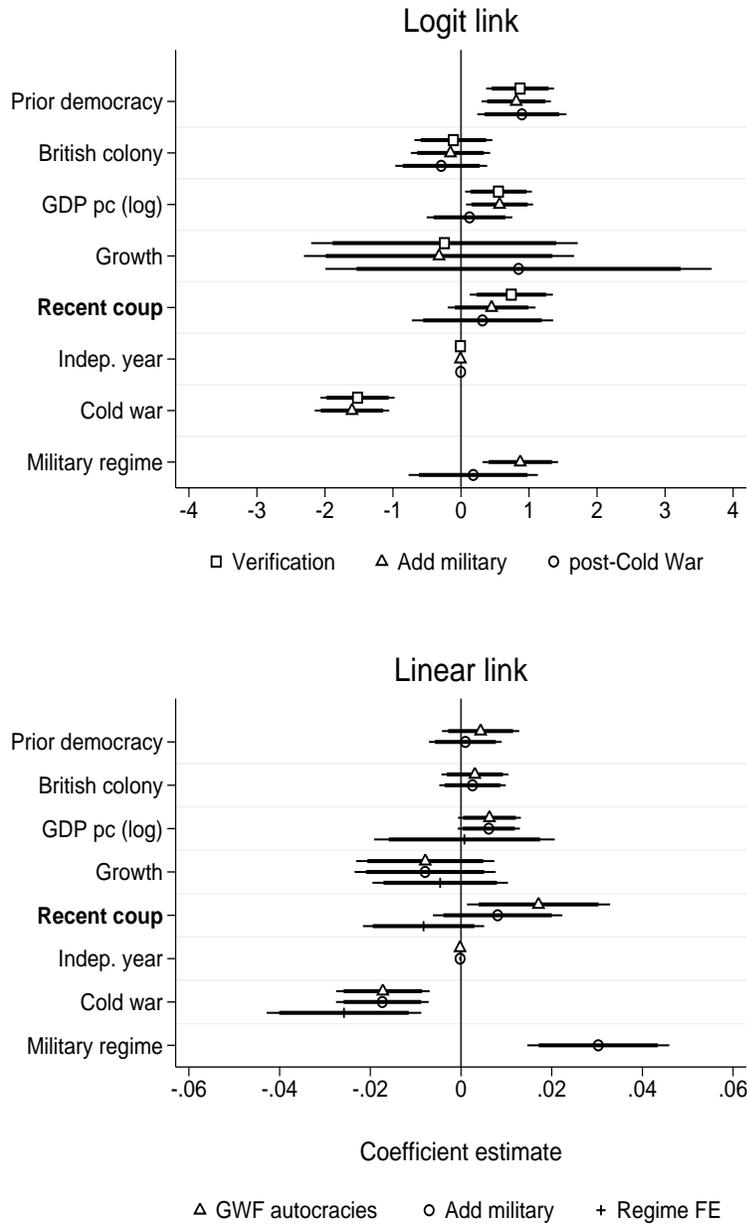


Figure B-1: Verification and extension of Thyne and Powell (2014).

Table B-1: *Verification and extension of Thyne and Powell (2014)*

	Verification	Add military	Post- Cold War	Only GWF autocracies	Add Military	Regime FE
	(1)	(2)	(3)	(4)	(5)	(6)
Recent coup	0.740*	0.450	0.316	0.017*	0.008	-0.008
	(0.31)	(0.33)	(0.53)	(0.01)	(0.01)	(0.01)
Military regime		0.871**	0.180		0.030**	
		(0.28)	(0.49)		(0.01)	
Prior democracy	0.869**	0.812**	0.895**	0.004	0.001	
	(0.25)	(0.26)	(0.33)	(0.00)	(0.00)	
British colony	-0.112	-0.155	-0.290	0.003	0.003	
	(0.29)	(0.30)	(0.34)	(0.00)	(0.00)	
Indep. year	-0.008**	-0.008**	-0.005+	-0.000*	-0.000*	
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	
Cold war	-1.521**	-1.603**		-0.017**	-0.017**	-0.026**
	(0.28)	(0.28)		(0.01)	(0.01)	(0.01)
GDP pc (log)	0.551*	0.567*	0.125	0.006+	0.006+	0.001
	(0.25)	(0.25)	(0.32)	(0.00)	(0.00)	(0.01)
Growth	-0.243	-0.323	0.847	-0.008	-0.008	-0.005
	(1.00)	(1.01)	(1.45)	(0.01)	(0.01)	(0.01)
No coup yrs	-0.003	-0.002	-0.012	-0.000	-0.000	0.000
	(0.02)	(0.02)	(0.02)	(0.00)	(0.00)	(0.00)
(No coup yrs) <sup>2</sup>	0.000	0.000	0.000	0.000	0.000	0.000
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
(No coup yrs) <sup>3</sup>	-0.000	-0.000	-0.000	-0.000	-0.000	-0.000
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Intercept	9.958*	9.589*	5.814	0.413*	0.379*	0.006
	(4.62)	(4.74)	(6.07)	(0.19)	(0.17)	(0.04)
Non-linear	✓	✓	✓			
Linear				✓	✓	✓
Regime-case fixed effects						✓
Log-likelihood	-390.546	-386.287	-221.932	3577.461	3595.657	3990.967
R <sup>2</sup>	0.205	0.286	0.250	0.015	0.024	0.194
N	4838	4838	1560	4134	4134	4134

Dependent variable is a binary indicator of democratization. Clustered standard errors in parentheses; p<0.1+ p<0.05\* p<0.01\*\*

## Appendix C: Coups and repression

In this section, we ask whether coups are associated with political repression. To do this, we restrict attention to autocracies that experience coups, and compare an arbitrary time period before a coup to one after the coup. This allows us to rule-out the possibility that a correlation between coups and repression arises because more violence-prone countries are more likely to have coups. We examine both annual and event data.

### Annual data

Our measure of annual repression in each country-year is the mean estimate from a latent measure of respect for human rights from Fariss (2014).<sup>18</sup> This measure accounts for the changing standards of accountability over time, as human rights norms and reporting standards have become stricter. We invert the scaled measure of repression, such that larger scores indicate higher repression.

We begin with four types of coup events: failed coups (F), dictatorship-to-dictatorship coups (A), democratizing coups (D), and reshuffling coups (N). Our goal is to compare repression in the post-coup year with repression in the pre-coup year. To do that, we let  $C$  denote coup type, and define:

$$E^C = \frac{\sum_{k=1}^{K^C} (R_{t+1}^k - R_{t-1}^k)}{K^C}$$

where  $k$  indexes  $K^C$  coup events of type  $C$ , and  $R_{t+1} - R_{t-1}$  is the difference in repression between the post- and pre-coup year. To recover an estimate of  $E^C$  (for coups of type  $C$ ), we fit the following linear model:

$$R_{i,t}^C = \alpha + \beta_1 \text{PreCoup}_{i,t}^C + \beta_2 \text{PostCoup}_{i,t}^C + \gamma_i + \xi_t + \epsilon_{i,t}$$

which includes binary indicators of pre- and post-coup years (with coup years as the reference category), and country ( $\gamma_i$ ) and time-period fixed effects ( $\xi_t$ ).<sup>19</sup> The estimated within-country change in repression over the 3-year coup window ( $t - 1, \dots, t + 1$ ) is the difference between the coefficients of the post- and pre-coup indicators. For example, the estimated change in repression around autocratic transition coups ( $C = A$ ) is  $E^A = \hat{\beta}_2 - \hat{\beta}_1$ . If this is positive, it indicates there is more repression in the post-coup than the pre-coup year. The results reported in Figure 2 of the manuscript (and in Tables C-1 and C-2 below) estimate this equation separately for each type of coup event (F, A, D, N).<sup>20</sup>

We fit several additional models as robustness checks. First, we estimate a country random effects model (Table C-3, Columns 1-2). Then, we fit models without country effects (Columns

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<sup>18</sup>The code to reproduce these results is contained in the file `repression_annual.do`.

<sup>19</sup>The time-period effects are dummy variables for each 5-year period between 1960 and 2010.

<sup>20</sup>Note the small number of observations in the post-Cold War models.

Table C-1: *Coups and repression, 1950-2010*

	All coups	D	A	N	F
	(1)	(2)	(3)	(4)	(5)
Post-coup	-0.011 (0.03)	-0.145+ (0.07)	0.069* (0.03)	-0.068+ (0.04)	-0.054 (0.03)
Pre-coup	-0.022 (0.02)	0.123 (0.07)	-0.126** (0.03)	-0.030 (0.04)	-0.039 (0.04)
Intercept	-0.072 (0.08)	-0.018 (0.03)	0.335* (0.15)	-0.229** (0.08)	0.169 (0.18)
Log-likelihood	-298.025	25.550	-28.613	19.310	-73.377
R <sup>2</sup>	0.324	0.673	0.366	0.726	0.194
N	655	42	216	143	329
Time-period fixed effects	✓	✓	✓	✓	✓
Country fixed effects	✓	✓	✓	✓	✓

Dependent variable is repression. Clustered standard errors in parentheses; p<0.1+  
p<0.05\* p<0.01\*\*

Table C-2: *Coups and repression: 1990-2010*

	All coups	D	A	N	F
	(1)	(2)	(3)	(4)	(5)
Post-coup	0.031 (0.05)	-0.264 (0.13)	0.197* (0.08)	-0.131 (0.07)	0.002 (0.05)
Pre-coup	-0.046 (0.05)	0.190+ (0.07)	-0.254* (0.09)	-0.171 (0.20)	-0.019 (0.06)
Intercept	0.909** (0.06)	-0.295* (0.07)	0.764** (0.04)	0.538* (0.10)	0.850** (0.10)
Log likelihood	6.971	9.815	13.259	13.257	30.930
R <sup>2</sup>	0.058	0.865	0.619	0.608	0.084
N	113	15	24	10	72
Time-period fixed effects	✓	✓	✓	✓	✓
Country fixed effects	✓	✓	✓	✓	✓

Dependent variable is repression; clustered standard errors in parentheses; p<0.1+  
p<0.05\* p<0.01\*\*

3-4), and models with year fixed effects instead of time-period effects (Columns 5-6). Last, we estimate specifications for the entire period in our sample, but include interaction terms to model the differential effect of Cold War versus post-Cold War coups (Columns 7-8). Further, for both time period samples (1950–2010 and 1990–2010) we re-estimate the autocratic transition coup model leaving out one country from the sample each time, for all countries in the sample (Figure C-1). This ensures the reported result does not hinge on any one case. In all these tests, the main result reported in the paper persists: autocratic transition coups (A) are followed by increased repression. On the other hand, the significant negative coefficient for democratizing coups (D) is not robust to year fixed effects, in part because of the small sample size and large number of parameters.

The repression data we use for our dependent variable are mean values from a latent measure of respect for human rights. Each mean value, though, is measured with uncertainty. To ensure our results are not driven by noise, we re-fit the main models (reported in Table C-1) using 1,000 random draws from the posterior distribution of each observation’s latent repression value. We then re-estimate the models in Columns 2-5 of Table C-1 1,000 times, each time using a different draw from the sampled values for repression. This provides 1,000 estimates of the change in repression (over each type of coup), which we use to describe the uncertainty in our reported repression effects.

Figure C-2 plots the median and (two-tailed) 95% confidence intervals for each model, corresponding to each type of coup. The point estimates (median) for autocratic transition coups (A) and democratic transitions coups (D) are almost identical to those reported in the main manuscript. The confidence intervals for democratic transition coups (D), however, now cross the zero line. This is not surprising, given the relatively small sample used in this model—there are relatively few democratizing coups. In short, the reported results are largely robust to accounting for uncertainty in the measure of our dependent variable, with the exception that we have less confidence in the previously reported decrease in repression over democratizing coups.

The approach to the annual repression data thus far estimates a separate equation for each type of coup event (F, A, D, N). This means the country- and time-period fixed effects are estimated for relatively small samples from the 3-year windows around each type of observed coup event. Furthermore, some observations are post-coup years for one type of coup event (e.g. a failed coup), but a pre-coup year for another type of coup event (e.g. an autocratic transition coup). To address these issues, we fit models that pool all of the coup windows together, allowing the fixed effects more observations within country (or time period), while also allowing different type of coup event windows to overlap in the same country. This pooled approach uses a sample of all coup windows with binary indicators for pre- and post-coup years for all four coup types, as well as binary indicators for coup years of three types (with failed coup year, F, as the omitted category necessary for identifying the model).<sup>21</sup> The results are reported in Figure C-3. The upper

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<sup>21</sup>We also test pooled models that drop all of the coup-year indicators, which is equivalent to treating all coup-years—F, N, A, and D—as the omitted category. This approach yields substantively similar results.

Table C-3: *Coups and repression: robustness tests*

	Country random effects		No country effects		Year FE		Cold War interaction	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post-D coup	-0.145+		-0.145+		0.084+			
	(0.08)		(0.08)		(0.04)			
Pre-D coup	0.128+		0.141		0.032**			
	(0.07)		(0.17)		(0.00)			
Post-A coup		0.067*		-0.003		0.078		
		(0.03)		(0.04)		(0.05)		
Pre-A coup		-0.126**		-0.173**		-0.118*		
		(0.03)		(0.04)		(0.04)		
1950-89 post-D coup							-0.079	
							(0.08)	
1950-89 pre-D coup							0.098	
							(0.11)	
1990-2010 post-D coup							-0.264*	
							(0.12)	
1990-2010 post-D coup							0.174**	
							(0.06)	
1950-89 post-A coup								0.056+
								(0.03)
1950-89 pre-A coup								-0.109**
								(0.03)
1990-2010 post-A coup								0.156*
								(0.07)
1990-2010 pre-A coup								-0.254**
								(0.08)
Intercept	-0.469+	0.354+	-0.395	0.273	-0.007	0.465**	-0.460+	0.348
	(0.25)	(0.21)	(0.28)	(0.30)	(0.01)	(0.17)	(0.26)	(0.21)
Log-likelihood			-40.178	-274.443	79.272	-11.317		
R <sup>2</sup>			0.521	0.073	0.975	0.460		
N	42	216	42	216	42	216	42	216
Time-period fixed effects	✓	✓	✓	✓			✓	✓
Year fixed effects					✓	✓		
Country random effects	✓	✓						
Country fixed effects					✓	✓	✓	✓

Dependent variable is repression; clustered standard errors in parentheses; p<0.1+ p<0.05\* p<0.01\*\*

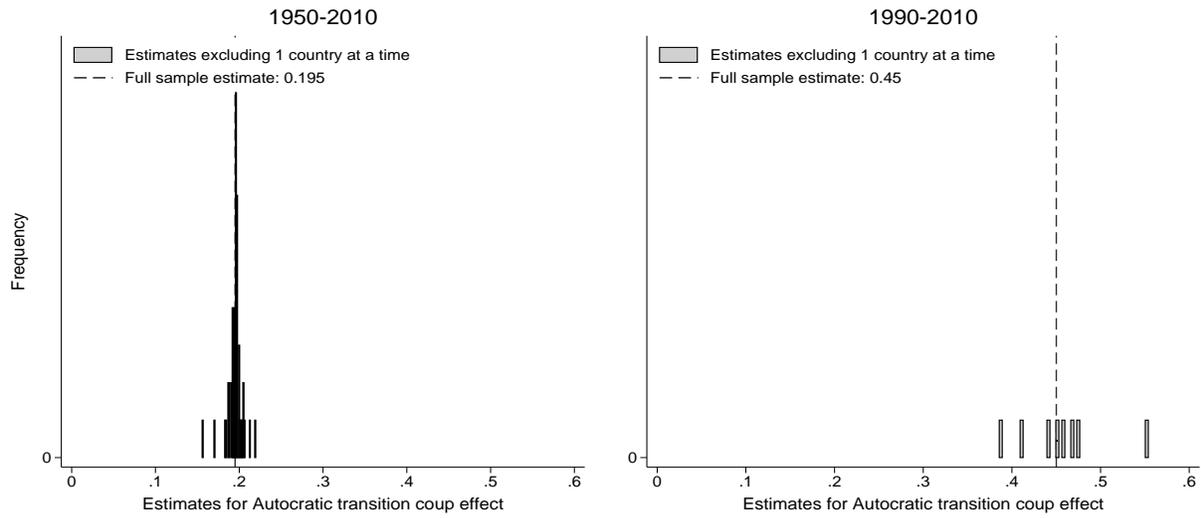


Figure C-1: Autocratic transition coup coefficient: leave-one-out tests

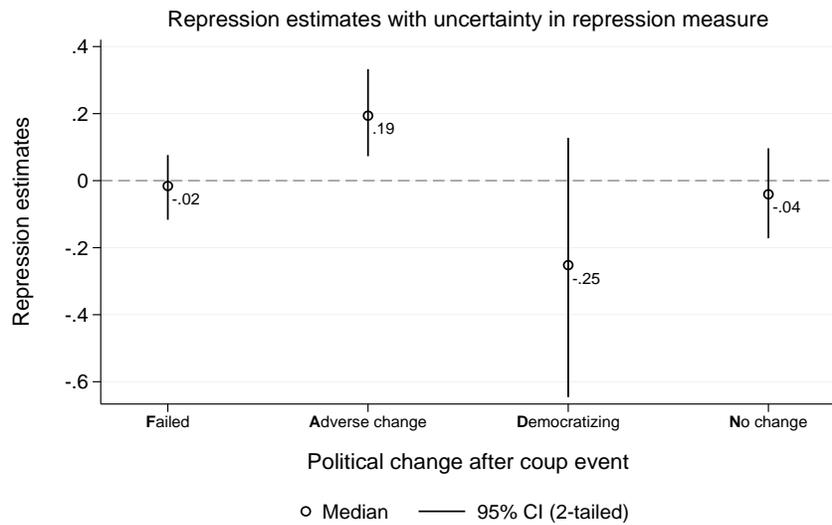


Figure C-2: Accounting for uncertainty in the repression measure

two panels report estimates from country fixed effects models, while the lower two panels report estimates from random effects models. Substantively, the pattern observed is similar to our main results.

Another approach to estimating the observed repression effects of different types of coups is to examine the short- and long-term effect of coups using an error-correction model (ECM). The ECM approach uses the differenced value of the dependent variable (repression), and includes the lagged and differenced values of the explanatory variables, as well the lag value of the dependent variable. For our purposes, we include indicators for the lagged and differenced coup-years as the primary explanatory variables. We use this specification with country- and time period fixed effects.

The results for the estimated short- and long-term effects of coups are reported in the replication code. There is a small short-term increase in repression resulting from failed coups and reshuffling coups as well as large short- and long-term positive effects of autocratic transition coups. Democratizing coups, on the other hand, appear to have no short-term effect. One way to summarize the long-run effect of coups is to estimate the long-run multiplier directly using a Bewley transformation. Figure C-4 reports estimates from these tests for the full sample period (1950-2010), and for the post-Cold War subsample (1990-2010). Both autocratic transition coups (A) and reshuffling coups (N) are correlated with a long-term increase in repression, while there appears to be little long-term effect for failed coups (F). Democratizing coups (D) are associated with a long-run decrease in repression—especially in the full sample period—but these estimates are not statistically significant.

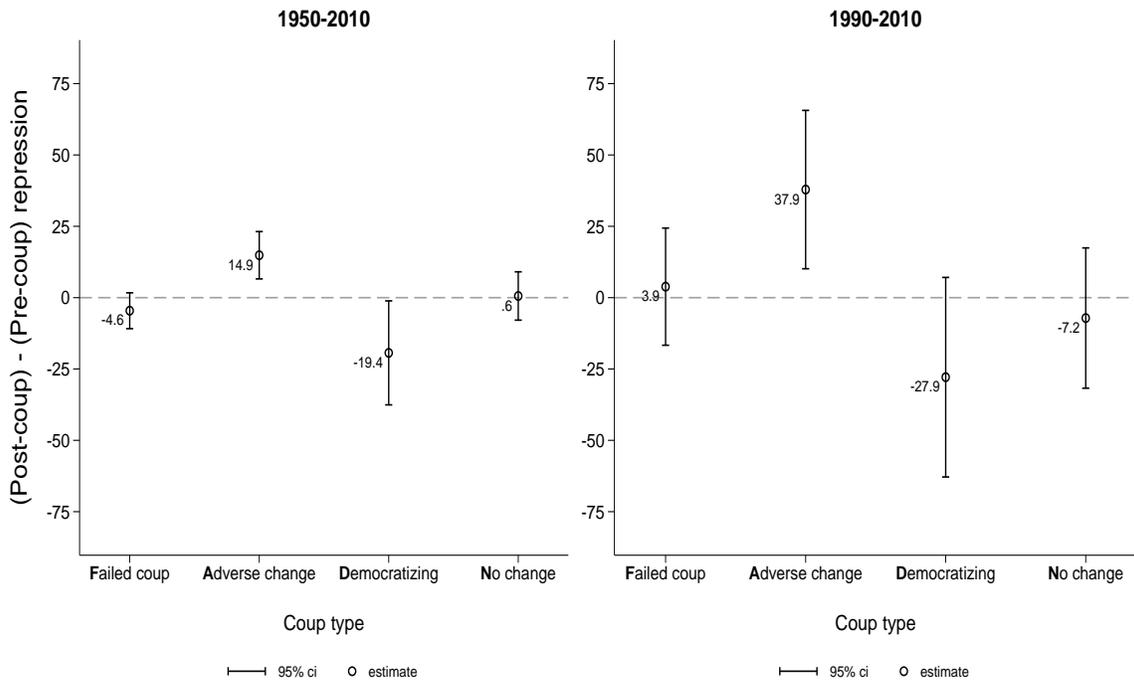
In sum, the various repression tests using annual data indicate a strong positive correlation between autocratic transition coups and repression. This finding holds over various time periods, and using a number of different estimators. The repression-reducing effect of democratizing coups is statistically more fragile: it is not robust to year fixed effects, to accounting for noise in the latent measure of repression, or to using an ECM. Finally, there are only five democratizing coups in the period 1990-2010, which makes any inference for the post-Cold War period relatively noisy.

## Event data

The analysis of the consequences of coups for citizens' welfare could benefit from a measure of *actual* government repression, free from the concerns accompanying the use of latent measures. In addition, annual indices may obscure informative dynamics in repression in the months preceding and following coups. For example, if repression increases immediately after a coup but subsides by the end of the calendar year, an annual measure may code this as no change. Yet, there is no reason why the welfare losses incurred by the country's citizens in this short time span should go unnoticed.

To do that, we look at monthly trends in observed instances of political violence in the 24-month window around each coup. Unfortunately, using event data restricts our sample to the years

Fixed effects



Random effects

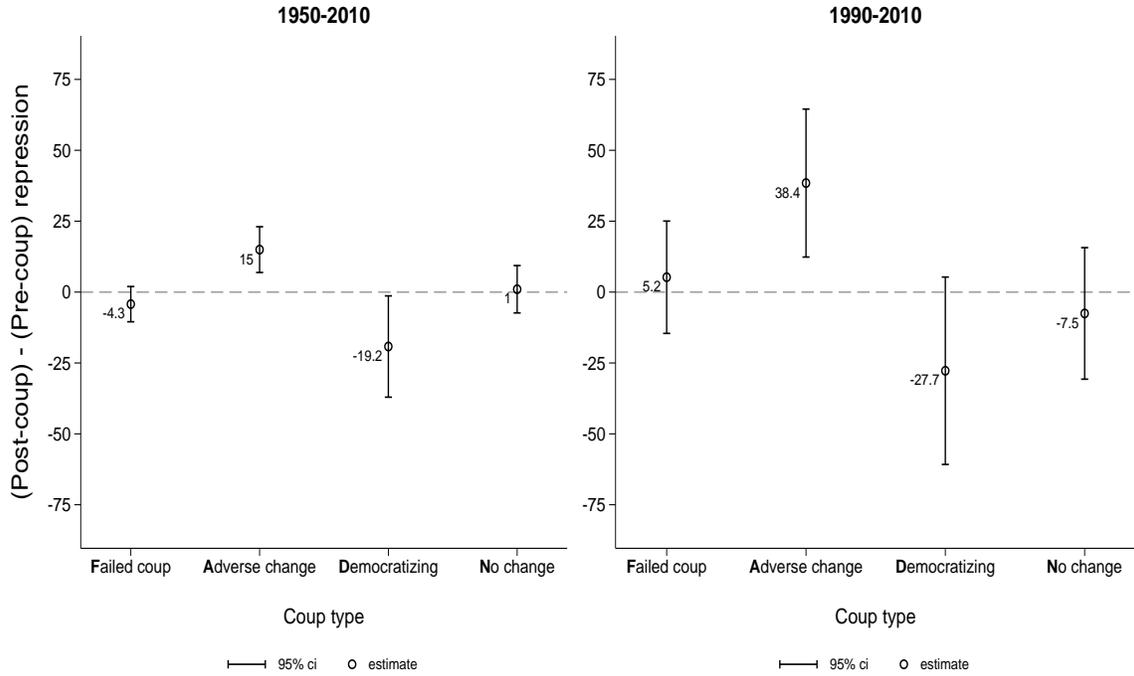


Figure C-3: Pooled coup windows

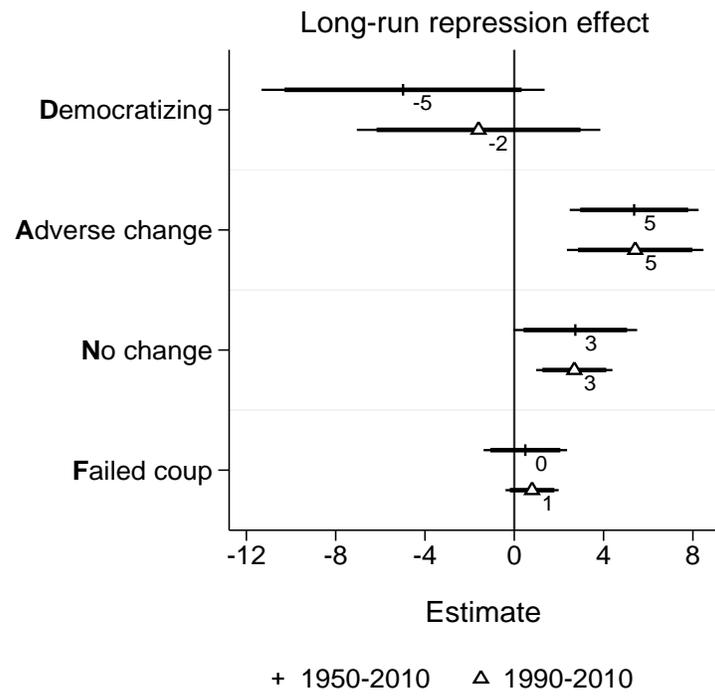


Figure C-4: Long-run effects of coups

1990-2014, as the systematic collection of this data starts later than other data types. Yet, this still leaves us with 50 coup events to analyze: 30 failed coups, 7 dictatorship-to-dictatorship coups, 7 democratizing coups, and 5 leader reshuffling coups.

In the main text, we describe the two sources used in our analysis: the Social Conflict in Analysis Database (SCAD) (Salehyan et al., 2012) for the years 1990-1994, and the Worldwide Atrocities Dataset (WAD) (Ulfelder and Schrodtt, 2009) for the years 1995-2014. From these datasets, we create a country-month variable, *deaths.state*: a count of deaths from pro-government violence in SCAD (1990-1994)<sup>22</sup>, or atrocities committed by the state or state-sanctioned actors in WAD (1995-2014).<sup>23</sup>

Figure 3 of the article shows the distribution of within-country changes in total *deaths.state* for the 12 post-coup months versus the 12 pre-coup ones. For a disaggregated view—albeit a pooled one—of where these changes come from, here we provide Figure C-5. It plots *deaths.state* in the 24-month window around each coup for all 49 coup events in our sample, classified by coup type. A line for the mean count is added in each plot.

As in Figure 3 in the article, we see that, on average, civilian deaths increase in some of the country-months after failed (F), dictatorship-to-dictatorship (A) and reshuffling coups (N). Indeed, aside from three country-months before one failed coup (Burundi, 4/2001), there are no coup events in our sample that are preceded by civilian deaths. On the contrary, several coup events of different types are followed by atrocities—hence the various spikes in the mean line in post-coup months.<sup>24</sup> The notable exception, as in Figure 3, are democratizing coups (D), for which no deaths are recorded either before or after any of the coup events.<sup>25</sup>

At this point, it is worth addressing three concerns with our analysis. (These also apply to Figure 3 of the article.) First and foremost, an issue raised in the main text: the lack of statistical confidence in our results. We attribute the large spread around the mean/median counts reported to three factors. First, the relatively narrow time period for which this type of data exists (25 years), and, hence, the small number of certain types of coup events we can analyze (as low as 5 for reshuffling coups). Second, the noise inherent in monthly time series, particularly for zero-inflated count variables, like civilian deaths. Third, the difficulties associated with coding state-sanctioned violent events in dictatorships, especially when relying on English-language media sources and going as far back as 1990, as WAD and SCAD do. Yet, despite the uncertain and purely suggestive nature of our evidence, we still believe that it strengthens our case: most types of coups, particularly the more frequent ones, are followed by repression against citizens.

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<sup>22</sup>That is, ‘distinct violent event[s] waged primarily by government authorities, or by groups acting in explicit support of government authority, targeting individual, or “collective individual”, members of an alleged opposition group or movement’ (Salehyan et al., 2012: 3).

<sup>23</sup>This includes the military, police, security forces and other state agents (Ulfelder and Schrodtt, 2009: 12).

<sup>24</sup>The relatively large number of observations in the Failed coups panel (30) draws the mean line toward the horizontal axis, and hence it is obscured by the scattered points. However, it is positive for almost all post-coup months, and zero for all but three pre-coup months (the aforementioned Burundi coup).

<sup>25</sup>Some deaths are noted *on* the month of one democratizing coup (Burkina Faso, 10/2014).

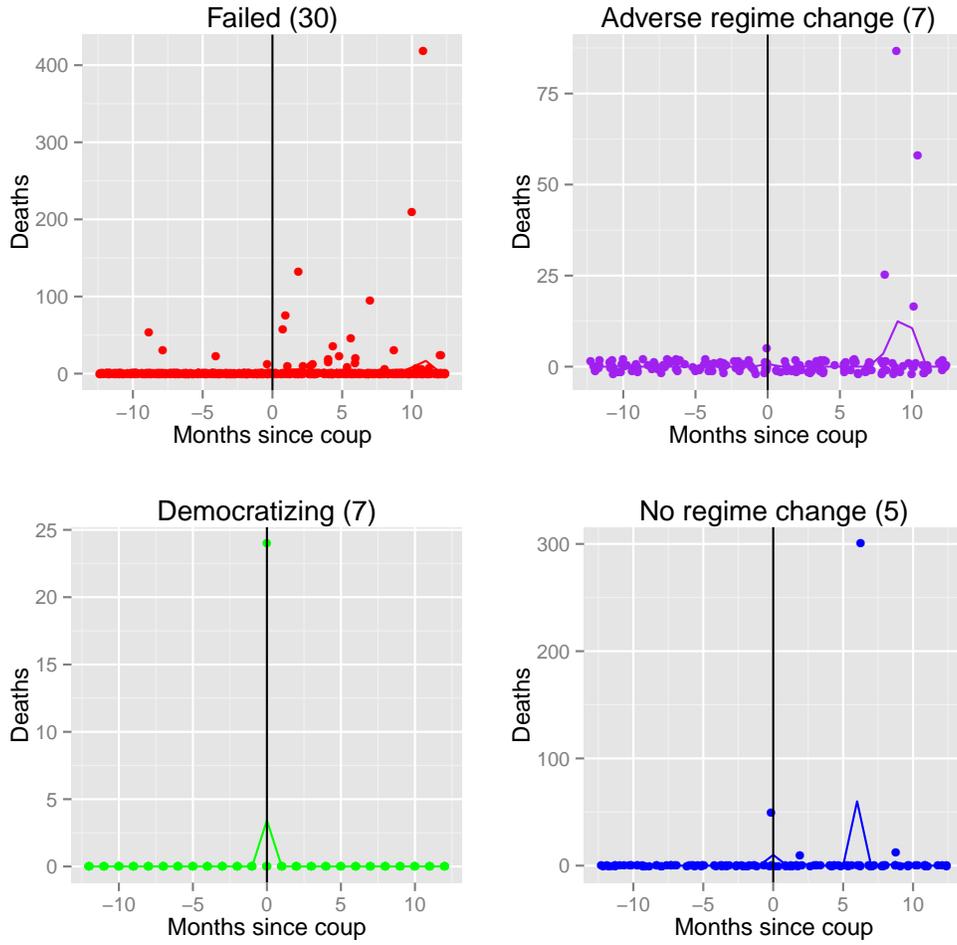


Figure C-5: Coup events and average monthly civilian deaths by state-sanctioned perpetrators

*Notes:* Colored lines denote the country-month average. Parentheses contain the number of coups of the respective type in the sample. Points are jittered. An outlier from the bottom-right plot has been removed to aid visualization. The vertical axis is scaled slightly differently in each panel.

Another concern is our use of two different event datasets. However, note that we do not “double-count” events from these two sources; we merely use SCAD to maximize sample size, as it allows us to study repression around coups in the period 1990-1994, which are outside WAD’s coverage. That said, one might worry that the two datasets measure different types of repression. For our purposes, though, this is not a large concern; we care mostly about within-country comparisons around coup events, and each of these comparisons uses data from a single source. Nevertheless, as a robustness check, we repeat all of our tests using events from the same source. We find no substantive differences from the results reported.

A third concern is the potential bias of event data collected after coups. Namely, the primary sources used by SCAD and WAD may overreport repression following coups—news outlets will monitor a country more closely when it is hit by some kind of shock, such as a coup. Thus, the jumps we note in reported civilian deaths post-coup may be owed to international observers’ closer scrutiny of pre-existing repression in autocracies. Repression may be the same after a coup—even lower—but people may be looking harder to find it.<sup>26</sup>

One way to address this concern is to weigh the counts reported by some measure of the news attention they receive. Even though each entry in both SCAD and WAD is meant to capture a distinct event, after coups it may be more likely that the same event gets coded multiple times, simply because coders expect to see more violence. Therefore, down-weighting death counts aggregated from a larger number of events can contain bias from coders’ “trigger-happiness”. For this reason, we repeat our analysis using a weighted measure of *deaths.state*, and see no substantive difference from our baseline results.<sup>27</sup>

Relatedly, and more worryingly, it is possible that the bias in post-coup reporting varies by coup type. Namely, the primary sources reporting the coded events may be more likely to see violence after failed, autocracy-to-autocracy and reshuffling coups than democratizing coups. This suspicion is strengthened by the fact that all coup types show almost no recorded deaths pre-coup, but only democratizing coups remain at zero post-coup. Indeed, this would bias the results displayed, but by making the period following democratizing coups appear relatively *less* repressive. In short, it is possible that even coups paving the way for competitive elections are followed by increased civilian harm.<sup>28</sup>

Concluding, we argue that our analysis using event data withstands the objections raised; with regards to its mixing of two datasets, and the potential sources of overreporting of repression after different types coups. Overall, the event data concur with the annual data: following all (most) coup

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<sup>26</sup>Note that using within-country comparisons, as in Figure 3 of the article, does not alleviate this bias. Nevertheless, it does address another potential bias—*cross-country* differences in foreign attention to repression—if events in more repressive countries are scrutinized more.

<sup>27</sup>We omit versions of the plots using this measure, as they are almost identical to the ones presented—only the scale of the axes changes, as the deaths counts are divided by event counts.

<sup>28</sup>Similarly, we cannot exclude the possibility that pre-coup repression levels are the same as post-coup ones, and hence repression does not increase following coups. Unfortunately, given the nature of the data, we cannot test for this possibility.

events (types), repression against civilians either increases or remains at its pre-coup level. Specifically, there is no evidence that lower repression ensues after failed, dictatorship-to-dictatorship, and reshuffling coups, while there is some evidence that repression often increases, particularly after dictatorship-to-dictatorship coups—the most frequent type of (successful) coup. In addition, the negative association between democratizing coups and repression found in some specifications that use annual data may be owed to reporting or coding bias following the announcement of competitive elections.

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