Repression and Backlash Protests: Why Leader Arrests Backfire

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This study investigates how different targets of state-sanctioned arrests shape the likelihood of collective action. We hypothesize that leader arrests are especially likely to result in backlash protests. Leader arrests symbolize the suppression of social collectives, they create collective grievances, and constitute focal points for mobilization. Building on a global sample of arrests of cultural identity group members, we qualitatively traced for each arrest whether it sparked a backlash protest. Drawing on coarsened exact matched models, we find that protests are significantly more likely following leader arrests. In contrast, mass arrests are not significantly linked to backlash protests. Additional tests show that organizational membership does not drive this findings, whereas the symbolic value of leaders is linked to protest outbreaks. Our findings cast doubt on the narrow focus on quasi-constant structural variables and make the case for the disaggregation of repression and the importance of triggering events.

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1 Introduction

Within hours, the arrest of Abdullah Öcalan, leader of the Kurdistan Worker's party (PKK), sparked a massive global protest wave by Kurds and the Kurdish diaspora in February 1999 (CNN, 1999; Guardian, 1999). Arrests of Gultan Kisanak, co-mayor of the Kurdish city of Diyarbakir, and Carles Puigdemont, former leader of the Catalan independence movement, evoked similar reactions, with thousands taking to the streets to protest. *Why do arrests of ethnic group leaders provoke backlash protest?*

Protest activity that erupts in the aftermath of government repression is a puzzling phenomenon, given the acute risk of a coercive response. The rich literature on the repression-protest nexus shows, however, that spontaneous and conflictive counter-reactions to government repression are not rare phenomena (Aytaç, Schiumerini, and Stokes, 2018; Carey, 2006; Rasler, 1996). But while there is a substantial body of research on structural factors that facilitate opposition mobilization (Anisin, 2016; Cederman, Weidmann, and K. S. Gleditsch, 2011; Collier and Hoeffler, 2004), we know comparatively little about which types of repression are associated with deterrence or incitement of backlash protests. In this article, we make the case that different forms of the same instrument of repression can have distinct effects on the likelihood of protest outbreaks. Our focus is on arrests, which may be indiscriminate and target a large number of ordinary group members or be more selective and target leaders. Our research investigates whether and to what extent these different types of arrests are associated with different propensities for backlash protests. Our focus is on ethnic, tribal, and religious groups, which face harsh government repression in many multi-ethnic societies, often in the form of deprivation of personal liberty (UNPO, 2016; CFR, 2021).

Inspired by the anecdotal evidence cited above, we expect that the arrest of ethnic group leaders elicits impulsive and conflicting reactions from affected cultural identity groups. Leader arrests symbolize the suppression of the social collective and are likely to be perceived as a direct

assault on cultural identity. Group members tend to identify with the fate of their leaders, resulting in collective perceptions of injustice or group-based grievances. Given that grievances are shared among group members who are connected to their leaders through mutual expectations of solidarity, we argue that leader arrests create a focal point for collective mobilization. Mass arrests, in contrast, are a less selective form of repression and decrease the likelihood of mobilization. Mass arrests impose costs on the group members themselves and make individuals suddenly and directly aware that they can be next. Such individual risk calculations could explain why the deterrent effect tends to prevail over the inciting effect in the case of mass arrests.

We explore two potential mechanisms through which leader arrest may spark backlash protests. A first perspective contends that decision-making infused by heightened collective emotions prompts people to respond to state repression (Pearlman, 2013, p. 388). Leader arrests are sudden and unexpected triggers of grievances, which are likely to provoke a collective emotional reaction and constitute focal points for collective mobilization. This is especially likely when those arrested hold significant symbolic value for group members and their cultural identity. While this perspective focuses on the nature of the triggering event, it places less emphasis on the role of organizations and pre-existing mobilization networks. This is contested by a second perspective which stresses the pivotal mobilizing role of organizations, resources, and communication channels (Sutton, Butcher, and Svensson, 2014; McAdam, 1999; Cunningham, 2013). Backlash protests emerge from pre-existing movement infrastructure through which bystanders are persuaded to join protests, perceived costs of participation are reduced, and coordination problems solved. What matters for backlash protests are organizational structures, which facilitate collective actions in response to the arrest of group leaders.

We test our theoretical arguments with a global sample of arrests targeting members of cultural identity groups between January 2016 and March 2021. We analyse which types of arrests incited backlash protests, accounting for trigger-specific and time-variant structural ex-

planations for backlash protests. Drawing on coarsened exact matching, cross-group comparisons, and within-group estimators, we find that backlash protests are significantly more likely following the arrest of group leaders. The inciting effect of leader arrests holds when we account for a variety of time-varying and structural variables that capture explanations related to socio-economic grievances and opportunity costs. In contrast, mass arrests tend to have a negative effect on the likelihood of backlash protests. While the negative effect of mass arrests is not statistically significant across all model specifications, our results suggest that mass arrests are unlikely to incite immediate group-based mobilization. Building on newly coded data on leaders' membership in formal organizations as well as their symbolic role in social collectives, our analyses show that leaders' organizational membership does not drive the findings, whereas arrests of highly symbolic leaders appear to increase the likelihood of a backlash protest.

Our article innovates on three grounds. First, building on the literature on triggering events, collective emotions, and grievances (Hess and Martin, 2006; Jasper, 1997; Pearlman, 2018; Simmons, 2014; Snow and Moss, 2014), we present a novel explanation for the divergent effects of leader arrests and mass arrests on backlash protests. Despite extensive research, there have been few efforts to conceptualize and measure repression in a more disaggregated manner, allowing researchers to assess which types of repressive actions demobilize protesters, and which end up causing the opposite (Earl, Soule, and McCarthy, 2003; Castro, 2022; Demirel-Pegg and Rasler, 2021). Second, we advance research on backlash protests by introducing a time-sensitive measure of backlash protests. In contrast to prior studies (e.g. Muller and Weede, 1990; Rozenas and Y. Zhukov, 2019), we code a protest only as backlash protest if there is explicit qualitative evidence suggesting that protests were triggered by a prior arrest. In so doing, we provide a stronger causal link between repression and protests relying on in-depth qualitative evidence on the substantive motivations of protests. Third, we respond to the demand for "more dynamic measures of some concepts" in the backlash literature (Chenoweth and Ulfelder, 2017, p. 22).

We measure structural grievances with temporally disaggregated variables that vary on a daily level, allowing us to empirically disentangle structural and trigger-based explanations of backlash protests.

2 State repression, grievances, and backlash protests

Conflictive responses by cultural identity groups to government repression are frequently explained by the severity of grievances resulting from political exclusion or socio-economic deprivation (Gurr, 2015 [1970]; Cederman, K. S. Gleditsch, and Buhaug, 2013). From the perspective of grievance-based approaches, protest behavior reflects the accumulation of grievances (Rudolfsen, 2021; Acemoglu and Robinson, 2006). Others have argued that grievances do not directly translate into conflictive mass behavior (e.g. Collier and Hoeffler, 2004). This criticism has usually revolved around the "consistency of discontent" argument (McAdam, McCarthy, and Zald, 1988) and the observation that "grievances abound while protest does not" (Mueller, 2000; Collier and Hoeffler, 2004; Van Stekelenburg and Klandermans, 2013, p. 889).

Opportunity-based approaches emphasize that the context in which a dissident movement operates is crucial to understand collective behavior (Meyer, 2004; Almeida, 2003; Slater, 2009). Levels of repression, organizational capacities and movement infrastructures, as well as framing and coordination skills by movement elites are key factors to explain backlash protests (McAdam, 1996; Anisin, 2016; Goldstone and Tilly, 2001; Rozenas and Y. Zhukov, 2019; Ives and Lewis, 2020). For instance, scholars have examined how levels of state repression affect protest sizes and frequencies, with some detecting a non-linear relationship: increasing repression levels increase protest levels only up to a certain threshold above which collective action becomes too costly (Gurr, 2015 [1970]; Muller and Weede, 1990; Lyall, 2009; Sullivan, Loyle, and Davenport, 2012; Shadmehr, 2014). Other scholars highlight the pivotal role of movement capacities and resource mobilization and show, for example, if opposition movements are cracked down before they can

organize themselves, backlash protest is unlikely to occur (Inata, 2021; De Jaegher and Hoyer, 2019; Aktan, 2022). Other prominent studies argue that repressive events convey true information on the government's intentions and weaknesses, which in turn motivates activists and shifts bystanders' beliefs on the effectiveness of the movement (Opp and Roehl, 1990; Khawaja, 1993; Lohmann, 1994).

We build our study on the argument by Snow and Soule (2010, p. 23) that "none of the various sets of conditions (...) is more important than the generation of deeply felt shared grievances" (Simmons, 2014; Snow and Soule, 2010, p. 23). However, we observe an incongruence between quasi-constant grievances stemming from political or socio-economic exclusion of ethnic, tribal or religious groups and spontaneously arising conflictive mass behavior in the context of state repression. From our perspective, this observed incongruence does not mean that grievances are irrelevant. It should rather direct our attention to short-term "incidental" grievances (Walsh, 1981; Bray, Shriver, and Adams, 2019; Hechter, Pfaff, and Underwood, 2016). Social movement scholars have highlighted that grievances are time and situation sensitive, meaning and emotion laden, which matters for explaining if, when and how mobilization occurs (Zomeren, Postmes, and Spears, 2008; Bergstrand, 2014; Simmons, 2014).

Grievances are powerful mobilizing factors when they occur suddenly and unexpectedly, representing "moral shocks" that move individuals emotionally and may spark eruptive collective action: "grievances are only grievances *because* they upset or outrage people" (Walsh, 1981; Jasper, 1997, p. 126).[1] According to this perspective, without a disruptive incident the protest that followed would not have happened in the way it did and at that particular point in time it occurred (Straus, 2015, p. 7). Triggering events crystallize emotions, prompting a certain state of action readiness in social collectives, which "tends to override other concerns, other goals, and other actions" (Polletta and Amenta, 2001; Gould, 2009; Frijda, 2007, p. 16). The concept of

[1] Emphasis by the authors.

repressive actions as triggering events has rarely been applied to tackle the "punishment puzzle" (Davenport, 2007, p. 8). Some authors have used the idea of disruptive events to explain the outbreak of revolutionary cascades or mass atrocities (Kuran, 1989; Straus, 2015; Bruun, 2013). From this angle, an act of repression is not simply an "accidental ingredient in [a] fire-prone situation" (Kimmel, 1990, p. 10), but an incident that has causal power itself. The event *itself* is felt by those affected to be sufficiently serious to warrant a prompt reaction.

Inspired by these studies, we take a distinctly processual perspective on the repression-conflict nexus and turn toward the idea of triggering events as proximate causes of collective action (Hess and Martin, 2006; Verhulst and Walgrave, 2009; Bruun, 2013). Arrests trigger backlash protests and work as critical catalysts for mobilization if such repressive events crystallize collective grievances (Jasper, 1997). Conceptualizing repressive actions as triggers, point-like occurrences in a confined space and time frame, allows us to distinguish different types of repression and to systematically analyze whether repressive events have backfired or not. As previous explanations of backlash protests have focused mostly on characteristics of the movement itself (Goldstone and Tilly, 2001), rather than on the characteristics of repressive events, our study makes several theoretical and empirical contributions to this body of research (Carey, 2006; Costalli and Ruggeri, 2015; Lohmann, 1994; Muller and Weede, 1990; Pearlman, 2013; Rozenas and Y. Zhukov, 2019).

In the next section, we develop a trigger-specific explanation of backlash protests in response to state repression. We explain why especially leader arrests tend to spark counter-protests, while mass arrests do not increase the likelihood of backlashes.

3 Towards a trigger-specific explanation of backlash protests

While state repression may either deter or incite dissent by simultaneously creating fear and anger (e.g. Aytaç, Schiumerini, and Stokes, 2018), we argue that the latter effect dominates in the case

of leader arrests. We define leader arrests as acts of deprivation of liberty that are sanctioned by state actors such as the police or the military and that target individuals perceived as leaders by identity-based groups. Leadership is regarded as a socially constructed role that does not need to be formally recognized. The defining criteria are that individuals are widely considered as leaders within the respective ethnic or religious group and that they exercise some degree of authority within these groups (Bob and Nepstad, 2007). This includes leaders of organizations that make claims for ethnic or religious groups such as politicians or movement activists but also ethnic group leaders who are not associated with any organization, such as intellectuals, shamans or preachers in some cases.

Leader arrests represent an especially salient type of repression in multi-ethnic societies that encapsulate the grievances of identity-based groups in a single dramatic event, creating a "moral shock" (Jasper, 2018, p. 87). For instance, hours after the arrest of Carles Puigdemont in March 2018, pro-independence supporters blocked the streets and fought street battles with riot police, "angered that they arrested Puigdemont [as] he is *our* highest representative", one protester is quoted (France24, 2018).[2] The arrest of a leader symbolizes the suppression of the entire group that is represented by the leader. To the extent that individuals identify with the respective group, the arrest of *their* leader may be perceived as an attack on their personal identity. Consequently, a leader arrest creates an indirect experience of repression, a sentiment of being targeted by proxy, which sparks widespread anger and indignation among ethnic group members. Put succinctly, "whenever a leader is removed it makes a larger number of people angry" (Siegel, 2011, p. 1005).

As a consequence, high-profile leader arrests provide a focal point around which citizens can organize (Esberg, 2021). Given that the risk of repression tends to be inversely related to the number of protest participants, citizens prefer to go to the streets when others do the same

[2]Emphasis by the authors.

(Kuran, 1997). However, citizens lack complete information about the intentions of others facing a coordination problem. Focal points can solve such coordination problems based on the mutual expectation of congruent actions (Schelling, 1980). Arrests of ethnic leaders such as Abdullah Öcalan, Gultan Kisanak, or Carles Puigdemont are highly salient events, making it plausible that group members anticipate that other group members will likewise be aware of them and feel similarly aggrieved and emotionally attached. Hence, citizen have a certain degree of insurance that others will perceive the situation in a similar light and act similarly. Overall, this lowers the hurdle to collective action compared to low-profile arrests that lack a coordination incentive.

Note that similar arguments could be made about other physical integrity rights violations directed against leaders such as assassinations or deportations. While we remain agnostic about the specific effect of other forms of repression, we argue that there is an element of reversibility that makes backlash protests especially plausible in response to leader arrests. In other words, detention is a reversible act, given that individuals can be released. This provides protests against leader arrests with a worthwhile and potentially realistic goal. In light of the empirical examples and our theoretical claims, we contend that leader arrests may be especially likely to trigger backlash protests. Hence, we derive the following hypothesis:

H_1: Leader arrests increase the likelihood of backlash protests.

Previous research suggests that the deterrent effect of repressive events increases when the certainty of punishment for individuals is higher (Ross and LaFree, 1986; Earl and Beyer, 2014; Shultziner and Goldberg, 2019). From this perspective, mass arrests are qualitatively different from leader arrests. In contrast to highly selective leader arrests, indiscriminate forms of repression target ordinary group members and impose costs on the group members themselves. To the extent that group members anticipate that they too may be arrested, they may place less value on the survival of the collective and their social identity than on their personal well-being

and safety. Indiscriminate arrests make individuals suddenly and directly aware that they can be next. This awareness of personal victimization is quite distinct from the more abstract identification with an ethnic group leader and his or her arrest. We expect this to give rise to stronger dispiriting emotions such as anxiety, fear, and sadness. When such emotions predominate within a social collective, the resulting action tendencies are obedience, inaction, or apathy (Pearlman, 2013). Research shows that the expectation of repression makes people more pessimistic about their personal risks and chances that others support their efforts (Johnson and Tversky, 1983; Lerner and Keltner, 2001; Druckman and McDermott, 2008; Y. M. Zhukov and Talibova, 2018; Young, 2019). For instance, when Turkish security forces launched the crackdown on the Kurdish community in the Diyarbakir region in the wake of the failed 2016 coup, many Kurds fled to the mountains for fear of imprisonment (Nordland, 2016).

However, this does not mean that mass arrests do not arouse any anger, resentment, and the motivation to protest. "If we were able to hold a free rally without pepper gas and all, we would have a million people", a Kurdish lawmaker is cited in the context of the purges (ibid., 2016, n.p.). Mass arrests and indiscriminate arrests of ordinary group members can be just as disruptive and shocking as leader arrests. However, the individual risk calculus includes weighing the expressive and instrumental benefits of protest against the expected costs of dissent. The latter might be regarded as a function of the severity and probability of negative sanctions that an individual might face (Young, 2019). Given that individuals anticipate that such sanctions tend to be significantly higher when their ethnic, tribal or religious group is indiscriminately targeted, we expect the repressive "net effect" to shift toward deterrence (Earl and Beyer, 2014).

Even if some individuals feel the urge to protest, we expect that overall *more* members in the collective are deterred than mobilized. While the simultaneous countervailing effects of incitement and deterrence apply both to mass arrests and leader arrests, we argue that mass arrests do not provide a comparable focal point for conflictive mass behaviour. Mass arrests lead

to behavioural challenges within the collective, as other group members may elect to step out to avoid their own arrest. When governments arrest large numbers of people, they demonstrate that they are willing and capable of implementing large-scale and indiscriminate repression. The high number of arrested people signals a high risk of participation in a backlash protest that not all group members are willing to accept in the same way (Sullivan and Davenport, 2017). Based on these arguments, we derive the following hypothesis:

H_2: Mass arrests decrease the likelihood of backlash protests.

4 Research design

To investigate whether leader arrests and/or mass arrests trigger backlash protests, we employ a research design that allows us to hold time-invariant structural factors constant. We study the likelihood of backlash protests in response to repeated arrests against members of the same identity groups within the same country. Why did the arrest of a group member at time point t spark a backlash protest while no backlash protest occurred after the arrest at time point *t+1*? Given that we compare responses to repeated arrests within the same political contexts over time, we aim to isolate time-variant and trigger-specific explanations of backlash protests. In the following sections, we first describe our case selection procedure, and subsequently introduce our dependent variable, and the key explanatory variables.

4.1 Case selection

Given that we are interested in collective responses to arrests, our sample is defined by the presence of arrests. We limit our sample to arrests that are directed against members of ethnic, tribal and religious groups, which are covered by the label of cultural identity groups.[3]

[3]We show a complete list of all cultural identity groups included in this study in the appendix.

Not only are arrests a frequent form of repression in multi-ethnic states, but given the shared cultural identity of targets and bystanders, they are particularly likely to trigger group-based protest activity. While the internal cohesion of cultural identity groups and their differentiation from others is the result of social construction, this does not mean that ethnic and religious identities are merely fleeting and easily malleable by elites (Gurr, 2000). Cultural identity groups, such as the Kurds in Turkey or the Mapuche in Chile are bound together by group loyalties, i.e., affective commitments that tend to persist (Jasper, 2011). Collective identities arising from overlapping or superimposed differences of religion, language, or descent are relatively fixed and not freely eligible due to their fairly high stability over time and the stabilizing effects they have on individuals' experience of life's contingency (Chandra, 2012; Hale, 2004). Cultural identity groups are collectives that may be regarded as indispensable because members are permanently and emotionally bound to them. As disputes affecting a cultural collectives identity strike at the very heart of the group members' sense of self, they are emotionally charged (Tajfel and Turner, 1997; Petersen, 2002). Hence, arrests as repressive acts against group members or leaders representing the group are easily perceived as moral shock, sparking collective counter-reactions against the attacker.

By confining our sample to arrests against members of cultural identity groups, we enhance the homogeneity of the studied cases. To construct our sample, we build on event-based data provided by the Armed Conflict Location and Event Dataset (ACLED) (Raleigh, Linke, et al., 2010; ACLED, 2021). We use the *arrest*-variable that is coded as subcategory of the *strategic development*-variable. We filter the subset of arrests that took place between January 2016 and March 2021 and that were directed against members of ethnic, tribal and religious groups.[4] Information on the targets of the arrests is taken from ACLED's actor specifications (*actor*, *assoc_actor*). The advantage of ACLED is that it provides systematic information on cultural

[4]We use this recent observation period in light of the fact that the quality of event-based data improved substantially over time (Fariss, 2019). Prior 2016 the coverage of arrest cases tends to be insufficient.

identity collectives by tagging them as ethnic, communal, indigenous, tribal, religious, caste, clan, or sub-clan groups. Organized actors that are not primarily or explicitly cultural, such as political parties, unions, or militias, are excluded based on a case-based assessment. In total, we derive a sample of 428 arrest events from 49 countries.[5] The sample covers 117 different cultural identity groups, which were targeted to different degrees. The identity groups with the highest numbers of arrest events in the sample are the Malay Muslim group in Thailand (61), the Kurdish ethnic group in Turkey (55), and the Oromo ethnic group in Ethiopia (25).

4.2 Dependent variable

Our dependent variable captures whether a backlash protest took place after a given arrest. We define a backlash protest as an *immediate, collective and conflictive reaction* that (a) occurred in a time period of no longer than seven days after an arrest, and (b) that was directly triggered by a given arrest. If a backlash protest occurred, we code the dichotomous variable as 1 and otherwise as 0. The period of seven days was chosen in order to exclude protests organized in the long term, while still taking into account that mobilization might require a certain time.

To operationalize backlash protests, we build on *protest* and *riot* events in ACLED in combination with an in-depth qualitative assessment.[6] In a first step, we coded whether a protest or a riot event took place in a time period of one week after a given arrest. In a second step, we carefully checked for each arrest case whether there is evidence that the protest events were directly sparked by a specific arrest. We studied newspaper articles on each individual protest event using LexisNexis and GoogleNews in combination with events descriptions provided by ACLED. For instance, according to the Times Singapore "Myanmar nationals staged a series

[5]Note that more than one individual can be arrested in a given arrest event.

[6]We are aware of the fact that event-based datasets suffer from systematic reporting biases, covering only a non-random subset of the true population (Gohdes and Price, 2013). In this study, these problems are alleviated by the fact that low-profile arrests that do not make it into the media, and thus reporting by ACLED, are unlikely to trigger large-scale protests. In addition, groups targeted with arrests might be systematically different from non-arrest-targeted groups.

of demonstrations [...] to protest against the Singapore Government's arrest [...] of leaders of the Arakan Association" in July 2019 (Yong, 2019). Similarly, according to the ACLED events description "a large number of the population close to the royal family of Gan vandalized two protestant churches in Loropeni following the arrest of the Gan traditional king (...)" (ACLED, 2021).

We code protest events only as backlashes if we traced a piece of evidence that states explicitly that a protest event was *directly* triggered by a given arrest. To the extent that the newspaper articles correctly assess the driving motivation of a protest event, this qualitative procedure allows us to causally link backlash protests to arrests.

In Figure 1, we illustrate the share of arrests that sparked backlash protests (red dot) to the share of arrests without counter-protests (gray dot) by country, sorted by regime type.[7]

[7]We use the V-DEM variable *v2x_regime* and values for 2018 to classify political regimes.

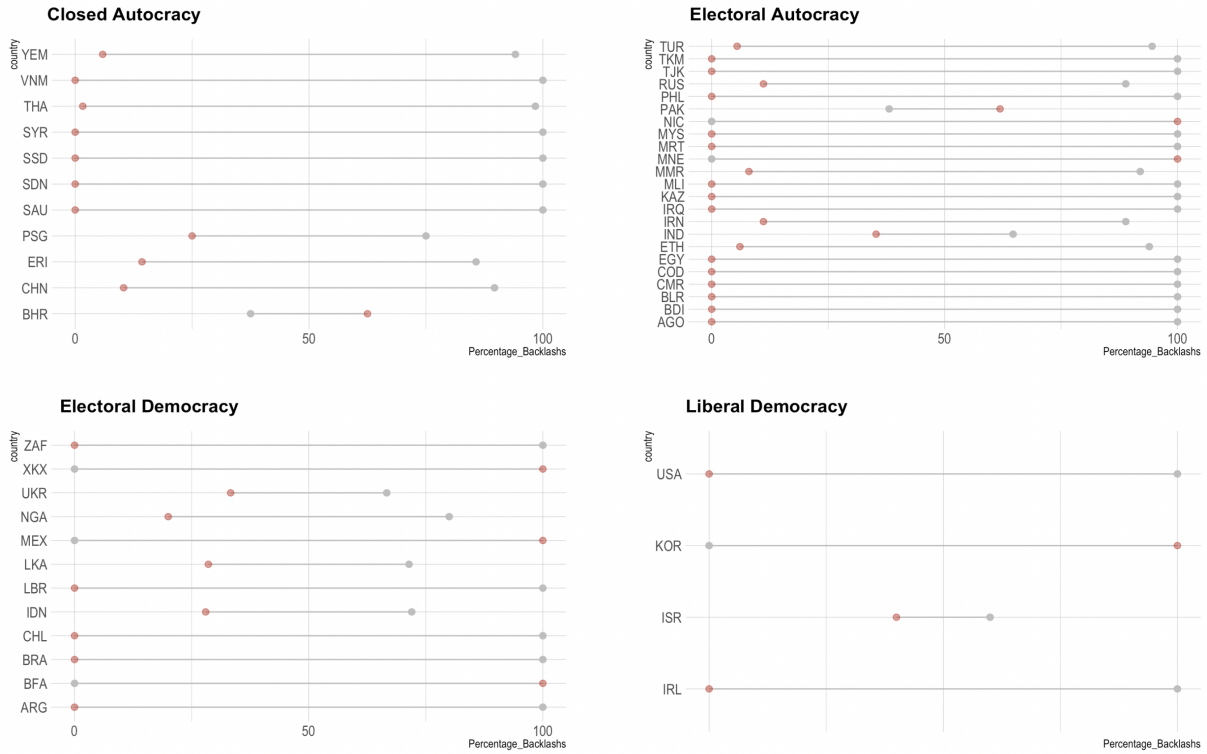

Figure 1: Responses to arrests by country and regime type

Figure 1 demonstrates that there is substantial variation both between and within countries in the responses to arrests. While the majority of arrests occur in electoral autocracies, the occurrence of backlash protests cannot be explained solely by regime type and related repression levels. While institutionalist and structuralist approaches can help us explain variation across political contexts, we also seek to explain variation *within* political contexts. We seek to understand why forms of the same type of repression (arrests) implemented in the same political contexts sometimes spark backlash protests and at other times not.

4.3 Independent variables

We use a variety of explanatory variables to analyse variation in both the type of arrest and in the structural conditions that may shape the likelihood of a backlash protests. Specifically, we

differentiate between leader arrests, mass arrests and arrests, which neither are mass arrests nor leader arrests.

We studied newspaper reports identified through LexisNexis and GoogleNews as well as ACLED's event descriptions of each individual arrest. We coded the binary variable *leader_arrests* as 1, if the sources contained explicit evidence that the detained individual is considered as a leader or head by the respective identity group. This may include politicians of ethnic parties, movement activists, intellectual figures, clerics, or traditional leaders. We coded the binary variable *mass_arrests* as 1, if the sources explicitly described the event as such or if there was evidence that at least 12 individuals were detained.[8] In total, we identify 88 leader arrests and 291 mass arrests. There are 15 cases that we coded both as leader and mass arrests, while for 64 arrests neither of these conditions is fulfilled (see table 2 in the Appendix). We include binary predictors for leader and mass arrests to our models and use other arrests as reference category.

The use of temporally and spatially highly disaggregated measures of structural variables allows us to hold time-invariant variables constant. In a first step, we measure grievances related to repression that have been linked to protest propensities (e.g., Carey, 2006; O'Brien and Deng, 2015). We use the variable *repression_10days*, which provides a count of the cumulative number of repressive events that occurred 10 days before a given arrest. We create this variable by aggregating all events of state repression against an ethnic or religious group in the 10 days pre-arrest time frame as reported by ACLED.[9] Following the same procedure, we created the variable *repression_50days*, which counts the cumulative number of repressive events that occurred 50 days before a given arrest.[10] By dividing the variable *repression_10days* through the variable *repression_50days*, we are also able to measure the development of repression in the pre-arrest

[8] We varied this threshold to 25, 50, and 100 arrests. Across all these definitions, mass arrests have a negative effect on the propensity of backlash protests (see Table 4 Appendix).

[9] More details on the coding procedure are provided in the codebook.

[10] Note that while reporting biases may weaken the cross-national validity of this measure, it is less prone to biases in within-country analyses. Under-reporting tends to result from a lack of infrastructure and geographical obstacles, factors that tend to remain relatively constant within states.

period labelled as *share_recent_repression*. Higher values than 1/5 of this quotient indicate that repression was increasing in the pre-arrest period, while lower values than 1/5 imply decreasing repression.[11]

To capture subsistence-related grievances that may trigger backlash protests, we use the variable *share_income_food_log*, measuring the share of income that citizens need for staple food. We rely on food prices because they allow for a direct and micro-economic measurement of grievances and have been identified as particularly relevant for social unrest (Weinberg and Bakker, 2015; Raleigh, Choi, and Kniveton, 2015; Bellemare, 2015). We chose the relevant staple food for each country and identified the nearest local market for each identity group based on their settlement patterns. Based on this information, we retrieved the staple food prices at the nearest local market in the month before a given arrest using the World Food Programme Database (WFP, 2021). Subsequently, we divided the staple food prices through the national average income per capita.[12]

Research suggests that protests are disproportionately likely to occur on important dates such as on national celebrations or on anniversaries that may represent focal points for dissidents (Truex, 2019; Schelling, 1980). Thus, arrests shortly before or after such meaningful days such as Nawroz for the Kurds in Turkey or the Liberation Sunday for Dalits in India could have a greater propensity of sparking a backlash protest. To account for such "dissident calendars", we qualitatively identified the most important holidays, festivals, or celebrations for each cultural identity group in our sample.[13] We created the variable *cultural_event* coded as 1 if an arrest occurred five days before, during, or five days after a cultural event that is meaningful for the respective identity group.

In addition to cultural events, we account for political events during which arrests may

[11] Given that the *share_recent_repression* is per definition collinear with the other repression proxies, we include it in a separate model as presented in Table 6 in the appendix.

[12] More details on the coding procedure are provided in the codebook.

[13] We present a comprehensive list of all identified cultural focal events in the appendix.

also have a different baseline probability of triggering backlashes. There is evidence that protests are especially likely in the proximity of elections (Hafner-Burton, Hyde, and Jablonski, 2018). We control for the *pre_election_period* and the *post_election_period* with binary indicators coded as 1 if an arrest took place 50 days before or, respectively, after an election.

Repression carried out during ongoing protest activity may encourage even more people to join a protest. There are numerous examples of counter-protests in which state repression heightened the intensity of ongoing upheaval waves (Aytaç, Schiumerini, and Stokes, 2017; Opp and Roehl, 1990; Beissinger, 2002). For instance, the harsh reaction of the Spanish government increased the protest activity of the Catalan independence movement (Balcells, Dorsey, and Tellez, 2021). To determine whether arrests occurred in the context of an ongoing protest, we code whether the arrest events in our sample are directly related to a cultural identity group's protest activity. Our *protest_context* variable is coded as 1 if a protest event related to a specific group occurred one day before or on the day of a given arrest of the same group. We base our coding on ACLED protest events, i.e., violent demonstrations, mob violence and peaceful protest (ACLED, 2021).

Finally, we include a few quasi-constant structural-institutional variables on the country-year level. While these variables are unable to explain short-term variation in backlash propensities, they account for broader structural differences between states, being useful for the cross-country models. We control for *democracy* with the electoral democracy index provided by V-DEM (Coppedge, 2021). We also use a measures of the capacities of the *security_apparatus* provided by the Fragile State Index (FFP, 2022) and control for the logged adjusted average net national income (*avg_income_pc_logged*) in US$ with data from the World Bank (World Bank, 2019). Additionally, we check for whether an arrest occurred in the context of an armed conflict as captured by the variable *armed_conflict_ongoing* coded with information from the UCDP/PRIO Armed Conflict Dataset (N. P. Gleditsch et al., 2002). We present summary statistics of all

included variables in Table 1 in the appendix.

4.4 Methods

We employ an empirical strategy that is increasingly demanding to test our hypotheses. We use five different specifications of logistic regression models: (1) a pooled model where no fixed effects are included, (2) a model with fixed effects for countries, (3) a model with fixed effects for cultural identity groups, (4) a model with two-way fixed effects for countries and years, (5) a model with two-way fixed effects for identity groups and years. We re-run the analyses with OLS regression models since country and time fixed effects may create problems in logistic regression models (see Magaloni and Rodriguez, 2020; Gomila, 2021). To obtain accurate variance estimates, we cluster the standard errors by identity groups.

5 Results

Our main findings are shown in a Table 1 and visualized in a coefficient plot presented in Figure 2.[14] Across all model specifications, we find that leader arrests have a positive and statistically significant ($p<0.05$) effect on the propensity of backlash protests. The coefficient plot with standardized coefficients shows that the effect of leader arrests is comparatively large. Based on model 1, leader arrests have the largest positive effect on the propensity of backlash protests compared to the other covariates. The finding holds across diverse combinations of fixed effects as shown in models 2-5.[15] While the findings must be treated with some caution in light of the limited sample size, we interpret them as suggestive evidence for hypothesis H_1.

Turning to our second key explanatory variable, we find that mass arrests are consistently

[14] We standardized all coefficients in the coefficient plot, which allows us to compare the effect sizes in relative terms.

[15] We also ran base models without any covariates and the positive and statistically significant effect of leader arrests persists (see Table 4).

negatively related to the propensity of backlash protests. The coefficient of the mass arrests variable is negative in all model specifications, however, only in one model does the effect reach statistical significance. Hence, while there is tentative evidence in support for hypothesis H_2, it remains unclear whether we can reject the null hypothesis. The pattern remains similar when we vary the threshold to define mass arrests: mass arrests with at least 25, 50, or 100 victims have likewise a negative, but statistically insignificant effect on the propensity of backlash protests.

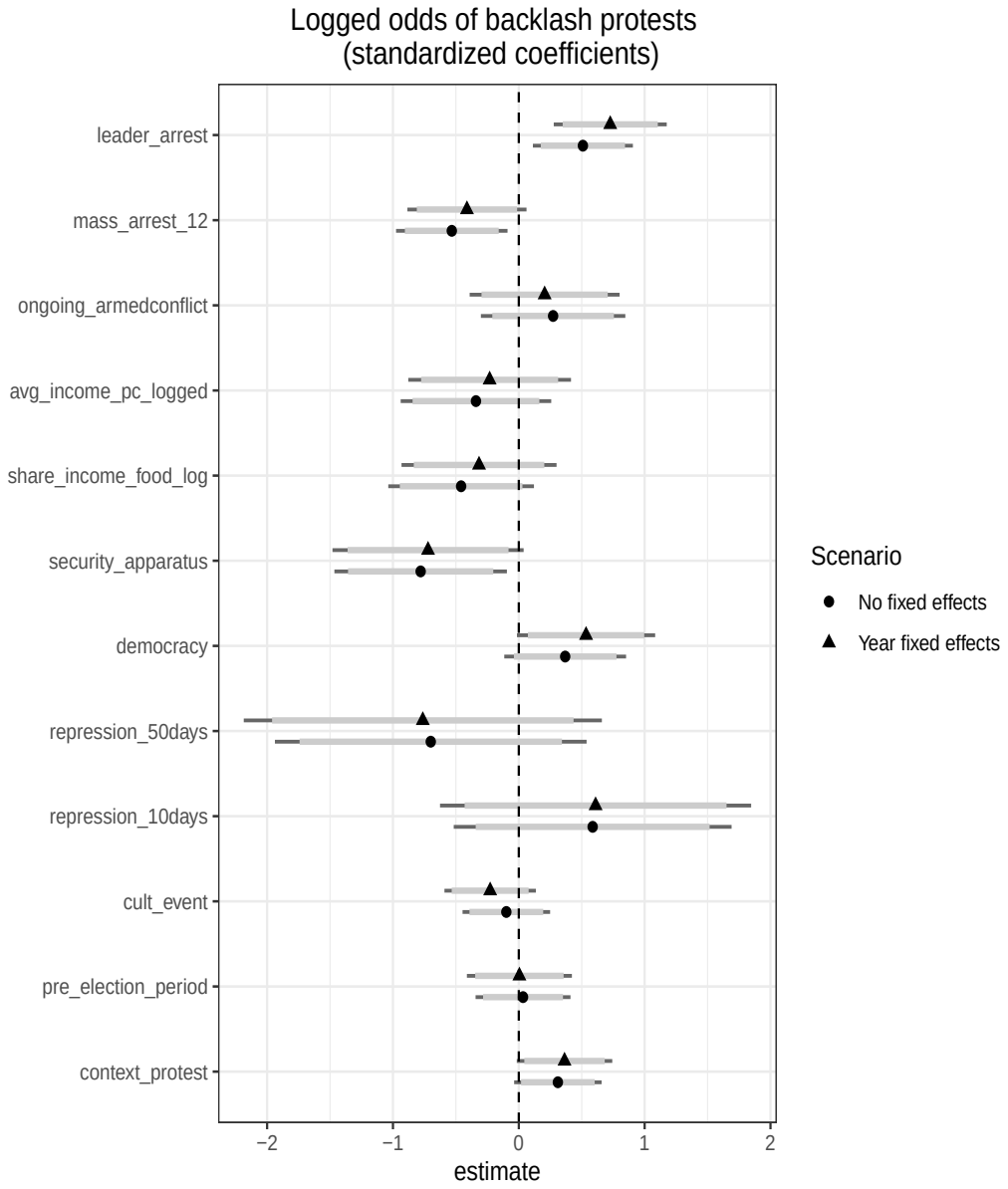

Figure 2: Coefficient plot of logistic regression model (based on model 1)

Note: Horizontal grey bars: 90% CIs, horizontal black bars: 95% CIs

Table 1: Logistic regression results

	Dependent variable:				
	Backlash protests				
	(1)	(2)	(3)	(4)	(5)
Leader_arrest	1.181*	1.922*	3.301*	2.785*	3.858**
	(0.707)	(1.043)	(1.744)	(1.568)	(1.837)
Mass_arrest_12	−1.244***	−0.865	−0.226	−0.893	−1.141
	(0.471)	(0.590)	(1.366)	(0.545)	(1.419)
Repression_10days	0.092	0.174	0.145	0.175	0.073
	(0.115)	(0.140)	(0.155)	(0.150)	(0.177)
Repression_50days	−0.020	−0.022	−0.033	−0.040	−0.040
	(0.021)	(0.026)	(0.034)	(0.029)	(0.025)
Share_income_food_log	−0.832	−1.828	−2.323	−1.581	−0.644
	(0.554)	(1.636)	(10.475)	(1.118)	(15.078)
Cult_event	−0.417	−0.867	0.964	−1.760*	1.273
	(0.674)	(0.831)	(1.175)	(0.979)	(1.220)
Pre_election_period	0.141	0.136	−41.089***	−0.557	−44.558***
	(0.909)	(1.765)	(2.453)	(1.803)	(2.755)
Post_election_period	−0.793	−0.070	−37.690***	−0.554	−40.164***
	(1.751)	(1.767)	(2.351)	(1.594)	(2.704)
Context_protest	1.228	1.486**	3.126**	1.874*	4.399**
	(0.748)	(0.727)	(1.538)	(0.957)	(1.903)
Democracy	1.873	4.449	−2.006	8.847	−5.150
	(1.833)	(8.732)	(8.098)	(7.760)	(6.620)
Security_apparatus	−0.633*	−3.274**	−3.917	−2.952	−15.881**
	(0.383)	(1.525)	(2.382)	(2.128)	(6.342)
Avg_income_pc_logged	−0.313	7.239	0.877	11.841	36.180
	(0.281)	(6.126)	(7.028)	(8.008)	(22.612)
Ongoing_armedconflict	0.614	−18.837***	−24.196***	−18.963***	−29.959***
	(1.000)	(3.237)	(3.159)	(2.944)	(4.048)

Constant	4.659	15.272	30.619	−22.363	−122.207
	(4.527)	(32.920)	(57.836)	(48.274)	(125.711)
Identity group fixed effects	NO	NO	YES	NO	YES
Country fixed effects	NO	YES	NO	YES	NO
Year fixed effects	NO	NO	NO	YES	YES
Observations	317	317	317	317	317
Log Likelihood	−90.129	−57.766	−32.754	−53.607	−28.036
Akaike Inf. Crit.	208.257	207.532	253.508	207.214	252.073
Note:					*p<0.1; **p<0.05; ***p<0.01

6 Robustness tests

We run several tests to probe the robustness of our key finding, i.e. the substantial and statistically significant effect of leader arrests on the propensity of backlash protests. First, we re-run the analysis with a different set of control variables. We add proxies for judicial accountability, economic inequality, and public service provision as further important long-term grievance variables in multi-ethnic societies (Østby, 2008; De Juan and Wegner, 2019). We also include a measure of the logged number of days since the last event of repression to control for the time-dependency of grievances related to previous repression. Further, we disaggregate state repression and account for the share of forced disappearances and fatalities per country population size. The positive effect of leader arrests remains statistically significant in all model specifications as shown in the Tables 6 and 9 in the appendix. The mass arrest variable remains consistently negative, however, the coefficient fails to reach statistical significance.

In a second step, we exclude one by one each of the identity groups to preclude that the findings are driven by a specific group. The positive effect of leader arrests remains statistically significant across all models, implying that it is not driven by outlier cases. Third, we re-run the main logistic regression models with OLS linear probability estimators. The findings remain robust as shown in Table 5 in the appendix. Fourth, we replaced year-fixed effects with month-

fixed effects and the findings remain unaffected (see Table 8 in the appendix). Fifth, we tested the predictive power of our model by using a separation plot, which represents a visual method for evaluating the predictive power of models with binary outcomes (Greenhill, Ward, and Sacks, 2011).[16] The separation plot presented in Figure 2 in the appendix suggests that our model has a high accuracy in predicting backlashes.

Finally, we employ coarsened exact matching to test the robustness of our findings to parametric modelling assumptions (Iacus, King, and Porro, 2012). While matching hinges just like regression on the conditional independence assumption, i.e. the assumption that all relevant confounders have been identified, it offers a non-parametric estimation approach. We match on the covariates *repression_10days*, *avg_income_pc_logged*, and *post_election_period*, which display a substantial imbalance between leader arrests and other types of arrests.[17] Figure 1 in the appendix shows that the covariate imbalance has been substantially reduced through the matching approach. Subsequently, we re-run the logistic regression models drawing on the pre-processed coarsened exact matched data. The findings presented in Table 7 in the appendix demonstrate that the effect of leader arrests remains positive and statistically significant in all model specifications. In contrast, the leader arrest variable remains consistently negative and it is statistically significant in two out of four model specifications.

7 Mechanism tests: Why leader arrests backfire

While our findings suggest that leader arrests have a higher propensity to spark backlash protests, the question arises *why* this is the case. In order to shed light on this unresolved question, we exploratively test two potential mechanisms that could link leader arrests to a higher propensity

[16] The separation plot provides a visual summary of the distribution of events and non-events (i.e. backlashes and non-backlashes in our case) and it illustrates whether observations with high predicted probabilities of experiencing backlash protests actually experienced the event.

[17] We also matched on a different set of covariates, i.e. *cultural_events*, *ongoing_armedconflict*, and *democracy* and the results remain robust as shown in the replication files.

of backlash protests. A first approach highlights the pivotal role of pre-existing organisations, a factor emphasized by many influential mobilization theories (McCarthy and Zald, 1977; Cunningham, 2013; Asal and Wilkenfeld, 2013; Chenoweth and Ulfelder, 2015; Sutton, Butcher, and Svensson, 2014). A second approach emphasises the magnitude of the triggering event and puts less importance on organisations and pre-existing mobilisation networks (Straus, 2015; van der Maat, 2018). While the two approaches are not mutually exclusive, they provide different explanations for why leader arrests backfire.

According to the first approach, individuals are mobilized through pre-existing organizational networks and communication channels, which shape capacities for collective action (Staniland, 2012; McAdam, 1999; Gledhill, 2018; Bell et al., 2013). For instance, Sutton and co-authors argue "that the likelihood of extreme repression backfiring against the government is a function of prior institution-building, especially institutions that facilitate communication and tactical adaptability" (Sutton, Butcher, and Svensson, 2014, p. 561). In terms of mobilization, organizational and communications infrastructure as well as networks of trust that bind members of close-knit social organizations improve their readiness to participate in high-risk collective action (Asal, Nagar, and Rethemeyer, 2014; Della Porta, 1988; Parkinson, 2013). Following this argument, backlash protests occur when organisational structures are targeted by state repression, provoking a counter-reaction by the other members of the organization. Building on the findings of our previous analysis, we expect that protests in response to the arrest of a leader are more likely to occur when the leader is member of a formal organization.

Following the logic of incidental grievances triggering conflictive mass behaviour, the arrest of an ethnic group leader prompts individuals to act (Straus, 2015; Jasper, 1997). Anecdotal evidence shows that this is often the case if those arrested hold significant symbolic value for group members and their cultural identity. Protests in response to the arrests of Carles Puidgemont (Spain) and the Gan traditional king (Burkina Faso), both highly symbolic leaders, illustrate this

assumption. From a socio-psychological perspective, this impulsive reaction is explained by the fact that cultural identities are particularly important for individuals' self-esteem, with perceived identity threats evoking negative emotions and eliciting fight reactions against the attacker (Tajfel and Turner, 1997; Goodwin, Jasper, and Polletta, 2001; Costalli and Ruggeri, 2015; Pearlman, 2013). Feelings of injured identity quickly spread and amplify within the social collective. Group members rapidly "infect" their ethnic peers with their collective emotions and, in a second step, with their action tendencies (Rimé, 2007; Straus, 2015; Rivera and Páez, 2007). According to this approach, a momentous triggering event is essential to trigger backlash protests (Straus, 2015; van der Maat, 2018). We expect this to be the case especially when the arrested leaders have a particularly high symbolic significance, which does not necessarily correspond to their organisation-based function.

To analyze the impact of those two distinct mechanisms, we carefully studied the descriptions of all arrest cases in our sample as well as information on arrestees and hand-coded two additional variables. In a first step, we determined on the basis of newspaper reports and ACLED's event descriptions whether those arrested are members of a formal organization. We define organizations as a connected group of people with a rule-based structure, some form of hierarchies, and a specific purpose. This includes, for instance, political parties, governmental and non-governmental organizations, companies, or militias acting in the name of a specific cultural identity group. This approach allows us to determine the organizational ties of arrested individuals.[18]

In April 2018, for example, an arrest sparked an upheaval among Pashtuns in Pakistan. The arrested individual was Ali Wazir, a leading figure in the Pashtun Tahafuz Movement (PTM), which qualifies the event as a leader arrest of a high-ranking organization member. In contrast, the arrest of Alexander Gabyshev, a shaman from Yakutia, by Russian police forces in Septem-

[18]More details on the coding procedure are provided in the codebook.

ber 2019 did not spark a counter-protest, possibly because the arrestee is not affiliated with any organization. To avoid falling for false claims and government propaganda, we only code organizational membership if we have explicit evidence that the arrested are or have been members of a specific organization.

To code the *symbolic_leaders* variable, we examined case-wise whether the arrested leader has a high symbolic value for the group, based on the information available to us. Even though this is notoriously difficult to determine, we contend that there are differences in degree that can be determined through a careful qualitative assessment. In a first step, we derive these differences from a leader's official function and code all clerics and traditional leader as *symbolic_leaders*. We contend that imams, preachers, tribal or indigenous leaders are important identification figures for cultural identity groups. For instance, figures such as the Al-Aqsa preacher Sheikh Ekrima Sabri or Jeon Kwang-hoon, representative of the Christian Council of Korea, have a high symbolic value, with repression directed against them easily triggering a feeling among ordinary group members of being targeted by proxy. In a second step, we studied all other leader arrests in our sample, as also political, social, economic or military elites may have significant symbolic importance that cannot be explained solely by their official (organisation-based) role. For instance, Carles Puidgement as leader of the Catalan independence movement had a higher symbolic status at the time of his arrest than other regional presidents and Abdullah Öcalan is more for the Kurds than just a former PKK leader. We coded the binary *symbolic_leaders* variable as 1 for all arrested individuals with such a high symbolic value for their respective cultural identity groups.

To investigate to what extent the effect of leader arrests on backlash protests is driven by those respective mechanisms, we conduct the following empirical tests: In a first step, we add the *organization_membership* variable to a logistic regression of backlash protests on leader arrests. If the effect of leader arrests on backlash protests would be driven by pre-existing organizational networks, we would expect that the leader arrest variable loses statistical significance, once we

account for organizational membership. In a second step, we interact leader arrests with *organization_membership*. If the effect of leader arrests on backlash protests would be conditional on organizational membership, we would expect a positive and statistically significant interaction effect. In a third step, we run a model with the *organization_membership*-variable that includes the vector of covariates from the main analysis. Subsequently, we conduct the same three tests with the *symbolic_leaders* variable.

The results of these additional tests are presented in Table 2. They suggest that the effect of leader arrests on backlash protests is not driven by leaders' organizational membership. The leader arrest variable remains statistically significant when we account for membership in organizations of arrested individuals (see model 1 and 5). We also do not find a statistically significant interaction between leader arrests and organization membership (see model 2). In contrast, the evidence suggests that the effect of leader arrests on backlash protests is driven by symbolic leaders. According to model 3, the leader arrest variable loses statistical significance, once we account for the arrest of symbolic leaders. Further, we find evidence for a statistically significant interaction effect between leader arrests and symbolic leaders, suggesting that the effect of leader arrests on backlash protests is conditional on whether leaders have an important symbolic role.

While these tests represent a first empirical exploration of the mechanisms linking leader arrests to backlash protests, we call readers to interpret the findings with caution. Our sample of arrests is comparatively small, implying that these additional subset analyses suffer from a limited statistical power. Our analyses only provide indirect and tentative evidence for the two mechanisms, requiring additional quantitative and qualitative investigation in future research.

Table 2: Logistic regression results, mechanism tests

	Dependent variable:					
	Backlash protests					
	(1)	(2)	(3)	(4)	(5)	(6)
Leader_arrest	1.111***	0.486	0.746	0.620	1.499**	1.546**
	(0.376)	(0.752)	(0.572)	(0.659)	(0.703)	(0.777)
Organization_membership	0.221	−0.166			0.655	
	(0.367)	(0.499)			(0.527)	
Leader_arrest:organization_membership		1.061				
		(0.814)				
Symbolic_leaders			0.965**	−13.442***		0.786
			(0.466)	(0.709)		(0.653)
Leader_arrest:symbolic_leaders				14.579***		
				(0.945)		
Constant	−2.158***	−2.095***	−2.146***	−2.124***	4.655	4.165
	(0.363)	(0.381)	(0.327)	(0.333)	(4.304)	(4.190)
Covariates	NO	NO	NO	NO	YES	YES
Observations	424	424	428	428	313	317
Log Likelihood	−167.638	−166.784	−166.022	−165.150	−91.668	−92.281
Akaike Inf. Crit.	341.276	341.568	338.043	338.301	209.335	210.563
Note:						*p<0.1; **p<0.05; ***p<0.01

8 Conclusion

While a rich literature shows that spontaneous counter-reactions to government repression are not rare phenomena, we know comparatively little about which types of repression are associated with deterrence or incitement of backlash protests. Taking a nuanced perspective, we made the case that different forms of the same instrument of repression, arrests, can have distinct effects on the likelihood of protest outbreaks in multi-ethnic societies. We argued that arrests as a salient form of state repression become triggering events when they represent incidental grievances and hence provoke a collective emotional reaction among cultural identity groups. This reaction is particularly likely in the case of leader arrests. Cultural group leaders symbolize the suppression of the social collective and thus evoke a strong feeling of injured identity among a large number of group members. Mass arrests, in contrast, are less supra-individual and make group members fear that they themselves could be affected by repression. This leads more individuals to reconsider their protest participation, which makes collective action more difficult.

To explain *why* leader arrests backfire, we tested two potential mechanisms. A frequently used argument in the literature on the repression-protest nexus is that individuals are mobilized through pre-existing organizational networks and communication channels, implying that the capacity of movements to mobilize is shaped by their access to existing societal organizations. We tested the empirical implication of this approach, which states that backlash protests are more likely to happen when members of formal organizations are detained. A second perspective revolves around people's emotional and spontaneous reactions to repressive events. This approach assumes that momentous triggering events increase peoples willingness to participate in collective action. We have argued that the arrest of leaders with high symbolic value constitute such "moral shocks" (Jasper, 2018, p. 87).

Our statistical models yield three main findings. First, backlash protests are significantly more likely following the arrest of group leaders. The positive effect of leader arrests holds when

we account for a variety of factors that capture socio-economic grievances and opportunity costs. Second, mass arrests tend to have a negative effect on the likelihood of backlash protests. While the negative effect of mass arrests is not statistically significant across all model specifications, our results suggest that mass arrests are unlikely to incite immediate mobilization among cultural identity groups. Third, our findings suggest that the effect of leader arrests on backlash protests is conditional on whether leaders play an important symbolic role. In contrast, we do not find evidence for a substantial role of an arrested leaders' organizational membership. In other words, it matters for backlash protests not only whether a leader is arrested, but also who this leader is.

Despite these consistent findings across a variety of robustness tests, important limitations must be considered. The explanatory power of our study is limited by the fact that we cannot make any statement about the reasons for the arrests, which may impact the likelihood of a backlash protest. While our study represents a first empirical exploration of the mechanisms linking leader arrests to backlash protests, we only provide indirect and tentative evidence for the two mechanisms. Moreover, the empirical evidence is limited to a relatively small sample of arrests and backlash protests by ethnic, tribal, and religious groups. Future research should investigate whether our findings hold with other scope conditions and can be generalized to other settings such as ideology-based opposition groups or social movements. We are also aware of systematic reporting bias in event-based datasets. While these issues are mitigated in this study by the fact that low-profile arrests that do not make the news are unlikely to spark large-scale protests, arrests of prominent leaders and the resulting backlash protests may be overrepresented in our media-based data. Being aware of these limitations, we present the first systematic empirical evidence suggesting that leader arrests are more likely to backfire than other types of arrests. While the targeting of high-profile leaders may be considered as an effective tool to suppress the demands of cultural identity groups, we demonstrate that leader arrests tend to incite opposition and backfire against repressive regimes.

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