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# From bad to worse? How protest can foster armed conflict in autocracies ☆

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ABSTRACT

Many protest movements have brought down dictatorships and paved the way for democracy. However, protests can also foster large-scale violence at the level of civil war. How can we explain the development from protest to armed conflict? In this paper, we use geographically fine-grained data to examine how collective mobilization leads to civil war violence at the local level. We argue that two mechanisms can explain this. First, in a *protest escalation* dynamic, confrontations between protesters and state security forces increase the willingness of protesters to ramp up the use of force. Second, in a *protest capture* mechanism, protests attract attention and resources from the state, thereby providing other local non-state actors with the opportunity to use violence. We test our theoretical expectations in a spatial analysis of protests and armed conflict in autocracies from 2003 to 2014. Our results show that protests increase the risk of local armed conflict when violently repressed. Further analysis reveals that the second mechanism, protest capture, accounts for the majority of escalations to armed conflict we see in our data.

In February 2021, the military seized power in Myanmar. In the coup's wake, protests demanding the reinstatement of the democratically elected government led by Aung San Suu Kyi erupted across the country. The military junta responded with a brutal crackdown on protesters. Months after these events, it became evident that these protests had failed to reach their goal, leading many protesters to believe that the regime needed to be fought violently (Beech, 2021; McPherson & Wongcha-um, 2021). It is not the first time we have seen such a radicalization; similar dynamics were happening in Myanmar in 1988 and in many other countries around the globe. For example, the civil wars triggered by the Arab Spring uprisings in Libya and Syria in 2011 failed to bring about democracy and resulted in major international humanitarian crises. How can we explain why peaceful protests sometimes turn into armed conflict?

In this paper, we theorize and empirically investigate how protest at the local level can develop into violent conflict in autocracies. Our theoretical starting point is that protests in autocracies inherently have a high potential to lead to organized violence. Protests signal widespread disaffection with the sitting regime that may previously have been unknown. Since political change through formal channels is blocked, this new information can embolden non-state actors to use violence. Moreover, protesters have taken a significant risk by revealing their opposition to the regime and are therefore unlikely to back down, even when confronted with severe violent repression.

We posit that the development from protest to armed conflict can occur through two trajectories. The first centers on the dyadic interaction between the autocratic government and the protest movement. In this *protest escalation* mechanism, confrontations between the protesters and state security forces impact the willingness of protest participants to escalate the use of force (Della Porta, Donker, Hall, Poljarevic, & Ritter, 2017). In this trajectory, protest-repression dynamics lead radical protesters to a tactical shift from peaceful protests to organized armed conflict. The second trajectory, *protest capture*, takes into account the multi-actor dynamics that we frequently see in conflict situations (Cunningham, Bakke, Seymour, Pearlman, & Cunningham, 2012). Ongoing protests can provide other armed non-state actors with the opportunity to use violence, either when the government reallocates state capacity to protests or when armed conflict actors compete for local resources and power with protesters.

We test our theoretical expectations in a subnational analysis of protest and armed conflict in autocracies from 2003 to 2014. We use protest data from the Mass Mobilization in Autocracies Database (Weidmann & Rød, 2019, Chapter 4) and armed conflict data from the UCDP Georeferenced Event Dataset (Sundberg & Melander, 2013). The data

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we use is temporally and geographically fine-grained, allowing us to analyze the relationship between protest and armed conflict at a high spatial (grid cells) and temporal (months) resolution. Our results show that protests, when met with a violent response from the state, increase the chances of armed conflict locally, thus supporting our theoretical argument. Our empirical analysis underlines state repression as a crucial scope condition: There is no evidence to support a link between protest and armed conflict in the absence of violence against protesters. We demonstrate the robustness of our findings through alternative model specifications, placebo simulation, and sensitivity analysis.

In addition, we investigate which of the two mechanisms can better account for the relationship between protest and armed conflict. We do so in two ways. First, in a qualitative examination of the cases in our data, we find that the majority fits the second trajectory, namely that protests shape the opportunities and motivation of existing rebel groups to engage in armed conflict. Second, we argue that the different mechanisms are likely to be affected by different contextual conditions. In regions with excluded ethnic groups, where strong organizations to enforce non-violent discipline are absent, protests should primarily lead to armed conflict through the protest escalation mechanism, with (some) protesters resorting to violence. In contrast, protest capture should be more likely in poor regions, where different non-state actors compete for scarce resources, and contenders can start to fight once an opportunity opens up. The analysis shows that protest is more likely to lead to armed conflict in poor regions, but we fail to find an effect in regions with excluded ethnic groups. The results of our analysis to distinguish between the mechanisms therefore largely support the second trajectory.

This article makes several contributions to the academic literature on the spatial determinants of collective mobilization and its non-violent and violent forms. First, we theorize and provide empirical evidence for trajectories of collective action from protest to armed conflict. Peaceful and violent forms of contention are often studied separately, although there are some notable exceptions (see Leventoğlu & Metternich, 2018; Moore, 1998; Vüllers & Krtsch, 2020). However, our analysis shows they are closely related and that collective (and often non-violent) mobilization bears the risk of further military conflict. Second, we show that government responses to protests and local conditions play an essential role in this development. If governments repress protests with physical violence, they contribute to a spiral of escalation that goes well beyond the groups involved in the protest itself. Third, from a methodological perspective, we show how different types of political mobilization can be analyzed with fine-grained data, using rigorous fixed effects estimation and other robustness tests to strengthen conclusions based on a large sample. The findings we present here alert academics and decision-makers to protests with a heightened risk of deteriorating into armed conflict, a development that compromises prospects for peace and democracy.

In the next section, we define key concepts and review the pertinent literature. Second, we outline the logic of the protest escalation and protest capture trajectories. Finally, we describe our empirical strategy and discuss the results of the analysis.

## Literature review

Before reviewing the relevant literature, it is necessary to define the key concepts in our study briefly. *Protests* are public gatherings of a group of people opposing the government (Weidmann & Rød, 2019). These protests are predominantly non-violent but can involve a degree of violence (e.g. property damage, clashes with government security forces). Importantly, the protests we study are not limited to maximalist demands (overthrowing the government) but also include protests against government policies (e.g. higher wages for public officials, fuel price hikes). *Armed conflict*, in contrast, is defined as the use of armed force between two parties, one of which is the government of the state (i.e., a "state-based" armed conflict). The critical difference between protest and armed conflict is the degree of militarization and the systematic use of armed force on the side of the political opposition. While a defining feature of civil war is violence, this does not apply to political protest.

Our aim is to explain when armed conflict erupts in the wake of protests, i.e. when the two occur in close temporal sequence. As we elaborate below, this can be the result of a tactical shift from peaceful to armed means of conflict by protesters (Moore, 1998; Ryckman, 2020) or the use of military violence by other non-state actors in opposition to the government.[1]

Several academic literatures offer important insights for studying the relationship between protests and armed conflict, particularly research on state repression, social movements, and civil resistance. These strands of research offer insights into how mechanisms at the individual (emotional and cognitive processes), organizational (resources), and contextual level (opportunities) pave the way for violence during protest waves. However, to our knowledge, only a handful of studies directly look at the relationship between protests and armed conflict. Della Porta et al. (2017) outlines a broad theoretical framework for understanding how social movements can lead to civil war and offer case study evidence from four countries (Syria, Libya, Yemen, and Yugoslavia). The book stresses how armed conflict emerges from the interactions between social movement activists and government actors, particularly how violent repression leads to radicalization and escalation of conflict tactics. McAdam (1983) and Della Porta (2008) similarly view escalation from protest to large-scale political violence as a reciprocal process of adaptation.

The relationship between repression and dissent has been studied intensely (see e.g. Davenport, 2007; Demeritt, 2016; Earl, 2011, for reviews), and there is ample evidence that violent indiscriminate repression can increase dissent (Bell & Murdie, 2018; Carey, 2006; Francisco, 1995, 1996; Lichbach, 1987; Rasler, 1996; Sutton, Butcher, & Svensson, 2014), as well as the use of violence by protesters (Ives & Lewis, 2020; Moore, 1998).

However, research has also uncovered heterogeneities in the effect of repression on dissent. State repression can sometimes decrease dissent by imposing unbearable costs on dissenters (Lichbach, 1987; Pierskalla, 2010). Young (2019) studies Zimbabwe and shows that repression can lead to fear, hindering dissent. Rozenas and Zhukov (2019) finds that political opportunity structures condition the effect of repression on dissent by studying Stalin's collective punishment in Ukraine. Repression can also lead to tactical changes. Faced with repression, dissenters may perceive one form of dissent as too costly and opt for more subtle or radical dissent activities (Moore, 1998). For example, after months of unrelenting government repression of protesters in Syria, protests escalated into armed conflict. In Nepal in the 1980s, on the other hand, repression may have successfully decreased protests in the streets but triggered more subtle dissent. One of the tactics employed was "blackouts": dissenters turned off all lights in a given time period of the day to signal support for the pro-democracy movement (Schock, 2005). Some scholars have found that the relationship between repression and dissent varies over time. Rasler (1996) finds that repression suppresses dissent in the short term but increases it in the long term. Furthermore, Rozenas, Schutte, and Zhukov (2017) have shown that repression can have inter-generational effects on political behavior, whereas Finkel (2015) argues that targeted state repression over time creates "skilled resisters" who have developed a tool-kit to sustain dissent under repression. Further, it has been shown

[1] Following Pearlman and Cunningham (2012, p.3), we define a non-state actor as "an organized political actor not connected to the state, but pursuing aims that affect vital state interests".

that the severity of repression is related to the frequency and degree of violence by dissenters (Carey, 2010; Davenport, 1995).[2]

In sum, while heterogeneities exist, many studies support the notion of an upwards spiral in the use of violence between opposition groups and the state. Della Porta et al. (2017) argues that escalatory dynamics of this kind are more likely when the state is weak and lacks legitimacy, when there is strong social or ethnic fragmentation, and when military networks and resources are available. Further, in a study of how peaceful protests developed into an armed conflict in Northern Ireland, White (1989) finds that repression leads to violent rebellion because the government response was viewed as illegitimate and peaceful dissent as ineffective by a large number of people. While these arguments generally find support in the broader literature on civil war onset (Cederman, Wimmer, & Min, 2010; Hegre, Ellingsen, Gates, & Gleditsch, 2001; Rudloff & Findley, 2016), empirical tests of escalation dynamics from protest to armed conflict are few and far between. As far as we are aware, the analysis in this paper is the first to study the relationship between protest and subsequent armed conflict using a large number of countries over time.

A growing body of research on civil resistance has also studied how and when protest movements integrate violent tactics into their repertoires of contention (Tilly, 1986). It is generally acknowledged that a turn to violence harms a movement's ability to reach its goals (Chenoweth & Schock, 2015; Chenoweth & Stephan, 2011). Nonetheless, recent empirical contributions have shown that there are multiple paths to increased reliance on violent tactics. Ives and Lewis (2020) underline the importance of government repression and lack of movement organization, Gustafson (2020) looks at socio-economic factors such as food prices hikes and unemployment, Sullivan (2019) studies the effect of state coercive capacity and legitimacy, and Ryckman (2020) finds that military resources and lack of progress towards movement goals are key factors. Still, this work has focused on changes in the behavior of individual groups, and therefore does not capture escalations involving other groups that are external to the protest movement. We add this broader perspective in our theoretical framework below.

In the next section, we build on this existing research and outline two theoretical trajectories through which protest in autocracies can lead to armed conflict.

## Theory

Our starting point is that protests in autocracies carry with them a potential for armed conflict. This holds for a number of reasons. First, protests signal a lack of popular support for the regime, which may previously not have been public. This new information can create a space for existing organized armed groups seeking to capitalize on anti-regime protests. Second, a significant share of protesters in autocracies are likely to be both highly committed and skilled resisters after long periods of repressive rule (Finkel, 2015). In dictatorships, people falsify their political preferences since they run a high risk if they reveal opposition to the regime (Kuran, 1989). Protest participants, especially first movers but also many others, who publicly identify themselves as political opponents by taking to the streets, are aware that they could be the targets of future repression. As a result, a sizeable group of protesters are unlikely to back down even in the face of extreme sanctions, such as violent police repression. Third, constraints on repression is often weak in autocracies, increasing the chances of its use (Poe & Tate, 1994).[3] The use of violence is likely to increase support for the protest movement and reduce regime legitimacy. These three factors, revealing a lack of popular support, committed protesters, and autocrats with access to a weakly constrained repressive apparatus, make escalation from protest to armed conflict likely.

These reasons also imply that we do not expect similar dynamics from protest to violence in democracies. Political opposition is known and inherent to democratic political systems, and protests usually do not reveal new information about government popularity. Moreover, political protest is a regular, low-risk political activity in democracies. Protesters, therefore, do not expose themselves to high risk by joining a protest movement. Furthermore, in contrast to autocracies, citizens in democratic countries can influence politics through formal channels. Finally, repression is robustly regulated through institutional checks and balances and normative expectations in democracies, which makes a spiral of government violence and protester violence unlikely.

Our theory focuses on how protest can lead to armed conflict at the local level, identifying two possible mechanisms. For each mechanism, we discuss how protest and repression affect the motivation and opportunity for organized violence carried out by (a) protesters or (b) other existing armed groups. In the first mechanism, our focus is on dynamics within the protest movement. We argue that confrontations between protesters and the government increase the chances that radical factions of the protest movement shift from primarily peaceful to violent tactics. In the second, we argue that mass protests provide existing armed groups with the opportunity and motivation to escalate force against the government. Both trajectories lead us to expect a heightened risk of local armed conflict following protests.

In the first mechanism, which we call *protest escalation*, protesters make a tactical shift from largely peaceful tactics to organized violence. Such a tactical change indicates that many protesters believe that nonviolence has failed, and that violence is feasible and effective to achieve their goals. How can we explain a shift from peaceful protest to armed conflict? We posit that the primary mechanism relates to how violent repression of protesters redefines political opportunities for armed conflict by widening the gap between moderates and radicals. Such a violent trajectory is especially likely in autocracies since strong mobilizing institutions that can enforce united non-violent discipline are lacking (McLauchlin & Pearlman, 2012; Pearlman, 2012; Sutton et al., 2014).

Repression increases mobilization costs, making existing preferences about tactics more salient (DeNardo, 1985). Radicals are likely to respond to increased costs with calls for violence (Della Porta, 1995), while moderates prefer a steady course. Forcing a split between protesters is likely part of the aim of repression. If autocratic governments can sow disunity between protest moderates and radicals, they can cater to these divisions by offering the moderates concessions and denouncing radicals. The wider the gap between moderates and radicals becomes, the more likely it is that radicals form a separate, violent faction. In this faction, beliefs about violent tactics that seemed extreme in the early stages of mobilization now become acceptable, especially since peaceful tactics have proven ineffective (White, 1989).

Psychological processes underpin the formation of new tactical preferences. As discussed, protesters in dictatorships are aware of the risks

[2] When reviewing the literature, which in large part consists of comparative studies using observational data (like ours), it is worth noting the difficulty of isolating causal effect of repression on dissent because of endogeneity (see, e.g., Ritter & Conrad, 2016).

[3] While autocrats face several challenges related to the coercive apparatus, such as principal–agent problems (Carey & Mitchell, 2017), loyalty (Nepstad, 2013) coordination and communication problem within and between units (Greitens, 2016), and resource scarcity (Conrad & DeMeritt, 2013; DeMeritt & Young, 2013), autocratic leaders often exert significant control over the repressive apparatus. State security forces in autocracies are often created by the leader and have an official association with the regime (Geddes, Wright, Wright, & Frantz, 2018). Such forces are tied to the regime through material incentives, exploitation of communal ties, and social exclusivity (Greitens, 2016; Makara, 2013) and often replace or 'counterbalance' the regular military (Böhmelt & Clayton, 2018, 198).

associated with taking to the streets. For example, a Libyan rebel during the Arab Spring remarked that while the prospects for political reform were uncertain, "we knew that our attempts to try would be hard and bloody" (Pearlman, 2013, p. 389). Therefore, it is likely that violent repression of peaceful protests will be perceived as an illegitimate use of force, triggering emotions such as anger, disgust, and hatred. These emotions are powerful motivators of action and affect how people prioritize their interests and process information. For example, anger at the government and hatred and desire for revenge may lead citizens to rebel at the expense of going to work. Such emotions also propel people to take larger risks and attack their opponents. Attacks are not necessarily physically violent, but they nonetheless increase the risk of violent retaliation by security forces. Through violent interactions with the government, armed conflict becomes attractive.

In the civil war literature, conflicts have traditionally been conceptualized as described above: dyadic interactions between a government and an internal challenger. Our discussion highlighted how a single actor (the protest movement) could make a tactical shift from protests to armed conflict. However, civil war scholars have long recognized that multi-actor dynamics also shape conflict. Multiple groups are frequently competing against the government and potentially against each other (Cunningham et al., 2012; Dowd, 2015). This has been shown to affect civil war duration (Cunningham, 2006), violence against civilians (Wood & Kathman, 2015), alliance formation (Christia, 2012), side switching (Otto, 2018), and inter-group competition (Cunningham & Weidmann, 2010). Most of this literature emphasizes conflict intensity, but others focus on the opportunities that arise for some actors due to the involvement of others. In particular, if governments are busy dealing with a group of internal challengers, a window of opportunity can open up for competing groups to mobilize. The literature on ethnic conflict diffusion, for example, has addressed the question of the intra-state spread of ethnic conflict, where the government's dispute with one group increases the likelihood that conflict with other groups emerges (Bormann & Hammond, 2016; Lane, 2016).

We follow the literature on the multi-actor dynamics of civil war to develop the second mechanism, *protest capture*. We argue that mass protest against the regime provides opportunity and motivation for the use of military violence by other non-state groups. For example, in the Israel–Palestine conflict, several attacks by Hamas in 2021 followed protests in Jerusalem against forced expulsion of Palestinian families from their homes. In this case, the protests provided Hamas an opportunity to attack when their opponent was under pressure from other actors. At the same time, Hamas needed to mark their territory as the primary defender of Palestinians and challenger of Israel.

There are two main reasons why the risk of armed conflict with other challengers increases following protest mobilization in autocracies. The first relates to state capacity and opportunities for armed conflict. When a government is confronted with protests, it is forced to reallocate and tie up security force resources to manage the challenge. As a result, mass protests can lead other non-state groups to reassess their odds of success in a military dispute, and consequently engage in armed conflict. However, protests can also harm state capacity by lowering government legitimacy. In autocracies, the threat of violence keeps opponents of the regime from revealing their true preferences. Unbeknownst to the regime and most citizens, dissatisfaction with the regime is often widespread and small acts of resistance that reveal widespread opposition can trigger calls for revolution (Kuran, 1989). When protests provide cues about the autocratic regime's unpopularity, a space for violent mobilization is opened for non-state groups, especially if the government reacts with a repressive crackdown. Armed non-state groups can capitalize on violence against peaceful protesters by arguing that peaceful means have failed to achieve political change.

The second reason relates to how competition between non-state actors affects the motivation for armed conflict. Non-state actors are involved in dual competition, one with the government and one with other government challengers (Fjelde & Nilsson, 2012; Krause, 2013). The maximal goal is policy concessions from the government, such as free elections or the removal of the incumbent. At the same time, non-state actors pursue proximate goals in competition with each other over local resources and power, such as domestic and international support, members, funding, and media attention (Cunningham, Dahl, & Frugé, 2017). When one non-state actor publicly challenges the government, other actors' motivation to mobilize increases because it threatens their local resources and power. In our case, protests should therefore increase the chances of political violence by competing non-state actors. In sum, the discussion of the two mechanisms leads to the following hypothesis:

> Protests increase the chance of armed conflict occurrence locally, especially if the government responds to protest with violent repression.

## Research design

To test the hypothesis presented in the previous section, we use data on protest and armed conflict in dictatorships from 2003–2015. The theoretical argument requires a geographically disaggregated approach that analyzes variation in protest and violence *within* countries. While in principle, it would be possible to use administrative subdivisions as spatial units of observation, we rely on artificial grid cells to avoid problems of inter-country comparability due to different sizes of administrative units. We use the well-known PRIO-GRID with its resolution of $0.5 \times 0.5$ decimal degrees, which corresponds to about $50 \times 50$ km at the equator (Tollefsen, Strand, & Buhaug, 2012).

As most spatial analyses with static units, ours too is susceptible to the "modifiable areal unit problem" (MAUP, see for example O'Sullivan & Unwin, 2003, Ch. 2). The MAUP is caused by the fact that results from spatial analysis can be affected by the size of the spatial units it is based on. One way to address the MAUP is to run analyses with varying spatial resolution; for our analysis, however, we believe that the PRIO-GRID's resolution is well suited to capture the theoretical mechanism at work. We do not believe protest and armed conflict to occur at exactly the same place, but in close proximity. The PRIO-GRID's cells are larger than a single city, but much smaller than entire provinces or countries. In addition, the PRIO-GRID's resolution is fixed, which is why it is very difficult for us to change it. We observe the PRIO-GRID cells with a monthly resolution, assigning protest and civil war events from two other datasets to these cell-months. The fine-grained design allows us to analyze the location of protest and armed conflict within countries as well as the timing of violent escalations.

We restrict our analysis sample to grid-cell months not affected by recent civil war violence. In the main analysis, we identify these observations by using a cut-off at more than two years since fighting last occurred. The sample restriction allows for a strict test of our theoretical argument in which protest leads to civil war.[4] Since none of the included grid-cell months have ongoing conflict, our models estimate the effect of protest on local conflict *onset*. In addition, this restriction reduces endogeneity concerns. Using the entire sample of data, we would risk that the estimated effect of protest on armed conflict was merely a reflection of civil war dynamics. In such a setup, it would not be possible to estimate the causal effect of protest on armed conflict reliably. After restricting our sample to observations not affected by civil war violence, we have approximately 4.2 million grid-cell months for analysis.

For our dependent variables, we use data on armed conflict from the UCDP Georeferenced Event Dataset, version 20.1 (Högbladh, 2020;

[4] In robustness tests, we vary the analysis sample in additional ways. We use an alternative cut-off of more than one year since fighting last occurred, conduct analyses with restrictions on armed conflict violence in neighboring grid-cells, and run the analysis with all observations.

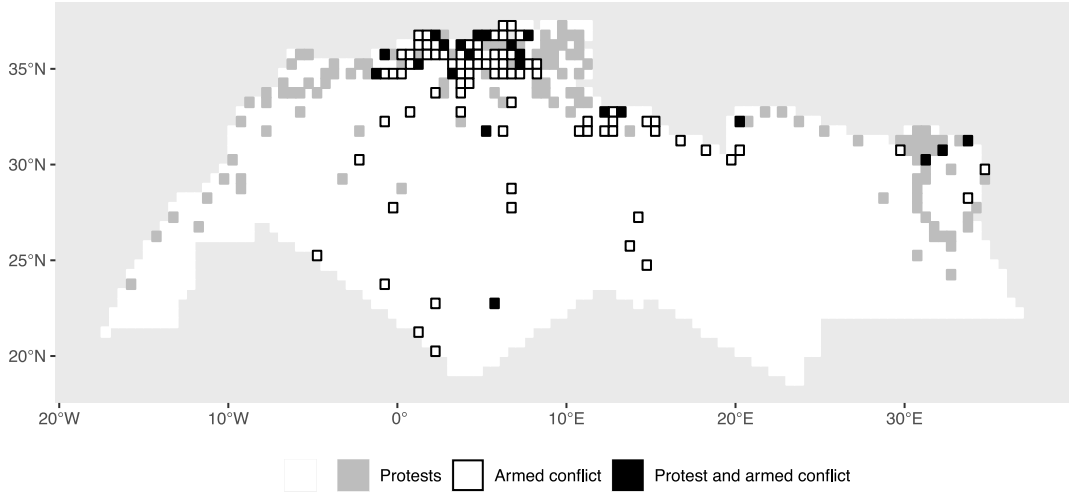


**Fig. 1.** Map of protests and armed conflict violence in Morocco, Algeria, Tunisia, Libya, and Egypt 2003–2014.

Sundberg & Melander, 2013). We restrict our analysis to those events coded as "state-based armed conflict", which is defined as "a contested incompatibility that concerns government and/or territory where the use of armed force between two parties, of which at least one is the government of a state, results in at least one battle-related deaths in a calendar year" (Högbladh, 2020, p. 28). We only use those events coded with a high level of spatial precision (level 1 or 2), since the other events cannot be assigned to grid cells with sufficient certainty. We create dependent variables from GED that measure either the number of events or the occurrence (yes/no) in a cell-month, resulting in two dependent variables. We display the results using number of events as dependent variable in the main text and the tables with occurrence in the Appendix. The choice of dependent variable does not impact the findings.

Data on our independent variables (protest) is provided by the Mass Mobilization in Autocracies Database (Weidmann & Rød, 2019, Chapter 4), which covers sub-national data on mass mobilization events in autocracies. The data is publicly available and can be accessed at https://mmadatabase.org. We use events coded as "anti-government" from Version 2 of the MMAD, which provides data on protests from 2003 to 2014.[5] The MMAD also allows us to distinguish protests met with government repression from those that were not: we categorize as repressed protests those where security forces used physical or lethal violence. Our universe of cases includes all autocratic countries as defined by the MMAD dataset. Our main independent variables *Protest* and *Repressed Protest* count the number of protest events (all events or only the repressed ones) in each grid-cell month. We lag both variables by one month.

To what extent do the events coded as armed conflict by UCDP and protest by the MMAD overlap? In our view, overlapping events in the two data sources is a minor issue. Protest and state-based armed conflict are conceptually distinct, and the differences are observable. State-based armed conflict involves organized use of armed force by rebels against the government. Protests, on the other hand, involve civilian actors and most of them do not involve violence by participants.

Fig. 1 maps data on armed conflict and protests in Morocco, Algeria, Tunisia, Libya, and Egypt. The colors indicate whether a grid-cell saw protests (gray), armed conflict (white with black outline), or both protests and armed conflict (black). The map shows that in North Africa, protest is more frequent than armed conflict violence. The same pattern emerges in the complete data set. There are 1079 conflict cases in 896 unique grid-cells in the data, and we observe almost six times as many grid-cell months with protest as with conflict violence (6256 vs. 1079). Protests are also more geographically spread out, occurring in 1464 unique grid-cells.

While protests in most places do not lead to armed conflict, Fig. 1 shows that a considerable proportion of grid-cells in the five North-African countries experience both protests and armed conflict between 2003–2014. Whereas Morocco and Tunisia primarily experience protests, we observe that protests and armed conflict oftentimes occur in the same locations in Egypt, Libya, and Algeria. In the full data set, 178 grid-cells in 32 countries experience both protest and armed conflict violence, corresponding to 12% of all grid-cell with protest.

We use OLS and linear probability models to estimate the effect of protests on armed conflict. Our most restrictive models include fixed effects for grid cells and years to account for unobserved differences between units and to net out secular trends in the occurrence of violence. We also cluster standard errors by grid cell to take into account the correlation of errors over time. Compared to many other high-resolution spatial analyses, this is a conservative estimation strategy. It eliminates underlying variation between cells and allows us to estimate within-cell effects. An advantage of this approach is that we can keep our model specification otherwise sparse since structural, static indicators are captured by the fixed effects. Nonetheless, our models also include a set of control variables that account for protest and armed conflict dynamics in time and space. First, using a spatial lag, we control whether there are protests in neighboring grid-cells since protests and armed conflict can be influenced by protest activity in other locations. Second, we also take into account armed conflict history by adding variables that capture recent intensity in the neighborhood (spatial lag) and time since fighting last occurred locally (with a set of different polynomials, see Carter & Signorino, 2010).

## Analysis

We now turn to the presentation and discussion of the empirical results. In the main analysis reported in Table 1, we look at how protest affects local armed conflict, the latter operationalized as the number of armed conflict events. The sample is restricted to observations where there was no armed conflict violence for at least 24 months. In Models 1 and 3, we cluster standard errors by grid-cell, and in Models 2 and 4, we also include grid-cell and year-fixed effects. In Models 1 and 2, we see that recent protests increase the number of conflict events and the probability of armed conflict, indicating support for our hypothesis. This result holds when controlling for the history of violence in the grid cell and the neighborhood. We also include a spatially-lagged

[5] While protest data from the MMAD is also available for later years, coverage of the current PRIO-GRID ends in 2014.

**Table 1**
Estimated effect of protest on armed conflict. Excluding observations with armed conflict in the past 2 years.

| | *Dependent variable:* | | | |
|---|---|---|---|---|
| | Armed conflict nr. events | | | |
| | (1) | (2) | (3) | (4) |
| Protest t-1 | 0.003* | 0.003* | | |
| | (0.001) | (0.001) | | |
| Repressed protest t-1 | | | 0.007* | 0.007* |
| | | | (0.003) | (0.003) |
| Not repressed protest t-1 | | | −0.0003 | −0.0002 |
| | | | (0.001) | (0.001) |
| Protest s-1 | 0.001** | 0.001** | 0.001** | 0.001** |
| | (0.0002) | (0.0002) | (0.0002) | (0.0002) |
| Armed conflict s-1 | 0.004** | 0.004** | 0.004** | 0.004** |
| | (0.001) | (0.001) | (0.001) | (0.001) |
| Time since armed conflict | −0.0002** | 0.0001** | −0.0002** | 0.0001** |
| | (0.00002) | (0.00003) | (0.00002) | (0.00003) |
| Time since armed conflict sq. | 0.00000** | −0.00000 | 0.00000** | −0.00000 |
| | (0.00000) | (0.00000) | (0.00000) | (0.00000) |
| Time since armed conflict cu. | −0.000** | 0.000 | −0.000** | 0.000 |
| | (0.000) | (0.000) | (0.000) | (0.000) |
| Constant | 0.016** | | 0.016** | |
| | (0.001) | | (0.001) | |
| Grid cell SE | Yes | Yes | Yes | Yes |
| Year FE | No | Yes | No | Yes |
| Grid cell FE | No | Yes | No | Yes |
| Observations | 4,149,980 | 4,149,980 | 4,149,980 | 4,149,980 |
| Adjusted $R^2$ | 0.007 | 0.023 | 0.007 | 0.023 |

*Note:* $^{*}p < 0.05$; $^{**}p < 0.01$; $^{***}p < [0. ***]$.

independent variable (protest in neighboring cells) to separate cell-level effects from neighborhood-level effects of protest.

When we distinguish between protests that were repressed and those that were not (Models 3 and 4 in Table 1) with the same controls as before, we obtain positive estimates for repressed protests but not for protest events without repression. In other words, the conflict-inducing effect of protests is limited in scope to cases where the government responds with violent repression. As before, the results are robust to controls that measure recent armed conflict activity in the grid cell and neighborhood. Moreover, the estimated effect of repressed protests is much larger than for all protests (Models 1 and 2): the coefficients are more than two times as large.

Substantively, the estimates indicate that for each additional 15 repressed protests, the model predicts an increase of one armed conflict event. Since armed conflict happens only rarely (1079 grid cell-months of over 4 million) and multiple events at an even lower rate (149 grid cell-months), this is a considerable effect. Our models with a dichotomous armed conflict indicator (see Table A2) provide a more intuitive substantive effect: Three repressed protest events increase the chances of armed conflict by 1%. While these numbers appear small, one should keep in mind that our analysis treats different localities (grid cells) and months in a country as separate. With protests often occurring in many places simultaneously, the effects we estimate for each of these places add up. For example, if protest increases the risk of armed conflict by 1% in *five* cells in a country, the overall risk that the country sees armed conflict somewhere increases to 5%.[6] A similar logic applies to the temporal dimensions. In short, these estimates should be considered lower bounds of protest's effect on subsequent violence.

[6] $1 - (1 - 0.01)^5$

The results support our theoretical argument, underlining that violent government responses to protests make escalation to armed conflict more likely. As discussed in the theory section, repression makes escalation more likely in both trajectories by making tactical preferences between moderates and radicals in a protest movement more salient and legitimizing armed resistance.

### *Robustness*

We conduct a series of additional tests with our main analysis to check the robustness of our findings.

*Alternative dependent variable, sample restrictions, and independent variable.* In order to ensure robustness, we make several changes to the main analysis. First, we conduct our analysis with a dichotomous dependent variable. The results, displayed in Table A2, are similar to those in the main analysis.

A second variation on our main analysis is an alternative definition of our sample. In Models 1 and 2 in Tables A3 and A4, we use 12 months since last armed conflict event to restrict the sample. Moreover, in Models 7 and 8 in Tables A3 and A4, we include all observations regardless of previous fighting. The results are robust to these changes. Further, we may be worried that controlling for conflict in neighboring cells may not be a strict enough test. Therefore, we estimate the main models on even more restrictive data samples that exclude neighboring observations with armed conflict in the past two years. The grid-cell months in this analysis are those in which no armed conflict violence occurred in the past two years, and which do not have neighboring cell months with violence in the past two and one years. The results, displayed in Models 3–6 in Tables A3 and A4, show that the main results are robust to these changes. Another potential issue involves government repression of protest elsewhere, which may affect protesters tactical considerations. To test whether repression in other

grids which is driving the results, we include a spatial lag of protest repression in our main analysis in Table A8. Results are once again unaffected by this change.

We also model the effect of cumulative protest activity in the medium term by changing our independent variable from protests in the last month to the number of protests over the past six months. The results in Models 9 and 10 in Tables A3 and A4 show that the main findings remain unchanged. Finally, we modify our protest variable to capture protests targeted at the national level. We do so because not all protests have the same risk of repression, or signal the same broad discontent with the regime. By selecting only protests aimed at the national government, potential endogeneity should be reduced. The results reported in Table A5 show that the results are robust to the change.

*Conflict history.* Despite the many sample restrictions discussed in the previous section, we may be concerned that the above results could be affected by conflict history, where protests are more likely to occur in cells with a recent history of armed conflict. While our main analysis excludes grid-cells with recent conflict and controls for conflict history, we repeat the main models while considering prior conflict in additional ways. If the outcome in our analysis is largely driven by recent civil war violence and not by protest, the effect of protest on violence we estimate in our analysis should only occur in locations with a recent history of conflict. To find out, we interact our main independent variable, protest in the grid cell, with an indicator measuring the number of months since the previous armed conflict.[7] Table A7 shows the results in regression table output. Figure B1 displays the change in the probability of armed conflict violence for increased protest activity. The results show that the effect of protest on future armed conflict is not conditional on time since armed conflict in the past. If conflict dynamics mainly drove our results, we would expect a more substantial effect on average close to fighting in the past. In sum, the results confirm that protest systematically heightens the risk of armed conflict, also in locations that have not recently seen fighting.

*Placebo simulation.* We conduct a placebo test to check whether our result could have been generated by chance. For this test, we randomly allocate protest events to cell months within a window of three years, such that the total number of simulated protest events in a cell is the same as the realized one throughout the 2003–2014 period. We generate 1,000 different Monte-Carlo datasets and run the same models as above. Figure B2 plots the distributions of the coefficients for our main independent variables obtained from these models. The plot shows the distribution for Model 2 in Table 1, where we estimated a coefficient of 0.0026 on the realized dataset. As we can see, this coefficient is highly unlikely to have been generated by chance, with the simulated data generating coefficients well below the estimated value.

*Sensitivity analysis.* We also attempt to gauge the omitted variable bias needed to overturn the main results, following the procedure laid out by Cinelli and Hazlett (2020).[8] The sensitivity analysis results are shown in Figure B3. As in the placebo simulation, we use Model 2 in Table 1 for the test.

The left plot in Figure B3 shows how the point estimate for our main protest variable in Model 2 in Table 1 would change if we included a hypothetical confounder as strong as the spatial lag of protest in the regression analysis. In the right plot, we also show how the $t$-value would change. We chose previous protest in neighboring cells as a baseline since it is a robust predictor of our models' outcome and correlated with protest. The left plot shows that a hypothetical confounder three times as strong as protest in neighboring cells would reduce the estimate from 0.0026 to 0.0022. In other words, the point estimate is likely to remain positive even in the presence of strong confounders that are possibly omitted from our model. Looking at the right plot in Figure B3, we see that statistical significance would be reduced below the 0.1 level for a confounder two-three times as strong as protests in neighboring cells. Nonetheless, given these results, we consider the evidence for our hypothesis quite robust to omitted variable bias. In sum, the sensitivity analysis provides additional confidence in the main results.

*Adjudicating between the two mechanisms*

Our two mechanisms posit that violence increases after protests but make different predictions about the actors involved. In the first trajectory, groups that protest (or parts thereof) later fight due to a tactical shift. In the second trajectory, groups that protest differ from those that fight.

We take a two-fold approach to distinguish which of the two mechanisms is at work in our data. The first, more qualitative approach is to take a closer look at the groups that resort to armed conflict violence in the aftermath of protests. In a second step, we modify our main analysis to distinguish whether the first or the second trajectory (or both) accounts for the effect of protest on armed conflict. We do so by identifying moderating conditions linked to one or the other mechanism.

In order to take a closer look at the actors, we zoom in on violent events that occur within six months of protests in our data. In this data, we find 73 unique grid-cell/armed group combinations. Sixty-six of these combinations are "new", meaning that a group has not used violence in a given grid-cells before (≥6 months ago) in our analysis period. We used the UCDP-GED actor database (Sundberg & Melander, 2013) and the Foundations Of Rebel Group Emergence (FORGE) dataset (Braithwaite & Cunningham, 2020) as sources to identify the armed groups.

Half of the violence in the data is carried out by armed groups founded two or more years before the protest events, such as the al-Qaida Organization in the Islamic Maghreb (AQIM) and Hamas. These observations best fit the protest capture mechanism, in which protest creates motivation and opportunity for existing groups to use violence in a locality. Newer groups such as the FLRN (The National Liberation and Reconstruction Front) in Haiti, the National Congress for the Defence of the People (CNDP) in DRC, and the Sudan People's Liberation Movement-in-Opposition (SPLM/A-IO) also fit this mechanism.

Only two groups in the grid-cell/armed group combinations can be attributed to the protest escalation mechanism: The National Transitional Council of Libya (NTC) and "Syrian insurgents". As protests mounted against the Syrian dictatorship in 2011, many observers described how massive government repression led protesters to engage in armed conflict (for example Lynch, 2013). For example, in the city of Jisr ash-Shughur, mass protests had been organized for months, resulting in the death of many civilians. In June 2011, dissenters entered the local police station and seized weapons. The opposition also set fire to a building which caused the death of several members of the Syrian security forces. These events led to the defection of security force members that joined the emergent armed uprising against the Syrian government. The NTC emerged under similar conditions in Libya in 2011. It was formed by a former Minister of Justice, Mustafa Abdul Jalil, as a political umbrella organization working for a revolution in Libya. The armed wing included several loosely organized armed groups that coordinated their efforts under NTC to oust Gaddafi.

In sum, our investigation of the actors reveals that most local instances of armed conflict in our data can be attributed to the protest capture mechanism. The main road from protests to armed conflict in autocracies is thus paved by changing opportunities and motivation for existing rebel groups to engage in violence. Nonetheless, we also find a

[7] The indicator counts the number of months since armed conflict in a cell. Cells that do not see any armed conflict violence or have not experienced conflict up until a certain point are counted as not having armed conflict since January 1, 1989, which is the starting date for the UCDP GED data.

[8] We rely on the `sensmakr` package (Cinelli, Ferwerda, & Hazlett, 2020).

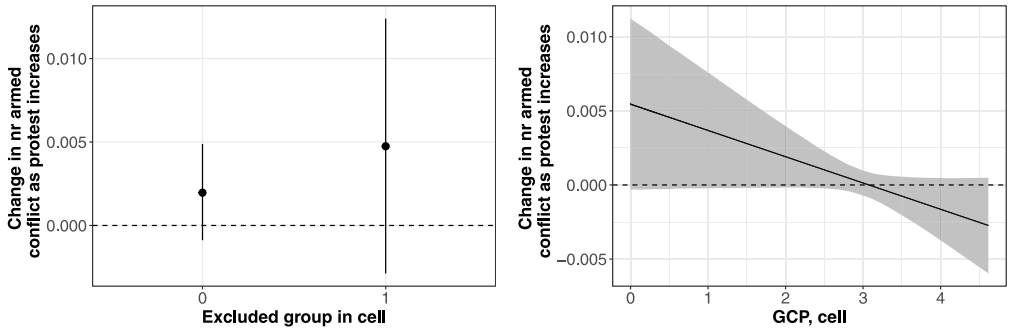


**Fig. 2.** First difference plot: The effect of protest conditional on excluded groups (left) and wealth (right).

few cases that fit our first mechanism in which protesters form armed groups to fight dictators.

A modification of our main analysis where condition on local circumstances can shed further light on the dominant mechanism. We consider two local factors here: economic development and ethnic exclusion. Regions with low development levels and politically excluded groups provide fertile ground for rebellion because locals may harbor grievances against the government for political exclusion and poor living conditions. We are interested in whether these local conditions also make one or the other trajectory from protest to violence more likely, or in other words if they moderate the escalation effect we have posited above.

Consider regions with politically marginalized ethnic groups, i.e., those excluded from political power at the central government. A rich literature has shown that ethnic exclusion increases the risk of armed conflict (see for example Cederman et al., 2010). However, ethnic exclusion can also provide a fertile ground for protest to escalate to military violence. Ethnic groups that are politically represented have access to more resources to build strong organizations, a critical factor in enforcing non-violent discipline among protesters (Sutton et al., 2014). As discussed in the protest escalation mechanism, such discipline reduces the probability of extremist factions resorting to violence. On the other hand, in regions with excluded groups, organizations should be weaker. If the protest escalation mechanism is at work, then protests in areas with excluded ethnic groups should have a higher probability of protest escalation due to lower non-violent discipline.

Further, consider the local level of development. Some research has shown that local development reduces the risk of conflict, a relationship that has been shown both with spatial estimates (Buhaug, Gleditsch, Holtermann, Østby, & Tollefsen, 2011), but also with survey data (Tollefsen, 2020). We believe that the multi-actor dynamics we discussed concerning our protest capture mechanism are particularly important in these regions. Recall that one of the reasons why protests can escalate to armed conflict is that armed challenger groups can compete with the protest movement. Suppose ongoing protests now attract fungible resources (e.g., supporters, funds from local businesses). In that case, this comes at the expense of these external actors, which in turn may be compelled to start fighting. Competition between different internal challengers will be much more severe in regions with few resources and low levels of economic development. If resources are scarce, competition and the likelihood of protests escalating to armed conflict should increase.

We test the impact of these local circumstances by using additional models that capture the contexts where protest leads to armed conflict. We re-estimate the main models but interact our main predictors with the different moderator variables. The interactions help us illustrate the conditions under which the effect of protest on violent conflict is particularly pronounced to better distinguish between our two mechanisms. We condition the effect of protest first on ethnic exclusion and then on local economic development.

Data on the conditional variables are from PRIO-GRID (Tollefsen et al., 2012), which integrates data from the Ethnic Power Relations Data (EPR Vogt et al., 2015) and the G-Econ data set (Nordhaus, 2006). *Ethnic exclusion* is a dichotomous indicator taking the value one if there is at least one excluded ethnic group in a grid-cell. Ethnic exclusion is frequent in the data. Approximately 50% of the observations are coded as having at least one excluded ethnic group. Our measure of local economic development, *GCP* is the logged gross cell product (GCP), measured in USD using purchasing power parity. The variable has a strong right skew, indicating that most of the grid-cells in our analysis have a low level of economic development. The log-transformed variable takes vales between 0 and 4.6, and only 5% of grid-cells takes values higher than 1.

First, we test whether our main effect is more pronounced for locations with excluded ethnic groups. The left plot in Fig. 2 shows the marginal effect of protest on subsequent violence, conditional on whether an excluded ethnic group is present in the grid cell (full results in the Appendix in Models 1 and 2 in Table A6). We do not find evidence for our expectations in this analysis: protest is not more likely to induce armed conflict in locations with excluded ethnic groups. If our theoretical assumption is correct, the protest escalation mechanism implies that exclusion has a positive interaction effect; failing to find this effect means that there is no empirical support for it.[9]

Second, we include the level of economic development as a moderator in our analysis. The right plot in Fig. 2 shows the marginal effect plots from the regression results in Appendix in Models 3 and 4, Table A6. Here, we see evidence that poorer regions are more likely to see an effect of protest on armed conflict: the effect is positive for low values of GCP. Moreover, we see that the effect vanishes as economic development increases. In line with our theoretical argument, the result indicates that sustained peaceful protest, without escalation to violent armed conflict, is more effective and feasible in affluent than in poor locations, which we interpret as support for the protest capture mechanism. People in richer areas have access to material and social capital that reduces the risk that they take to arms.

## Conclusion

Popular uprisings are frequently – and rightfully – hailed as drivers of democratization (Rød, Knutsen, & Hegre, 2020). But successful uprisings do not always bring about benign regime change (Chenoweth & Schock, 2015). On the contrary, dictators pushed out of office by protests are often replaced by new dictators, and popular uprisings at times pave the way for armed rebellion. To improve our understanding

[9] One caveat worth mentioning is that the results could be affected by the spatial resolution of our study. Excluded ethnic groups are likely located in peripheral regions of a country while impactful protests often take place in central regions. It is possible that protests in peripheral regions where excluded ethnic groups reside are of a type that are unlikely to lead to escalation altogether. In the Table A9, we try to address this concern by seeing whether interacting ethnicity with the spatial lag of protest affects our results. The results do not change, which mitigates this concern.

of how protests can successfully bring about democratization, we also need knowledge about how they can fail. Failed popular uprisings can be devastating. For example, ten years after the Arab Spring, armed conflicts in Libya, Syria, and Yemen have led to approximately 400,000 deaths, according to numbers from the UCDP GED (Högbladh, 2020; Sundberg & Melander, 2013).

How can we explain the development of largely peaceful protest into armed conflict? In this paper, we have studied the relationship between protest and armed conflict in autocracies. We argue that protests in dictatorships carry with them an inherent potential for organized violence. Political repression keeps opponents silent and popular support for regimes is therefore frequently assumed to be high. Protest outbreaks reveal that this is not the case: since protesters run a high risk by taking to the streets, they signal radical discontent with the regime that has latent potential for violent escalation.

We investigate two mechanisms of how protest can turn into armed conflict. First, in a *protest escalation* dynamic, we propose that violent interactions between protesters and government security forces can lead to a tactical shift from peaceful to violent dissent. In the face of repression, protest movement members will over time conclude that nonviolence has failed and that violence is the way forward. In a second *protest capture* trajectory, we argue that protests can pave the way for organized violence by other actors by tying up state resources, legitimizing violence against the state, and triggering competition over local resources between non-state actors. Our empirical analysis supports the hypothesis that armed conflict is more likely to occur in the wake of protest, but only when violent state repression is used. Overall, our analysis reveals that protest capture is more common than escalation. Our empirical evidence thus suggests that protests can lead to armed conflict by creating space and incentives for existing armed groups to fight the government. As far as we know, the article offers the first large-N, systematic analysis of the relationship between protest and armed conflict at the subnational level.

These findings have implications for policy-makers and democracy activists worldwide. The most evident implication of our research is that the risk of escalation from protest to civil war can be reduced by avoiding violent clashes between state security forces and protesters. However, powerful systemic incentives for violence against political opponents exist in autocracies. In particular, dictators often create paramilitary groups that have much to lose from regime change (De Bruin, 2021; Greitens, 2016). Recruitment to these groups is often based on loyalty to the regime, thereby reinforcing identity politics. A reduction in violent repression is, therefore, more likely to be achieved through democratization. Our finding that the main path from protest to armed conflict relates to existing rebel groups also points to democratization as a way forward. The results reinforces the urgency with which such groups should be demobilized and enabled to influence politics through conventional channels. Another implication of our research is that local economic development can reduce the chances of escalation to armed conflict. The risk of civil war is higher in poor regions, but poverty also increases the chances that protest turns into a violent confrontation. Therefore, investment in material and social capital can reduce the risk that peaceful collective mobilization can have dangerous consequences.

## Data availability

Data and syntax for the main analysis is available at https://github.com/espengroed/how-protest-can-foster-armed-conflict-in-autocracies.

## Appendix A. Supplementary data

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.polgeo.2023.102891.

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