

Squeaky Wheels and Troop Loyalty: How Domestic Protests Influence Coups d'état, 1951–2005

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Abstract

This article considers how domestic protests influence coups. Protests signal regime illegitimacy, which incentivizes coups and provides a favorable climate for postcoup reforms. Protests also ease coordination obstacles among coup plotters and make international actors less likely to punish coup leaders. We expect these signaling processes to be strongest when protests take place near the capital or are non-violent. Our empirical analyses introduce event-level protest data from the Social, Political, and Economic Event Database project into the coup literature. Examining a global sample of coup attempts from 1951 to 2005, we find strong support for our theoretical expectations. Our discussion provides implications for scholars studying coups and nonviolent movements more generally. It also speaks to the influence of external actors on social uprisings and highlights the importance of geographical disaggregation in the study of dissident behavior.

Keywords

domestic politics, internal armed conflict, political leadership, political survival

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In June 2013, fourteen million protesters flooded the urban centers of Egypt to oust their leader. Following the military's removal of Mohammed Morsi from office, the media and many policy makers insisted on calling this event a "popular" or "democratic" coup, rather than a military coup (Varol 2012). This terminology implies a tight link between protest activity and the military's decision to stage a coup. While scholars sometimes treat coups and mass protests as distinct events (Goemans 2008; Escriba-Folch 2013; Svolik 2009), they also recognize that there is often a swelling of protests in the days leading up to a coup attempt (Russell 1974; Casper and Tyson 2014). Even among studies that acknowledge a link between protests and coups, it remains unclear why some protests signal support for regime upheaval more clearly than others. While scholars have established a link between protests and coup activity, we seek to improve our understanding of the process by which popular protests influence elite decisions to overthrow the executive and how variations in protests influence this process.

We make two primary contributions. Recent works from scholars like Casper and Tyson (2014), Frantz and Ezrow (2011), and Powell (2012) focus our attention on coordination problems among elites who are contemplating a coup. Popular protests are expected to ease elite coordination obstacles by revealing information about support for regime upheaval. Although all protests should serve in this manner, we move the literature forward by considering how the strength of signals sent by protesters to coup plotters varies based on protest locations and levels of violence. We argue that protests should provide the most credible information to coup plotters about the public's support for military intervention when they are centered near the capital or are nonviolent. Our second contribution is empirical. We introduce event-level, geo-coded protest data into the coup literature (Nardulli, Leetaru, and Hayes 2014). These data allow for more refined analyses than were previously possible. Like recent efforts in the civil war literature that take timing and spatial considerations seriously (e.g., Sundberg and Melander 2013; Raleigh, Linke, and Hegre 2010), we expect these factors to enhance our understanding of coup activity.

This work expands on three main bodies of literature. First, while recent advances in the coup literature have enhanced our understanding of civil-military relations considerably, most studies focus almost exclusively on elites (e.g., Powell 2012; Pilster and Böhmelt 2011; Belkin and Schofer 2003). We expand this work by explaining how average citizens can influence elite decisions to defect. Second, we expand work on social movements. Recent studies on nonviolent protests recognize that the military often plays an integral role in determining whether the movement is successful (e.g., Stephan and Chenoweth 2008; Chenoweth and Stephan 2011). However, we currently lack theoretical and empirical work to understand when and why militaries are apt to side with protesters. Finally, our study has important implications for policy makers. We have evidence that external actors can both spur coups (Thyne 2010) and play an important role in the postcoup state (Thyne and Powell 2016; Goemans and Marinov 2014). Our work provides

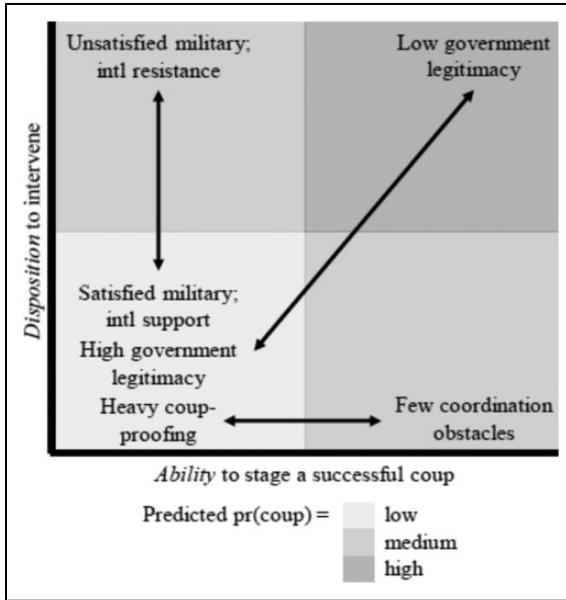


Figure 1. Coup plotter decision-making factors.

information on what policy makers should expect from popular uprisings, shedding light on appropriate stances to support democracy.

Theoretical Framework

Coups are risky. About half of all coup attempts fail and the consequences for launching a failed coup include prison, exile, and death, while punishments often extend beyond the coup plotters themselves (Svolik 2009). Following past work, we expect coup attempts to be most likely when the benefits of overthrowing the government are high and when coup plotters expect a high probability of success (Thyne 2010; Powell 2012; Lichbach 1995; Casper and Tyson 2014). To help guide the discussion, in Figure 1, we present the primary decision-making factors for coup plotters as characterized by past work.

Focusing on the y-axis, the perceived benefits of overthrowing the government are often referred to as the military’s “disposition” to intervene, which is determined by coup plotters’ expected payoffs from staging a successful coup (Powell 2012, 1021-22). Work focuses on three levels to explain the military’s incentives for a coup. First, scholars like Decalo (1990) and Miller (1970) argue that coups are motivated by individual-level decisions, with officers using coups to garner power and prestige. Second, researchers focus on the organizational interests of the military, primarily considering military resources (Nordlinger 1977; Thompson 1973;

Perlmutter 1977; Huntington 1991). Finally, state-level discussions focus on international legitimacy of the government, arguing that coups become more likely as hostility from international actors increases (Thyne and Powell 2016; Finer 1962; Belkin and Schofer 2003; Lindberg and Clark 2008; Huntington 1968; Londregan and Poole 1990).

Moving to the x-axis, the second main body of work predicting coups focuses on the military's ability to stage a successful coup. Irrespective of the individual or organizational level, scholars recognize that leaders often lack resources to placate coup plotters. Thus, significant attention has been paid to how leaders "coup-proof" their regimes by creating structural obstacles that increase coordination costs (Pilster and Böhmelt 2011). Such maneuvers include building counterforces that check and balance each other (First 1970; Jenkins and Kposowa 1992; Kposowa and Jenkins 1993; Belkin and Schofer 2003), rotating military officers (Pollack 2002), and increasing military specialization (Welch 1976; Quinlivan 1999). While these maneuvers are primarily focused on the military, others have recognized that elite coordination, which includes nonmilitary components of the government, is likewise needed for a successful coup (Frantz and Ezrow 2011; Weeks 2008; Casper and Tyson 2014).

Finally, scholars have considered how the government's domestic legitimacy influences both the disposition and ability to stage a coup. When the population signals that the government no longer has the "right to make rules" (Belkin and Schofer 2003, 607), the military is likely to intervene to protect its organizational interests (Norris 1999; Booth and Seligson 2009; Thompson 1973; Welch 1970; Finer 1962). The military is also more likely to succeed because the population is apt to support the putsch (Weatherford 1987; Powell 2012; Sutter 1999; Roberts 1975).

The framework presented in Figure 1 is useful because it focuses on the two key processes that drive coups: disposition and ability. Coup plotters must ask themselves two key questions when contemplating a coup. First, will the putsch succeed or fail? Second, if the coup succeeds, will the plotters have the ability to improve the status quo? Previous work expects protests to yield a positive response to each of these questions, which should increase the likelihood of coup attempts. In an attempt to further unpack the protest–coup relationship, we next describe how the past work has characterized the influence of protests on coups. We then add to the discussion by considering how variations in protests influence coup-plotter calculations.

Protests and Coup Attempts

Scholars have gained significant ground in understanding the protest–coup relationship. Consistent with this work, we focus on three primary actors. First, leaders hold power and seek to retain power (Svolik 2012; Bueno de Mesquita et al. 2003). Second, regime elites, or potential "coup plotters" as we refer to them below, include both military and civilian actors on which the leader's power rests. We assume that elites wish to retain their privileged positions and, if possible, increase

their power (Wig and Rød 2014; Acemoglu and Robinson 2001; Lichbach 1995; Thyne 2010; Powell 2012; Casper and Tyson 2014). Third, citizens are nonelite members of the population. Although it is possible for citizens to protest for thrill seeking or to blow off steam, we assume that protests are signals of government illegitimacy and that protesters seek to affect policy change (Lohmann 1993). Aside from these three primary actors, recent studies looking beyond the coup state have shown that international actors can play decisive roles in the postcoup political trajectory (Thyne and Powell 2016; Goemans and Marinov 2014). We therefore integrate international actors into our discussion and assume that plotters consider international audiences when calculating their disposition to stage a coup.

Having laid out our actors of interest, we next consider the influence of protests on coups. Classical work argued that militaries are apt to intervene when the public shows overt dissatisfaction with the regime (Welch 1970; Finer 1962; Sutter 1999). Consistent with our theoretical framework, subsequent work has focused on both disposition and ability as mechanisms to clarify the protest–coup relationship. Regarding disposition, protests are apt to increase the likelihood of coups along one of the two paths. First, protests may provide a window to revise policies in the coup plotters' favor, providing a spark to activate latent dispositions to intervene among individual officers (Decalo 1990; Miller 1970) or among the military establishment as a whole (e.g., Thompson 1973; Huntington 1991). Second, even in the absence of underlying displeasure with the executive, plotters may launch a coup out of fear that protests may spur revolutions, risking the plotters' elite status altogether (Acemoglu and Robinson 2001; Wig and Rød 2014).

Plotters' dispositions to intervene amid protests may also be influenced by international actors. Because coups are the foremost reason that democracies fail (Onwumehili 1998; Kieh and Agbese 2005), both states and international organizations (IOs) seeking to protect democracies have instituted policies to punish leaders following a coup.¹ However, we continue to see significant variation in whether these policies are actually enforced (Shannon et al. 2015). Coup leaders who take power amid mass protests can claim to be responding to the will of the people. Such take overs have been dubbed “democratic coups” (e.g., Varol 2012), and leaders taking over under conditions of protests are less likely to face the consequences of anticoup policies from states and IOs. Past work suggests that responses to coups should be more positive when coups come about amid protests, thus heightening plotters' dispositions to stage a coup.

The second pathway by which protests may spur coups is by increasing plotters' ability to successfully overthrow the government. Recent work characterizes protests as direct “signals” of discontent (Thyne 2010; Powell 2012). Such signals are important because they increase the perceived likelihood that the coup will be successful. More specifically, Casper and Tyson (2014) suggest that protests often precede coup attempts because they fill an information gap between the elites. This information increases the likelihood of coups because elites update their beliefs about the strength of the opposition and the likelihood that other elites will support

a coup. As this information is revealed, obstacles that make elite coordination difficult diminish, increasing the likelihood of coup attempts.

One might wonder whether the regime leader is simply a bystander in this process or whether the leader can act to prevent coups in the face of protests. As noted earlier, leaders seek minimize coup risks by coup-proofing their regime, so why do we not see increased coup-proofing efforts in the face of swelling protests? Two primary reasons make this unlikely. First, coup-proofing is likely to draw backlash and weaken elements of the military apparatus that are needed to protect the leader during precarious periods (Wig and Rød 2014; Powell 2014b). Rotating or purging officers when their support is needed most, simply put, would be unwise. Second, protests are generally fleeting in nature, while creating structural coup-proofing obstacles is accomplished over longer periods. Increasing military specialization (Welch 1976) and building “armies into numerous, mutually suspicious rival forces that check and balance one another” (Belkin and Schofer 2003, 596) cannot happen overnight. Thus, we expect leaders to be both unwilling and unable to engage in enhanced coup-proofing in the face of protests, instead being forced to rely on the measures they already have in place to prevent coups when protests arise. This argument is consistent with the empirical reality of coup-proofing measures (Belkin and Schofer 2003, 2005), which change quite slowly overtime. Further, if leaders indeed think strategically in this manner, such maneuvers should only weaken the expected relationship between protests and coups.

Taken together, past work on the protest–coup relationship tells a consistent story. Via the disposition or ability mechanism, protests are apt to increase the likelihood of coups. Empirical work provides strong evidence supporting this relationship (e.g., Thyne 2010; Powell 2012; Casper and Tyson 2014, Wig and Rød 2014). However, there remains a lack of knowledge about the type of protests that are most apt to spur coups. We argue that protest location and violence serve as useful proxies to capture variations in signal strength.

Protest Locations and Coups

The first variation we consider is the location of protests. As described earlier, protests are overt signals that the regime is no longer a legitimate governor of the peoples’ interests. When coups are launched during protests, plotters should predict a high likelihood of success and a favorable climate for reform. Moreover, coup plotters should expect little backlash from international actors. The signal from domestic actors in support of a coup should be strongest when protests are near the capital for three main reasons.

First, costs increase the credibility of signals (e.g., Schelling 1960) and the costs of protests increase as they move toward the capital. Knowing this, leaders fearful of coups frequently create loyal paramilitary forces with the sole purpose of defending the leader (Belkin and Schofer 2003). For example, Hussein moved the most skilled soldiers from the Iraqi army to his Baghdad-based Republican Guard (Powell

2014b), and Zairian President Mobutu refused to send his elite Special Presidential Division forces away from the capital even as the Armed Forces of Liberia rebels won battle after battle (Wrong 2002). In contrast to neglect of peripheral areas, paramilitary forces are quite capable of crushing dissent when protesters gather near the leader's seat of power. During the Arab Spring uprisings in Libya, for example, Colonel Gaddafi's weak regular units had little role in quelling the unrest and even their defection to the demonstrator's side did little to tilt the balance. However, the loyal and well-equipped paramilitary units, including the Khamis Brigade led by his son, proved effective when protesters rallied in Tripoli (Finn 2011; Hosenball 2011).²

Second, protests near the capital send the sharpest signal about the public's support for a coup because the target of the protesters' wrath, the executive, is clearer when the protests take place near the leader's seat of power. Capital cities typically include vast segments of various ethnic groups and interests, and protests within the capital will commonly find these segments represented. In contrast, protests in areas outside of the capital are often segmented by ethnicity or religion and the causes behind the protests are often parochial. For example, the protests in Cairo that led to Morsi's ouster were notable for their size and diversity, factors that are strongly related to the success of protest movements (Chenoweth 2013; Besinger, Jamal, and Mazur 2013). In contrast, many other protests happened in governates outside of Cairo during the same period: butchers blocked a major highway in Sohag, residents protested lack of security in South Sinai and Suez, and citizens in Gharbia demanded improved working conditions (Al-Monitor 2012). Unsurprising, given the history of neglecting rural areas, these protests did not provide the push needed for military intervention (Ayeb and Bush 2014). When protests arose in the capital, however, military elites saw an opportunity to increase their power and were forced to act to avoid a large-scale revolution (Acemoglu and Robinson 2001).

Finally, protests that take place near the capital are impossible for militaries to ignore. They must act, either by remaining loyal or by siding with the protesters. This argument stems from a concept known as "urban bias," which has informed previous studies about the unique importance of political activity in capital cities and other urban centers (Lipton 1977; Bates 1981; Bezemer and Headey 2008). Urban bias theories suggest that the central concern of leaders is staying in power; thus, they respond quickly to those who have the capability to unseat them. These capable groups are generally identified to be the military, the upper and middle classes, and urban dwellers (Bates 1981; Stasavage 2005). In connection to our theory, urban bias is not directed at the leader, but at the military. Massive upheaval threatens the rule of law, and part of the military's edict is to maintain law and order. Thus, the military is unable to ignore protests in urban centers because they threaten the strength of the state. Because of these threats, we expect the military to be more likely to respond to demands of protesters who are active in the capital versus those in peripheral areas.

If protests near the capital provide clear and credible signals about the public's support for a coup, then we should expect these types of protests to have a strong

influence on plotter decision-making. Relating this to our theoretical framework, protests near the capital should ease elite coordination because they provide clear information to waffling elites about other elite preferences. This easing of coordination problems should increase plotters' abilities to stage coups successfully. Second, protests centered near the capital should increase plotters' dispositions to intervene because they provide plotters with ample evidence that they can improve the status quo. Likewise, instead of automatic sanctions, coup leaders who come to power amid centralized protests can more credibly stake claim on being the guardians of the people, giving international actors space to ignore policies that are meant to protect legitimate rulers. This discussion yields our first hypothesis:

Hypothesis 1: Compared to peripheral protests, protests centered near the capital should have a stronger effect on the likelihood of coups.

Protests Violence and Coups

The literature on violent and nonviolent protests has recently burgeoned, and the introduction of new data on nonviolent movements point to fruitful paths for further development (Chenoweth and Lewis 2013). This work largely focuses on how the decision to use violence influences the movement's success, finding that nonviolence is surprisingly effective (Stephan and Chenoweth 2008; Chenoweth and Stephan 2011). Scholars have also shown that protests become more effective when the military joins the protesters, which provides a tight link to our discussion. We depart from this work in two primary ways. First, we move back a step in time to consider what causes the military to join the regime challengers in the first place. Second, our focus ends with the military defection, leaving future work to consider whether movements are ultimately successful.³ Closely paralleling work on nonviolent movements generally, we expect protest violence to temper the strength of signals sent by protesters to coup plotters for three main reasons.

First, while popular protests signal declining regime legitimacy, legitimacy is not a one-way street. Protesters must also signal legitimacy by demonstrating that a postcoup state would be preferable to the status quo, and the behavior of regime challengers signals whether overthrowing the regime will result in a better or worse situation. Other scholars have noted the relationship between the nature of dissidents and posttransition conditions. For example, Bob (2005), Lasley and Thyne (2015), and Cunningham (2013) explain that dissidents change their goals, tactics, and messages to signal legitimacy. Protesters signal their own level of legitimacy when they decide to use either peaceful or violent methods. Nonviolent protests encourage broad participation because protester preferences are viewed as less extreme, which broadens their appeal (Stephan and Chenoweth 2008; O'Donnell, Schmitter, and Whitehead 1986). In contrast, violent protests often include a smaller subset of the population because they are perceived as having extremist goals, which casts an unappealing net for support (Abrahms 2006; Weinstein 2007; DeNardo 1985).

Second, elite coordination obstacles ease during nonviolent protests. This is because the forces risk backlash if they comply with orders to repress nonviolent protesters. The internal solidarity of mobilization movements should harden if government forces bully nonviolent protesters, and nonmobilized sectors should be more likely to join the movement. Likewise, repression of nonviolent movements generates rifts among regime supporters. As explained by Nepstad (2013, 339-40), resisters can draw on many sources to raise the costs of repressing nonviolent movements, including emphasizing the religious immorality of attacking unarmed protesters and highlighting the “honor costs” of remaining loyal to a regime that harms unarmed civilians. When commanded to repress violent protests, militaries are apt to comply. Not only are militaries trained to counter violent threats, but they also have strong incentives to do so (Summy 1994; Zunes 1994). Soldiers who defect to a violent movement risk being killed or tortured by protesters, and regimes are unlikely to forgive soldiers who defect under these conditions (Stephan and Chenoweth 2008). Thus, pressure by governments to repress nonviolent movements is apt to generate rifts among loyalists, opening space to be exploited by coup plotters. When protests are violent, however, both soldiers and civilian elites are likely to rally behind their leadership.

Finally, coup plotters must look outside their state when making predictions about their ability to meet their goals once taking power. Just as protest violence signals illegitimacy about the protesters’ intentions to domestic constituencies, we expect violent protests to send important signals to international audiences. Militaries are expected to remain loyal to counter illegitimate, violent protesters, and external actors have a long history of supporting governments being challenged by violent resisters (Chenoweth and Stephan 2011). In contrast, staging a coup amid nonviolent protests may be welcomed by the international community. Such defections reveal that the military refused orders to repress, instead choosing to serve as a guardian of the people. Such guardianship is tenuous to be sure, as coups have ushered in many repressive regimes. However, coups can also spur democratization (Thyne and Powell 2016; Powell 2014a) and elections (Goemans and Marinov 2014). External actors know this, and they also realize that military forces have few good options when faced with nonviolent resisters. Loyally repressing nonviolent resisters will almost certainly draw an international backlash, and disregarding repressive orders will yield punishment from the government (Chenoweth and Stephan 2011). Thus, fully defecting by staging a coup is apt to be the least costly option.

Taken together, we expect nonviolent protests to have a stronger effect on coup attempts than violent protests. Coup plotters should expect a greater likelihood of success when protests are nonviolent. This is because militaries are less likely to follow orders to repress nonviolent protesters, preferring instead to defect and, thus, easing coordination obstacles among coup plotters. Regarding disposition, coup plotters responding to violent protests should expect little support for postcoup reforms because it is unclear whether the population views violent protesters as more or less legitimate than the government. In contrast, juntas responding to

nonviolent protests can stake claim on being the guardians of the people. Likewise, coup plotters should expect little resistance from external actors following a coup if their defections appear to represent the interests of legitimate, nonviolent citizens. This discussion yields our second hypothesis:

Hypothesis 2: Compared to violent protests, nonviolent protests should have a stronger effect on the likelihood of coups.

Research Design

Our theoretical framework predicts coups to be more likely during periods of heavy protest activity, which has found support from a variety of previous studies. In addition to providing more refined data to test this claim, we push this expectation further with two more innovative hypotheses. We expect the influence of protests on coups to increase if protests are centered near the capital (Hypothesis 1) or are nonviolent (Hypothesis 2).

We test these hypotheses by first defining our unit of analysis as country-month for all states for which we have data.⁴ This includes 150 states from 1951 to 2005, which produces 79,404 country-months for analysis. Our dependent variable, *coup attempt*, comes from the Powell and Thyne (2011, 252) coup data set, which defines coups as “illegal and overt attempts by the military or other elites within the state apparatus to unseat the sitting executive.” We see 403 coup attempts during our period of study.⁵ Given that our dependent variable is dichotomous, we use logistic regression with standard errors clustered by country. We include time since the last coup attempt, time squared, and time cubed to account for temporal dependency (Carter and Signorino 2010).

Independent Variables

Following the conventional wisdom, our first expectation is that protests should increase the likelihood of coups. Data issues from past work have unfortunately left a large gap between the theoretical conceptualization and measurement of protests. Early work often operationalized public discontent with abstract indicators of economic performance or quality of governance (Norris 1999; Booth and Seligson 2009; Belkin and Schofer 2003; Thompson 1973; Weatherford 1987). More recent work comes closer to capturing overt antipathy toward a regime with Bank and Wilson’s (2015) annual count of riots, protests, and strikes (Thyne 2010; Powell 2012; Casper and Tyson 2014). However, these data collapse a great amount of information into a single, yearly indicator, making it virtually impossible to consider important variations associated with protest events.

Our first independent variable, *domestic protests*, introduces event-level data from the Social, Political, and Economic Event Database (SPEED) Project to the coup literature.⁶ Coded for 165 countries from 1946 to 2005, these data capture

“human-initiated destabilizing events,” which include “*happenings that unsettle the routines and expectations of citizens, cause them to be fearful, and raise their anxiety about the future*” (Nardulli et al. 2014, 1, italics in original). These data are collected using a hybrid computer–human approach, with autoextraction of news reports that are categorized into event data by human coders using carefully structured protocols. Events are associated with information critical to analyzing our research questions, including type, location, and use of violence.

The original SPEED data include around sixty-two thousand events coded by initiator, target, date range, and location (*inter alia*). For our domestic protests variable, we are interested only in how nonstate, domestic actors responded to the government. Our second step, therefore, was to purge the data to capture only events where domestic actors demonstrated a negative response (either “political expression” or “political attacks”) toward the government.⁷ This reduced our observations to 18,509 events. Having captured the event of interest, our third step was to calculate the sum of protest events taking place in each state-month.⁸ The measure ranges from zero to sixty-four (anti-Vietnam protests in the United States, May 1970). Consistent with the conventional wisdom, we expect to see a positive and significant coefficient for this variable.

Moving beyond the conventional wisdom, our first hypothesis predicts the influence of protests on coup attempts to be strongest if protests are centered near the capital. Using the latitude and longitudinal coordinates for each available protest event provided by the SPEED data, we calculated the distance from each protest location to capital city.⁹ We then split the protest variable into three categories: *no protests*, *protests near the capital*, and *protests away from the capital*, using a mean of three miles as the threshold between “in” and “away” from the capital.¹⁰ Our analyses exclude the “no protest” measure, and our hypothesis is tested by comparing the coefficients on “protests near the capital” and “protests away from the capital.”

We expect the influence of protests on the likelihood of coup attempts to decrease if the protests are violent (Hypothesis 2). The SPEED data include a categorical variable that captures weapons used in each event, including such things as fire, explosives, and blunt instruments. We simplify this measure by capturing whether or not a weapon was used in any of the protest events during each country-month.¹¹ Like the distance measure, we then created three categories for analysis: *no protests*, *violent protests*, and *peaceful protests*.

Control Variables

Past research has found a number of factors to consistently influence the likelihood of coup attempts.¹² Important among these is the economic condition of a state (Londregan and Poole 1990; Belkin and Schofer 2003; Powell 2012). We capture economic performance by including a measure for *gross domestic product per capita* (ln; K. Gleditsch 2002). We expect the probability of coup attempts to

decrease as the economy strengthens. Next, military regimes are more likely than other regime types to experience a coup attempt (Belkin and Schofer 2003; Thyne 2010). Thus, we include a dummy variable, *military regime*, that is coded a 1 if a state has a military regime and a 0 otherwise (Geddes, Wright, and Frantz 2014). Past research has shown that anocracies are more apt to experience coups compared to authoritarian regimes and democracies (Powell 2012). We include measures for *democracy* and *authoritarian*, leaving *anocracy* as the excluded category (Marshall, Jaggers, and Gurr 2009). Next, it is possible for our protest measure to capture more extreme varieties of protests, which are more akin to civil conflict than what we describe theoretically. To isolate protests from these types of conflicts, we include a dummy variable that captures whether or not the government faced an ongoing *civil conflict* (N. P. Gleditsch et al. 2002). Finally, we expect the likelihood of coups to vary by region, particularly due to regional IOs like the African Union and Organization of African States putting in place policies to condemn coups. Thus, we add regional dummies to our model, leaving Europe as the excluded category.

Results

We present our primary findings in Table 1. Consistent with the conventional wisdom, we first see that the likelihood of coup attempts increases with domestic protests (model 1, $p < .003$). This finding indicates that coup plotters perceive a higher likelihood of success and a favorable postcoup environment when they stage their putsch in the midst of widespread discontent. Following Hanmer and Kalkan's (2013) observed values approach, we calculate marginal effects for each case by changing each significant independent variable while keeping all others at values observed for the case. We present the substantive effects for these predictions in Figure 2. In the absence of protests, the likelihood of a coup in each month is quite small (.0007), which is unsurprising given the infrequency of coups at the country-month level. The predicted likelihood of a coup increases by 16.4 percent (to .0008) when the state experiences three protest events in a given month.

Moving to our first hypothesis, we expect the influence of protests on coups to be strongest when the protests are primarily in or near the capital. We find support for this expectation in model 2 after splitting protests between those centered "near" and away from the capital. While both types of protests are significantly more likely to produce coups compared to stable periods (the excluded category), the coefficient on protests near the capital is seven times greater than what we see away from the capital ($p < .005$). In substantive terms, a month with three protests near the capital increases the likelihood of a coup by 246 percent over the baseline (to .003), while the same number of protests away from the capital increases the likelihood of a coup by only 16.5 percent (to .0008).

One way to illustrate this finding is to consider the contrasting the events leading to the coup attempt in Portugal (November 1975) and those against Hussein following the first Gulf War (1991). After overthrowing the Estado Novo regime in 1974,

Table 1. Influence of Domestic Protests on Coup Attempts, 1952 to 2005.

	Model 1	Model 2	Model 3
Domestic protests	0.059* (0.020)		
Protests near capital (H1)		0.417* (0.126)	} $\Pr(\beta_{near} = \beta_{away}) < .005^*$
Protests away from cap. (H1)		0.057* (0.020)	
Violent protests (H2)			0.049* (0.022)
Peaceful protests (H2)			0.200* (0.046)
			} $\Pr(\beta_{viol} = \beta_{space}) < .005^*$
GDP/capita	-0.193 [†] (0.109)	-0.197 [†] (0.109)	-0.195 [†] (0.109)
Democracy	-0.561* (0.194)	-0.566* (0.194)	-0.568* (0.194)
Authoritarian	-0.436* (0.108)	-0.441* (0.107)	-0.441* (0.108)
Military regime	0.357* (0.111)	0.360* (0.111)	0.358* (0.111)
Civil conflict	0.387* (0.147)	0.379* (0.147)	0.385* (0.146)
Middle East	1.407* (0.473)	1.414* (0.472)	1.412* (0.472)
Africa	1.297* (0.482)	1.301* (0.481)	1.301* (0.481)
Asia	0.898 [†] (0.481)	0.887 [†] (0.479)	0.887 [†] (0.479)
Americas	1.710* (0.463)	1.705* (0.459)	1.690* (0.462)
Constant	-3.669* (1.003)	-3.648* (1.002)	-3.672* (1.001)
Area under ROC	0.809	0.809	0.811
Observations	79,404	79,404	79,404
Pseudo R ²	0.106	0.106	0.107
LR χ^2	254.2*	256.7*	295.7*

Note: Robust standard errors clustered by country in parentheses. Coup months, coup months² and coup months³ not shown. GDP = gross domestic product; ROC = receiver operating curve; LR = likelihood ratio.

*p < .05 (two-tailed).

[†]p < .10.

the ruling bodies of Portugal faced a series of crises in their transition to democracy. Protests from procommunist elements against President Costa Gomes escalated dramatically in November 1975 (Howe 1975; New York Times 1975), culminating in a coup attempt on November 26. Similar protests were seen following Iraq's

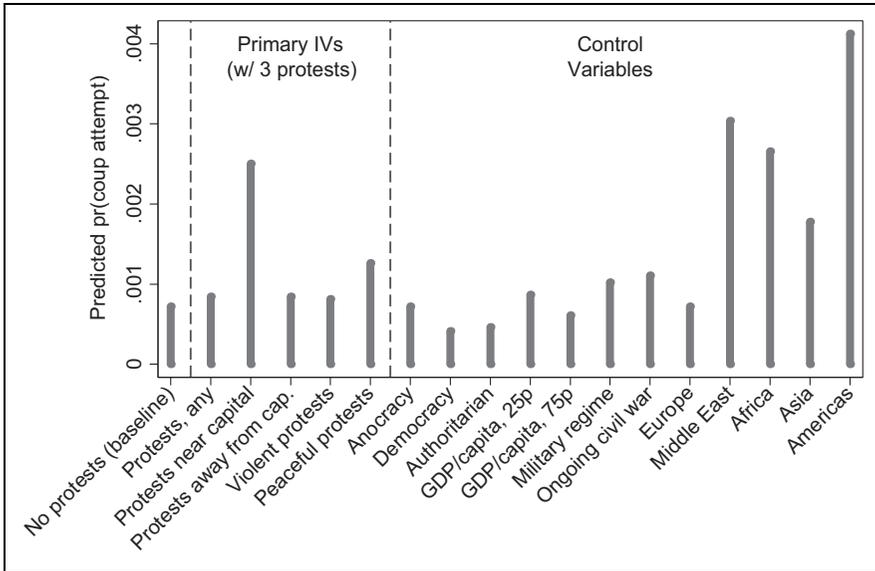


Figure 2. Influence of domestic protests on coup attempts: substantive effects.

defeat to US-led coalition forces in 1991, as an estimated 75 percent of Iraq's population gathered in the streets to protest Hussein's rule (Bengio 1993). While these protests eventually led to civil conflicts in the Kurdish and Shia areas, unlike Portugal we saw no coup attempt in Iraq. Consistent with our theory, one way that we can make sense of these disparate outcomes is by examining where the protests took place. In Figure 3, we plot the location, frequency, and size of protests events in Portugal and those following the end of coalition's hostilities in Iraq. Although the size of the protests in Iraq dwarfed those that we saw in Portugal, the notable difference is that Baghdad remained silent, while almost all Portuguese protests centered in Lisbon. So while the military in Lisbon fractured, Hussein remained secure at home and was able to use his troops to quell dissenting Kurds and Shia far from his seat of power (Quinlivan 1999).

Moving to our second hypothesis, we expect the influence of protests on coup attempts to be strongest if the protests are peaceful. We find strong support for this expectation. Months that include both violent and peaceful protests are significantly more likely to lead to coups versus stable periods. However, the coefficient on peaceful protests is significantly greater than the coefficient for violent protests ($p < .001$). Compared to a baseline scenario with no protests, for example, adding three protest events increases the likelihood of a coup by 74.2 percent if the protest is peaceful (.0007 to .001). If weapons are used in the protests, however, the same number of protest events increases the likelihood of a coup by only 12.8 percent (to .0008). This finding coincides well with Chenoweth and Stephan's (2014, 101)

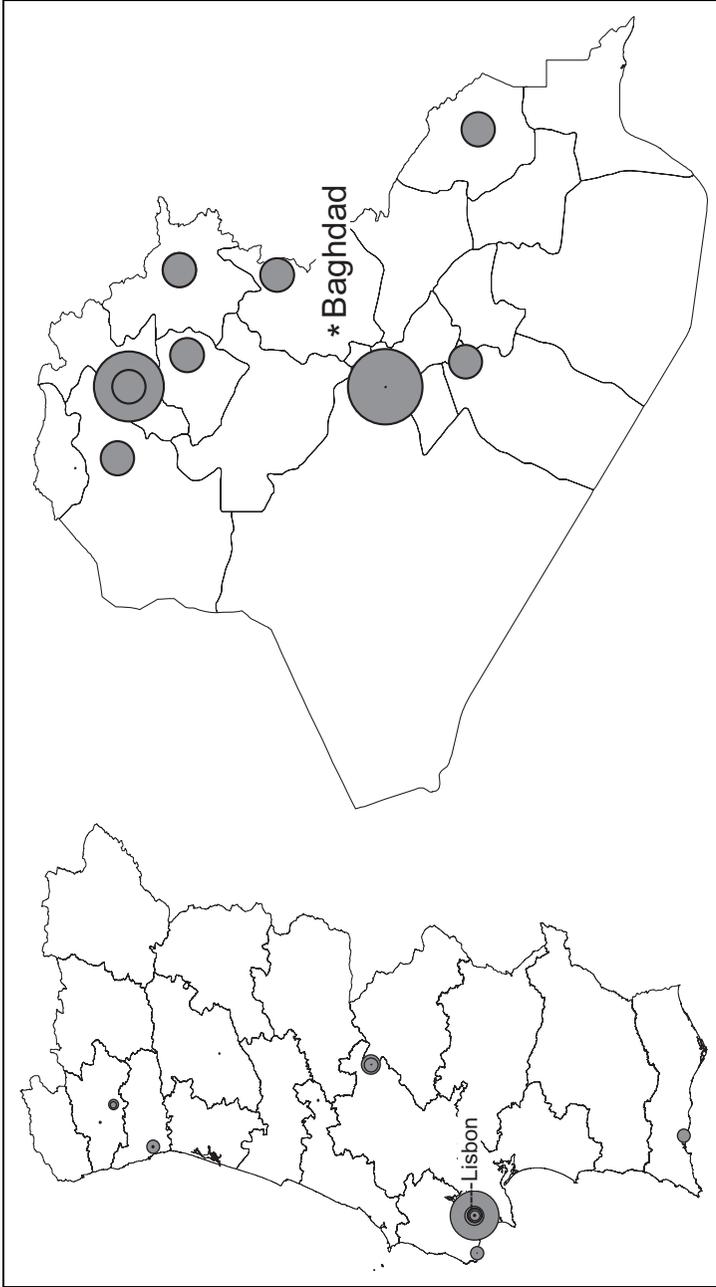


Figure 3. Protests size and locations in Portugal (1975) and Iraq (1991). Events for Portugal include those in November prior to the November 26 coup attempt. For Iraq, we present all protests following the end of coalition hostilities on February 27.

discussion of the importance of Egyptian's adherence to nonviolence in overthrowing Mubarak: "... had the Tahrir Square protests turned violent," they argue, "the situation in Egypt might well have been far worse than it is today. Armed uprising tend to reinforce the power of the military even more quickly, discouraging defections."

Regarding control variables, our results largely support previous analyses. Although p values are only marginally significant across models, the likelihood of coup attempts declines as the state's level of wealth grows. This finding is consistent with several past studies (Londregan and Poole 1990; Powell 2012; Collier and Hoeffler 2007; Gasiorowski 1995; Przeworski et al. 1996), suggesting that development can deter coup activity.¹³ Likewise, anocracies appear to have more tenuous civil-military relations when compared to their democratic and autocratic counterparts. Next, military regimes are found to be significantly more likely to face coups compared to all other regime types, which is consistent with previous findings from Geddes (1999) and Thyne (2010). Civil conflicts also increase the likelihood of coup attempts. This measure was primarily used to isolate protests that might be conflated with civil conflicts within the SPEED data set. Thus, we suspect that more study would be needed to analyze the true effect of civil wars on coup attempts. Finally, we see all regions are more coup-prone than Europe (the excluded category).¹⁴

Robustness and Causation

At this point, our results show a correlation between protests and coups. However, two key factors remain unclear. First, rather than causing coups, it is possible that protests and coups are simply part of the same process. Lagging the independent variables helps in this regard, but doubt remains. Protests may arise due to perceived weakness with the government in anticipation of coups, for example, or because the population feels that the military is particularly unlikely to follow orders to repress, instead choosing to defect. Second, it is unclear whether variations within countries are driving our findings or whether we are capturing cross-sectional variation. The latter would be particularly problematic because news coverage of events is likely to vary depending on country-specific factors (e.g., wealth or population), which would be related to the measurement of our primary independent variable. If within-country variations explain our findings, in contrast, we can be far more confident that protests cause coups rather than both being part of some underlying country-level factor.

We address these uncertainties in Table 2. These models reestimate the findings from Table 1 with five alternative estimation approaches.¹⁵ First, we add fixed effects by country (models 1–3) and year (models 4–6) to model unit heterogeneity. The findings for the latter set of models are nearly identical to our primary findings, which is unsurprising because we have little reason to think that the relationships will depend on the year. In terms of direction, we also see little change with

Table 2. Influence of Domestic Protests on Coup Attempts: Robustness Checks.

	Fixed effect and matching								
	Fixed effect by country			Fixed effect by year			Matching		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Domestic protests	0.095* (0.019)			0.060* (0.017)			0.568* (0.110)		
Protests near cap		0.346* (0.157)			0.412* (0.146)			0.376* (0.146)	
Protests away		0.094* (0.019)			0.058* (0.017)			0.060* (0.017)	
Pr($\beta_{near} = \beta_{away}$)		<.110			<.015*			<.030*	
Violent protests			0.085* (0.020)			0.050* (0.020)			0.044* (0.019)
Peaceful protests			0.324* (0.062)			0.199* (0.047)			0.191* (0.049)
Pr($\beta_{viol} = \beta_{peace}$)			<.001*			<.003*			<.005*
Observations		45,540			79,404			72,682	
	Within-/between-country effects								
	Excluding \bar{Y}_i			Including \bar{Y}_i					
	(10)	(11)	(12)	(13)	(14)	(15)			
Protest _{Dev}	0.091* (0.027)			0.093* (0.026)					
Protest near cap _{Dev}		0.355* (0.152)			0.374* (0.141)				

(continued)

Table 2. (continued)

	Within-/between-country effects				
	Excluding \bar{Y}_i		Including \bar{Y}_i		
	(10)	(11)	(12)	(13)	(14) (15)
Protests away _{Dev}		0.094* (0.029)			0.091* (0.026)
$\Pr(\beta_{\text{near dev.}} = \beta_{\text{away dev.}})$		<.091 [†]			<.049*
Violent protests _{Dev}			0.092* (0.033)		0.086* (0.028)
Peaceful protests _{Dev}			0.277* (0.053)		0.297* (0.045)
$\Pr(\beta_{\text{viol. dev.}} = \beta_{\text{peace dev.}})$			<.002*		<.001*
Protest s_i	-0.317 [†] (0.187)			-0.254* (0.110)	
Protests near cap _i		1.363 (0.862)			-0.936 [†] (0.486)
Protest away _i		-0.501* (0.255)			-0.187 [†] (0.101)
Violent protest s_i			-0.764 [†] (0.415)		-0.370* (0.164)
Peaceful protest s_i			0.361 (0.338)		-0.081 (0.195)
Coups(Y_i)				85.61* (9.69)	89.87* (9.89)
Observations		79,404			79,404

Note: Control variables (consistent with Table 1) not shown.

* $p < .05$ (two-tailed).

[†] $p < .10$.

country-level fixed effects, though the difference between near and away from the capital become borderline insignificant ($p < .110$) with country-level fixed effects.

We attempt to test causality more directly using a matching technique in models 7 and 8. This approach identifies units that have similar profiles across the range of independent variables but have different treatment groups. We employ Iacus, King, and Pooro's (2012) method of coarsened exact matching, which matches among observations that are nearly balanced for variables that are near each other, but not identical. Unmatched observations are omitted, and weights allow multiple matches with the same profiles. Our data match based on the control variables from Table 1 using a dichotomized protest variable as the treatment variable. Our estimations include the variables used for the matching as independent variables because the matching produces treatment and control groups that are not perfectly balanced.¹⁶ As we can see in models 7 and 8, the matching technique produced findings that were quite similar to our primary analyses, again lending support to protests having the anticipated effect on coups.

One problem with the first set of models in Table 2 is that they omit observations where states had no coups in our time frame (models 1–3) and where observations could not be matched (models 7–9).¹⁷ To rectify this, we follow Wright, Frantz, and Geddes (2013) who describe an alternative way to model unit heterogeneity that is akin to the conditional logit models used in models 1 to 6. This approach simultaneously models both between- and within-country effects by including the country means for explanatory variables in the model (e.g., Protests_i captures the mean of each states) and the deviations from country means to capture within-country differences ($\text{Protests}_{i,t-1} - \text{Protests}_i$), which we label with subscript "Dev" (Protests_{Dev}). We also include the means of coups (\bar{Y}_i) in models 13 to 15 to account for country-specific intercepts. In this approach, the mean of our protest measures estimates between-country effects, while the deviations capture within-country effects. The results from this approach support our baseline findings. The deviations reported at the top of the table largely mimic our primary findings, suggesting that within-country effects have a strong effect on coups and countering the idea that certain states are simultaneously prone to both protests and coups. Likewise, country means reported on the lower half of these models suggest an inconsistent (or even negative) influence of protests on coups.

Although these additional tests help support our causal argument, we have provided no panacea. The matching analyses assume confounding from observables, which may not be the case if unobserved factors are driving both protests and coup propensity. Measures like regime type and wealth are likely to capture some of these factors, but others may remain. Likewise, our fixed effect models cannot control for confounding factors that may vary over time. One final way to consider causation, therefore, is to think theoretically about the processes of interests. In spite of our robustness checks, it may still be the case that protests and coups are part of the same process. However, it is unlikely that our expectations—how *variations in* protests

influence coups based on location and violence—are wrapped up in the same process. Even the savviest of protesters is unlikely to choose protest locations based on expectations about the military's underlying willingness to stage a coup, preferring instead to protest near their homes, we suspect. Likewise, we very much agree with the idea that protesters strategically choose nonviolent tactics to encourage military defections. This is, of course, a key component of our argument. Whether this choice is made based on protester predictions about the underlying propensity for the military to stage a coup, however, we very much doubt. Protesters against Mubarak in Egypt may have refrained from violence because they suspected the military to be on the verge of a coup anyway, for example, though Mubarak's nearly thirty years of rule with military support prior to the Arab Spring uprisings would make such an expectation unlikely. Taken together, our analyses show a strong relationship between protest attributes and the likelihood of coup attempts, and our subsequent analyses further suggest a causal connection between these variables. As we discuss below, more work on the connection between protests, coups, and (perhaps) international responses is warranted before we can be assured of a causal link between protests and coups.

Conclusion and Implications

The purpose of this article was to examine and unpack the relationship between protests and coup attempts. Our theoretical framework focused on the military's disposition and ability to intervene, and how variations among protests can influence these factors. Supporting the conventional wisdom, our analyses reinforce the notion that protests increase the likelihood of coup attempts. We further showed that protests are most likely to spur coups when they are centered near the capital and nonviolent.

This work provides important implications for a variety of literatures. For coup scholarship, we provide a theoretical explanation for when protests are particularly likely to spur coup activity. We also broaden the focus beyond elites, showing how average citizens and international actors can play a role in decisions to overthrow the government. Our work also speaks to the burgeoning work on nonviolent social movements. Although this work recognizes that the military plays a role in the success of protests, how and when the military is apt to defect from the government has received less attention. We show that nonviolent movements can push the military to intervene. Future work would do well to consider how the interplay between domestic protesters and the military influences long-term outcomes, however. Following Thyne and Powell (2016), we suspect that coups inspired by domestic protests are apt to begin democratic transitions, as we saw in diverse places like Paraguay (1989) and Mali (1991). However, it is also possible for coups to hijack or undermine social movements, as may be the long-term outcome of the Arab Spring uprisings in places like Egypt.

Our effort to disaggregate protests geographically likewise proved fruitful, as we now have evidence that protests are most likely to spur coups if they are centered near the capital. This finding dovetails nicely with developments in the civil war literature, which have focused on how geographical dispersion of factors like ethnic dominance influences civil war emergence (e.g., Buhaug, Cederman, and Rød 2008; Cunningham and Weidmann 2010). We suspect that future efforts to link these literatures, perhaps by simultaneously considering how the location of protests influence multivariate outcomes (e.g., civil wars, secessionism, and coups) would be fruitful.

Finally, our study speaks to a growing literature on how international actors influence coups. Past work has shown that international actors can spur coups (e.g., Thyne 2010), and more recent work sheds light on how and why international actors respond to coups (e.g., Shannon et al. 2015). Our discussion suggests that protest-inspired coups should be less likely to draw international ire, particularly when the protests are nonviolent and challenge nondemocracies. Such coups may even then yield elections (Goemans and Marinov 2014) and democratization (Thyne and Powell 2016). However, much work remains. Our study measures neither international reactions to protests nor coups. Moreover, previous work on the interactions between external actors and coup plotters ignores domestic protests. We expect future work considering all three actors—domestic elites, the population, and external actors—at once to be particularly fruitful.

Authors' Note

Replication files and the online appendix are available at jcr.sagepub.com and www.uky.edu/~clthyne2

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Supplemental Materials

The online [appendices/data supplements/etc] are available at <http://jcr.sagepub.com/supplemental>.

Notes

1. Such policies include a 1993 US law forbidding assistance to countries “. . . whose duly elected Head of Government is deposed by military coup or decree,” and similarly strong resolutions passed by the Organization of African States (OAS; 1991), OAS (1997), and African Union (AU; 1999; 2002 AU Charter; Piccone 2004).
2. One might deem the protests in Libya to be successful given that Gadhafi was ultimately overthrown. However, in the absence of North Atlantic Treaty Organization intervention, the protests would likely have been crushed similar to those in Bahrain (Daalder and Stavridis 2012).
3. A third, more minor point of departure is the distinction between what Chenoweth and Stephan (2011) call “military defections” and how researchers commonly define coups. Laying down arms and joining the protests would be military defections in the former characterization, but they are not always coups because they are not necessarily overt and illegal attempts to unseat the executive (Powell and Thyne 2011).
4. Given that we have precise data coded at the daily level for both the dependent and independent variable, we opt for country-month as the unit of analysis to take advantage of this precision. Analyses at the country-day level produce similar findings, though the inflated number of observations artificially reduces our standard errors. We also tested the more conventional country-year approach. This also produces similar findings. However, unlike more large-scale conflicts, coups often spring up unexpectedly and terminate within hours. Using protest data aggregated at the yearly level, therefore, gives us only a minimal understanding of the independent effect that protests have on coup attempts.
5. Given that our theory speaks primarily to military coups, we also tested our hypotheses after removing eighteen nonmilitary (mostly “palace”) coups from the data set. This altered our results very little (see Online Appendix Table 1). This is unsurprising because while the military may not have played the most prominent role in these coups, all coups take place with at least the tacit support of the military. It is also possible that coups that take place amid protests will have higher success rates because they are viewed as more legitimate by domestic and international actors. Thus, we also ran our analysis on successful coups only. As shown in Online Appendix Table 2, this alternative specification of our dependent variable influences our results little.
6. We lack the space to provide a thorough explanation of the Social, Political, and Economic Event Database (SPEED) data. See Nardulli, Leetaru, and Hayes (2011) and Nardulli, Althaus, and Hayes (2015) for an excellent summaries. In addition to analyzing SPEED, we also ran our analyses using the Social Conflict Analysis Database (Salehyan et al. 2012). Although this alternative database limits our analyses both temporally (1990–2014) and geographically (Africa and Latin America only), the results are largely consistent with what we present in this article (see Online Appendix Table 3).
7. In terms of the source data, our data set include events where “INI_TYPE” equals one to focus on nongovernmental actors and cases where “EV_TYPE” equals one or two to capture expression/attacks toward the government. We also analyzed our data after removing verbal/written expressions (cases where EV_TYPE equals one only) to assure

that lower-level protests were not driving our findings. Results remain consistent with this approach (see Online Appendix Table 4).

8. In addition to this measure, we tested measures for the mean number of protesters and total protesters. Results using these approaches reveal substantively identical results. Likewise, we analyzed the number of protests logged, protesters/population, and estimations after potential outlier removal (see Online Appendix Table 5). Estimations largely remain consistent with our primary analyses. We chose to present the event count for two main reasons. First, we might expect the number of protesters to be influenced by the population of the area in which the protest arises, which is difficult to capture by specific location within each country. Second, capturing the number of protesters is exceedingly difficult, and we expect estimations to be more accurately reflected in states that have a continual media presence. In contrast, capturing whether or not a protest happened is a simpler endeavor—one that is less apt to fall prey to Western media bias.
9. The original distance measure has a mean of 323 miles ($SD = 144$). Around 25 percent of all protests happen within three miles of the capital.
10. Although three miles is admittedly an arbitrary cut point, this seems like a reasonable distance for protests to present a credible challenge to the executive. We tested several alternatives, both closer and further from the capital. Closer thresholds were generally more likely to produce a significant difference between “near” and “away” protests (see Online Appendix Figure 2).
11. From the raw data, we see weapons used in 35.5 percent of protest events.
12. Our models include few control variables to keep the analyses as simple as possible. To assure that our results are robust, we also tested models including ongoing militarized disputes (Palmer et al. 2015), military expenditures (Singer, Bremer, and Stuckey 1972), and counterbalancing (Pilster and Böhmelt 2012). Inclusion of these measures alters neither the direction nor the significance of the measures included in our analyses. All independent variables are lagged by one unit (month for the protest measures and year for the year-level control variables).
13. Effects for control variables in Figure 2 were calculated from Table 1, model 1.
14. Subsequent analyses reveal that the Americas are more coup-prone than Africa and Asia, and both the Middle East and Africa are significantly more coup-prone than Asia. Coefficients for all other possibilities show insignificant differences.
15. We omit control variables from Table 2 for presentational reasons. These findings can be produced with our replication files. Results for control variables are consistent with what we see in Table 1.
16. The matching technique produces 72,682 matched observations (13,399 with protests/treatment). This excludes around 18 percent of our observations and has L_1 statistic of 0.57. The highest L_1 statistic for the individual variables gross domestic product per capita is 0.12.
17. Models 4 to 6 (fixed effects by year) could potentially lose observations if there were a year without a coup attempt. This has not happened since 1950 (the start of the Powell/Thyne data set).

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