Original Article

Military Conscription and Nonviolent Resistance

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Abstract

Nonviolent campaigns against repressive regimes often turn on the military's decision to either defend the ruler or make common cause with the ruled. Yet surprisingly little scholarship investigates opposition expectations for the military's likely response to mass protest. We theorize that some determinants of the military's willingness to repress are more observable to activists than others. In particular, we identify conscription as a highly salient indicator that soldiers will refuse to fire on protesters and hypothesize that nonviolent campaigns are more likely to materialize against regimes with conscripted armies than those with volunteer forces. We substantiate this theory with two sources of evidence: (1) a survey experiment conducted during the 2019 Algerian Revolution and (2) a cross-national analysis of the positive association between conscription and nonviolent campaign onset from 1945 to 2013.

Keywords

social movements, military and politics, Middle East, democratization and regime change, conscription

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> "The army was not going to intervene. […] The soldiers are ordinary citizens coming from working-class districts, whom the raffle chose. They are there just for a year, it is not their job. They are not indoctrinated, [or] trained in repression. It is not their daily life."
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> Tunisian activist Yassine Ayari (2011)

From the Color Revolutions and the Arab Spring uprisings to recent resistance campaigns in Hong Kong, Algeria, and Sudan, the 21st century has become an "age of global mass protests" (Brannen et al., 2020). These and many other contentious episodes have heavily shaped the course of international politics, so much so that mass protest has surpassed the coup d'état as a leading cause of regime change (Chenoweth & Stephan, 2011; Kendall-Taylor and Frantz 2014). This surge of "people power" campaigns, a trend that only continues to accelerate (V-Dem Institute, 2020), has rejuvenated scholarly interest in the causes and consequences of civil resistance.

Yet while inspirational success stories in places like Ukraine (2004) and Tunisia (2011) are widely lauded, violent crackdowns in China (1989) and Bahrain (2011) illustrate that rulers are hardly helpless in the face of mass uprisings. These cautionary tales highlight the military's paramount importance during revolutionary episodes, the outcomes of which often hinge on the military's willingness to repress popular demands for change. For this reason, recent scholarship has explored how the military decides whether to "defect or defend" (Barany, 2016; Bellin, 2012; Brooks, 2013; Lee, 2014) in response to mass protests.

Surprisingly little work, however, has considered this question from the opposition's perspective: how do activists assess whether the military will side with the ruler or the ruled? As violent repression can impose prohibitively severe costs on peaceful protesters, activists' beliefs about the military's resolve to repress likely influence their decision to take to the streets. Yet we know very little about how activists evaluate whether the military would welcome or oppose popular demands for reform.

To that end, this article is one of the first attempts to directly investigate protesters' expectations for the military's response to mass mobilization. Among many factors that shape the military's loyalty (Barany, 2016; Nepstad, 2013), we identify military conscription as an especially salient signal that increases activists' confidence that the military will not repress. As exemplified by Tunisian activist Yassine Ayari above, activists infer that conscripted soldiers are less willing to defend the regime and fire on demonstrators than volunteer careerists. We therefore argue that activists should be more likely to mobilize nonviolent campaigns against regimes with conscripted armies than those with volunteer forces.

We test our theory with both survey experimental and cross-national data. We first test the individual-level micro-foundations of the theory through an

online survey experiment fielded during Algeria's 2019 Hirak protests. We find that priming Algerians to recall that the Algerian military is largely conscripted reduced their expectations of military repression, and in turn increased their stated willingness to protest. Further analysis reveals that the conscription prime led respondents to perceive a difference between commanding officers and low-level soldiers—while officers may wish to defend the regime, activists expected conscripted soldiers to identify with protesters and therefore to resist orders to repress.

We then assess the generalizability of our argument through a cross-national analysis of conscription and mass protest. We extend existing data on conscription (Toronto, 2014) through 2013, to match available time-series data on nonviolent campaign onset (Chenoweth & Shay, 2019). We find that conscription positively correlates with nonviolent campaign onset, roughly doubling the likelihood of protest onset in a given country-year even when controlling for other factors thought to determine military recruitment practices and mobilization onset. We also demonstrate that conscription is associated with other important aspects of nonviolent campaigns, including protest size, security force defections, and success, and present evidence suggesting that activists strategically employ nonviolence, not violence, when facing conscripted militaries. Combined, the survey experiment and cross-national analyses provide strong evidence that conscription decreases opposition expectations of repression, in turn increasing the likelihood of mass nonviolent mobilization.

Our findings advance the study of civil resistance and civil-military relations in at least two ways. First, they demonstrate that conscription is fraught with trade-offs. Rulers may implement conscription in order to combat external security threats, or in an attempt to generate nationalist sentiments at home. Yet by binding the military more closely to the people, conscription leaves regimes less able to repress—and thus more likely to face—popular uprisings. Our findings thus echo Talmadge (2015) and Greitens (2016)'s path-breaking works on the trade-offs rulers face when structuring their coercive apparatuses.

Second, our research opens the door to further integration of the literature on civil resistance and civil-military relations. These fields are largely estranged, and our work suggests that the overlap between them is fruitful ground for new scholarship, especially regarding the "potential endogeneity of protests to civil-military relations" (Brooks, 2017). This article focuses on conscription, but future studies could generate and test additional hypotheses about how protesters, regimes, and militaries strategically interact.[1]

Mass Protests and the Military

In recent years, protesters have hit the streets from Hong Kong to Khartoum, seeking to overthrow dictators or force major institutional change. In turn, scholars have redoubled their efforts to understand the conditions that give rise

to popular uprisings. How do we explain variation in protest onset? Are some states more susceptible to civil resistance campaigns than others?

One of the most widely discussed factors in the contentious politics literature is the threat of repression.[2] Would-be protesters who anticipate that regimes will crack down on demonstrations may be more reluctant to join. Indeed, autocrats use repression not just to quell ongoing protests, but also to preemptively deter mobilization (Nordås & Davenport, 2013; Danneman & Ritter, 2014; Ritter & Conrad, 2016; Young, 2019). In formal models of mobilization, the threat of repression is almost universally acknowledged to shape protester behavior (e.g., Aldama et al., 2019; DeNardo, 1985; Kuran, 1991; Pierskalla, 2010).[3]

However, the sources of opposition expectations for repression have received less attention. According to the "Law of Coercive Responsiveness," regimes invariably meet demands for systemic change with repression (Davenport, 2007). But the form, scope, and severity of repression can all vary greatly (Earl, 2011), and scholars have not sufficiently considered how would-be-protesters anticipate the degree of repression they are likely to face. Instead, scholars have typically modeled protesters' expected costs for repression in a general sense, as a function of either (1) the observed size of demonstrations, that is, "safety in numbers" (Kuran, 1991); or (2) repression itself, that is, whether the regime repressed or offered concessions in the recent past or in response to certain resistance tactics (Lichbach, 1987; Ginkel & Smith, 1999; Pierskalla, 2010). These are valuable insights, but many other factors could inform opposition beliefs about the risk of repression.

Of particular importance is how protesters assess the likelihood of repression from the military, as opposed to the police or other security forces. Armed for war and trained to kill, militaries can wield tremendous violence against demonstrators, imposing uniquely high costs of repression—if the military agrees to repress, hundreds if not thousands of dissidents may die. Moreover, the military's behavior has an outsized impact on movement outcomes. As the regime's "repressive agent of last resort" (Svolik, 2012), the military's choice to repress or rebel can "make or break" revolutions (Barany, 2016; Lee, 2014; Nassif, 2021; Nepstad, 2013). The military's behavior thus carries great weight both for protesters' costs of repression and for the broader likelihood of campaign success.

In turn, dissidents' expectations for the military's response to mass protests likely influence their decision to mobilize in the first place. Mobilization is a far more attractive option, both for individual demonstrators and for the movement as a whole, if the opposition expects the military to join the cause. Yet the literature is surprisingly silent on how protesters assess the likelihood of military repression.

Some initial hypotheses can be drawn from a parallel literature on how militaries respond to mass uprisings. Particularly since the Arab Spring,

scholars have considered why some militaries repress protesters and others join them (Brooks, 2017). Much of this literature links the military's behavior to institutional characteristics, such as whether it is professional (Bellin, 2012), conscripted (Barany, 2016), counterbalanced by the police (Brooks, 2013; Nassif, 2021), ethnically stacked (Harkness, 2018; McLauchlin, 2010; Nepstad, 2013), internally divided (Albrecht & Ohl, 2016), or concerned about its institutional prerogatives (Grewal, 2019a; O'Donnell & Schmitter, 1986) or prestige (Pion-Berlin & Trinkunas, 2010).

While each of these factors may influence the military's behavior, it remains unclear whether they will have corresponding effects on the opposition's beliefs about repression or their decision to protest—we should not assume that these factors are self-evident. Indeed, a number of high-profile cases, such as the 1989 Tiananmen Square massacre or the 2011 crackdown in Bahrain, suggest that the opposition can misjudge the true likelihood of military repression. Such miscalculation may occur because dissidents cannot easily acquire first-hand information about military officers' (private) preferences. Direct communication with officers can be prohibitively risky for activists hoping to avoid attention from the security apparatus. Moreover, the military is often physically separated from society—soldiers leave home for barracks, and officers may eventually move their families into military enclaves (Khaddour, 2015; Sayigh, 2012)—thus limiting the social ties through which activists could discern attitudes within the armed forces.

Accordingly, many of these factors, such as internal rivalries in the military, are likely unknown to the public. Others, like the military's corporate interests and patronage spoils, may only be partially visible. Corruption may be hidden from public view through secret military budgets, control over black markets or other illicit profits, and even ownership-by-proxy of key industries, obscuring the full extent of the regime's efforts to buy the military's loyalty.[4] Even seemingly "well-known" factors, such as ethnic stacking, may be intentionally obscured, as with Hafez al-Assad's selective promotion of a handful of high-profile Sunni officers to downplay the Alawite domination of the officer corps (Bou Nassif, 2015; Phillips, 2015).

Still, other factors may be more visible to the opposition. In the next section, we theorize that military conscription is a particularly salient indicator that the military is likely to side with the people. In turn, dissidents are more likely to initiate nonviolent campaigns in countries with conscripted militaries than those with volunteer forces.

A Theory of Conscription and Mass Protest

Following Toronto (2014), we understand conscription to broadly include any national policies that coerce citizens into military service. These policies can take many forms, such as drafts or selective service programs, universal

service requirements of varying length and scope, as well as indirect laws and regulations that effectively compel citizens to serve as a prerequisite for obtaining basic public goods.[5] Today, about 60 countries employ some form of conscription (Figure 1).

Our theory of conscription and protest onset rests on two core assertions. First, we review the literature's existing finding that conscripted militaries are less willing to repress mass demonstrations than volunteer forces. Second, we argue that because conscription is a highly salient feature of the military, dissidents will likely recognize and anticipate this dynamic. Because the threat of violent repression can deter opposition mobilization, we infer that, all else equal, activists should be more likely to mobilize nonviolent campaigns against regimes with conscripted armies than those with volunteer forces.

Conscription and Repression: Of the People, For the People

Protest scholars have long suspected that conscripted militaries are unreliable agents of repression. Nepstad (2011, p. 129) observes that in East Germany (1989), "many soldiers were not military careerists; a sizable number were conscripts who had friends and family members in the movement. Moreover, these soldiers often supported demonstrators' goals…this made it difficult for troops to view civil resisters as unreasonable radicals." Likewise, Binnejdik and Marovic (2006, p. 417) argue that in Serbia (2000) and Ukraine (2004), "Nationwide conscription …meant that a bulk of both armies consisted of relatively young recruits who remained in contact with friends and families and whose political affiliations often mirrored those of their peer civilians." Barany (2016, p. 141) contends that "Egypt's [2011] conscript army has so many ties to society at large that, even had the generals been willing to shoot

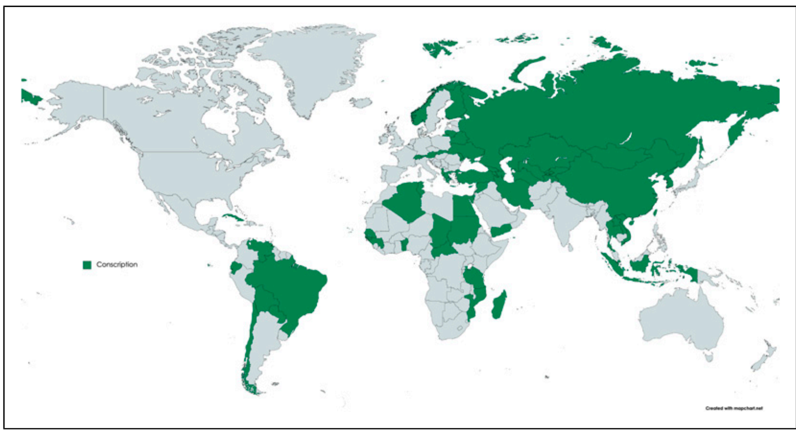

Figure 1. Countries with Conscription, 2013 (author extension of Toronto, 2014).

demonstrators, many officers and enlisted men would probably have refused to obey such an order." And in Syria (2011), Bou Nassif (2015) and Ohl et al. (2015) document how Syrian conscripts were mistreated by officers and denied privileges afforded to careerists, indignities that led many to defect. Indeed, Barany (2016, p. 29) argues that "virtually all major rebellions in the twenty-first century bear out this hypothesis" that conscripted armies will side with the people, and Lutscher's (2016) cross-national analysis of military defection during nonviolent campaigns from 1975 to 2006 offers further evidence in this direction.

These studies illustrate four interrelated mechanisms through which conscription weakens the military's resolve to repress. The first and most basic of these is a difference in motivation: conscripts were forced into service, while volunteers chose it. In war, conscripts are more likely to desert than volunteers (McLauchlin, 2015). For the same reason, conscripts are more likely than volunteers to desert or defect in the face of mass protests—many conscripts had not wanted to serve at all, let alone fire upon their countrymen.

Second, conscripted militaries are generally more representative of the people than volunteer forces. Volunteer armies draw recruits from a relatively narrow subset of society, that is, those who would willingly opt into the regime's service and the possibility of committing violence in its defense. In contrast, conscription draws recruits more broadly and irrespective of individuals' beliefs about the regime, thereby creating more diverse armies. As a result, conscripted forces should better reflect the true distribution of societal preferences over regime change.[6] Put another way, conscripted militaries are armies "of the people," drawn from the same disaffected masses that fuel nonviolent campaigns. Both Barany (2016) and Lutterbeck (2012, p. 33) argue that conscripts are more broadly representative of society, and so "will be more open to popular pro-reform movements,"[7] and Brooks (2017) observes that "overlap between the composition of the [conscripted] military and society at-large, complicates repression against a mass uprising."

Relatedly, conscripts are more likely to maintain their preexisting social networks than volunteers. For volunteers, military service often becomes a life-long career, through which they form new identities and friendships, typically within the institution. Conscripts, meanwhile, are recruited for limited periods of service (1–2 years), after which most return to civilian life. They are therefore more likely to maintain their preexisting social networks during their period of military service, which in turn make them more likely than volunteers to personally identify with the protesters.

Lastly, conscripts have fewer career incentives to repress. For volunteer careerists, the army can bring financial stability and upward mobility as promotion into the upper ranks entails higher salaries and other professional perks. Conscripts, by contrast, will soon leave the service and thus are less swayed by the patronage spoils that may come with loyalty and promotion. At

the same time, conscription imposes opportunity costs on recruits, who can be forced to delay or even forego more lucrative employment options until the completion of their service (Poutvaara & Wagener, 2007a). Thus, while careerists may prefer to preserve their privileges by protecting the regime, conscripts have no such incentive.

Activists Know That Conscripts Won't Shoot

For these reasons, conscripted militaries may be less resolved to repress popular uprisings than volunteer forces. Yet as Kurzman (1996) reminds us, "political opportunities" as construed by social scientists may not be perceived as such by dissidents themselves. We now argue that the opposition is indeed likely to recognize this dynamic—activists are aware of conscription, and consequently anticipate that the military will be unwilling to repress.

Most importantly, unlike other aspects of the military, conscription is well known to the public—by its very nature, conscription is conspicuous. Conscription is usually promulgated and enforced by law. Universal service requirements are transparent by design, and draft or lottery systems require all eligible citizens to register, even if they are not ultimately chosen to serve. Some may evade conscription only by taking steps to avoid it, perhaps by paying a bribe, extending secondary education, or working abroad during eligible years.[8] Because compulsory military service threatens a direct and substantial disruption to ordinary life, conscription is likely to be one of the most salient features of the military.

At the same time, a (likely inadvertent) byproduct of conscription is increased civilian awareness that conscripted soldiers would be unwilling to repress mass protests. The reason is that conscription tends to involve cycling a wide array of recruits through brief periods of service, such that a relatively large number of eligible citizens are ultimately made to serve (Choulis et al., 2021). These individuals return to civilian life with first-hand experience of life as a conscript, including knowledge of conscript morale and potential internal frictions between conscripts and career officers. Thus, not only does conscription depress the military's resolve to repress, but it also disperses many ex-conscripts who personally understand this dynamic back into the civilian population. In so doing, conscription also increases the likelihood that civilians with no military experience know former conscripts who did serve, from whom they might learn about conscripts' dispositions. In short, by increasing the proportion of citizens exposed to military service, conscription ultimately produces a society more attuned to the dynamics we outlined above about why conscripts defect.

Anecdotally, there is evidence of activists anticipating the relationship between conscription and military repression in line with our theory. Yassine Ayari, a leading activist in the 2011 Tunisian revolution and now a member of

Parliament, explained that it was because of conscription that he knew that the military would not fire on protesters:

> "The army was not going to intervene. [...] The soldiers are ordinary citizens coming from working-class districts, whom the raffle chose. They are there just for a year, it is not their job. They are not indoctrinated, [or] trained in repression. It is not their daily life" (Ayari, 2011).

Ayari's comments suggest that Tunisian activists understood that conscripts would be unwilling to repress, and were in turn more confident that nonviolent protests would not meet repression. This was certainly the case for Ayari, who goes on to state that he "encourage[d] everyone to go out in the street, and push[ed] everyone to fraternize with the army, something that I had no doubt [would work], which was later confirmed. It worked, it helped put an end to ZABA [President Zine El Abidine Ben Ali]" (Ayari, 2011).

In Egypt as well, several protesters in 2011 expected that conscript soldiers would side with them against Hosni Mubarak. "We know them and we know they are on our side now," one protester said.[9] Asked if he had friends in the army, the activist Saad Eddin Ibrahim responded, "of course," concluding therefore that "it is not in the tradition of the Egyptian army to fire on citizens."[10] Indeed, the links between society and the military were so strong that protesters explicitly chanted, "the army and the people are one hand" (Ketchley, 2014). And they would prove to be right. Defense Minister Hussein Tantawi reportedly told Mubarak: "the military is disinclined to fight the people in the streets; all our officers and soldiers have brothers or relatives demonstrating in Egypt's squares."[11]

In sum, we argue that conscription forges broader and denser linkages between military and society, thereby increasing the general public's knowledge of the military and its potential for defecting in the face of mass protests. In touch with conscripts and ex-conscripts, the general public is more likely to expect the military to defect, in turn encouraging them to protest. As a result, conscription undermines the regime's deterrent threat of military repression, inviting popular uprisings.

Toward Empirical Testing

Our argument that conscription increases rulers' vulnerability to mass protests raises questions about why rulers would ever adopt conscription. Existing literature suggests that the choice between conscription and volunteerism depends on a number of factors. Perhaps the clearest motive is security—states conscript when volunteerism cannot provide sufficient manpower to repel external threats or conquer adversaries (Posen, 1993; Asal et al., 2017). Beyond security, economic factors, such as high wage costs of volunteerism or highly

regulated labor markets, may incentivize conscription (Cohn & Toronto, 2016; Mulligan & Shleifer., 2005; Ross, 1994). Variation in societal values may make conscription more or less attractive, such as those imparted by colonial legacies (Asal et al., 2017). Rulers may also turn to conscription for state development, using the military as a "school of the nation" to instill civic virtues, foster nationalism, or otherwise educate the masses (Ardemagni, 2018; Leander, 2004; Posen, 1993), though the efficacy of nation-building through military service is empirically disputed (Krebs, 2004).

This discussion raises potential complications for testing our theory. Cross-national time-series analyses of conscription and nonviolent campaign onset may be confounded by other factors, such as external threats, that influence both the ruler's decision to conscript and the opposition's decision to mobilize. Though regression analyses can control for known confounders, this confounding cannot be fully alleviated via a solely correlational approach.

In light of this concern, we test our theory along two fronts. First, we conduct a survey experiment examining the individual-level micro-foundations of the theory, that is, that conscription decreases dissidents' expectations for military repression, thereby increasing their willingness to protest. The experimental framework allows us to identify this core causal mechanism free of contamination from confounding variables. We then turn to a cross-national regression analysis that examines the key implication of our argument, that is, that conscription is associated with increased nonviolent campaign onset. We find evidence to this effect even after accounting for other known confounding factors.

Algeria Survey Experiment

This section presents results from a survey experiment conducted during the 2019 Algerian Hirak protests that toppled 20-year President Abdelaziz Bouteflika.[12] We show that reminding Algerians about conscription decreased their expectations for military repression, and in turn increased their willingness to protest.

Why Algeria?

Algeria is a fruitful venue for our research for several reasons. First, protests were ongoing during the survey, so our survey captured respondents' expectations for military repression in the moment, not in hindsight or hypothetically. Because repression would directly affect their own lived experiences, respondents' expectations should have a high degree of internal validity—Algerians had almost certainly considered the risk of a crackdown before answering our survey. Relatedly, it should also be more difficult to influence respondents' real-world expectations for repression, as compared to those elicited from a hypothetical scenario. As such, evidence that an

informational prime about conscription influenced respondents' expectations for repression would constitute strong support for our theory.

In addition, existing scholarly accounts of the Algerian uprising postulate that conscription was one important reason why the military turned on Bouteflika. Though officers may have wanted to preserve Bouteflika, the soldiers who would have been responsible for repression were conscripts. As Ghanem (2019b) observed: "Many junior officers and enlisted men showed their support to the people because they identify with the average citizen. The Algerian army's ranks are filled by ordinary citizens, mostly from the lower and working classes, as conscription for 12 months is mandatory for all men from the age of 19." Grewal (2019b) likewise noted that "Nearly 70 percent of the army are conscripts. […] Conscription is universal, not just from one narrow ethnic or regional background. […] The military's composition […] may have rendered soldiers unwilling to fire." Al-Marashi (2019) concurred: "if the ranks of the military are conscripted from homogenous societies," like in Algeria, "soldiers would have trouble shooting at protesters who they see as their fellow citizens."

Thus, Algeria is a useful case to test our core hypothesis: that protesters, too, recognize conscription as evidence that the military will not repress. Available qualitative evidence provides initial support for this hypothesis. For instance, protest leader Mustapha Bouchachi emphasized the representative nature of the Algerian Army, telling protesters: "Don't lose hope. They will go. This is not an army that represents only one particular clan, as in Syria" (Dworkin, 2019). Similarly, protesters literally chanted "the army and the people are brothers, brothers" (Grewal, 2019b; McDougall, 2019). While this slogan mirrors those from earlier Arab Spring protests, it also reflects the opposition's recognition that the bonds between Algerian soldiers and society are relatively strong.

Survey Sample

Though illuminating, Algeria is also a challenging environment for research on civil resistance, due to both extensive state repression and animosity toward foreign researchers. Given concerns about enumerator safety, we chose to conduct our survey online, recruiting Algerians through Facebook advertisements. Facebook ads have become an increasingly common survey recruitment device, in both the US and the developing world (i.e., Cassese et al., 2013; Guiler, 2020; Samuels & Zucco, 2014). Following this scholarship, we purchased advertisements on Facebook (see Supplementary Appendix) shown to all adult Algerians.[13] Clicking on the advertisement took users out of Facebook and into Qualtrics, where they first agreed to a consent form, and then viewed the survey.[14] Between April 1 and 3, 2019, 1113 Algerians completed the survey experiment (see Supplementary Appendix for sample verification and validation).[15]

Our sampling strategy entails both benefits and limitations. On the one hand, the sample is not nationally representative. Only 45% of Algerians (19 million) are active monthly users of Facebook. These 45% skew younger, more male, and likely also more urban, wealthy, and educated. We followed Zhang et al. (2018) in implementing age and gender quota sampling during recruitment, bringing our sample more in line with the population on observable demographics (see Supplementary Appendix). Still, there are likely other unobservable differences between Facebook users and non-users, so we do not claim that our sample is nationally representative.

Still, our convenience sample remains analytically useful, for two reasons. First, our experiment identifies the effects of our informational treatments on respondents' expectations for repression, even if our sample does not permit descriptive inference about the full Algerian population. Second, the sample bias has some desirable properties for our study. In our case, the Facebook bias—urban, educated, connected—had the effect of oversampling protesters.[16] 58% of our survey sample claimed to have protested at least once since February 22. While there is no definitive figure on participation rates in the Hirak, it is likely that our survey oversampled protesters 5–10 fold. That is particularly useful since our theory speaks to the calculations that lead one to protest. Our sample therefore captures a substantively meaningful subset of the Algerian population—it matters how protesters understand civil-military relations, even if their perceptions differ from those of other Algerians.

Experimental Design

The 1113 respondents were randomly assigned to either a control group or one of five treatment groups, with about 185 in each group. The control group received no text. Each of the treatment groups received a factual, informational prime highlighting one of five characteristics thought to shape expectations of repression (Table 1). We anticipated that respondents were already aware of the information provided, but that reminding them would briefly heighten the salience of these factors as they answered the subsequent questions.

The first treatment group, Conscription, primed respondents to think about how the Algerian army is largely conscripted and thus representative of the people. It then asked respondents whether they personally knew any conscripted soldiers, to which 64% answered yes, validating our intuition that conscription is a highly salient feature of the Algerian Army. If our theory is correct, respondents primed about conscription should be less likely to expect military repression relative to respondents in the control group, and in turn be more willing to protest. If so, we also expect effects to be strongest among those who answer that they personally know a conscript, in line with our theoretical predictions.

The four remaining treatment groups primed other factors that could plausibly influence expectations about the military's behavior. While we did

Table 1. Priming Experiment.

Treatment	Text	Sample Size	Question
Control		216	NA
Conscription	"Military service is mandatory for all Algerian males. Nearly 70% of the army is composed of conscripts, making it representative of the people. Do you personally know any conscripts?"	175	Yes (*N* = 112, 64%) No (*N* = 62, 35%)
Corruption	"Many believe that military officers profit from corruption. In 2018, for instance, nearly a dozen senior military officers were dismissed on charges of illicit enrichment. Do you believe there is corruption in the military?"	186	Yes (*N* = 118, 63%) No (*N* = 63, 34%)
Past repression	"In October 1988, the military opened fire on protesters, killing several hundred Algerians. The military also cancelled elections in 1992 and killed thousands in the 1990s. Do you personally know anyone who was killed by the military in these events?"	187	Yes (*N* = 25, 13%) No (*N* = 161, 86%)
Russia	"Russia supplies more than 80% of the Algerian military's equipment. On March 19, Russia pledged support for Bouteflika's roadmap and expressed concern that the protests were destabilizing the country. Given Russia's support for the regime, the United Nations is unlikely to support democratization. Did you know about Russia's position?"	182	Yes (*N* = 71, 39%) No (*N* = 110, 60%)
United Nations	"At the Arab League summit on March 31, United Nations Secretary General Antonio Guterres said that he "welcomes the efforts toward a peaceful and democratic transition in Algeria." Did you know about this UN statement?"	167	Yes (*N* = 44, 26%) No (*N* = 123, 74%)

Final column does not include DK/refuse (*N* = 1, 5, 1, 1, and 0, respectively).

not pre-register our hypotheses, we expected each of these primes to shape respondents' attitudes, and thus to help benchmark the magnitude of the conscription effect. First, the Corruption condition reminded respondents about a recent, high-profile event where 12 officers, including five major generals, were dismissed on corruption charges (Ouali, 2018), and then asked

respondents whether they believed that there was corruption in the military (63% said yes). Empirically, corruption tends to produce vertical divisions during times of protest, with senior officers wanting to preserve their material interests, but junior officers and soldiers being more supportive of defection (Nassif, 2021). Second, the Past Repression condition highlighted the most recent and most prominent episode of military repression—the crackdown on protesters in 1988 and the coup and subsequent civil war in the 1990s—and then asked respondents whether they personally knew someone killed in these events (13% answered yes). This period was repeatedly invoked by the regime in the spring of 2019 to deter mobilization (Zeraoulia, 2020), and we would expect in the experiment for it to increase expectations of repression and discourage protest intentions.

The last two treatments highlighted international factors. The Russia condition primed respondents to think of foreign support for regime repression, noting that Russia is a major supplier for the Algerian Army and that Russia had also recently expressed concern about destabilizing protests. Only 39% reported knowing about Russia's position. Finally, the United Nations condition primed respondents to think of the UN's support for the protests, which likewise only few respondents (26%) knew about. We expected Russia's position to increase respondents' beliefs that the military would repress, in turn discouraging mobilization, while the UN treatment would do the opposite.

We subsequently traced the effect of these primes onto four variables. First, as our dependent variable, we asked respondents: "how likely are you to protest in the coming days?" on a four-point scale from very unlikely (1) to very likely (4). About 72% said they were likely or very likely to protest in the future, in line with the massive protests that continued for nearly a year after Bouteflika's ouster. If our theory is correct, respondents primed to think about conscription should report higher intentions of protesting.

To capture the mechanism, we asked three questions regarding expectations of military repression.[17] The first asked: "Suppose, hypothetically, that military personnel are ordered to repress the protesters. Do you think the military would agree or refuse to repress the protesters?" Respondents answered on a five-point scale from "very likely to refuse" to "very likely to agree." Expectations of repression were low: 74% said the military would refuse, while only 6% said it would agree (the remaining 20% answered "neutral").

We then asked two questions that help to disaggregate who in the military would refuse, and why. The first asked: "Soldiers would not repress the protesters because they are brothers." This question closely tracks with our theory that conscript soldiers will not fire because they identify with the protesters. About 86% agreed or strongly agreed, while only 3% disagreed or strongly disagreed.

The final question asked: "While officers may wish to preserve the system, soldiers will not fire on their countrymen." This question likewise emphasizes the likely refusal of soldiers, but puts it in contrast with the officers, to see if

respondents are attuned to vertical divisions within the military between the career officers v. conscript soldiers.[18] Indeed, 75% agreed or strongly agreed with this statement, with 7% disagreeing or strongly disagreeing. If our theory is right, reminding respondents that the military is conscripted should decrease expectations of repression across all three of these variables.

Results

Table 2 displays the results of a multivariate linear regression analysis of each dependent variable on our informational primes (for list of covariates, see Supplementary Appendix Table 8). The findings provide strong support for our theory. First, respondents primed to think of conscription were significantly (p = .005) more likely to say they would protest in the coming days compared to those in the control group (Model 1). The effect size is substantive, about 0.27 points on the 1-4 scale; dichotomizing the scale, treated respondents are about 13.4 percentage points more likely to say they will protest.

The results also suggest that this increased willingness to protest may be driven by lowered expectations of military repression. Respondents primed to

Table 2. Algeria Survey (April 1–3, 2019).

	Dependent variable			
	Will protest	Military will repress	Soldiers won't shoot	Officers may, soldiers won't
	(1)	(2)	(3)	(4)
Primes				
Conscription	.27*** (0.09)	−.21** (0.10)	.14* (0.08)	.22* (0.12)
Corruption	.12 (0.09)	−.20** (0.10)	.06 (0.08)	.28** (0.11)
Past Repression	.13 (0.09)	−.15 (0.10)	−.01 (0.08)	.16 (0.11)
Russia	.16* (0.09)	−.14 (0.10)	−.03 (0.08)	.34*** (0.11)
United Nations	.10 (0.10)	−.05 (0.11)	0.13 (0.08)	.15 (0.12)
Covariates	✓	✓	✓	✓
Observations	1011	1011	1009	1010
R^2	0.38	0.14	0.17	0.09
Adjusted R^2	0.36	0.12	0.15	0.06

*p < .1; **p < .05; ***p < .01. For list of covariates, see Supplementary Appendix, Table 8.

think about conscription were significantly less likely to say that the military would agree to repress protesters (Model 2, $p = .042$). Substantively, reminding respondents of conscription decreased expectations of repression by 0.21 points on the 1-5 scale, translating when dichotomized to respondents being about 8 percentage points more likely to say the military would refuse to repress.

Moreover, respondents primed to think of conscription were also more likely to pick up on who in the military would refuse, and why. Those respondents were more likely to say that the soldiers will not repress protesters because they are brothers, and that while the officers may want to repress, the soldiers will not (Models 3–4, $p < .1$). In other words, not only did priming conscription reduce expectations of military repression, but it appeared to do so in line with our precise theory about the preferences of soldiers, rather than officers.

These results paint a consistent story: conscription appears to reduce expectations of military repression and embolden respondents to protest. But is the increased willingness to protest driven by the reduced expectations of repression? We examine this causal pathway in two ways. First, following Baron and Kenny (1986), we show that the effect of the conscription prime weakens when controlling for any of the three mechanism questions, and that each of those strongly correlate with a willingness to protest (see Supplementary Appendix, Table 9). Second, more formally, we run a causal mediation analysis (Imai et al., 2010). Using any of the three mechanism questions, the analysis (Supplementary Appendix, Table 10) shows a significant mediated effect through decreased expectations of repression. While we should note that there is still considerable variation left unexplained, expectations of repression appear to be at least one of the links between conscription and willingness to protest.

That said, none of the other primes see consistent results across our outcome variables. In line with our expectations, priming respondents of corruption in the military reduced expectations that the military would repress (Model 2), and in particular appeared to highlight divisions between officers and soldiers (Model 4). However, military corruption had no impact on the likelihood of protesting. Meanwhile, priming respondents of the military's ties to Russia marginally increased protest intention (Model 1), as well as highlighted divisions between officers and soldiers (Model 4), perhaps because officers are the ones benefiting from Russian training. However, Russian ties did not appear to shape respondents' expectations of whether soldiers or the military at large would repress.

More surprising was the null effect of the past repression prime, given the centrality of repression to both the protest literature and Algerian history. Many scholars, for instance, have argued that the military's past repression deterred mobilization during the 2011 Arab Spring uprisings (Pearlman, 2013). One explanation for the null results in our survey could be that the military's past behavior was already a practically ubiquitous concern for

Algerians, even those in the control group. Indeed, the regime repeatedly warned protesters throughout 2019 of the civil war that erupted the last time Algerians mobilized for change. Accordingly, our treatment likely did not meaningfully increase the salience of repression for respondents.

Supplementary Results

To shed further light on why the conscription prime reduced expectations of repression and increased protest intentions, we divide the treatment group by whether they personally know a conscript. The analysis (Supplementary Appendix, Table 11) reveals that all results were driven by respondents who reported knowing a conscript, confirming our theory that interpersonal ties allow for better inferences about the military's expected behavior.

Finally, we examine two counter-explanations: whether the conscription prime might have increased anger toward the regime (for employing forced labor), or might have convinced respondents the military is not professional.[19] However, we find that the conscription prime had no effect on whether respondents believed that "most military personnel are professional" and likewise had no effect on whether they believed protesters should fight back if they were to be repressed, which we interpret as a proxy for anger. This latter finding likewise aligns with our intuition that conscription encourages nonviolent, but not violent, protest, something we explore further in the cross-national analysis.

Cross-National Analysis

To establish that our theory generalizes beyond Algeria, we now turn to a cross-national regression analysis of the relationship between conscription and protest mobilization. We find that conscription is positively associated with nonviolent campaign onset, even when controlling for a number of other potential confounding variables.

Data and Variables

Our analysis relies on several data sources. To start, the Nonviolent and Violent Campaigns and Outcomes (NAVCO) 2.1 dataset (Chenoweth & Shay, 2019) provides country-year data on 384 resistance campaigns from 1945 to 2013.[20] From these, we generate our primary dependent variable, *NVOnset*, a binary indicator that captures whether or not a nonviolent campaign was initiated in a given country-year.[21] This measure of campaign onset includes campaigns seeking regime change, major institutional and policy reforms, secession, and greater autonomy, and excludes anti-occupation movements. These latter campaigns lie beyond the scope of our theory, in that the

opposition is generally confronting foreign militaries. We also use NAVCO 2.1 to generate several secondary dependent variables, discussed below.

We pair this data on nonviolent campaigns with the Military Recruitment Data Set (Toronto, 2014), which provides *Conscript*, a binary indicator of military recruitment that serves as our primary independent variable. Per the Toronto (2014) codebook, *Conscript* identifies whether compulsory or coerced service is "the principle means of satisfying the military's manpower requirement" in a given country-year, though this does not require that a majority of soldiers are conscripted. The Toronto data initially ranged from the mid-1800s to 2008, but we extended the conscription indicator through 2013, to match NAVCO.[22] This leaves 1945–2013 as our temporal universe of cases.

Several notes are in order. First, Toronto's dichotomous coding of conscription, and consequently our analysis, masks a diverse array of conscription practices. Among other factors, conscription can vary in the form of recruitment (universal vs. selective), the extent of training, the length of service, and the type of deployment (reserves vs. active duty) (Toronto & Cohn, 2020), and this variation may well influence how dissidents perceive conscripts' allegiance to the regime. Data limitations prevent us from exploring this variation more fully. Still, our analysis is a reasonable first cut at the topic, as aggregating over variation in conscription should, if anything, make it more difficult to recover our hypothesized mechanism—including countries with highly selective recruitment and relatively few conscripts likely dilutes any observed effect of conscription on opposition mobilization.

In addition, and as mentioned earlier, states' use of conscription is thought to depend on political, economic, and cultural factors, some of which may also influence opposition behavior. Though our observational analysis cannot fully eliminate omitted variable bias, our models also include a set of controls intended to account for prominent confounding factors. We discuss these controls and confounding factors extensively in Supplementary Appendix.

Results

Table 3 presents results for our primary dependent variable, *NVOnset*. Model 1 demonstrates that conscription is associated with a significant increase ($p <$ .01) in the likelihood of nonviolent protest onset. Model 2 demonstrates that this correlation persists when controlling for covariates, including year fixed effects. This effect of conscription on protest onset appears substantive, as visualized by Figure 2; conscription roughly doubles the predicted probability of campaign onset in a given country-year, from 1% to 2% (recall that nonviolent campaigns are relatively rare events). This finding is also robust to different model and dependent variable specifications (see Supplementary Appendix Table 12).

Table 3. Conscription and Nonviolent Campaigns.

	Dependent Variable: Nonviolent Campaign Onset	
	(1)	(2)
Conscript	.011*** (0.003)	.009** (0.003)
War		−.011** (0.006)
Rivalry		−.009** (0.004)
MilSize (log)		−.113 (0.275)
MilSpend (log)		−.003** (0.001)
Democracy		−.020*** (0.005)
MilRegime		.010* (0.006)
BritCol		−.003 (0.004)
GDP (log)		.003 (0.002)
GDPChange		−.001*** (0.000 2)
PopSize		.003*** (0.001)
Youth		.181* (0.101)
Urban		.009 (0.010)
Mobile (log)		.001 (0.002)
Diffusion (log)		.011** (0.005)
PastSuccess		.004* (0.002)
PastDefection		−.002*** (0.001)
Region(Americas)		.008 (0.006)
Region(East Asia and Pacific)		.005 (0.006)
Region(Europe and Eurasia)		.011 (0.008)
Region(Middle East and North Africa)		.001 (0.007)
Region(South and Central Asia)		.016* (0.009)
Year fixed effects		✓
Constant	0.011*** (0.002)	−0.049 (0.032)
Observations	9857	7244
Countries	182	157
R^2	0.002	0.031
Adjusted R^2	0.002	0.021

*p < .1; **p < .05; ***p < .01. Robust SEs computed via Huber-White sandwich estimator.

To further explore the effect of conscription on nonviolent campaigns, we conduct several additional analyses within NAVCO's universe of campaign-years, displayed in Table 4. We first assess the relationship between conscription and nonviolent campaign Size, which estimates total popular participation in each nonviolent campaign-year.[23] Though protest size is likely affected by many dynamic and endogenous factors that arise during a nonviolent campaign, our theory would generally predict a positive association. We find some

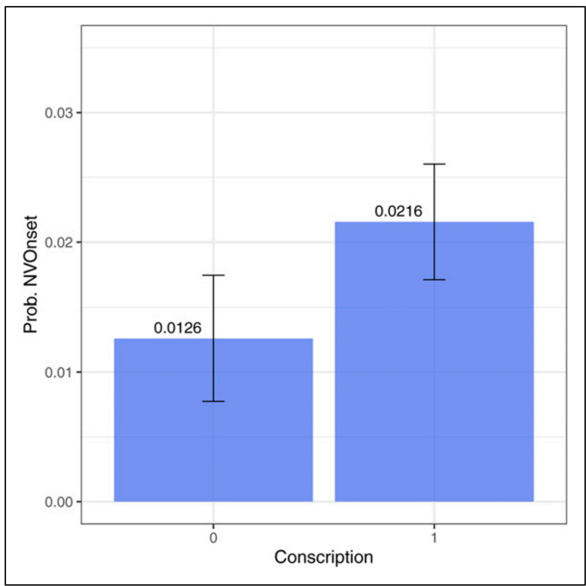

Figure 2. The effect of conscription on the probability of campaign onset. *X*-axis = conscription (1 = yes conscription); *Y*-axis = predicted probabilities from full model (Model 2), calculated via the R "prediction" package. Error bars represent 95% CIs.

Table 4. Campaign-Year Analysis.

	Dependent Variable		
	NV Campaign Size	Defection	NV Campaign Success
	(1)	(2)	(3)
Conscript	.689* (0.390)	.137** (0.065)	.138** (0.069)
Controls	✓	✓	✓
Constant	1.685 (3.569)	2.201*** (0.724)	1.712** (0.807)
Observations	316	330	338
Countries	73	71	73
R^2	0.390	0.319	0.241
Adjusted R^2	0.219	0.138	0.045

*$p < .1$; **$p < .05$; ***$p < .01$. Robust SEs computed via Huber-White sandwich estimator. For full results, see Supplementary Appendix B.2, Table 13.

evidence to this effect, as the conscription coefficient is positive and marginally significant in the presence of controls (Model 1, $p = .078$).

We also explore whether conscripted militaries are more likely to defect in response to nonviolent campaigns using *Defect*, which indicates whether a campaign provoked major security force defections that year. As the prevailing wisdom expects, we find that conscription is positively and significantly associated with security force defections in response to nonviolent campaigns (Model 2, $p = .03$). We subsequently examine whether conscription is associated with nonviolent campaign *Success*, defined as whether the nonviolent campaign achieved its maximalist goal that year. In line with our theoretical expectations, we find that conscription is positively associated with nonviolent campaign success (Model 3, $p = .045$).

A related implication of our argument is that conscription should influence activists' choice of resistance tactics. Our data suggests that when confronting conscripted armies, nonviolent campaigns are especially likely to elicit military defections and ultimately achieve their goals. In contrast, violent rebellions may not enjoy these benefits—it is far easier to justify repression of armed rebels than peaceful protesters (Chenoweth & Stephan, 2011), and even conscripts may have few qualms about repressing violent opposition if their lives are at stake.[24] Opposition activists may also identify more strongly with representative conscript militaries and thus reject violent resistance that would endanger conscripted friends and family.[25] For these reasons, we expect that conscription will be positively associated with nonviolent resistance tactics, as opposed to violent resistance.

We analyze this implication in two ways, presented in Table 5. Using our full country-year dataset, we first show that although conscription correlates with nonviolent campaign onset, it is not associated with violent campaign onset (Model 1). Then, among NAVCO campaign-years (including both

Table 5. Nonviolence versus Violence.

	Dependent Variable	
	Vio Campaign Onset	Campaign Tactics
	(1)	(2)
Conscript	−.002 (0.004)	.060*** (0.022)
Controls	✓	✓
Constant	.133*** (0.041)	−.250 (0.196)
Observations	7244	2018
Countries	157	103
R^2	0.039	0.208
Adjusted R^2	0.029	0.178

*$p < .1$; **$p < .05$; ***$p < .01$. Robust SEs computed via Huber-White sandwich estimator. For full results, see Supplementary Appendix B.2, Table 14.

nonviolent and violent campaigns), we analyze *Tactics*, a binary indicator for whether a campaign used primarily nonviolent or violent tactics that year. We find that conscription significantly increases the likelihood that a resistance campaign employs nonviolent tactics (Model 2, p = .02). Together, these findings support the intuition that opposition activists strategically employ nonviolent resistance against regimes with conscripted militaries. Overall, our results offer strong evidence in support of our hypothesized relationship between conscription and nonviolent campaign onset.

Conclusion

Recent scholarship has sought to explain why some militaries are more loyal agents of repression than others. Building on their insights, this article has shown that certain factors that dictate the military's loyalty may likewise shape activists' expectations for repression, thereby influencing the likelihood of nonviolent mobilization. We find that conscription is an especially salient indicator that the military is unwilling to repress peaceful protests. As a result, nonviolent mobilization is more likely to occur against regimes with conscripted militaries than those with volunteer forces.

To conclude, we discuss two implications of our argument for scholarship on civil-military relations and mass protest. First, our argument advances a growing body of research on the trade-offs rulers face in developing their coercive apparatuses. Forging the optimal security force requires rulers to strike a delicate balance—weak militaries cannot defend against the ruler's enemies, but strong ones may simply claim power for themselves (Feaver, 1999). Recent scholarship suggests that autocrats who coup-proof their security forces may succeed in avoiding coups but inadvertently weaken their ability to fight external wars (Talmadge, 2015) or effectively police their own citizens (Greitens, 2016). Greitens (2016) further observes that regimes are often conscious of these trade-offs and structure their coercive apparatuses in line with the balance of threats at the time of regime formation.

In this vein, our findings point to another trade-off between external and internal security in the design of coercive institutions. Conscription provides the manpower needed to fight wars and deter enemies, so rulers that face higher external threats are incentivized to adopt conscription. We show that this choice comes at the expense of the military's reliability as an agent of domestic repression and may therefore increase the risk of future domestic unrest. Moreover, conscription is a relatively stable institution—though conscription has become less common over time, Asal et al. (2017, p. 1473) find that states only rarely adjust their recruitment strategy, perhaps because doing so entails transition costs. As a result, conscription may leave rulers vulnerable to popular uprisings long after the initial threat that justified the choice of conscription has passed.

The second implication concerns the overlap between scholarship on civil resistance and civil–military relations. This article is one of the first studies to examine the links between military characteristics and opposition behavior, and the topic is ripe for future study. For instance, counterbalancing and ethnic stacking are two widely discussed coup-proofing tactics thought to condition the military's loyalty, but scholars have yet to explore corresponding implications for both the prevalence and tactics of opposition mobilization. Similarly, scholars have debated at length the importance of military "professionalism", yet we know little about whether dissidents understand the military in such a manner. This article posits that some military characteristics, such as conscription, are generally more observable to society than others. Building on this idea, future scholarship could explore variation in public awareness of other theoretically relevant military attributes. It could be that certain military characteristics become more or less visible in different contexts or in response to mediating factors. For instance, focal events (economic crises, high-profile court cases, etc.) could temporarily increase the salience of military corruption or composition, with implications for opposition expectations for military repression.

As a final note, we encourage scholars of civil–military relations to devote greater attention to the opposition. Much existing work focuses on military-regime linkages, treating military loyalty as primarily a puzzle for the regime to solve. These insights are invaluable but neglect the fact that civil–military relations also influence opposition behavior in meaningful ways. Though it can be difficult to obtain individual-level data on opposition behavior, especially in repressive states, this paper demonstrates that online surveys represent one (albeit imperfect) solution to this problem. We hope that future scholarship builds on our and others' efforts in this regard.

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Notes

1. Along these lines, Brooks & White, (n.p) contend that regimes that strike "grand bargains" between the military will experience greater protest onset.
2. For a helpful review, see Davenport (2007).
3. An expansive literature documents that repression sometimes invigorates mobilization, it what is known as the "backlash effect"—see Rasler (1996); Hess and Martin (2006); Lawrence (2017). Though our findings speak to this topic, we set the deterrence-backlash debate aside in favor of a more narrow focus on how opposition activists assess the likelihood of military repression before it materializes.
4. For instance, see Sayigh (2019) on the Egyptian Army's colonization of major industries, and Wehrey et al. (2009) on the Iranian IRGC's domination of black market trade.
5. For a fuller account of the diversity of conscription practices, see Toronto and Cohn (2020).
6. If the regime bends recruitment rules in order to exempt loyalists from conscription, then conscripted units will consist even more heavily of regime opponents. Tunisia even used conscription as a form of punishment for the opposition; see the case of Mohsen Marzouk.
7. Similarly, Vazquez & Powell, (Forthcoming) contend that because conscripted armies are more "socially representative" than volunteer forces, they are more likely to assent to democratization following coups.
8. These example strategies to avoid the draft were frequently given during author interviews with Syrian opposition activists.
9. Quoted in Arrow (2011).
10. Quoted in Weymouth (2011).
11. Quoted in Nassif (2021, p. 173).
12. Sparked by Bouteflika's nomination for a fifth term in February 2019 despite his evident infirmity, the Hirak successfully ousted Bouteflika on April 2. Weekly Friday protests then continued for nearly a year, as Algerians rallied for deeper, systemic political change (Davis, 2019; Ghanem, 2019a; Boubekeur, 2020). For more background on Algeria's protests, see Zoubir (2019) and Grewal et al. (2019).

13. Separately, we also targeted advertisements to Algerians Facebook believes are interested in the military, in an attempt to oversample military personnel. For this article, we remove all military respondents. However, many civilians were also recruited through this ad, who we leave in the survey sample, controlling for recruitment through the "military" ad. Results are also robust to removing these respondents entirely (results available from authors).
14. There are important ethical concerns about the data Facebook collects on its users. Because our study is conducted in Qualtrics, all Facebook learns is whether users engaged with or clicked on the advertisement. Facebook does not learn their answers to the survey.
15. The survey thus flanks Bouteflika's resignation, occurring in the evening of April 2, by 1 day on each side. We include a post-Bouteflika resignation control in every model.
16. This may simply be because protesters tended to mobilize using social media. See, for example, AFP (2019).
17. The first was asked prior to the future protest question; the latter two, after.
18. Anecdotally, at the same time that protesters chanted that the "army and people are brothers, brothers" they also chanted "Generals to the dustbin [of history], Algeria will regain its independence."
19. We are indebted to reviewers for raising these possibilities.
20. NAVCO defines a "campaign" as a movement with maximalist objectives, that has greater than 1000 participants in each year, and that demonstrates coherence in activities and organization over time.
21. Most NAVCO variables are measured at the country-year level—campaigns are nonviolent in some years, but violent in others. NVOnset includes all campaigns that began nonviolently, though some may subsequently become violent. Other dependent variables presented below operate at the country-year level.
22. We matched Toronto's (2014) data collection process as closely as possible, as described in the Supplementary Appendix.
23. To reduce missingness in NAVCO's "total.part" variable, we fill some missing values with the lower bound of the range provided by that campaign-year's "camp.size" variable when available.
24. Note also that violent insurgencies and rebellions in the NAVCO dataset are on average smaller and less diverse than nonviolent campaigns, weakening the "representativeness" link between these rebellions and conscript militaries.
25. We are indebted to an anonymous reviewer for this suggestion.
26. Separately, we also ran one advertisement that was targeted toward military personnel. We exclude all military personnel from this paper, though we include any civilians who were recruited through this ad (controlling for their ad in all regressions).
27. R code to detect duplicates obtained from https://github.com/andrewflowers/survey-fraud/blob/master/r_scripts/percentmatch.R.

28. Thompson and Dreyer's original rivalries data is right-censored, stopping at 2010. However, interstate rivalries are highly stable over time—the vast majority of rivalries ongoing in 2010 had existed for decades prior, and continue to this day. Our analysis therefore extends the original rivalries data by assuming that a state's rivalry status in 2011–2013 matches its value in 2010. Censoring the analysis to 2010 and/or excluding the rivalries control produces similar results.
29. While military crackdowns are uncommon in strong liberal democracies, repressive violence can occur in weak democracies or transitioning states, cases that are often of substantial import. For more on instances of so-called "murder in the middle," see (Davenport, 2007, p. 11).
30. On the causes of nonviolent mobilization, see Cunningham (2013); Chenoweth and Ulfelder (2015).

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