

No Easy Way Out: The Effect of Military Coups on State Repression

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Military coups are often advocated as solutions for ending state-sponsored atrocities. Yet, we know little about coups' precise consequences. This article estimates the effect of coups on state repression by exploiting the element of chance in whether an attempted coup succeeds or fails. Contrary to popular views of coups as remedies against repressive autocrats, I find no evidence that coups have a pacifying effect on state repression. Rather, coups appear to make matters worse, even when targeting leaders who commit large-scale human rights violations. This article contributes to studies of political violence, authoritarianism, and civil-military relations by resolving a long-standing "good coup versus bad coup" debate. It also advances literature on coups and their consequences through an innovative empirical design that leverages exogenous variation in coup outcomes, combined with an extreme bounds analysis, overcoming conventional challenges of causal inference using observational data.

Can military coups improve human rights conditions? In recent years, a cautiously positive view of coups has emerged in the comparative politics literature. Coups, according to this view, may enhance democracy and human rights by removing autocratic rulers (Collier 2008; Miller 2012) as well as triggering constitutional reform (Varol 2012) and democratic elections (Chacha and Powell 2017; Marinov and Goemans 2014; Thyne and Powell 2016). The argument that coups might help political and economic development echoes canonical theories in political science that regarded coups as distasteful yet useful means for achieving modernization and building political order (Huntington 1968).

This article supports a more pessimistic view of coups and their potential for improving human rights conditions in repressive regimes. I find that coups increase state repression, even when they target repressive autocrats. This finding is based on a robust research design that sidesteps common challenges to studying the causal effect of coups. On the one hand, many acknowledge that current scholarship lacks a complete model of the determinants of military coups (Gassebner, Gutmann, and Voigt 2016, 293; Harkness 2016, 589). On the other hand, ongoing studies on coups' consequences widely rely on multivariate regression, implicitly assuming that the determinants of coups—and therefore the list of potential confounding variables—are well known.

This article develops an alternative approach that leverages naturally occurring randomness in whether an attempted coup succeeds. I build on important existing studies that show how the micro-dynamics of coups resemble coordination games and how whether an attempted coup succeeds or fails involves an important element of chance (Boix and Svolik 2013; Casper and Tyson 2014; Geddes 1999; Little 2015; Singh 2014). My identification strategy combines difference-in-differences and extreme bounds analyses to derive causal estimates that are not sensitive to the choice of conditioning variables (Dunning 2012; Samii 2016).

The main finding is that successful coups tend to increase state repression. Among countries where military coups were attempted since 1950, I find that on average, a successful coup increased state repression by about a tenth of a standard deviation for the following year, compared to a coup that fails. This effect is small but consequential because it challenges expectations that coups can achieve social peace in times of instability. This positive quantity is also surprising because it indicates that a successful coup leads to more repression than a failed coup, that is, that if an attempted coup that fails is "bad" for human rights (Balcels 2017; Shen-Bayh 2018), then one that succeeds is worse.

In addition to identifying a "local" effect of coups on state repression within the population of country-years where

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coups were attempted, I show evidence of a “global” effect for the entire population of country-years, that is, including country-years that have not experienced coups. This harmful effect holds similarly for coups overthrowing both autocratic and democratic regimes as well as in contexts where large-scale human rights abuses are already occurring. In other words, there is little evidence to support the notion that removing a repressive ruler can only make matters better in the short term.

This article’s empirical analysis contributes to a growing literature on coups’ effects. Engaging with recent debates on coups’ effects during the post–Cold War era (Derpanopoulos et al. 2016, 2017; Miller 2016; Thyne et al. 2017), I find that although coup dynamics might have changed since the end of the Cold War, the effect of coups on state repression does not become negative after 1991. I also discuss mechanisms which suggest that these effects might occur because the military faces a power struggle shortly after seizing power, consistent with studies of authoritarianism that highlight the commitment problems that new rulers face during their early years of tenure (Albertus 2015; Haber 2006; Magaloni 2008; Slater 2010). Most importantly, this article develops an approach to estimating the causal effect of coups that sidesteps methodological challenges to conditioning on observables, which researchers can use to examine outcomes of interest beyond state repression, such as economic growth, trade (Baccini and Chow 2018), and democratization (Miller 2012; Thyne and Powell 2016), as well as civil and international conflict (Thyne 2015).

The article is organized as follows: First, it reviews existing scholarship on coups and state repression, identifying competing arguments for why coups may both increase and decrease state repression. I also clarify the methodological challenges that this question raises. Second, I present my empirical strategy, discuss data used, and establish the plausibility of the identifying assumption. Third, I proceed to estimate the effect of coup success compared to coup failure on state repression through a difference-in-differences analysis. This section includes robustness checks and explores whether the negative effect of successful coups depends on the type of regime where the coup occurs or prior levels of state repression. Fourth, I estimate a lower bound on the effect of coup attempts on state repression within the population of country-years that have experienced a coup attempt, using an extreme bounds analysis. Fifth, I estimate the “global” effect of coup attempts within the entire population of country-years using fixed-effects regression, clarify the scope conditions of my analysis, and address the question of whether post–Cold War coups have become virtuous. In a brief discussion of mechanisms, I consider why successful coups might increase state

repression and provide suggestive evidence that the military’s efforts to build a civilian support base may explain this effect. The conclusion discusses future directions for research and policy implications.

EXISTING LITERATURE

Existing scholarship on civil-military relations, political violence, and authoritarianism presents two competing hypotheses about the relationship between coups and state repression. The first hypothesis is that coups reduce state repression; the second is that coups increase state repression.

Good coups

Coups might reduce state repression for various reasons. First, a “good coup” may demobilize social forces and thereby reduce incentives for state repression (*Economist* 2015). This argument harkens back to Samuel Huntington, who argued that coups could ease social tensions and reduce violence in times of rapid social and economic change. A coup, according to Huntington, “serves to halt the rapid mobilization of social forces into politics and into the streets . . . and, by removing the target and the immediate stimulus of the escalation, to defuse the explosive political situation” (Huntington 1968, 216).¹ By stabilizing expectations, a coup restores order without the need for repression. The feeling in the immediate aftermath of a military coup is often one of “relief and harmony” (Huntington 1968, 217) because “at least, the slide downward would be stopped” (Trager 1963, 321; quoted in Huntington 1968, 218).

Second, coups may strengthen the effect of international pressures to respect human rights (Thyne et al. 2017). The incoming junta will seek to maintain good relations with international sponsors and avoid spectacular acts of violence that could threaten the flow of aid and investments into the country (Dunning 2004; Thyne and Powell 2016, 196). Norms against military coups have strengthened after the end of the Cold War and especially since 2000, when member states of the current African Union adopted the Lomé Declaration condemning coups and stipulating sanctions against governments established through coups (Derpanopoulos et al. 2017; Marinov and Goemans 2014; Miller 2016; Shannon et al. 2015; Souaré 2014; Thyne et al. 2017). Under international pressure, a ruling junta may choose to hold elections and quickly return to the barracks to avoid sanctions (Marinov and Goemans 2014; Miller 2012). Such democratization should reduce repression (Davenport 2010). Finally, coups that specifically target leaders

1. Here Huntington is referring specifically to coups happening in radical praetorian societies.

responsible for extensive human rights abuse can trigger leadership change and reduce repression (Collier 2008).

Bad coups

However, there are ample other reasons to believe that coups might increase repression. First, the new leader's initial vulnerability may provide incentives in favor of using repression rather than against it. Having broken with existing rules of politics while seeking to set the foundations for new ones, the incoming ruler may be especially intolerant of mobilized opposition. Indeed, "keeping the popular classes from becoming actively involved" becomes a primary concern for an incoming junta (Farcau 1994, 141). Second, the coup may galvanize opposition rather than calm it when the coup spurs the mobilization of forces sympathetic to the ousted incumbent. Third, a coup may generate unrealistic hopes for improved material conditions. Popular expectations of short-term improvement may clash with the new rulers' longer-term objectives, such as fostering capital investment, leading to increased repression as a means of securing acquiescence (Luttwak 1979, 183–91).

The nature of military rule also provides arguments that coups increase repression. Militaries are often ill-equipped to police social unrest. Indeed, scholars have found that regimes ruled by the military tend to be more repressive than other types of regimes (Davenport 1995; 2007, 490–91). Moreover, a coup typically relaxes constraints on executive power in the months following it because the military often rules by decree; such reduction of executive constraint might increase repression (Davenport 2010).²

Coups and the problem of causal inference

The previous section has shown that existing scholarship supports two competing hypotheses. On the one hand, coups may decrease repression because the military's seizure of power demobilizes social forces and because the new rulers seek to avoid costly confrontation with the population and maintain foreign investment. On the other hand, coups may increase repression because of relaxed constraints on executive power, unrest caused by the military's inability to deliver short-term economic benefits, and mobilization by supporters of the ousted incumbent.

2. The existing literature's understanding of "good" versus "bad" coups incorporates additional nuances. In their study of coups' impact on democratization, for instance, Thyne and Powell (2016) discuss two causal quantities: (i) the effect of a coup that succeeds compared to a similar situation in which no coup is attempted; and (ii) the effect of a coup that fails compared to a similar situation in which no coup is attempted. Consistent with this scholarship, this article disambiguates among distinct effects of coups and the related counterfactuals.

Adjudicating between these two hypotheses—whether coups increase or decrease state repression—presents a problem for causal inference. If coups bring "relief and harmony," following Huntington's formulation, it is because citizens believe that violence would have been worse had the coup not occurred. The problem is that this counterfactual scenario of what would have happened in the absence of a coup is never observed. It can only be inferred from comparative cases and requires adjusting for systematic differences between cases in which coups do and do not occur using control variables.

Whereas choosing valid controls is crucial for causal inference, selecting the correct model is especially challenging in the case of coups. A rich literature finds that the set of variables that correlates with coups—and therefore the variables that could confound results—is very large.³ These variables include poor economic performance (Galetovic and Sanhueza 2000; Londregan and Poole 1990; Miller 2012), exposure to international markets (O'Kane 1993), resource curses (Dunning 2008), interstate conflict (Desch 1999; Piplani and Talmadge 2016), civil wars (Bell and Sudduth 2015), mass protests (Casper and Tyson 2014; Dragu and Lupu 2018), and precedents of coups (Londregan and Poole 1990; Singh 2014) in influencing coup risks. Scholars have also stressed the importance of regime type (Bell 2016; Galetovic and Sanhueza 2000; Geddes 1999; Menaldo 2012), as well as the size of the military (Besley and Robinson 2010; Bienen 1969), its funding (Besley and Robinson 2010; Bove and Nistico 2014; Leon 2014), and coup proofing (De Bruin 2017; Horowitz 1985; Quinlivan 1999). Debates remain about the size and direction of many of these effects. Natural resource wealth, for instance, has been argued to reduce coup risk because oil-rich leaders have the resources to pay for the military's support (Bazzi and Blattman 2014; Fearon and Laitin 2003); however, these resources might also increase the risks of coups by increasing the "prize" attached to capturing the state (Besley and Persson 2010). Similarly for the size of the armed forces, some contend that a larger military is more

3. See appendix sec. A.1 for a detailed discussion of this literature. Important studies include those of Acemoglu, Ticchi, and Vindigni (2010), Albrecht (2014), Albrecht and Eibl (2018), Bazzi and Blattman (2014), Boix and Svolik (2013), Bove and Rivera (2015), Brownlee, Masoud, and Reynolds (2015), Bueno de Mesquita and Smith (2017), Casey (2020), Chacha and Powell (2017), Cotet and Tsui (2013), Dunning (2008), Fossum (1967), Gandhi (2008), Geddes (2009), Geddes, Frantz, and Wright (2014), Geddes, Wright, and Frantz (2014), Girod (2014), Grewal and Kureshi (2018), Herb (1999), Hollyer, Rosendorff, and Vreeland (2015), Houle (2016), Johnson, Slater, and McGowan (1984), Kim (2016), Lehoucq and Pérez-Liñán (2014), Levitsky and Way (2012), Lindberg and Clark (2008), Little (2015), Miller (2011), Needler (1966), Nordlinger (1977), Nordvik (2019), Pilster and Bohmelt (2011, 2012), Powell and Chacha (2016), Roessler (2011), Svolik (2012), and Tomashevskiy (2017).

capable than a smaller one to stage coups (Besley and Robinson 2010; Bienen 1969), while others reason that a larger military faces greater logistical obstacles to staging coups (Powell 2012). Because of these challenges, extant scholarship does not reach a consensus on the causes of coups. As a recent study by Gassebner et al. (2016, 293) points out: “About one hundred potential determinants of coups have been proposed, but no consensus has emerged on an established baseline model for analyzing coups.”

Such lack of consensus on the determinants of coups, in turn, has posed challenges for studying the consequences of coups on state repression. In the absence of such an “established baseline model,” it remains unclear which variables researchers should control for when studying coups’ effects. Specifically, it becomes difficult to adjudicate between the two competing hypotheses about the relationship between coups and state repression mentioned above: whether coups increase or decrease state repression.

EMPIRICAL STRATEGY

My empirical strategy sidesteps the difficulties of choosing controls through a natural experiment design (Dunning 2012) that compares successful coups to failed coups. I draw on recent advances in scholarship on the microdynamics of coups that shows there is an important element of chance in whether an attempted coup succeeds or fails (Singh 2014). Small unforeseen events—a military unit’s delay in reaching its assigned position or “weather so miserable . . . that loyal forces have time to organize an effective resistance”—can have a determining impact on whether a coup attempt succeeds (Kebschull 1994, 570). This degree of fortuitousness substantially weakens the statistical relationship between coup success and structural variables that could confound the relationship of interest and offers opportunities for causal identification.

Coup success as a natural experiment

Whether a coup attempt succeeds or fails involves an element of chance. Coup plotters must coordinate their action amid great uncertainty; rumors and misperceptions play an enormous role in this context. New unexpected events can cause officers to reverse their assessment of the coup’s chances of success, creating a self-fulfilling prophecy leading to mass defection. These coordination game dynamics inject serendipity into whether an attempted coup succeeds and reduce the impact of structural factors on the coup’s probability of success.⁴ When choosing between defecting or remaining

loyal to the incumbent, an officer’s personal grievances or political opinions matter less than what he thinks his colleagues will do. An officer may complain about low salary and poor work conditions and generally dislike the incumbent, yet he will not join a conspiracy he believes is destined to fail (Geddes 2009; Singh 2014). Conversely, the officer may join a conspiracy aimed at overthrowing a ruler he privately supports. These incentives—which emanate both from concerns for self-preservation and a desire to protect the organizational integrity of the armed forces—weakens the influence of ideology and grievances, as well as that of the broader economic, social, and political environment, on the officer’s decision to join an ongoing coup attempt.

Coups, therefore, are prone to sudden reversals of fortune when unanticipated events cause conspirators to update their beliefs about what their colleagues will do. History is replete with failed coups that nearly ousted the incumbent yet failed at the last minute because of serendipitous events.⁵ On November 27, 1992, mutinous Venezuelan officers mistakenly broadcast a “garbled tape” on national television instead of the prerecorded public announcement that they intended to air (Singh 2014, 31). The coup attempt came close to succeeding, but this mistake proved fatal because it signaled incompetence and cast doubt on the coup’s prospects for success. The plot unraveled as the perception that the coup plotters were disorganized propagated through the ranks, engendering a self-fulfilling prophecy that the coup would fail. A few months earlier, on February 4, 1992, another coup attempt had failed when the airplane of President Carlos Andrés Pérez was rerouted due to bad weather, forcing him to land at Caracas’s international airport instead of the Francisco de Miranda Air Force Base where mutinous officers were waiting to capture him. This accidental change of plans allowed the president to evade capture and thus foil an elaborate conspiracy against him that enjoyed broad support across different branches of the military (Baburkin et al. 1999; also Kebschull 1994, 574–75). More recently, in Turkey on July 15, 2016, the call that President Tayyip Erdogan made to a Turkish television station appears to have caused the rebelling officers to lose control of the narrative about the coup, damaging their ability to propagate the belief that their success was assured. Had President Erdogan been unable to place this call, it is conceivable that the coup

4. On these coordination dynamics, see Boix and Svolik (2013), Casper and Tyson (2014), Geddes (1999), Little (2015), Singh (2014).

5. Perhaps the most dramatic historical example of a failed coup is German officers’ failed attempt against Hitler in 1944. The assassination attempt at the center of the plot failed because the briefcase that contained the bomb was misplaced, which allowed Hitler to survive the blast. I am grateful to one of the reviewers for raising this example.

might have succeeded. In executing a coup, “there are many points along the way . . . for things to go wrong” (Kebschull 1994, 570). This sensitivity of coup success to small exogenous shocks generates unpredictability in whether attempted coups succeed. As Singh (2014, 5) aptly observed, “No coup attempt is guaranteed to succeed, no matter what the circumstances.” This unpredictability, in turn, can be exploited for causal identification.

Data

I measure state repression using the Human Rights Protection Scores (version 2.04), a latent index constructed from over a dozen measures of state repression that covers the period 1949–2013 (Fariss 2014). I chose this data set for three reasons. First, it covers a much longer time period than most human rights data sets. Second, it is continuous and therefore captures more fine-grained differences in repression than other data sets that use binary or ordinal scales. Third, the data set corrects for changing standards of accountability in human rights over time (Fariss 2014). I invert the human rights protection scores so that a higher score reflects higher levels of repression.⁶

I define a coup attempt as an extralegal and overt attempt at overthrowing the incumbent by members of the military. My list of failed and successful coups is based on Belkin and Schofer (2003, 2005), as this data set most closely corresponds to this definition.⁷ Unlike other data sets that include coups by government actors outside the armed forces, the dynamics of which may differ from the above-described coordination dynamics, Belkin and Schofer’s definition focuses on coups by military actors. In the appendix (available online), I present additional tests using the Powell and Thyne (2011) data set (1950–2017), which includes nonmilitary coups and extends temporal coverage beyond 2000, and find similar results for the effects of repression. There are 310 coup attempts from 1950 to 2000 (183 successes and 127 failures).

Assessing covariate balance. To check the plausibility of the identifying assumption, I test for systematic differences between the pretreatment characteristics of successful and failed coups. If the success of an attempted coup indeed approximates a random process, we should expect these two groups to be similar. I chose characteristics based on available scholarship on the determinants of coups (see section A.1 in

the appendix for a detailed discussion of these variables). For clarity, I sort the main coup-related variables into six categories, relating to economic factors, demographic factors, regime type, military-related factors, international environment, and domestic environment. Table 1 summarizes the variables considered (34 in total) and the data sources.

Table 2 presents difference in means tests between the treatment (coup success) and control (coup failure) groups for the 34 variables. Consistent with the identifying assumption, the differences in means are indistinguishable from zero for most covariates. Only three of the *p*-values are significant at the .05 level (oil and gas production, military expenditures per capita, and the number of past failed coups). Given the large number of tests conducted, these results are consistent with the null hypothesis of no statistical relationship between coup success and the variables. To make this assertion precise, I performed a randomization inference test, in which I conducted 1,000 simulations, assigning an artificial “success” label to randomly selected subsets of coup attempts. I then calculated the proportion of simulations giving at least three *p*-values below .05. This proportion is 0.179, which is insufficient to reject the null hypothesis that there is no statistical relationship between coup success and the covariates. The results are consistent with my claim that coup success, conditional on coup attempt, does not strongly correlate with background structural variables.⁸

As test of joint significance, I performed logistic regressions of coup success on a selection of variables from table 1.⁹ Table 3 presents the results. As shown, the covariates do not jointly predict the probability of coup success better than the null model, as demonstrated by the *p*-values on the *F*-tests, which are greater than .1 for all the models (the *p*-values range from .13 to .43). These results are consistent with the claim that the variables do not strongly predict, either jointly or individually, whether an attempted coup will succeed or fail.¹⁰

Estimating the effect of coup success. The previous section established the plausibility of the identifying assumption.

6. See appendix sec. A.1 for detail on the latent measure of state repression.

7. See the appendix for detail on data sources and coding rules.

8. In appendix sec. A.2, I present results from a nonparametric combination (NPC) test that also fails to reject the null at 0.05 (Caughey, Dafoe, and Seawright 2017).

9. For variables that measure the same underlying concept in table 1, I select the variables that most strongly correlate with coup success in table 1.

10. These results are also consistent with the findings of previous scholars who have noted that “few variables have a consistent and statistically significant effect on coup success” (Bell 2016, 1187; see also Powell 2012; Singh 2014, 55).

Table 1. Data Sources

Category and Variable	Data Sources
Economic:	
GDP per capita	Maddison GDP data (Coppedge et al. 2016)
GDP growth	Maddison GDP data (Coppedge et al. 2016)
Exports share of GDP	COW trade, v4.0 (Barbieri, Keshk, and Pollins 2009)
Oil and gas production	Ross and Mahdavi (2015)
Demographic:	
Ethnic fractionalization	Ethnic Power Relations (Wimmer, Cederman, and Min 2009)
Population	COW NMC, v4.0 (Singer 1987)
Population growth	COW NMC, v4.0 (Singer 1987)
Regime type:	
Democracy	Cheibub, Gandhi, and Vreeland (2010)
Presidentialism	Cheibub et al. (2010)
Military regime	Geddes, Wright, and Frantz (2014)
Party regime	Geddes, Wright, and Frantz (2014)
Monarchy	Geddes, Wright, and Frantz (2014)
Personalist regime	Geddes, Wright, and Frantz (2014)
Legislature	Cheibub et al. (2010)
Military factors:	
Military personnel, share of population	COW NMC, v4.0 (Singer 1987)
Military personnel, total	COW NMC, v4.0 (Singer 1987)
Military expenditures, per capita	COW NMC, v4.0 (Singer 1987)
Military expenditures, per soldier	COW NMC, v4.0 (Singer 1987)
Military expenditures, share GDP	COW NMC, v4.0 (Singer 1987)
Military expenditures, total	COW NMC, v4.0 (Singer 1987)
Military expenditures, change	COW NMC, v4.0 (Singer 1987)
Counterbalancing, Pilster and Böhmelt	Pilster and Böhmelt (2011)
Counterbalancing, Belkin and Schofer	Belkin and Schofer (2003)
Counterbalancing, Powell	Powell (2012)
Counterbalancing, De Bruin	De Bruin (2017)
International:	
Interstate war	COW War Data, v4.0 (Sarkees, Reid, and Wayman 2010)
Cold War	
Domestic	
Civil war	COW intrastate wars v4.1 (Sarkees et al. 2010)
Civil war	UCDP PRIO, v4 (Gleditsch et al. 2002; Pettersson and Wallensteen 2015)
Unrest	Banks and Wilson (2017)
Purges	Banks and Wilson (2017)
Past successful coups	Belkin and Schofer (2003, 2005) and Powell and Thyne (2011)
Past failed coups	Belkin and Schofer (2003, 2005) and Powell and Thyne (2011)
State repression	Human Rights Protection Score (Fariss 2014)

Note. COW NMC = Correlates of War, National Material Capabilities Dataset; MID = Militarized Inter-State Dispute; UCDP PRIO = Uppsala Conflict Data Program, Peace Research Institute Oslo.

This section proceeds to estimate the effect of coup success on state repression through a difference-in-differences approach. This approach addresses the concern that the natural experiment may not have removed all confounders. For instance, the ability of the coup plotters could confound the results if it increased both the officers' chances of success in their coup attempt and their capacity to peacefully defuse political ten-

sions once in power.¹¹ The difference-in-differences analysis is robust to the presence of such unobserved confounders provided that they do not vary over time.

11. Alternatively, greater ability could also mean that the coup plotters are more skilled at repressing.

The quantity of interest is the (local) average treatment effect (LATE) on state repression of coup success relative to coup failure, within the population of country-years that experienced at least one coup attempt.¹² This quantity is $\lambda \equiv E[Y_{i,t=1}^1 - Y_{i,t=1}^0]$, where $Y_{i,t=1}^1$ is the level of state repression attained one year after the coup attempt if the attempt succeeds and $Y_{i,t=1}^0$ is the level of state repression attained one year after the coup attempt if it fails (i indexes country-years that experienced a coup attempt between 1950 and 2000, and t denotes the time since or until the coup attempt). The fundamental problem of causal inference is that we only observe one of the two potential outcomes $Y_{i,t=1}^1$ or $Y_{i,t=1}^0$. To estimate the quantity of interest, I make the standard difference-in-differences assumption that average state repression in the treatment (coup success) and control (coup failure) groups follow parallel trends in the absence of treatment, that is $E[Y_{i,t=1}^0 - Y_{i,t=-1}^0 | D_i = 1] = E[Y_{i,t=1}^0 - Y_{i,t=-1}^0 | D_i = 0]$, where $D_i \in \{0, 1\}$ is a binary indicator that takes the value 1 if the coup attempt was successful and 0 otherwise. Concretely, this assumption implies that average state repression would have changed by the same amount from the year before to the year after the coup attempt in the two groups had the coups failed everywhere.

With the parallel trends assumption, the average treatment effect on the treated (ATT) is identifiable from observed quantities: $ATT = E[Y_{i,t=1} - Y_{i,t=-1} | D_i = 1] - E[Y_{i,t=1} - Y_{i,t=-1} | D_i = 0]$, where Y_i without the superscript denotes the observed level of state repression. If we assume that treatment assignment is independent of treatment effects, then the ATT is equal to the ATE (Morgan and Winship 2015, 69).¹³ I estimate it using linear regression

$$\Delta Y_i = \alpha + \lambda D_i + X_i' \beta + \epsilon_i, \quad (1)$$

where $\Delta Y_i \equiv Y_{i,t=1} - Y_{i,t=-1}$ is the first difference in state repression between the year after the coup attempt and the year before, α is a constant, λ is the quantity of interest, D_i is a binary indicator that denotes whether the coup succeeded or failed, X_i is a vector of exogenous controls, β is a vector of coefficients, and ϵ_i is the error term.

The next section presents results from the difference-in-differences analysis where the dependent variable is the first difference in state repression between the year after the coup attempt and the year before. To account for potential hetero-

skedasticity, I report bootstrapped standard errors based on 500 replicates of the data. To address the possibility of non-parallel trends, I compare results after reweighting observations using the covariate balancing propensity score (CBPS) (Imai and Ratkovic 2014), and adding controls for lagged values of military regime, youth population (Hill and Jones 2014; Nordas and Davenport 2013), democracy (Davenport 2010), per capita military expenditures, Cold War, counterbalancing (De Bruin 2017), past successful coups, and past failed coups. If the identifying assumption is correct, adding controls and reweighting observations should not change the estimates. To address issues of missing data (which are most severe for measures of counterbalancing), I generated 80 imputations of the missing data using the *mice* package in R, and recombined the results using Rubin's rule (see appendix sec. A3). Note that in the data that there are 94 instances of coup attempts occurring in consecutive years. For those cases, I calculated the first difference in state repression between the year after the last coup attempt and the year before the first coup attempt of the sequence, treating the entire sequence of coups as a single observation. The observation is coded as coup success if at least one of these coup attempts was successful.¹⁴ Since more coup attempts implies a higher probability of treatment, I stratify the data into three groups corresponding to observations with one, two, or three or more coup attempts and I reweight observations using the inverse probability of treatment estimated within each stratum.¹⁵

RESULTS

Model 1 in table 4 presents the baseline model without controls and without reweighting observations using the covariate balancing propensity score. It shows that the first difference in state repression is 0.144 points larger for coups that succeed than for coups that fail. The effect is significant at $p < .01$. Although not large,¹⁶ this quantity is nevertheless substantially meaningful because it represents the increase in state repression relative to a coup that fails, which often prompts repression (Shen-Bayh 2018). Model 2 controls for military regime, youth population, democracy, military expenditures per capita, Cold War, counterbalancing and the number of previous successful coups (all measured before the coup attempt).

14. See Jones and Olken (2009) for a similar approach.

15. See Morgan and Winship (2015, 228). The estimate of the propensity score is the proportion of treated observations within each stratum. The regression includes a fixed-effect indicator of the number of coup attempts. The results hold without reweighting (table A1) and when discarding consecutive coups (table A2).

16. An increase of 0.144 corresponds to 0.11 standard deviations of the Human Rights Protection Scores.

12. For consistency throughout this article, I use the term "local" and the subscript i to refer to the population of country-years that experienced coup attempts.

13. Note that the ATT is identified under the parallel trends assumption even if treatment assignment correlates with treatment effects (Morgan and Winship 2015, 61–62).

Table 2. Are Successful and Failed Coups Similar?

Variable	Success	Failure	Difference	<i>p</i> -Value on Difference
GDP per capita (log)	3.160 (.321)	3.245 (.393)	-.085 (.045)	.051
GDP growth	1.278 (6.277)	.398 (6.245)	.880 (.786)	.240
Exports share of GDP (log)	-1.245 (.381)	-1.224 (.430)	-.021 (.051)	.674
Oil and gas production	.110 (.314)	.233 (.424)	-.122 (.047)	.010
Ethnic fractionalization	.460 (.282)	.449 (.294)	.011 (.036)	.754
Population (log)	3.859 (.519)	3.864 (.532)	-.006 (.066)	.928
Population growth	.029 (.031)	.031 (.072)	-.003 (.007)	.673
Democracy (CGV)	.261 (.441)	.250 (.435)	.011 (.055)	.930
Presidentialism	.142 (.350)	.177 (.384)	-.035 (.046)	.504
Military (CGV)	.398 (.491)	.476 (.501)	-.078 (.062)	.220
Party (GWF)	.276 (.449)	.278 (.450)	-.002 (.056)	1.000
Monarchy (GWF)	.075 (.264)	.067 (.251)	.008 (.032)	1.000
Personalist (GWF)	.321 (.469)	.389 (.490)	-.068 (.060)	.366
Legislature	.633 (.483)	.581 (.495)	.053 (.061)	.420
Military size, share of population (log)	-2.503 (.509)	-2.420 (.403)	-.083 (.058)	.116
Military size, total (log)	4.362 (.764)	4.452 (.655)	-.091 (.089)	.270
Military expenditures, total (log)	6.306 (.769)	6.469 (.830)	-.163 (.100)	.090
Military expenditures, share of GDP (log)	-3.754 (.357)	-3.668 (.392)	-.086 (.047)	.061
Military expenditures, per capita (log)	-.569 (.514)	-.415 (.590)	-.155 (.069)	.021
Military expenditures, per soldier (log)	1.918 (.418)	2.006 (.406)	-.087 (.052)	.074
Military expenditures, change (cube root)	27.181 (86.272)	42.620 (102.056)	-15.439 (11.858)	.183
Counterbalancing (Pilster and Böhmelt)	1.507 (.581)	1.620 (.712)	-.113 (.082)	.293
Counterbalancing (Belkin and Schofer)	.430 (.611)	.514 (.703)	-.083 (.083)	.520
Counterbalancing (Powell)	-.523 (.857)	-.279 (1.220)	-.244 (.132)	.304
Counterbalancing (De Bruin)	.478 (.477)	.618 (.512)	-.140 (.062)	.097
Interstate war	.022 (.148)	.040 (.198)	-.018 (.022)	.568

Table 2 (Continued)

Variable	Success	Failure	Difference	<i>p</i> -Value on Difference
Cold War	.929 (.258)	.866 (.342)	.063 (.038)	.100
Civil war (COW)	.156 (.363)	.161 (.369)	-.006 (.046)	1.000
Civil war (PRIO)	.233 (.424)	.298 (.459)	-.065 (.055)	.255
Domestic unrest	1.613 (3.580)	2.017 (4.173)	-.404 (.488)	.392
Past successful coups	1.459 (1.609)	1.583 (1.576)	-.124 (.200)	.501
Past failed coups	.781 (1.329)	1.236 (1.650)	-.455 (.188)	.010
Purges	.351 (.719)	.467 (.978)	-.115 (.108)	.273
State repression (lagged)	.574 (.920)	.681 (.977)	-.107 (.119)	.336

Note. The table shows the average and standard error (in parentheses) of each variable for successful and failed coups. An observation is a coup attempt. Each variable is measured the year before the coup attempt. The last column reports *p*-values on differences in the mean from two-sided unpaired *t*-tests (for continuous variables) and χ^2 tests (for binary variables). All logarithms are in base 10. The change in military expenditure was transformed by taking the cubic root to minimize the influence of outliers. CGV = Cheibub, Gandhi, and Vreeland; GWF = Geddes, Wright, and Frantz; COW = Correlates of War; PRIO = Peace Research Institute, Oslo.

Adding these controls barely changes the point estimate: the first difference in state repression is 0.136 points greater for coups that succeed relative to coups that fail.

Models 3 and 4 reweight each observation using the covariate balancing propensity score (Imai and Ratkovic 2014). Such reweighting increases covariate balance between the treatment and control groups while correcting for differences in the probability of treatment. Importantly, the estimated coefficients do not change after reweighting observations (model 3, 0.136) and adding controls (model 4, 0.137). Models 5 and 6 control for linear and squared trends in state repression. Again, the coefficient estimates barely change and remain significant at .01. That the estimates remain stable across specifications and when using weights is consistent with the identifying assumption that the probability of coup success does not correlate with secular trends in state repression.

Heterogeneous effects

In this section, I test whether the effect of coup success varies depending on where the coup takes place. One might expect coups in democracies, which have better human rights records than autocracies, to have a larger marginal effect on state repression than coups in authoritarian regimes, which are already quite repressive. I examine this possibility by reproducing the analysis on two subsets of the data: coups in autocracies and coups in democracies. I report 90% bootstrapped confidence intervals. Figure 1 shows that the esti-

mated effect is similar regardless of the type of regime where the coup happens, suggesting that the results are not driven by cases of democratic breakdown.

Does the effect of coup success vary depending on the pretreatment level of state repression? One might expect coups in countries where repression is widespread and extensive to have a smaller marginal effect than coups in more peaceful countries. It might even be possible that coups in extremely repressive settings could mitigate repression. After all, this conjecture forms the basis of the “good coup” perspective, which argues that when repression is already severe, removing the leader can only make things better. I examine this hypothesis by adding an interaction between coup success and the pretreatment level of state repression in equation (1) (model 1 in table 4) and then computing the marginal effect of coup success for different values of pretreatment repression.¹⁷ Figure 2 presents the results, along with 95% (light gray) and 90% (dark gray) confidence intervals.

17. To account for measurement uncertainty, I took 10 draws from the posterior distribution of state repression measured pretreatment and performed the analysis separately on the 10 imputed data sets (Crabtree and Fariss 2015; Fariss 2014; Schnakenberg and Fariss 2014). I combined the results from the 10 data sets following the rules by Rubin (1987). Observations were reweighted using the propensity score, defined as the probability of treatment conditional on the number of attempted coups during the observation period.

Table 3. Logistic Regression of Coup Success

	Dependent Variable: Coup Success			
	(1)	(2)	(3)	(4)
GDP per capita (log)	-.503 (.474)	-.824 (.683)	-.336 (.709)	-1.163 (.955)
GDP growth	.024 (.020)	.023 (.021)	.023 (.022)	.031 (.025)
Exports share of GDP (log)	.156 (.331)	.191 (.357)	.167 (.394)	.130 (.427)
Oil and gas production	-.664* (.397)	-.586 (.430)	-.612 (.447)	-.551 (.485)
Ethnic fractionalization	-.219 (.474)	.028 (.583)	-.089 (.520)	.495 (.738)
Population (log)	.001 (.249)	-.010 (.296)	.262 (.298)	.247 (.357)
Population growth	-1.129 (2.345)	-1.041 (2.450)	-2.496 (2.588)	-2.464 (2.872)
Democracy (CGV)	.262 (.492)	.271 (.510)	.244 (.533)	.272 (.553)
Presidentialism	-.375 (.526)	-.657 (.595)	-.466 (.575)	-.652 (.643)
Military (CGV)	-.260 (.316)	-.309 (.335)	-.440 (.356)	-.399 (.375)
Legislature	.024 (.305)	.030 (.318)	-.110 (.365)	-.139 (.376)
Military size, share of population (log)			.008 (.376)	-.147 (.430)
Military expense, per capita (log)			-.282 (.434)	-.157 (.478)
Counterbalancing (De Bruin)			-.767* (.411)	-.934* (.517)
Interstate war			-.804 (.782)	-.665 (.846)
Cold War			.617 (.465)	.455 (.483)
Civil war (PRIO)			-.245 (.369)	-.135 (.400)
Domestic unrest			-.009 (.039)	.012 (.047)
Past successful coups			.096 (.103)	.062 (.106)
Past failed coups			-.145 (.099)	-.145 (.111)
Purges			-.244 (.173)	-.331* (.184)
State repression (lagged)			-.064 (.183)	-.070 (.193)
Region fixed effects	No	Yes	No	Yes
Observations	310	310	310	310
<i>p</i> -value: <i>F</i> -test on all variables	.33	.43	.13	.25
<i>p</i> -value: <i>F</i> -test on all variables (except fixed effects)	.33	.37	.13	.21

Note. Multivariate regressions of coup success for the years 1950–2000. Missing values were imputed using multivariate imputation by chained equations (MICE), as implemented by the *mice* package in R. Estimates, standard errors, and *p*-values of the *F*-tests were computed based on the rules given by Meng and Rubin (1992; Rubin 1987). The regions are defined by the Varieties of Democracy's *e_regionpol* variable (Coppedge et al. 2016). Logarithms are in base 10. Constant not shown. CGV = Cheibub, Gandhi, and Vreeland; PRIO = Peace Research Institute, Oslo.

Table 4. Effect of Coup Success on State Repression, Difference-in-Differences Analysis

	Dependent Variable: First Difference in State Repression					
	Baseline Models		With CBPS Weighing		With Trends	
	(1)	(2)	(3)	(4)	(5)	(6)
Success	.144*** (.042)	.136*** (.044)	.136*** (.043)	.137*** (.043)	.140*** (.043)	.139*** (.044)
Military regime (CGV)		-.046 (.056)		-.033 (.057)		-.049 (.056)
Youth population		-.007 (.007)		-.010 (.008)		-.007 (.007)
Democracy (CGV)		-.016 (.057)		-.004 (.058)		-.023 (.059)
Military expenditures, per capita (log)		-.030 (.059)		-.058 (.063)		-.028 (.060)
Cold War		.033 (.077)		.027 (.082)		.082 (.117)
Counterbalancing (De Bruin)		-.030 (.060)		-.030 (.062)		-.030 (.061)
Past successful coups		.002 (.017)		.012 (.017)		.003 (.018)
Past failed coups		.001 (.017)		.014 (.017)		.001 (.018)
Time trend					-.001 (.001)	-.007 (.012)
Time trend ²						.0001 (.0002)
CBPS weighting	No	No	Yes	Yes	No	No
Observations	225	225	225	225	225	225
Treated	148	148	148	148	148	148

Note. The dependent variable is the first difference in state repression (between after and before the coup attempt). The main independent variable is a binary indicator of coup success. Models 3 and 4 show results after reweighting observations using the covariate balancing propensity score (CBPS). Columns 5 and 6 show results after adding time trends to the regression model. Logarithms are in base 10. In models 2, 3, 4, and 6, missing values were imputed using the *mice* package in R (80 imputations). Estimates were obtained using the rules by Rubin (1987). Columns 1 and 5 report bootstrapped standard errors. Constant not shown. CGV = Cheibub, Gandhi, and Vreeland.

* $p < .1$.

** $p < .05$.

*** $p < .01$.

The results are striking: successful coups increase state repression even when they overthrow repressive leaders. The estimates of the marginal effects are positive and statistically significant for a wide range of pretreatment values of repression in figure 2. The 95% confidence intervals do not cover zero for pretreatment levels of repression that reach values of 1 and 1.5, meaning that coup success has a positive and statistically significant effect at those values of repression. Scores of 1 or 1.5 characterize regimes that are widely known for imprisoning, harassing, and often killing political

opponents. China in 2012, for instance, receives a score of 1.19 on the repression scale.

Robustness

I tested the assumption that the assignment of treatment is independent of secular trends in state repression in several ways. First, I plotted pretreatment trends in state repression and looked for evidence of nonparallelism in the pretreatment period trajectories that would signal a violation of the

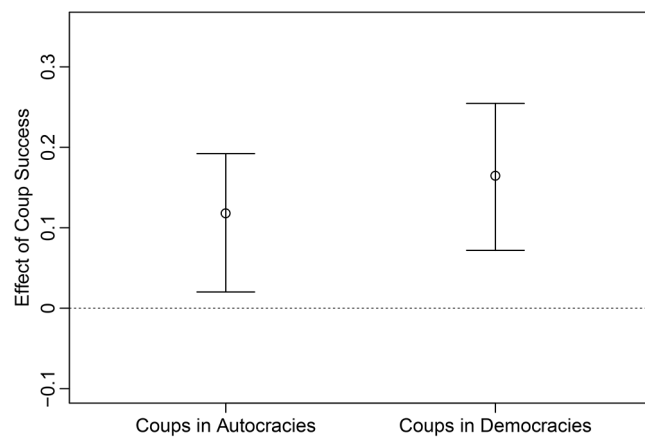


Figure 1. The figure shows the effect of coup success on state repression for coups taking place in autocracies (*left*) and democracies (*right*), with 90% bootstrapped confidence intervals. Autocracy/democracy is measured one year before the coup attempt.

identifying assumptions.¹⁸ Figure 3 shows no such divergence. On the contrary, the two trends are parallel over the pretreatment period. The trend in state repression for successful coups shifts upward at $t = 0$, the year of the coup attempt, as one would expect if the successful coup caused an increase in state repression.

Second, I added country-specific trends to the regression models to account for unobserved country-specific confounders. Doing so removes all information from countries that experienced only one event, making it harder to detect an effect. Despite the decrease in efficiency, the estimated coefficient is still positive and significant at $p = .011$ after controlling for linear and quadratic country-specific trends, respectively (table A3; tables A1–A5, B1–B3 are available online).

As an additional test of robustness, I replicate the analysis using the Powell and Thyne (2011) data (see appendix sec. B). With this alternative data set, I find that successful coups increase state repression, relative to failed coups (table B1). I find that this effect is similar in democracies and non-democracies (fig. B1; figs. A1–A4, B1–B4 are available online) and that the effect holds for coups happening in repressive settings (fig. B2).

18. For each observation in the differenced data set, I calculated the average of one-, two-, and three-year lags (as well as one- and two-year leads) of state repression before the coup attempt for the two groups (coup success and coup failure). To ensure that posttreatment trends are comparable, I removed observations for which coup attempts occurred in consecutive years. Trends are left (right) censored at the previous (next) coup attempt.

EFFECT OF COUP ATTEMPTS

The previous section demonstrated that coup success increased state repression relative to coup failure locally (i.e., within the population of country-years that experienced coup attempts between 1950 and 2000). In this section I consider the effect of coup attempts (relative to no coup attempts). This quantity addresses an important policy question, namely, should coups be promoted against highly repressive leaders? Answering this question requires estimating the ex ante impact of a coup attempt, that is, the expected effect of attempting a coup before knowing whether it will succeed. This quantity is $\nu_i \equiv \pi Y_{i,t=1}^1 + (1 - \pi)Y_{i,t=1}^0 - Y_{i,t=1}^n$, where π_i is the probability that the coup will succeed, $Y_{i,t=1}^n$ is the posttreatment level of repression when the military abstains from attempting a coup, $Y_{i,t=1}^0$ is posttreatment repression attained if the military attempts a coup and the coup fails, and $Y_{i,t=1}^1$ is posttreatment repression attained if the military attempts a coup and the coup succeeds. Figure 4 presents the tree of potential outcomes.

Extreme bounds analysis

In the absence of randomized treatment, estimating $E(\nu_i)$ presents major challenges. As argued above, many factors influence a military's decision to stage a coup or not. To reduce reliance on modeling decisions, I perform an extreme bounds analysis to estimate a lower bound on the effect of coup attempts (see Gassebner, Lamla, and Vreeland 2012, 2016; Miller, Joseph, and Ohl 2016). I begin by checking for evidence of a “benign” (i.e., ≤ 0) effect of failed coups relative to no coup attempt on state repression by running over 3 million regressions that control for a large combination of variables plausibly related to state repression and coups. I find no support for the hypothesis that failed coups improve human rights relative to no coup attempt. At best, failed coups seem to have no effect on state repression. This lower bound on the effect of failed coups implies that the ex ante effect of coup attempts must also be bounded. Specifically, it must be strictly positive, given the results of the difference-in-differences analysis. I elaborate on this point in appendix section A.6, which clarifies the following simple intuition. If successful coups increase state repression relative to failed coups, and if failed coups do not decrease state repression relative to no coup attempts, then it follows that coup attempts strictly increase state repression relative to no coup attempts.

My list of controls for the extreme bounds analysis includes the covariates listed in table 1, as well as four variables conventionally associated with state repression: the percentage of youth in the population (Hill and Jones 2014; Nordås and Davenport 2013), a binary indicator for common law

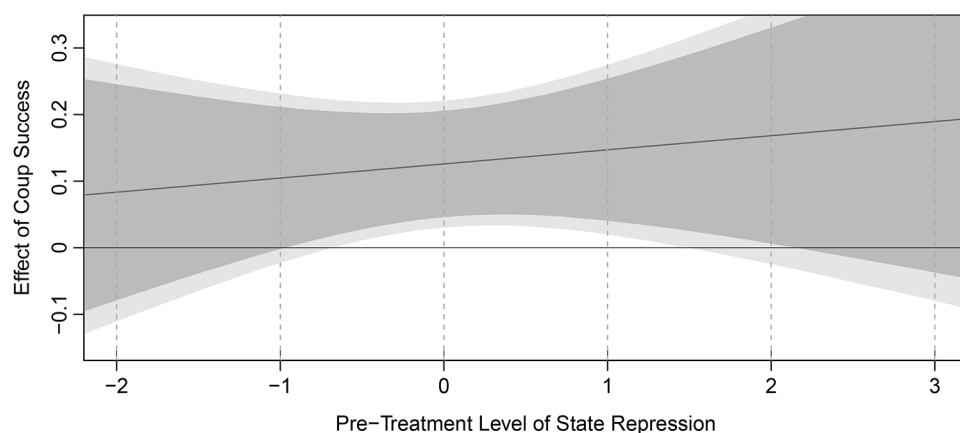


Figure 2. The figure shows the marginal effect of coup success on state repression as the pretreatment level of repression changes. Pretreatment repression is measured one year before the coup attempt.

legal tradition (Mitchell, Ring, and Spellman 2013), a binary indicator for ratification of the International Covenant on Civil and Personal Rights (Simmons 2009; Simmons and Hopkins 2005), and the measure of constraints on executive power provided by Polity IV (Davenport 2010), for 38 variables in total. I run 3,345,616 regressions, corresponding to all subsets of size 6 or less of the set of these 38 control variables. The dependent variable is the level of state repression, and the independent variables of interest are failed coup and successful coup, both lagged one year. Given that the treatment is lagged one year, I use two-year lags of controls to avoid conditioning on posttreatment variables. The quantities of interest are the average effects for failed and successful coups (both relative to no coup attempt), which I denote as ϕ and ψ , respectively. Errors are clustered by country.

Figure 5 summarizes the results. It presents the distribution of t -values for the coefficients of interest (the effect of failed coups ϕ and the effect of successful coups ψ) across the 3,345,616 regressions. It shows that the t -values for the ϕ coefficient aggregate on the right (positive) side of the graph, which suggests that failed coups either increase or have no effect on state repression relative to no coup attempt.¹⁹ In fact, none of the estimates of ϕ is both negative and significant at .05, which suggests that failed coups either worsen or have no effect on human rights relative to no coup attempts. Combined with the findings of the previous section, this result suggests that the *ex ante* effect of coup attempts is positive locally. In other words, coup attempts between 1950 and 2000 have harmed human rights (compared to the counterfactual of no coup attempt). In appendix section A.5, I show that this result holds regardless of the pretreatment

level repression, that is, coup attempts harmed human rights even when they occurred in highly repressive settings (table A5). In appendix section B.2, I also present a replication of this analysis using the coup data by Powell and Thyne 2011 that confirms these findings (fig. B3, tables B2, B3).

Global effects of coup attempts

Do the “local” effects identified in the previous section extend to the population of country-years that never experienced coup attempts? This section estimates the “global” effect of coup attempts, namely, the average effect of coup attempt within the full population of country-years.

I present results from a fixed-effects panel regression along with placebo tests. The treatment is a binary indicator of coup attempt, lagged one year, and the controls are year and country-specific dummy variables, as well as country-specific cubic trends. I conduct a falsification test in which I replace the lag indicator of coup attempt with a lead indicator. If treatment assignment is ignorable conditional on fixed effects, future treatment should not predict past outcomes after conditioning on controls, and the coefficient on this lead indicator should be indistinguishable from zero (Angrist and Pischke 2008, 237–38). To address serial correlation, I report robust standard errors clustered by country (Bertrand, Duflo, and Mullainathan 2004).

Table 5 presents the results of the fixed-effect panel regressions. Model 1 is the placebo test, while model 2 presents the estimate of the causal effect. Consistent with the identifying assumption, model 1 presents no evidence of an anticipatory effect of future coup attempt on state repression (the p -value on lead coup attempt is .525). In contrast, model 2 shows that the coefficient on lag coup attempt is positive and significant at $p < .01$, suggesting a positive average effect. Between 1950 and 2000, coup attempts increased state repression by 0.076 points on the repression scale, compared to

19. Note that the t -values for ψ are also positive and tend to be larger than those for ϕ , which is consistent with the claim that successful coups increase state repression by a larger amount than failed coups.

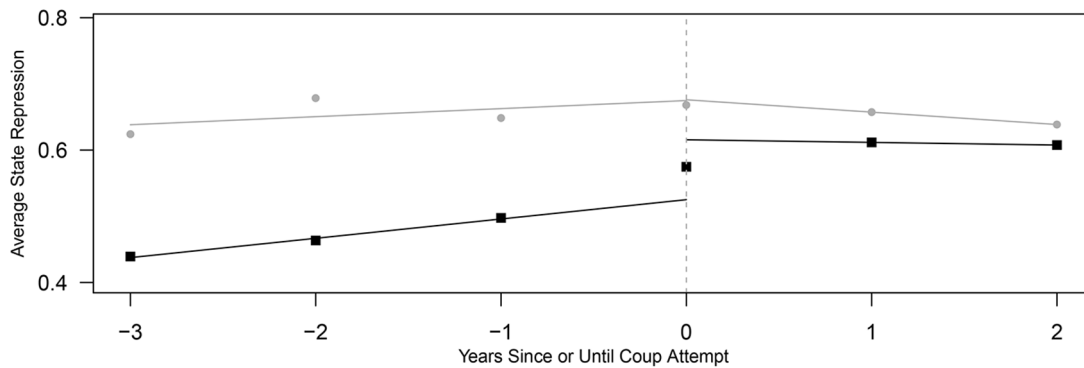


Figure 3. Average state repression for successful and failed coups before and after a coup attempt. The plot shows the mean level of state repression for each year before and after a coup attempt for the control (*gray*) and treatment (*black*) groups. The points represent the average lag and lead values of state repression for the two groups. The trends were calculated by fitting a line through average values of state repression at $t < 0$ and $t > 0$ for the two groups.

the counterfactual of no coup attempt. In the appendix, I show that this effect holds for coup attempts that occur in repressive settings (fig. A2).

Coups after the Cold War

Do these results generalize to the post–Cold War period? A recent and important development in the coup literature notes how the end of the Cold War brought a shift in international norms altering coup dynamics, with empirical analyses suggesting that a “good coup” effect may hold for the post-1991 period (Marinov and Goemans 2014; Miller 2016; Shannon et al. 2015; Thyne et al. 2017; also see Derpanopoulos et al. 2016, 2017). It is reasonable to expect that the harmful effects of coups are attenuated for the post–Cold War period. This attenuation might induce bias in estimates of the “global” effect of coup attempts in the analysis above, which only extends to 2000 and thus includes only a decade of potentially good coups.²⁰

To address this concern, I estimate the global effect of coup attempts using the Powell and Thyne 2011 data, which extend to 2013 for available data on state repression. As discussed previously, the Powell and Thyne data include coups by government actors who are not members of the military. Therefore, the quantity estimated is based on a more expansive definition of coups than previous analyses (see above on the definition and data set selection rationale for this article). With this caveat in mind, I find no support for the claim that the end of the Cold War reversed the harmful effect of coups. Table 6 presents results of fixed-effects regressions for two time periods— during the Cold War (models 1 and 2) and after the Cold War (models 3 and 4). The coefficient on lag of a coup attempt is positive and significant for both time periods, and placebo tests in models 1 and 3 suggest that these coefficients have a causal interpretation.

20. I am grateful to one of the reviewers for highlighting this point.

In the appendix, I further explore conditional effects using fixed-effects regression and the Powell and Thyne data. I estimate the effect of coup attempt on state repression for different levels of pretreatment state repression. I find no indication that coup attempts might decrease state repression in the global population of country-years for the 1950–2013 period (fig. B4).

MECHANISMS

This section explores possible explanations for why military coups worsen state repression. Although it is beyond the scope of this article to test causal mechanisms, I propose a “power consolidation” hypothesis that efforts to consolidate power in the period shortly after the coup are responsible for this effect. In appendix section A.8, I discuss alternative mechanisms, including a “regime transition” hypothesis which posits that the effect is caused by transitions to military regimes. I find stronger support for the former “power consolidation” hypothesis than the latter, although further research is necessary to disambiguate causal mechanisms.

Studies of authoritarianism have highlighted the intense power struggles that new leaders face during the early years of their tenure (Haber 2006). Shortly after capturing power, the new leader must reassure powerful groups that he will

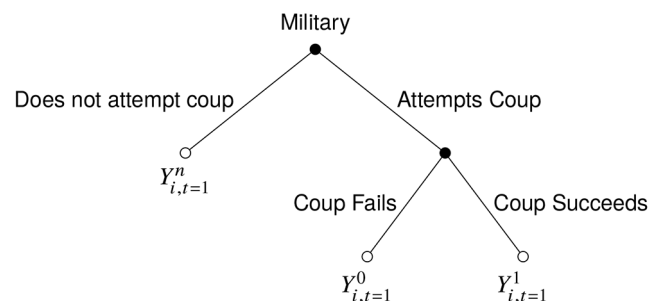


Figure 4. Tree of potential outcomes

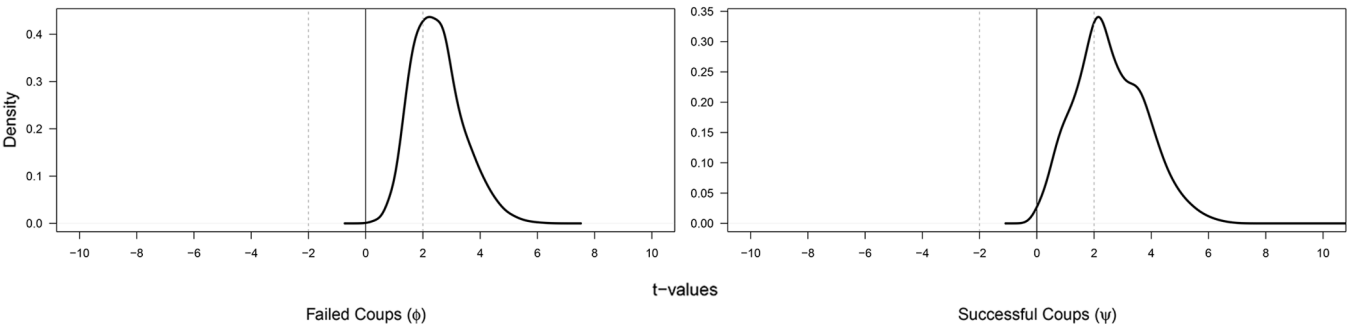


Figure 5. Distribution of *t*-values across 3,345,616 regressions

protect their interests (Albertus 2015; Haber 2006; Slater 2010). Performing these tasks is especially challenging for a military junta lacking the organizations and institutions to make credible commitments to potential allies (Magaloni 2008). Given these limitations, the military may resort to violence for maintaining order, until it builds organizations that can channel popular demands and disincentivize powerful groups from rebelling.

One implication of this power consolidation hypothesis is that a successful coup’s effect should be strongest in the immediate aftermath of the coup, when power struggles are most acute. The effect should decrease over time, as the new leader consolidates civilian support, either creating a party (Geddes 2009) or redistributing assets to elite and nonelite supporters (Albertus 2015), while weakening groups that threaten its survival (Haber 2006). It is also possible that the military eventually returns to the barracks after enshrining constitutional principles that protect its corporate interests (Nordlinger 1977). In all instances, we can expect repression to spike shortly after the coup and decrease over time.

Figure 6 shows patterns that align with these expectations. It presents the results of a fixed-effects regression with leads and lags of successful coups for two years before to five years after a coup for the period 1950–2000.²¹ The fixed-effects panel regressions suggest that the effect of a military coup is strongest the year immediately after the coup and diminishes over time. This relatively short-lived effect provides tentative evidence that the effect has roots in the leader’s struggle to consolidate power, as these power struggles occur early on.

DISCUSSION AND CONCLUSION

The key findings of this article caution against overly optimistic views of “good” coups. Many coups begin with the

promise of better days to come. However, such change rarely involves improvements for human rights. I find that (1) successful coups increase state repression compared to failed coups, (2) failed coups do not improve human rights relative to no coup attempts, and that (3) these results hold even for coups occurring in repressive regimes. These findings carry clear policy implications. Heated debates arise over whether coups should be encouraged against authoritarian rulers responsible for large-scale abuses of human rights. My analysis demonstrates that there is little evidence to show that a successful coup will make matters better in the short term, even if it removes a repressive autocrat. Rather, human rights abuses tend to worsen in the year after the coup.

The methodological approach of this article contributes to literature in political science regarding coups and their consequences. Although coups have been extensively studied, scholars have struggled with demonstrating causal relationships between coups and quantities of interest. Coups do not occur at random, and assessing the effect of coups typically

Table 5. Estimating the Effect of Coup Attempts on State Repression, Fixed-Effects Panel Regression

	Dependent Variable: State Repression	
	(1)	(2)
Coup attempt _{<i>t</i>+1}	.016 (.025)	
Coup attempt _{<i>t</i>−1}		.076*** (.020)
Country fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
Cubic trends by country	Yes	Yes
Observations	6,743	6,743
Adjusted <i>R</i> ²	.965	.965

* *p* < .1.
** *p* < .05.
*** *p* < .01.

21. The regressions control for year and country fixed effects, as well as country-specific cubic trends. Vertical bars represent 99% confidence intervals. Errors are clustered by country. See fig. A3 for replication of these results using the data by Powell and Thyne 2011 and Archigos, v4.1.

Table 6. Estimating the Effect of Coup Attempts on State Repression during and after the Cold War: Fixed-Effects Panel Regression Using the Powell and Thyne Data

	Dependent Variable: State Repression			
	During the Cold War		After the Cold War	
	(1)	(2)	(3)	(4)
Coup attempt _{t+1}	.016 (.019)		.052 (.033)	
Coup attempt _{t-1}		.055*** (.016)		.069*** (.021)
Country fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Cubic trends by country	Yes	Yes	Yes	Yes
Observations	5,266	5,266	4,168	4,168
Adjusted R ²	.975	.975	.990	.990

* $p < .1$.** $p < .05$.*** $p < .01$.

requires strong assumptions about the factors that make coups more or less likely. This article has provided a research design that reduces reliance on such assumptions by leveraging naturally occurring randomness in whether a coup, once attempted, succeeds.

This article's findings and methods provide several avenues for future research. First, the findings have left open the question of whether coups by civilians similarly increase repression. It is possible that civilian coups, such as so-called palace coups, have different effects; estimating these effects may be more challenging because civilian coups require the coordination of smaller groups of conspirators and may therefore be less uncertain than military coups. Second, although the method adopted here is best suited for studying phenomena affected by coups in the short run, such as state repression, it could be refined to understand coups' effects

on other quantities of interest to social scientists, including democracy and development that likely experience coups' effects more gradually.

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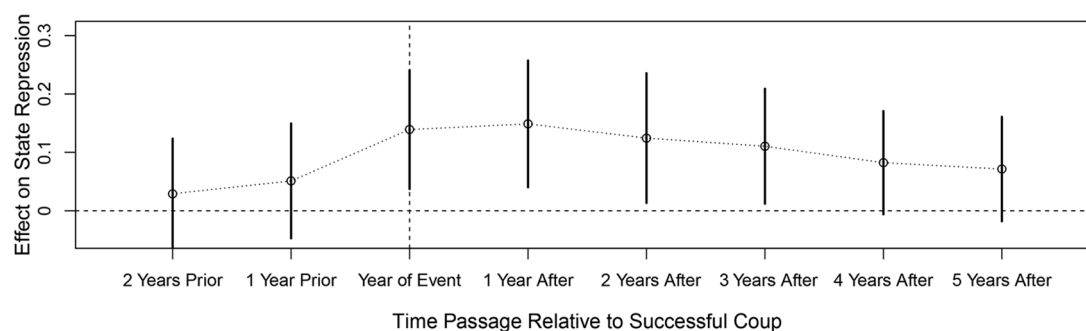


Figure 6. Estimated effect of successful coups on state repression for years before and after the event

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