

Virtuous Shirking:
Social Identity, Military Recruitment, and Unwillingness to Repress

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Abstract

This study examines the effect of the breadth of military recruitment on the military's willingness to obey orders to use lethal violence to suppress nonviolent mass uprisings. Building on existing experimental evidence from social psychology, I theorize that lower social distance between the military and the protesters should result in a greater likelihood of virtuous shirking, or refusal to obey orders to kill civilians. I operationalize social distance in terms of whether the military is recruited through conscription and the degree to which the regime uses ethnic stacking to recruit the officers and rank-and-file soldiers. I also theorize that strategic anticipation of shirking by the military should affect the regime's decision of whether to employ the military against a mass uprising, the timing and tactics of mass uprisings, and the recruitment policy that is created in the first place. To test the theory, I use large- n statistical tests that employ original data on a number of key variables in one of the first-ever quantitative approaches to the subject. I also employ a four-country comparative case study that highlights the causal mechanism at work.

The theory receives mixed support in each of the stages – predicting military design, uprisings, and response to uprisings. As expected, internal threat to the regime tends to result in a military that is socially more narrowly recruited, and external threat tends to result in a military that is more broadly recruited, but this is only true for certain forms of each type of threat, and not the ones initially thought to be most important. The timing of nonviolent uprisings is more a function of idiosyncratic foreign and domestic factors and shocks than of anticipations of the military's response, although there is some evidence that ethnic stacking in the military tends to deter uprisings, and once mobilized, protesters facing conscript armies expect to be able to win the soldiers over to their cause. The statistical tests show no generalizable pattern in the response to uprisings, while the case studies reveal that while social distance matters in the direction expected, its effect is both highly contingent on other factors such as protester tactics and subject to countervailing efforts and tactics by the regime, including tactics attempting to artificially generate social distance between the military and the protesters.

To all human rights defenders: those willing to stand up to both tyranny and apathy so that they may “undo the heavy burdens, let the oppressed go free, and break every yoke.”

To my wife, for her very patient support through the long years of graduate school, and for the joy she takes in motherhood – a work that changes the world more effectively and lastingly than any research publication ever could.

Contents

1	<i>Virtuous Shirking</i>	1
1.1	Introduction	1
1.2	Virtuous Shirking and the Design of Military Recruitment Policy	4
1.3	Strategic Anticipation by Opposition Movements	7
1.4	Deployment Decisions:	10
1.5	Scope	12
1.6	Connections to Various Research Areas	13
1.7	Outline of this Dissertation	17
1.8	Policy Implications	18
2	A Model of Social Distance and Revolutions	20
2.1	This Study's Contributions	20
2.2	Prevailing explanations	24
2.3	A Model of Social Distance	27
2.3.1	Impact of Threat Environment on Social Distance (Stage 1)	28
2.3.2	Social Distance and Opposition Movements (Stage 2)	34
2.3.3	Social Distance and the Response to Nonviolent Uprisings (Stage 3)	35
3	Testing the Design Stage of the Theory	44
3.1	Introduction	44
3.2	Research Design	45
3.2.1	Unit of Analysis	45
3.2.2	Sample	45

3.2.3	State Leaders and Their Policies	46
3.2.4	Omission of Paramilitary Forces	47
3.2.5	Key Variables	48
3.2.6	Control Variables	57
3.2.7	Summary Statistics	59
3.3	Methods	61
3.4	Results	62
3.4.1	Relationships among the Dependent Variables	62
3.4.2	Determinants of Conscription Use	63
3.4.3	The Determinants of Ethnic Exclusion in the Officer Corps	70
3.4.4	Determinants of Ethnic Exclusion in the Rank and File	77
3.5	Discussion and Conclusion	85
4	Testing the Uprising Stage of the Theory	88
4.1	Introduction	88
4.2	Sample	89
4.2.1	Revolutionary Campaigns	89
4.2.2	Corroborating Campaign Onsets and Active Years	91
4.3	Methods	97
4.4	Key Independent Variables	98
4.5	Control Variables	99
4.6	Results	101
4.6.1	Revolutionary Campaigns	101
4.6.2	Robustness Check 1: Broadening the Sample to Include All NAVCO Campaigns	107
4.6.3	Robustness Check 2: Cross-Sectional Tests	116
4.6.4	Robustness Check 3: Accounting for Possibility of Endogeneity	118
4.7	Conclusion	119
5	Testing the Response Stage of the Theory	123
5.1	Introduction	123
5.2	Sample	125

5.3	Research Design, Dependent Variables, and Methods	126
5.3.1	Deployment	127
5.3.2	Shirking	128
5.4	Key Independent Variables	129
5.5	Control Variables	130
5.5.1	New Variables	130
5.5.2	Variables Previously Introduced	131
5.6	Results	133
5.6.1	Deployment	133
5.6.2	Shirking	137
5.7	Conclusions	139
6	Exploring the Causal Mechanisms at Work	141
6.1	Introduction	141
6.2	Case Selection	143
6.3	Military-Design Stage	146
6.3.1	East Germany	146
6.3.2	Romania	147
6.3.3	South Korea	148
6.3.4	Jordan	150
6.4	Uprising Stage	152
6.4.1	East Germany	152
6.4.2	Romania	154
6.4.3	South Korea	156
6.4.4	Jordan	158
6.5	Deployment and Military-Response Stage	159
6.5.1	East Germany	159
6.5.2	Romania	160
6.5.3	South Korea	162
6.6	Conclusions	164

6.6.1	Military-Design Stage	164
6.6.2	Uprising Stage	165
6.6.3	Deployment and Military-Response Stage	166
7	Conclusions	168
7.1	Theory: Recap	170
7.2	Summary of Findings	171
7.2.1	Military Design	171
7.2.2	Mass Protests	172
7.2.3	Deployment and Shirking	172
7.3	Recommendations for Future Work	174

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Chapter 1

Virtuous Shirking

1.1 Introduction

After failing to quell the 2013-14 Euromaidan mass protest using internal security forces, the fate of the Ukrainian regime rested on the willingness of the country's conscript military to follow through on orders to crush the opposition. This protest movement, over an issue that came to be seen as a tug-of-war between the European Union and Russia for influence in Ukraine, had by this point lasted for three months. Although there had been some police-protester clashes and an incident involving looting of weapons from a military armory,¹ the protest movement had primarily relied on nonviolent tactics. As the protests had continued through February, it became clear that the capacity of the internal security forces was not going to be sufficient to force an end to the campaign, despite those forces' demonstrated willingness to brutalize civilians encamped in Independence Square.² In response to police violence, the protest movement had swelled to the hundreds of thousands, and the protesters' demands now included the extra-constitutional removal of the president. Yanukovych had to either call in the military to crush the protests or accede to their demands.

Faced with orders to deploy against the protesters, the head of the armed forces Colonel General Volodymyr Zamana refused to pass on the deployment orders. He was immediately dismissed and replaced with someone more pliable, Admiral Yury Ilyin.³ Military deployments to Kiev began,

¹Charles Miranda, "Army on Civil War Footing," *Herald Sun* (Australia), 21 February, 2014.

²For example, see Oksana Grytsenko, "Ukraine's Bloody Crackdown Leads to Call for Sanctions," *The Guardian*, 30 November, 2013. <http://www.theguardian.com/world/2013/nov/30/ukraine-bloody-backlash-sanctions-eu> (November 11, 2015).

³Reuben F. Johnson, "Ukraine Sacks Reformist CGS for Refusing to Send in Troops," *IHS Jane's Defense Weekly*,

but the actions of the initial group of soldiers who were deployed indicated that carrying out the full-scale deployment would likely result in widespread mutiny: “. . . [D]espite Ilyin’s willingness, the army did not grapple with the demonstrators. A few convoys of paratroopers left their barracks in buses and trains but were stopped on the way by protesters’ roadblocks and some of the soldiers and officers refused to carry out their orders . . . [These initially deployed] paratroopers succeeded in keeping the military from intervening in the internal conflict that is tearing Ukraine apart.”⁴ In response to these units’ refusal to use violence against the protesters blocking them from reaching Kiev, both military and civilian leaders anticipated similar disobedience from the rest of the military if deployed. With his regime’s last remaining support having failed, Yanukovych fled the country and found asylum in Russia. Shortly afterward, a journalist commenting on the events provided this diagnosis: “Ultimately, Mr. Yanukovych . . . forgot the critical lesson from all recent revolutions: That a national military which depends on conscripts is always reluctant to fire on its own people.”⁵

Ironically, within weeks the leaders in the new Ukrainian regime found themselves in a similar situation. Although the situation has since degenerated into full-fledged civil war, initial attempts to send the military to crack down on the popular rebellion in eastern Ukraine witnessed similar defections and refusal to fire on civilians. For instance, in April 2014, Ukrainian infantry fighting vehicles were sent into the city of Kramatorsk to restore central-government control, but they were immediately surrounded by a crowd of unarmed civilians intent on preventing the soldiers from carrying out their mission. After a long standoff, and with no other options than to either open fire on the civilians or to surrender, the soldiers opted for the latter.⁶ When the captured vehicles rolled through neighboring Slavyansk flying Russian flags, accompanied by the same Ukrainian military personnel, one of the soldiers explained to a Reuters press agent that “All the soldiers and the officers are here. We are all boys who won’t shoot our own people.”⁷

Anecdotal evidence abounds for the role of social distance affecting military response to mass protests in nondemocracies. In many of these cases, the military identified with the populace

20 February, 2014. <http://www.janes.com/article/34418/ukraine-sacks-reformist-cgs-for-refusing-to-send-in-troops> (May 28, 2014).

⁴Anshel Pfeffer, “Amid Crisis, Ukrainian Military’s Neutrality Hangs in the Balance,” *Haaretz* (Israel), 24 February, 2014. <http://www.haaretz.com/news/world/.premium-1.575987> (May 28, 2014)

⁵Jonathan Eyal, “Ukraine Too Important to be Left Alone,” *The Straits Times* (Singapore), 24 February, 2014.

⁶Alec Luhn, “Ukrainian Troops ‘Demoralised’ as Civilians Face Down Anti-Terror Drive,” *The Guardian*, 16 April, 2014.

⁷“Pro-Russia Rebels Claim Army Defections, Tank Seizures in Ukraine’s East,” Al Jazeera America, April 16, 2014. <http://america.aljazeera.com/articles/2014/4/16/ukraine-russia-rebels.html> (May 28, 2014).

because of widespread recruitment from conscription and refused to carry out orders to violently repress the protests. For instance, the Egyptian military that Mubarak ordered to deploy in 2011 against the masses assembled in Tahrir Square was, like the Ukrainian example above, recruited through conscription. This meant that beside the senior officers, “the rest of the army is really a cross-section of the Egyptian people. Because of conscription, young men from all classes, from all groups of society . . . get into the army.”⁸ In addition, “[a]lmost every family in Egypt has a son, brother, cousin or other relative either in the army or who has served in a force that is regarded highly as a national institution.”⁹ One scholar put it this way: “Egypt’s conscript army has so many ties to society at large that, even had the generals been willing to shoot demonstrators, many officers and enlisted men would probably have refused to obey such an order” (Barany 2011, 32-33). Instead of firing on the protesters, most soldiers refused to shoot into the crowd and at times even physically intervened to protect the protesters from the state police.¹⁰

In other cases, state leaders had assiduously put as much social distance between the population and the military as possible in anticipation of having to use the military against mass protests, and the military followed through with orders to repress. For example, the Bahraini military is overwhelmingly drawn from the ruling Sunni minority, and it did not hesitate to use violence to quash the predominantly Shi’ite incipient revolution when ordered to do so. Bahrain’s ruling family has been very deliberate about its policy of over-recruiting from one ethnic group to the exclusion of the other main group: the military in Bahrain “is a fighting force of Sunni Muslims who are charged with protecting a Sunni ruling family and Sunni political and business elites in a country . . . where about three of every four or five people are Shia . . . [and] Bahrain has no conscription precisely because its ruling elites do not want Shias bearing arms and receiving military training” (Barany 2011, 35-36).

The influence of social ties between the military and the populace appears to be a strong factor in shaping the military’s response to mass protests, but this factor is certainly not determinative or causally sufficient, as shown for instance by the case of China in 1989. The Chinese military

⁸Mary-Jane Deeb, “As Egypt’s Protests Spread, All Eyes on Army’s Allegiance, Next Moves,” PBS NewsHour, January 28, 2011, www.pbs.org/newshour/bb/world/jan-june11/egyptguests2_01-28.html (May 8, 2012).

⁹Eva Haroun, “Egypt: A New Tomorrow Dawns,” *allAfrica*, February 19, 2011, al-lafira.com/stories/printable/201102190012.html, (May 8, 2012).

¹⁰“The Role of the Army in Popular Uprisings,” *Temasek Review*, February 13, 2011, www.tremeritus.com/2011/02/13/the-role-of-the-army-in-popular-uprisings/ (May 8, 2012).

was recruited through conscription at that time (Dreyer 1989, 648-649) and from the Han Chinese ethnicity that makes up the majority of the population (Heaton 1981, 176). When student protests arose in Tiananmen Square, the force initially deployed against the protesters was the 38th Army, which was stationed in Beijing where the protests were taking place. Ties between that force and the protest movement were strong, even to the point that “[s]ome soldiers had friends and family members in the movement” (Nepstad 2011b, 25). After martial law was declared, as one journalist who was present during the protest relates, “There were four or five attempts to bring garrison troops into Beijing that night, and the commanders and the soldiers didn’t want to do it. I saw them myself turn back. They didn’t have the stomach for it . . . [because] the People’s Liberation Army had always ‘swam in the people’ as Mao said . . .”¹¹ Up to this point, in the same way as in the above cases, social ties between the soldiers and the protesters were helping stave off violent repression. In order to put down the pro-democracy movement, the Chinese leadership eventually brought in military units from distant parts of China, who were willing to shoot into the crowd. The degree of social distance between these replacement units and the protesters was minor, though – not, as some (e.g., Nepstad 2011b, 35-36) claim, due to any difference in ethnicity (Dreyer 1989, 651-652). The soldiers who were willing to kill hundreds of protesting students shared with their victims the same national and ethnic identity, and were drawn from a conscript military indoctrinated with the view that it existed to serve and protect the people.

1.2 Virtuous Shirking and the Design of Military Recruitment Policy

Do social ties between the military employed by a non-democratic regime and a mass-protest movement challenging that regime make that military more likely to refuse to carry out orders to crack down? If so, then how does the prospect of virtuous shirking by the military affect state policies regarding military recruitment? These are the first two research questions that this dissertation addresses. To show the need to address these questions, we first need to set this phenomenon in its broader context. As earlier studies have done (e.g., Feaver 1998, 2003; Gates 2002; Pion-Berlin and Trinkunas 2010;

¹¹Peter Ellingsen, “Anniversary of Tiananmen Square,” June 4, 2014, ABC Radio (Australia), <http://www.abc.net.au/radionational/programs/latenightlive/anniversary-of-tiananmen-square/5500516> (17 June 2014).

Weinstein 2007), we can cast the question of whether security forces will obey or disobey orders in principal-agent terms. The principal (here, the state leadership) employs the agent (here, the military) to do a job that the principal is not able to perform on its own. In this context, that job is to protect the state – and, in nondemocracies, that includes the regime as well – from security threats.

Obedience by the agent to the principal’s wishes is normally termed *working*, while disobedience is termed *shirking*. The origin of this framework in economics is apparent from these terms, as business owners are mainly threatened by the possibility that their employees will stand idle, given the opportunity, while still getting paid for it. However, shirking includes any form of disobedience to the principal, not just idleness.

Two factors enable shirking to take place. The first of these is that principal and agent do not fully share the same interests. The second factor is private information. Divergent interests are a necessary condition for shirking, and these two factors may be considered jointly sufficient. Economic applications of the principal-agent framework take divergent interests as a given, since employees are assumed to want more pay for less work and the reverse for their employers, and as a result, economists focus primarily on the role of private information. Workplace security cameras and employee drug tests are means by which employers attempt to reduce the space for shirking to take place by reducing the amount of information about employee behavior that is private to the employee.

Expertise creates one particularly important form of private information. When an agent is hired to do a job that the principal lacks not only the time but also the skill or knowledge to accomplish, the agent’s behavior is opaque; even if the principal could continually monitor the agent, it would not be clear at all, at least in the short run, whether the agent is actually carrying out the principal’s wishes or not. This aspect of the principal-agent framework is what studies of bureaucratic delegation tend to focus on, since the main purpose of bureaucracy is to provide elected leaders with professional expertise required to implement government programs. Members of Congress, for instance, have great difficulty trying to micromanage federal scientific agencies.

Despite the usual focus on private information, divergent interests play an even larger part. If the agent had exactly the same interests as the principal, private information would be rendered harmless, because the agent would be trying to achieve the same objective that the principal

would seek. Further, shirking can take place in the absence of private information. If the principal makes himself or herself particularly vulnerable to the agent, then a betrayal, although completely open to the principal’s view, may be disastrous. In the same way that celebrities and politicians are vulnerable to their own bodyguards, citizens in democracies choosing their own leaders place themselves in those leaders’ hands, running the risk that those leaders will use their delegated power to overthrow the democracy. As Lane (2008, 10) states, “When the principal cannot get rid of the agent, it may not matter much that the principal can monitor the agent.”

The military constitutes an essential pillar of support for any nondemocratic regime. Loss of the military’s support has been described as necessary although not necessarily sufficient for a domestic revolutionary movement to overthrow the regime (Barany 2011; 2016; Blanga 2014; D’Anieri 2006; Nepstad 2011b, 2011a; Russell 1974). Empowering the military to make it strong enough to confront the domestic opposition creates potential for moral hazard, as the military may choose instead to turn the guns against the regime, leaving the state’s leadership defenseless (Svolik 2013). Thus, the principal-agent relationship present between the state and the military represents a severe case of vulnerability of principal to agent.

State leaders certainly recognize this vulnerability. The threat of coups usually prompts nondemocratic regimes to build up large and ideologically fervent paramilitary forces to serve as counterweights against attempts by renegade officers and their loyal factions to capture the capital (Belkin and Schofer 2003; De Bruin 2014; Powell 2012). However, in a contest between the regime and a mass-protest movement, if the movement has swelled beyond the capabilities of the police and paramilitary forces to control, the military represents the last and most powerful bulwark of support for the regime. Should the military as a whole defect in such a situation, there are not enough paramilitary forces to be capable of disciplining the mutinous soldiers. Divergent interests are, therefore, an essential starting place to begin in analyzing military obedience in the context of mass protest.

In most applications of the principal-agent framework, shirking is portrayed as normatively bad. However, in a context where faithfully following through on the principal’s orders results in large-scale human-rights abuses, the normative connotation is reversed. Refusal to use violence against civilians, especially unarmed civilians, is now the normatively good outcome. In other words, in this context *shirking is virtuous*.

For scholars and policymakers interested in protecting human rights and preventing violent repression, being able to explain the real-world variation that we see regarding the willingness of militaries to repress anti-government movements takes on great significance. To understand how confrontations between the military and mass-protest movements arise in the first place, we must first focus on the institutional-design aspect of the problem, which is directly under the control of the state leadership: how do autocratic states design the military to avoid virtuous shirking?

1.3 Strategic Anticipation by Opposition Movements

The next question that this dissertation asks is based on anticipation of military behavior should the military be deployed to put down mass protests by force: *How does the prospect of virtuous shirking by the military affect the willingness of civilians to join in opposition movements engaging in direct anti-government action, and the character that this action takes – violent or nonviolent?*

I assume that civilians are rational and weigh the costs and benefits of participating in protest movements before deciding to join. While some of the costs or benefits may be entirely psychological and expressional – such as if civilians fed up with persistent rights abuses or economic misery obtain utility from venting against their rulers, similar to the participatory motives that drive rational models of voting (Riker and Ordeshook 1968) – those civilians each individually face a very real cost from being jailed or shot if the regime cracks down on the protests.

There are frequently small handfuls of individuals who are willing, whether due to being naturally risk-acceptant, exceptionally grieved against the regime, or ideologically committed to its overthrow, to engage in anti-government action like protest or even insurgency, but these efforts usually do not get very far. Nondemocratic regimes, especially more cohesively authoritarian ones, tend to dedicate enormous resources into quashing organized resistance to their rule, so incipient revolutions never even make it off the ground most of the time. Police and paramilitary forces see to that. However, there is safety in numbers, and rational civilians who would like to protest the regime might choose to do so if they think that the movement will attract enough of their fellow civilians to make the movement strong enough to overcome attempts by the internal-security forces to put down the protests by force. This tends to be a self-fulfilling prophecy: assuming sufficiently widespread grievances, if everyone really thinks that a protest movement has a good chance to become large

enough to be impervious to challenge from the internal-security forces, then enough people join, and quickly enough, to bring that scenario to pass. This basic model also helps explain why protests of revolutionary proportion can materialize almost overnight.

The calculation of costs and benefits for joining an opposition movement is also contingent on everyone's anticipation of what the military will do. Since armies generally command much greater firepower than state police forces, mass protests of nearly any size are still vulnerable to being crushed by the military, if the military is willing to follow through on orders to turn its guns on the amassed civilians. To survive, a mass protest would have to inspire widespread defection within the military. Therefore, we could expect protests to grow large enough to pose an "end-game" scenario to the regime only when potential protesters anticipate that the military will not follow through if orders to repress the protests are given, or that the orders will not be given in the first place.

This is where choice of tactics comes in. If the hardcore set of individuals who tend to be the ones found leading opposition movements, come rain or shine, fail to rally large-scale support from the public, or anticipate doing so, we would expect them to be more likely to resort to violent tactics. Insurgency requires a much lower degree of support from the general public to get off the ground than nonviolent protest. On the other hand, if they believe, such as because of their expectations about military behavior, that they will be successful in convincing the public to rally with them in the streets or engage in mass strikes, then this would make them more likely to resort to nonviolent tactics.

This discussion so far highlights an interesting puzzle: If opposition leaders and civilians are able to anticipate military responses based on what they can directly observe about the strength of social ties between the military and society at large, why would mass protests ever materialize, only to be crushed by the military?

Several possible answers are worth mentioning in response to this puzzle. The first and easiest response is perhaps they simply got it wrong. Despite careful efforts to assess military loyalty, loyalty is ultimately an intangible factor that resides in the minds of the soldiers and is ultimately known to no one but the soldiers themselves – and perhaps not even to themselves until the moment of truth, when faced with having to actually pull the trigger. This is also true of social ties: while civilians may perceive strong ties between the society and the military, what really matters is how the military and individual soldiers perceive, and feel, the strength of those ties. Directly observable

factors that influence such ties are objective but imperfect indicators, so it is theoretically possible for opposition movements to miscalculate here.

Second, as a theoretically causal factor, the presence of strong social ties is not expected to be sufficient on its own, as highlighted for instance by the Tiananmen Square case described above. Other factors that also affect military loyalty to the regime, factors that may even be unobservable to the public, may completely outweigh the influence of social ties and persuade soldiers to side with the regime even to the point of shedding the blood of fellow countrymen and -women.

As discussed in Chapter 2, the current state of the literature on the subject of military response to mass protests highlights a wide variety of potentially causal factors, and although large- n analyses on the subject are few and far between, the extant case-study work has certainly shown many of these to be plausibly influential.

Another possible explanation centers on the complexity of military design. For the most part, studies of military loyalty treat the military as a cohesive unit and ask “what did the military do?” In reality, militaries are complex, bureaucratic organizations and contain hierarchical structures of command. By disaggregating the vertical structure by rank, we see the possibility that the interests of individuals at one level of that hierarchy, the enlisted soldiers, might be completely different from the interests of the officers who command them. Similarly, mid-level officers who directly command units may have very different interests than the top brass. State leaders place enormous weight on ensuring that the generals remain loyal, in order to prevent military coups, and less weight on the loyalty of field and company officers and the enlisted soldiers. In the event of orders to deploy against mass protests, the military may be divided internally based on the different interests of the different ranks comprising it, making the actual outcome the result of an unpredictable struggle of willing officers to motivate unwilling enlisted soldiers, or of willing senior commanders to motivate unwilling companies to carry out those orders.

A similar factor that is also based on military design is the possibility for horizontal differentiation of military forces. A single elite regiment, drawn narrowly from society to ensure its loyalty, may provide enough firepower to quell a protest many-thousands strong, and authoritarian regimes often maintain such regiments for the specific purpose of maintaining internal security. Especially if we add to this the possibility that the military may bifurcate into pro-opposition and pro-regime factions, it may be simply impossible to predict the behavior of every portion of the military and

how intra-military struggles will play out.

1.4 Deployment Decisions:

The final question that this dissertation addresses is also based on strategic anticipation of military behavior: *How does the prospect of virtuous shirking by the military in the event of mass protest affect state policies regarding military recruitment, and given actual mass protests, state leaders' willingness to deploy the military?*

This is where we return to the original framework proposed and address the set of choices and policies available to the principal to try to ensure loyalty from the agent, well before that loyalty is tested. The policy area that I focus on in this study, military recruitment, is crucial to examine to understand how the interests of the military are determined. The extent to which the soldiers and officers are drawn narrowly or widely from society should have a strong effect on whether the military's interests diverge or converge with the regime's interests regarding how to deal with mass protests.

State leaders presiding in turbulent times, as well as those who face a strong but latent possibility of revolution, know that their political survival may very well depend on the willingness of their military to defend them against domestic opponents. When threat of political violence or mass action is high, all else equal, we can expect state leaders to narrow the base of recruitment to those sectors of society that they consider the most loyal politically.

Whether the military is ultimately deployed against mass protests is also within the control of the state leader. This decision, based on the same consideration of ensuring the leader's and the regime's political survival, should be simple and straightforward: if the military's composition is favorable to the regime, then deploy. However, if the composition of the military is such that its loyalty to the regime cannot be ensured, and the opposition movement has swelled to end-game proportions, we would expect the leader to opt for some alternative, such as reaching for concessions to offer the protesters; or if their demands include the leader's removal from power, to begin working toward a negotiated transition from power that still leaves the leader with some influence (or, if nothing else, with his or her life).

Two additional puzzles emerge here, both of which I address in this study. First, why would state

leaders ever find themselves caught off guard? One vital answer lies in the same factor, international constraints, that Skocpol (1979) criticized studies of revolution a generation ago for not taking into account sufficiently. The role of the international environment in which the state operates, especially the security environment, is still under-theorized in studies of revolution, especially those that focus on military responses to revolution. States do not operate in a vacuum, and international security threats may constrain the leader's ability to mold a military suitable for domestic policing. As the force of last resort for both internal and external security needs, the military occupies a key place at the intersection of the domestic and the international. Unfortunately for state leaders, the logic of designing a military that can protect the regime against domestic enemies is expected to clash, under certain conditions, with the logic of designing a military that can protect the state from external threats.

In addition to its impact on military recruitment policy, the international security environment can also shape state leaders' responses to mass protests. This is where the next puzzle comes in: we see state leaders sometimes staking their political survival – and, potentially, their actual survival – on a military that they cannot fully trust to back them up. Often, leaders make that gamble and, just like opposition movements that end up getting crushed by loyal armies, leaders sometimes get it wrong. They should not be expected to take the risky step of deploying the military if they were anything less than absolutely confident that the ensuing confrontation would play out well for them. Again, international constraints have a vital explanatory role to play here. States often cannot seem to resist the temptation to meddle in their neighbors' affairs when those neighbors are wracked by domestic turmoil. While there is the possibility of the regime obtaining support from foreign patrons in its fight against a growing domestic opposition, there is also the possibility that rival states may throw their lot in with the opposition and provide the opposition movement with material resources or diplomatic support. In some cases, rival states may even choose to intervene militarily, sometimes under the pretext of protecting the protesters or the social groups that they represent from human rights abuses. Risky decisions to deploy a military of uncertain loyalty against a mass protest may represent an attempt to contain the conflict and prevent it from internationalizing.

1.5 Scope

This study is limited to revolutionary mass-protest movements, where the size of the protests and the severity of demands present a major threat to the regime.¹² Because of its focus on actual or incipient revolution, this theory would not necessarily travel to interactions between the military and small minority groups; although possible that it might, this is a question better addressed by future studies. Similarly, although military deployment against mass protests, sometimes resulting in virtuous shirking, is not unknown in democracies (Pion-Berlin, Esparza, and Grisham 2014; Pion-Berlin and Trinkunas 2010), the comprehensive treatment of the subject, from military design up to the loyalty decision, provided in the present study deals strictly with autocracies and saves the extension of the theory to democracies for future work.

In addition, because this study is focused on the loyalty of the military, rather than repression or success of the protest movement, I do not focus on the use or the behavior of internal security forces, the usual actors tasked with protest policing. While militaries sometimes have domestic policing as their primary role, the military in any state serves as its only self-defense against external threats. Usually the only body fulfilling the state's core function of providing defense, it is one of the most important symbols of the nation and its sovereignty. State police forces, on the other hand, are unambiguously the bulwark of the regime against insurrection and revolution. As a result, the nature of employment and self-identity for those employed in the state police or other internal security forces and their interactions with protesters is expected to be very different.

This study also focuses only on the street-level bureaucrats in the military, not military elites. The theory and empirics specifically address the behavior of the enlisted soldiers and the junior officers, those at the front lines, as opposed to the behavior of the senior officers, although the behavior of the latter could be in response to the actions of the soldiers and junior officers. Although senior officers generally pose a larger threat to the regime through the threat of a coup d'état, and while coups may take place in response to mass protests (Albrecht and Koehler n.d.; Hiroi and Omori 2013), I do not focus on coups in the context of the current study. The main causal mechanism under study in this work is the psychological factor of shared identity, and those who

¹²The focus in the final stage of the theory, modeling regime and military response to the mass protests, is solely on the nonviolent variety of this kind of movement; the intermediate stage focuses on the choice of violence versus non-violence.

are actually ordered to use force against the anti-government protesters are the ones who have to overcome internal restraints against harming fellow humans. Those in the higher echelons of the military are so far removed from the scene of the confrontation that the shared-identity factor is not expected to be at work in determining their behavior.

1.6 Connections to Various Research Areas

The theory that I present draws from two approaches that are sometimes portrayed as diametrically opposed, a social-psychological approach and a rational-choice approach. The decision to focus first and foremost on the psychological difficulty that individual soldiers might experience if required to shoot into a crowd of civilians, particularly civilians with whom they can identify because of shared social ties, might appear to some to be incompatible with rationalism. However, over the years since the rational-choice approach migrated from its origins in economics to political science applications, this approach has gradually become more sophisticated in its scope, even to the point of starting to include intangible factors. Recent work, for instance by Gates (2002) and Gates and Nordås (2010) regarding loyalty in insurgencies, has shown that psychological inputs can inform formal models. Social or ethnic distance, ideological or religious principles, or other ideas can represent the source of preferences that individuals hold regarding various courses of action, which we can then take into account using standard rational-choice methods (see also Fearon and Wendt 2002)

In this study, I draw from research on social psychology to help understand why soldiers in militaries with strong social ties to the society they are charged with either defending or policing would prefer not to crack down violently on protesting civilians, or in the alternate case, why they would have few qualms about doing so. The reliance in this theory on inputs that are largely ideational, as opposed to largely material, does not lessen the applicability of rational-choice assumptions to this context. For the most part, the remainder of the story that the theory tells about how we get from military design to protester-military confrontation is based on rather more concrete material and political incentives, such as, for the leader, remaining in power, or for the opposition, capturing power or at least not dying, but I do also draw on social distance as a factor influencing the preferences of foreign states who might be weighing the decision to intervene on behalf of the regime or the opposition.

The implications of this study extend directly to a variety of research topics. The most direct application is to studies of military loyalty during revolution, a small body of research that has roots going back for several decades. Until the 2011 Arab Spring protests, studies on this subject could hardly be called a literature, existing rather as disparate work all weighing in from various sub-disciplinary and even inter-disciplinary perspectives. Scholars of revolution, civil-military relations, authoritarian regime maintenance (and the closely related subject of democratization), repression / human rights, and nonviolent action had each contributed sporadically over the decades. After the Arab Spring – which was also the source of inspiration for the present study, with the military-protester-confrontation portion of the theory that I present having been inductively generated from those events – attention to this subject increased dramatically from academics and journalists alike. Proponents of various theories of military behavior in reaction to mass protests began to engage in mutual dialogue, and for the first time work on this subject began to operate as a somewhat cohesive literature. These studies widely tend to follow the traditional comparative politics / international relations divide and focus only on domestic factors, which means that they are not able to tell the whole story of how the military came to be in its present form and under what conditions it would actually be deployed against mass protests.

By focusing directly on the military-design and deployment aspect, this study speaks directly to the larger subject of institutions in authoritarian regimes (e.g., Svolik [2012](#)). Often, leaders of authoritarian regimes create or design institutions in ways meant to sustain their rule, but these institutions may then take a life of their own and can even sow the seeds for the regime’s own destruction. As an institution, the military in an autocracy is designed with regime-maintenance in mind, but policy changes in this area, especially recruitment policies, tend to have a slow-moving impact, as it may take years for individuals to filter out of the military once recruited or promoted. While state leaders have control over the military’s composition in the long run, short-term change is very difficult to achieve, which may prevent the regime from responding to a changing security situation that suddenly requires a different form of military composition to help preserve the regime.

This focus on the decisions that state leaders face regarding military design and deployment also speaks to the broader civil-military relations literature. Generally, this literature tends to be divided into research of civil-military interactions in developed, Western democracies on the one hand and interactions in autocratic or democratizing states on the other. Because of the evident

immunity of developed democracies to overthrow (e.g., Przeworski and Limongi 1993), the latter are primarily concerned with coups d'état, while the former are generally focused on maintaining Huntingtonian (1957) “objective” military control in the sense of giving the military autonomy over intra-organizational policies and preventing officers from becoming too vocal about their political views. Although the question of military design for protest policing is related more closely to the coup-proofing behavior that civil-military relations scholars focus on, it goes beyond the nearly exclusive focus of those studies on the military’s top brass and instead showcases the relevance of soldiers and company-level officers for understanding civil-military relations in autocracies. By introducing the mass public as a third actor into otherwise dyadic models of state-military interaction, the critical need becomes apparent to understand the behavior of “street-level bureaucrats” in the military and state policies geared toward influencing their behavior, since it is the soldiers and officers who directly interact with the mass public in the event of domestic revolution.

Ironically, though, given its scope limitation to autocracies, this dissertation bears a much closer resemblance theoretically to the civil-military discourse in the United States, where both scholars and journalists have spilled plenty of ink debating how concerned this country should be about the growing gap between the military and society (e.g., Bacevich 2013; Feaver and Kohn 2001). According to the standard narrative, this gap emerged in the decades after the (re)introduction of the all-volunteer force in 1973, with the military becoming ideologically, politically, and socially ever more distant from mainstream American society – which, depending on the author’s background, usually leads to a call to either liberalize the military or to revive civic duty and morality in American society. In this dissertation, I examine the role which similar identity gaps between the military and society – which may very well have been deliberately engineered by the state’s leaders – plays in shaping the military’s willingness to be used as an instrument of repression against that society.

Relating it now to the repression literature, this dissertation follows the call by Davenport and Moore (2012, 708) to disaggregate state-opposition interactions. Traditionally, studies of repression have overlooked the agents of repression themselves, assuming that the state and its security forces work as a unitary actor. Some recent work in this genre has begun to model these actors using a principal-agent approach. One strand of this research uses this approach to explain why security forces may engage in human-rights abuses contrary to the will of the political leaders who oversee

them, such as torturing prisoners (Conrad and Moore 2010; Mastroianni and Reed 2011), using heavy-handed tactics for protest policing (Cunningham and Beaulieu 2010), or committing sexual violence against civilians (Butler, Gluch, and Mitchell 2007). Another strand takes into account the reality that state leaders may deliberately set themselves up for agency loss by keeping loose control over their security forces while tacitly encouraging them to rape, pillage, and kill civilians, which allows these leaders to terrorize their political opposition while keeping their own image untarnished by maintaining plausible deniability (Carey, Colaresi, and Mitchell 2015; Kowalewski 1982; Mitchell 2004, 2004; Mitchell, Carey, and Butler 2014). Mitchell (2004) classifies the first strand as “can’t control” and the latter as “won’t control.” However, the topic examined in this dissertation falls into a third category, one that combines insights from both of these strands, recognizing that the state leadership may have malign intent and that the security forces may disobey orders from the principal. Agency loss in this third category implies virtuous shirking, and despite the normatively important application for those interested in preventing state-ordered repression, there has been very little work done in this vein (although see DeMeritt 2014).

This study adds to the nonviolent-action literature by focusing on an understudied question: What causes opposition movements to choose nonviolent methods over violent ones in pursuing their political goals? The theory I employ reverses the theoretical sequence presented by the authors of one prominent, recent study from this genre, Chenoweth and Stephan (2011), who argue that nonviolent movements are more successful because they are able to attract much larger crowds, making defections in the security forces more likely. Taking the opposite approach, the present study endogenizes the choice of opposition tactics to the anticipated behavior by the military: given a real possibility of inspiring defections in the security forces, it becomes easier to attract larger crowds to an opposition movement’s cause, thus making opposition movements more willing to attempt a nonviolent approach to challenging the regime.

Last, and from the broadest perspective, this dissertation occupies a place at the intersection of the international relations (IR) and comparative politics (CP) subfields. Much of the action that I focus on in this study falls under the heading of “second image reversed” (Gourevitch 1978), with state policies regarding the design and use of the military being shaped in considerable measure by international-security factors. Traditionally, IR research on state security policies has ignored domestic security threats such as revolution, while CP research on repression and revolution has for

the most part tended to ignore international security threats and constraints. Following David's (1991) argument for an "omnibalancing" conceptual framework that takes both levels into account, a growing body of work starts from the assumption that state leaders primarily seek their own political survival, and to do so, they must address both international and domestic threats to their tenure (e.g., Bueno de Mesquita et al. 2003; Maoz 2003; Miller and Toritsyn 2005). By introducing internal threat into their models, IR scholars have begun to show why states opt for military policies that would otherwise remain inexplicable, such as the reality that "underbalancing" against foreign enemies appears to be more common than balancing (Schweller 2006); that state leaders would prefer to promote inept rather than competent military leaders despite disastrous battlefield consequences of doing so (Hoyt 2007; Talmadge 2013, 2015); or that developing countries that can scarce afford it are still willing to spend heavily on expensive military technology to avoid having to arm their populace widely in order to meet external threats (Wendt and Barnett 1993). In a similar fashion, but from a comparative-politics starting point, I introduce external threat to help explain startlingly suboptimal security force design for protest policing.

1.7 Outline of this Dissertation

Chapter 2 reviews the existing literature on military loyalty during revolution and then presents the theory that I will be testing in subsequent chapters. As briefly outlined above, this theory takes place in three stages, beginning with the state leadership's decision process of how to shape military recruitment policy, then proceeding to the opposition's decision of whether and how to engage in direct action against the regime, and ending with the state leadership deciding whether to deploy the military against a mass protest that has arisen and the military's decision of whether to repress the protests violently as ordered.

These three stages are subjected to quantitative testing in Chapters 3-5, respectively. The theory-testing done in these three chapters represents one of the very first attempts at quantitative analysis of this subject. Because of severe limitations on existing data for most variables of interest, this statistical analysis uses original data collected for this project from a variety of secondary sources. One of those variables is the ethnic composition of the military, which I use as one of two

measures to represent the extent of social ties between the military and society,¹³ and which was gathered as part of a separate, co-authored project (Johnson and Thurber 2016b). While data for this variable were gathered on a time-series cross-sectional basis, because of the difficulty in collecting data on military ethnicity, our efforts here were limited to one region, the Middle East and North Africa (MENA) region. My second measure for social ties, the use of conscription for military recruitment, is available already on a time-series cross-national basis (Asal et al. n.d.; Toronto 2005). Consequently, for each stage of the analysis, I conduct it in two parts: first, using the military-ethnicity measure for a sample limited to the MENA region, and second, using the conscription measure for a global sample.

To show that any empirical patterns that I may find in support of the theory are the result of the actual process theorized, I supplement the statistical analysis with a comparative case study in Chapter 6 that uses process tracing to show whether the causal mechanism is actually at work in driving the outcome. For this illustrative case study, the choice of cases used is explained and defended in that chapter.

1.8 Policy Implications

Finally, it is worth asking whether, should the predictions made herein be substantiated, anything can actually be done about it. If militaries with stronger social ties to the mass public are shown to be less willing to repress mass demonstrations, how could this knowledge be made useful to policymakers and actors interested in preventing human rights abuses – especially when the design and recruitment of that military is nearly always in the hands of the same state leaders who would be the ones giving orders to repress? Outsiders generally have little leverage over security policies, especially in an area as sensitive as military recruitment, but there is some room to make a difference in certain cases. One of those cases is for post-civil war countries, where as part of their mission, peacekeepers are directly involved in facilitating the process of disarmament, demobilization, and reintegration (DDR) of former combatants. Negotiating an end to civil wars often includes explicit promises to integrate a certain number of rebel soldiers into the state’s military, which is meant to help prevent civil war from breaking out again by helping rebel soldiers find gainful employment

¹³I defend my choice of measures for this variable hereafter.

and by encouraging the civilian groups on whose behalf the rebels had fought to trust that the state will not use the military to oppress or destroy them once they have disarmed (Gaub 2011; Ghosn and Sciabara n.d.; Glassmyer and Sambanis 2008; Licklider 2014). By making sure that the state follows through on this promise, or encouraging future peace agreements to include such a provision, the resulting amalgamated military force will have broader ties to society.

A second, equally important opportunity where rights-respecting outsiders may be able to influence military recruitment policy is when democratizing states are conducting security-sector reform (SSR), where, besides dismantling extensive police-state bureaucracies and paramilitary forces, the democratizing state is attempting to professionalize the military. Without attention to the degree of social ties that the military will have to the rest of society after the reform, foreign advisors' efforts to promote military professionalism may leave the military as a still-useful tool for crushing political opposition, especially by ethnically excluded groups. This unfortunate scenario fits SSR in post-2003 Iraq rather well (Krieg 2014; Sullivan 2013). That reform effort, which enjoyed plenty of assistance and funding from the US, ultimately failed in ensuring Sunni representation, leaving Shi'ite-dominated forces to police restive Sunni-majority provinces. Given that the Iraqi regime had also deliberately disarmed the Sahwa (Awakening) Sunni tribal militias in 2010,¹⁴ the Sunni protest movement that began in December 2012 was left vulnerable to a harsh crackdown by the Iraqi military.¹⁵ This was a crucial development in explaining the sudden rise of the Islamic State (Krieg 2014), which has conquered portions of Syria and Iraq and has, to date, also launched or inspired major terrorist attacks against citizens of Russia, France, Germany, the US, and other countries.

Hence, by taking this theory into account when advising post-civil war countries or democratizing countries, at least in those settings outside advisors may be able to help prevent violent repression from taking place down the road.

¹⁴“Iraq disarms Sunni tribal militias,” Al Jazeera, June 6, 2010, <http://www.aljazeera.com/news/middleeast/2010/06/20106653940383435.html> (8 April 2015).

¹⁵Duraïd Adnan, “Deadly Turn in Protests Against Iraqi Leadership,” New York Times, January 26, 2013, Section A7. Tim Arango, “Dozens Killed in Battles Across Iraq as Sunnis Escalate Protests Against Government,” New York Times, April 24, 2013, Section A5.

Chapter 2

A Model of Social Distance and Revolutions

2.1 This Study’s Contributions

The importance of the military’s reaction to revolution¹ was highlighted by Russell (1974) in her landmark study on revolution. Writing partly as a response to the then-prevailing wisdom that armed popular rebellion should be sufficient to overthrow the regime, she showed that military defection from the regime was a necessary, although not sufficient, cause for mass revolts to succeed. Her comparison of seven cases of successful rebellion and seven cases of unsuccessful rebellion showed that while military defection was present in some of the unsuccessful cases and absent in others, all of the successful cases of popular rebellion experienced military defection. Concluding her study, she wrote, “The question that cries out for research, then, is what factors explain ‘the going over of the army to the insurrection?’ My study has shown only the significance of armed-force loyalty to the outcome of mass rebellion. It is for another study to uncover what factors determine whether the armed forces are likely to be loyal or disloyal” (81). She also suggested some specific factors for others to pursue to test their predictive power. Among these factors were four that relate specifically to social distance: “[1] the social-class composition of the armed forces on both

¹In this dissertation, I use the term *revolution* loosely to describe popular revolts (Barany 2013, 2016; D’Anieri 2006; Lehrke 2014; Nepstad 2011b; Ritter 2014; Russell 1974), not to connote social or political change that might or might not take place afterward (à la Skocpol 1979). I use the term interchangeably with *mass protest*, *mass uprising*, and *mass revolt*.

the officer and conscript level; . . . [2] the closeness of contacts between the armed forces and the civilian population; [3] whether the army is a long-service, professional army or a short-service, conscript army, or something in between; [4] the recruitment criteria, for example political versus military” (81).

Russell’s call for further research on the determinants of military loyalty during mass rebellion went largely unheeded for the first two decades after her study was published. It was not until the Color Revolutions that marked the end of Communist control in Eastern European countries, where largely nonviolent and mass-based protest movements managed to overthrow the regime in several countries thanks to defections among the military and the internal security forces, that interest in this subject began to grow. As mass-based challenges to the regime in non-democracies began to increase in frequency and size around the world, academic interest in the role of the military in these situations gradually increased. After the Arab Spring protest movements of 2011, academic and journalistic attention to military loyalty during revolutions increased rapidly.

The great majority of the research that has been published to date on the subject of military loyalty during revolution has consisted of small- n case comparisons; probably half of these studies all focus on the same set of Arab Spring cases. Despite the limited number of cases of mass revolts being compared, the list of theoretical determinants of military loyalty during revolution that has been posited is rather large. One author’s attempt to catalog all of these factors (Barany [2013](#)) included the following:

- factors at the level of the military: an ethnic or religious split within uniformed ranks, sociopolitical divisions among military elites, elite versus average units, regular militaries versus other parts of the security state, splits between branches of the armed forces, senior versus junior officers, professional soldiers versus conscripts, and generals’ decision-making autonomy;
- factors at the level of the state: the regime’s treatment of the military, the generals’ view of the existing regime, and the regime’s directions to the military;
- factors at the level of society: the size, composition, and nature of demonstrations, the army’s record of conduct toward society, the popularity of the revolution, and rebel efforts to win the army’s support.
- foreign factors: the potential for foreign intervention, revolutionary diffusion, and foreign exposure of officers.

It is easy to see that the state of research on this subject is suffering from a severe “too few cases, too many variables” problem. In addition, many of the currently prevalent explanations for military loyalty have not been tested outside of the original case or cases from which they were inductively generated, so we really do not know the extent of their generalizability.

The need for large- n analysis, such as that which the present study provides, in order to show the importance of each of these factors relative to the others is, therefore, particularly acute. To date, only a handful of published studies (Chenoweth and Stephan 2011; Hendrix and Salehyan n.d.; Koren 2014; Lutscher n.d.; Sutton, Butcher, and Svensson 2014) on this subject employ a large- n approach.² Together, they represent a rough “first cut” at the subject, and although each of these works is itself a major contribution as a pioneering effort, there remains a good deal of work to be done within this overall research topic, especially for thoroughly testing any particular explanation like the social-psychological one that I focus on.

Second, current research on the subject of military loyalty during revolution does not consider a starting point earlier than when the mass protests break out, even though, realistically, both the regime and the civilians would be expected to base their actions leading up to this point on their anticipations of the military’s response. Nearly all, or perhaps even all, studies of this subject only examine what I term the “confrontation stage,” where the military has been ordered to deploy against the mass protests, but this artificially truncates the sample and potentially underestimates the impact of any given explanatory factor. By only examining cases where the “dog barked” – where protests actually took place – we miss any influence that a given factor has on whether such confrontations arise in the first place, working through actors’ expectations of how that factor might impact what the military actually will do if put to the test. In this dissertation, I address this problem head-on by taking into account the effect of social distance on the regime’s decisions about military recruitment, the opposition’s decisions about whether to protest, and regime decisions to deploy the military to put down protests.

Third, the literature on this subject tends to discount the role of the actors who make up the largest proportion of the military, junior officers and enlisted soldiers, and as a result we have very

²In addition, Barany (2016), Pion-Berlin, Esparza, and Grisham (2014), and Russell (1974) may be said to use a “medium- n ” approach. There is also a small number of unpublished studies I am aware of that take a quantitative approach to the topic: Albrecht and Koehler n.d., Morency-Laflamme and McLauchlin 2016, Saha 2013, 2014, Tófalvi 2012, and White 2013.

little scholarship that can help us predict the effects of most recruitment policies. Traditionally, research on military loyalty (e.g., [Finer 1962](#); [Powell 2012](#); [Welch 1976](#)) examines the causes of coups d'état and the influence that officers can have on politics either through direct rule or through the threat of takeover. These works often discuss methods of keeping the military subject to civilian control. Because, almost invariably, coups are led by senior officers, research on civilian control of the military focuses almost exclusively on senior officers and ignores the possibility that commands passed down from the generals might be disobeyed.³ The present study joins a small but growing movement within the conflict studies subfield attempting to account for conflict outcomes by disaggregating the security forces (e.g., [Bou Nassif 2015b, 2015a](#); [Dahl, Gates, Nygård, and Strand 2013](#); [Grauer 2014](#); [Lyal 2010](#); [McLauchlin 2011](#); [Nepstad 2013](#); [Ohl 2014](#)) – in my case, to focus on the street-level bureaucrats who actually confront mass protests.

Last, this dissertation takes a familiar factor for explaining military behavior during mass protests, social distance, and gives it a more solid theoretical foundation than in the past.⁴ Social distance is not a new explanation – arguments in support of one military form versus another based on the resulting social distance between the military and the citizenry extend into ancient history;⁵ its role was briefly highlighted by early studies of revolutions;⁶ and contemporary research has provided some evidence for the importance of this factor, as discussed below. Yet, much remains to be clarified, especially how this factor works at the level of the individual soldier struggling over whether to pull the trigger. I do so below by linking this subject directly to decades of experimental work from social psychology, which all points in the same theoretical direction regarding what we should expect from real-world applications.

³Ironically, despite the fact that the civilian-control literature fundamentally understands the importance of relaxing the unitary-actor assumption, disaggregating the regime into its civilian and military components, the assumption is still maintained at a different level, since this literature treats the military itself as a unitary actor.

⁴[Saha 2014](#) is the most in-depth analysis from a social-psychological perspective that I have found.

⁵For example, political theorists writing in historical settings as varied as Florentine Italy, revolutionary America, and 19th- and 20th-century Marxist movements frequently highlighted social distance between soldiers and society as an important consequence of the state's decision of whether to use standing armies or citizen militias.

⁶For instance, in [Johnson's \(1966\)](#) account, losing control of the security forces may be one type of “accelerator” factor for revolutionary challenge to regimes that have long since lost popular legitimacy. He briefly highlights several possible factors determining military loyalty, one of which is the representativeness of military recruitment: “Is the army a caste-type of military elite or is it organized on the basis of some other exclusivist principle – i.e., a mercenary corps, a foreign army of occupation, or a domestic army recruited among an ethnic subgroup? Is it a mass army based on universal conscription? In what kinds of societies can a mass army be used to suppress a domestic revolt?” (100).

2.2 Prevailing explanations

Previous explanations for military loyalty during mass uprisings include a wide array of factors, which I discuss below.

Since social composition is the factor that the present study focuses on, I begin with a discussion of how previous research on military loyalty has used this factor. Identity-based recruitment is at least mentioned as an explanation for military loyalty in a majority of studies on the subject. However, all but a very small handful focus on what Nepstad (2013) calls a political-incentives explanation to account for the importance of this factor. As Enloe (1980) originally theorized over three decades ago, and as later work (Bellin 2004, 2012; Bou Nassif 2015a; Droz-Vincent 2011; Lou er 2013; Lutterbeck 2013; Makara 2013, 2016; McLauchlin 2010; Morency-Laflamme and McLauchlin 2016; Nepstad 2011b, 2011a, 2013; Quinlivan 1999; White 2013) applied to the context of mass uprisings, recruiting the military on the basis of identity aligns the interests of the military with the interests of the regime, because the privileges that the favored group enjoys are contingent on the maintenance of the current regime in power.⁷ This is true both for a group that is over-recruited because it shares the same ethnic identity as the regime in power and also for a favored group that is recruited along the lines of a martial-race pattern, a phenomenon discussed further below. The political-incentives mechanism is expected to function especially strongly when the civilian elite in the ruling regime come from an ethnic minority that would certainly lose power in the event of a regime change.

A second mechanism, referred to by Nepstad (2013) as a “moral costs” explanation, characterizes the impact of social distance in terms of the internal difficulties a soldier might face when ordered to shoot members of his or her own “in group.” Saha (2014) also draws on this mechanism using a within-country, quantitative comparison of civilian deaths at Iranian protests. Saha’s study demonstrates that, at least in the case of Iran, as a state security force is better able to identify with the amassed crowd, as opposed to seeing the protesters as an out-group, that security force is much less likely to resort to violent repression. She takes advantage of variation in both the composition of the military (broadly vs. selectively recruited) and the composition of the crowd (coming from a single social stratum vs. across the social spectrum) in Iranian protests to show that higher social

⁷A related but distinct explanation is given by Khelghat-Doost (2016), who proposes that in a military recruited and trained on the basis of ideology, the soldiers will be motivated to fight for the regime as a matter of deep conviction.

distance correlates positively with the number of civilian fatalities at protest events. Other authors (e.g., Barany 2016; Binnendijk and Marovic 2006; Blanga 2014; Bou Nassif 2015a; Lutterbeck 2013; Pion-Berlin, Esparza, and Grisham 2014; Prieur 2011; Tófalvi 2012) examine conscription as a potential influence on social distance, drawing on the same sort of moral-costs logic about why conscripts might be more hesitant to shoot.

Although the debate about the determinants of military loyalty is still in nascent form, alternative explanations can be grouped under a few major headings. One of the most popular factors is the economic incentives that the regime provides to the military, in terms of military expenditures, the wages and benefits that officers and soldiers receive, and opportunities for the military elite to use their position in the military institution to enrich themselves through graft, plunder of natural resources, or military-sponsored business ventures. While state leaders themselves appear to believe that it works, as seen through widespread practice of paying off the military to prevent defection, there is mixed evidence for whether economic incentives actually determine military loyalty during mass uprisings (Barany 2016; Bellin 2004; Blanga 2014; Bou Nassif 2013, 2015b; Brooks 2013; Makara 2013, 2016; McLauchlin 2010; Nepstad 2011b; Pion-Berlin, Esparza, and Grisham 2014; Powell 2012; Prieur 2011; Tófalvi 2012). This tactic may even backfire: for instance, in Egypt, after Hosni Mubarak’s use of economic incentives to maintain the military’s loyalty for decades, the military elite abandoned him in 2011 at least partly because they saw the sprawling business empire that they had established with state support as being threatened by the economic liberalization policies that Mubarak’s son and heir-apparent, Gamal Mubarak, was championing (Nepstad 2013).

Another factor that has received a good deal of attention is counterbalancing, a form of coup-proofing that involves building up parallel military forces in order to “guard the guardians” (Quinlivan 1999). While counterbalancing has been shown to reduce coup attempts (Pilster and Böhmelt 2015; Powell 2012) by making them more costly, its impact on military behavior during mass uprisings has not yet been fully established. We have competing theories about how this factor should operate during mass uprisings. On the one hand, counterbalancing should provide the regime with greater capacity to punish defectors and thereby keep soldiers from defecting out of fear of the consequences (Prieur 2011). On the other hand, resources spent building up parallel security forces come at the military’s expense, provoking jealousy, and the practice of counterbalancing itself shows an open distrust, so the military would have less of an incentive to stand by the regime (Bou

Nassif 2015b, 2015a; De Bruin 2014; Gaub 2013; Makara 2013, 2016; Tófalvi 2012). An example of this would be Tunisia in 2011, where the regime had kept the military small and underfunded while building up a massive police state apparatus, and as a result of built-up resentment among junior officers, they were more than happy to allow the regime to fall (Bou Nassif 2015b; Brooks 2013). Lutscher (n.d.) posits and finds evidence for the idea of a U-curve effect, where the likelihood of shirking (by the security forces in general) should be highest in the face of both low and high degrees of counterbalancing in the security forces: at low, because a unified military does not want to risk intra-organizational splits, and at high, because then repressing mass protests becomes a collective action problem among the various forces in charge of protecting the regime.

Violence or nonviolence of protests themselves is another leading explanation for military loyalty during mass uprisings and is theorized to matter in three specific ways. First, nonviolent protests are able to attract much wider participation from the populace, since the participation costs are much lower than for joining an insurgency, and larger protests are more likely to undermine the regime's claim to legitimacy (Bellin 2004; Chenoweth and Stephan 2011). Second, using violence against unarmed civilians violates fundamental notions of warrior's honor, and soldiers are likely to resent the regime for requiring them to shoot at civilians as opposed to armed rebels (Nepstad 2011b; 2013). This effect is expected to be heightened when the military has not traditionally been used for domestic policing, so that engaging in repression would tarnish the military's image (Brooks 2013; Pion-Berlin, Esparza, and Grisham 2014), or when the military's self-perceived role is that of defender of the nation or the "army of the people" (Pion-Berlin, Esparza, and Grisham 2014; Scobell 1992). Third, protests that are primarily nonviolent are psychologically more difficult for the military to suppress, because it is much more difficult for the regime to portray unarmed protesters as enemies of the state, terrorists, thugs, etc. (Binnendijk and Marovic 2006). These effects appear to be magnified as the size and infrastructural sophistication of the opposition campaign increases (Barany 2016; Bellin 2004; Sutton, Butcher, and Svensson 2014). The social breadth of the opposition campaign (Blanga 2014; Brooks 2013; Saha 2014) is also an important factor conditioning the effect of nonviolence.

A fourth leading explanation for military loyalty during mass uprisings examines the cohesion of the military in terms of the loyalty of officers to the military as an institution. Championed primarily by Lee (2005, 2009, 2015), this theory predicts that regimes that use divide-and-rule strategies to

maintain the loyalty of the military elite through personalist promotion instead of institutionalized, merit-based promotion cause intra-military competition and factionalism. Officers who lose out on these plays for office and spoils may choose to join forces with the domestic opposition, allowing mass uprisings to succeed with the support of the disaffected portion of the military. Originally generated inductively from the cases of Indonesia and China (Lee 2005), this theory has received some out-of-sample support (Barany 2016; Gaub 2013; Lee 2009, 2015; Lutscher n.d.; Pion-Berlin, Esparza, and Grisham 2014). A somewhat related version of this theory (Lehrke 2014), which applies to the level of the individual soldier, focuses on the loyalty to the regime or to the military as an institution that individuals serving in the military experience as a result of psychological bonding, which can compete with the individual’s loyalty and ties to society at large.

Many other, often overlapping, explanations for military loyalty in settings where loyalty means having to use violence against civilian protesters have been proposed. Some of these include the level of professionalization of the military generally (Bellin 2004; Lutterbeck 2013; Pion-Berlin, Esparza, and Grisham 2014); the legal role and recent history of the use of the military for domestic policing (Brooks 2013; Droz-Vincent 2011; Prieur 2011; Pion-Berlin, Esparza, and Grisham 2014); anticipated and actual opposition or support from foreign states (Ambrosio 2014; Barany 2016; Bellin 2004; DeMeritt 2014; Gartner and Regan 1996; Nepstad 2013; Pion-Berlin, Esparza, and Grisham 2014; Ritter 2014); popular perceptions of the legitimacy and durability of the regime (Barany 2016; Prieur 2011); and the interaction between the degree of threat posed by the dissenters and the factionalization of the security forces (Hendrix and Salehyan n.d.).

2.3 A Model of Social Distance

I now present a theory that examines the effect of the threat environment that the state faces on the way that it shapes the military’s social composition. Subsequently, the theory deals with how the social composition of the military affects the likelihood of mass uprisings, whether those uprisings will be violent or nonviolent, strategic calculations about deployment in response to mass uprisings, and, finally, whether there will be virtuous shirking by the military if deployed.

Although formulated qualitatively, the model is based on the notion of equilibrium behavior. State leaders are assumed to tailor the military to the specific configuration of internal and external

threat that the regime faces, and therefore, in equilibrium, would-be revolutionaries are deterred from attempting a nonviolent uprising, as they would face a military that they could not expect to sway. (Violent insurrections may still take place, as revolutionaries may hope to defeat the military by force of arms rather than by inducing shirking.) Nonviolent revolutions should be rare in real life based on this logic, an expectation which is borne out by the data: the sample that I analyze in Chapters 4 and 5 includes only 53 nonviolent revolutionary uprising onsets, out of a sample of over 4,000 autocratic state-years.

However, I also assume that there is a lag in the process of attempting to change the military's composition to meet a changing threat scenario. This means that exogenous shocks that change the levels of internal or external threat that the state faces can produce off-equilibrium behavior. This period of time where the state leader has not yet managed to change the military's composition to meet the new threat configuration is where we should see attempted nonviolent revolutions.

The concept of equilibrium behavior also applies to the third stage of the theory, where, given a nonviolent revolutionary uprising, the state decides how to respond. If the current composition of the military translates into low social distance between the military and the crowd, we should expect the state leader to refrain from sending in the soldiers with orders to use lethal violence against the protesters, because the leader would be deterred by the prospect of the soldiers shirking, or refusing to fire on the crowd. Therefore, we would not observe virtuous shirking in equilibrium, since a military that can be expected to shirk due to low social distance between the soldiers and the protesters would not be deployed in the first place. As I show in Chapter 5, in real life, even given the rarity of nonviolent revolutions, shirking is especially rare, indicating the prevalence of on-equilibrium-path behavior throughout the entire process.⁸

2.3.1 Impact of Threat Environment on Social Distance (Stage 1)

This theory takes into account both internal and external threat in predicting how state leaders will design the military. I make the assumption that the political tenure of leaders depends on successfully balancing against threats arising both from within the state as well as from other states. The military is the force of last resort in defending against both kinds of threat. In addition, I

⁸Deployment took place only about half of the time when there was a nonviolent revolutionary uprising (25 of 53 cases), and only one-third of the instances when the military was deployed witnessed shirking (8 of 25 cases).

assume that state leaders choose between potential security force designs and recruitment policies, particularly whether to use ethnic stacking and whether to use conscription, based on a cost-benefit calculation and that they are able to strategically anticipate the effect that their chosen design for the military will have on their likelihood of remaining in office.

States facing high external threat have an incentive to raise a large and capable military that can protect the state and defend its interests abroad. When hostile neighbors with large militaries pose a challenge, the home state must ensure that its own military capabilities are sufficient to mount a defense and, if necessary, carry the fight to the aggressor. The need to deal with high external threat, especially land-based territorial threat, generally necessitates a large standing army that is allowed to recruit broadly and promote the most capable individuals it can find. However, this need goes directly against the logic of military design for protecting the regime against internal threats.

Ethnic stacking, defined as over-recruiting from ethnic groups expected to be loyal and avoiding recruiting ethnic groups whose loyalty is not assured, tends to result in a military that is less effective at doing its job of winning foreign wars (Hoyt 2007; Talmadge 2013, 2015). This is because by recruiting and promoting officers on the basis of loyalty, the weight that is assigned to basic competence is diminished. On the other hand, when neighboring states menace, leaders are forced to both enlarge the military and place a premium on competence. In a pioneering study on ethnicity in the military, Enloe (1980) theorized that state leaders often seek to limit recruitment to the most loyal groups in society, but that the international threat environment places constraints on leaders' ability to be selective. A small military is easy to maintain as the preserve of a loyal ethnic group, but as threat increases, so does the optimal size of the military in preparation to meet that threat. Enloe predicted that leaders tap the more reliable groups for manpower first, but when those groups are tapped out, the state is forced to cast the net widely to bring in the manpower that it needs, regardless of the soldiers' political leanings or ethnic identity.

The diluting effect of rising manpower targets should be seen in both the rank and file and in the officer corps, with both becoming more representative because of the need for manpower and for expertise that can only be found through merit-based recruitment and promotion. Hoyt (2007) provides an example of this process in action in the context of the Iran-Iraq war, where Saddam Hussein, whose military had previously relied heavily on Sunni officers, especially from his

hometown of Tikrit, was becoming desperate about the need to start winning battles against Iran in a war that had so far gone poorly for Iraq. This sense of urgency led him to rely more heavily on recruiting Shi'ites as soldiers through general conscription and as officers through a shift toward merit-based promotion, despite his (largely unfounded) concern that Iraqi Shi'ites would naturally identify more with Shi'ite Iran than their home state.

Military conscription is another recruitment tactic directly related to the military's social identity and composition. Conscription becomes especially attractive to state leaders when the needed manpower levels in the military become very high, as happens in the case of war. The basic function of military conscription is to increase military manpower levels higher than would be possible, given the same operating budget, if the state were to rely on voluntary recruitment. This is because conscription shifts a portion of the burden of paying for defense to the soldiers who are drafted: by being forced to serve in the military, instead of only joining when the pay is high enough to meet each individual's personal reservation price, soldiers pay an in-kind tax of coerced labor (Lee and McKenzie 1992; Poutvaara and Wagener 2007; Ross, 1994). Conscription as a recruitment system has large fixed costs to implement (Mulligan and Shleifer 2005), another reason that it becomes relatively less expensive for the state compared to volunteer-based recruitment only when manpower targets must be made especially high (Lee and McKenzie 1992; Ross 1994).

Cohen (1985, 25) describes the importance of external threat as a determinant of conscription use as follows: "Perhaps the best (if crudest) predictors of a country's system of military service are the length of its land borders with potentially hostile neighbors and the size of its population relative to that of its neighbors. Countries . . . surrounded . . . by powerful and occasionally malevolent Great Powers, have almost always required and depended on conscription to raise large standing armies to defend themselves."

The expected effect of increasing external threat on military recruitment, therefore, is to induce state leaders to make the military larger in manpower terms. This directly conflicts with the logic of fighting internal threat by securing the military's loyalty through selective recruitment. As manpower targets increase, politically loyal segments of society come to be tapped out for potential recruits, and so the regime must draw in recruits from other segments to be able to meet those targets.

This discussion so far produces the following hypothesis:

H1: As external threat rises, the state is likely to trade loyalty for war-winning capability by adopting non-selective recruitment practices, which lower the degree of social distance between the military and the populace.

We could expect internal threat to have the opposite effect. The possibility that the military will be needed to protect the regime against domestic revolution suggests placing a premium on military loyalty. When internal threat is high, instead of recruiting soldiers mainly to fight against foreign enemies – who, on the basis of nationalism at least, can be cast as an outgroup – soldiers are here recruited to fight against their own countrymen. This reality calls for recruitment limited to the most politically loyal segments of society. Given that civilian mobs are usually unarmed or poorly armed, the military can be quite limited in size and still do the job effectively due to its much-superior weaponry. Since a relatively small military is sufficient for internal policing, military recruitment can be limited to politically loyal segments of society in order to counter internal threat.

Ethnic stacking’s purpose is to ensure that the soldiers remain loyal during mass uprisings. The need for loyalty is especially strong in countries where a minority ethnic group rules to the exclusion of other groups, since in this type of case the threat of revolution is especially high: not only more likely, since excluded groups are motivated to challenge the regime (Cederman, Weidmann, and Gleditsch 2011; Cederman, Wimmer, and Min 2010, 482), but also more severe, since any successful revolution against a minority-group regime can be expected to result in violent retaliation against the minority group (McLauchlin 2014).⁹

Ethnically selective recruitment may even be helpful for repressing uprisings originating from the ruling elites’ own ethnic group. Rulers facing challengers from their own ethnic group often find it convenient to rely heavily on soldiers and officers from small and peripheral ethnic groups, for the sake of removing the possibility of their military sharing a common identity with the masses in the streets. Britain and France traditionally used this sort of approach to pacify their conquered territories, referring to recruited groups as “martial races” due to qualities that the groups were alleged to possess that made them more fit for military service (Enloe 1980, ch. 2; Horowitz 1985,

⁹Examples of minority-ruled countries that have relied on a military stacked with co-ethnics to maintain minority rule include Syria (Nepstad 2013), Bahrain (Louër 2013), Guyana (Enloe 1976), Kenya (Nepstad 2011b, 107), South Africa during apartheid (Peled 1998, ch. 2), and others.

445-449; Janowitz 1964, 52-53). Because martial-race groups were usually located on the economic margins of society, military service appeared attractive to them as a method to achieve greater social mobility. Martial-race ethnic groups usually also came from the geographic periphery as well, their homeland being far from the population center of the colony, “so that local sympathies would not interfere with the performance of duty in the event of unrest” (Horowitz 1985, 447). The combination of economic benefits and newly minted family heritages of military service tied the group’s fate to that of the regime, ensuring loyal support.

Along the same lines, many rulers throughout history, including in recent times, have employed foreign mercenaries to fill the same role as martial races in repressing mass uprisings. Their non-citizen identity, inherent in the definition of mercenary, makes mercenaries particularly useful, because it decreases the chances that they will identify with the local populace. For instance, Saha (2013) provides evidence that this lack of common identity makes mercenary soldiers more willing to shoot protesters, with states that employ mercenaries experiencing a higher average number of protest deaths per year. In a real-world example, Muammar Qaddafi of Libya was very deliberate about recruiting (non-Arab) African mercenaries who would be willing to use lethal force against the majority-Arab Libyans who rebelled in 2011.¹⁰

Besides making ethnic stacking in the military more attractive, high internal threat makes military conscription less attractive. The short duration of conscript service implies that the recruit’s identity remains primarily civilian – citizen-soldier, as opposed to career soldier – thereby making a conscript military more likely to identify with the people than the regime in the event of a contest between the two. A conscript military also maintains stronger social ties to the populace, again due to the short duration of service (Binnendijk and Marovic 2006; Lutterbeck 2013). While professional soldiers have the opportunity over years and even decades of service to forge strong ties to their fellow soldiers, to the military as an institution, or even to the regime, conscript soldiers are more likely to maintain strong ties to the “folks back home” and have less opportunity for long-term bonding to the military and the regime (Lehrke 2014).

The importance of these two factors,¹¹ citizen-soldier identity and stronger ties to the populace,

¹⁰Kareem Fahim and David D. Kirkpatrick, “Qaddafi Massing Forces in Tripoli,” *New York Times*, February 24, 2011.

¹¹A third factor, discussed by Wendt and Barnett (1993), making conscription use potentially dangerous for states facing high internal threat is how a high-manpower military facilitates the spread to potential rebels of the military weaponry, training, and organization needed in order to successfully challenge the state.

is emphasized by scholars of civil-military relations who write about a growing gap in the United States between military and society as a result of the transition away from conscription in 1973 (e.g., Feaver and Kohn 2001). Others have noted how conscription ties the military more closely to society. For instance, we have evidence that compared to older democracies, newly democratic regimes are more likely to use conscription for military recruitment specifically to tie the interests of the military to those of the citizenry in order to prevent military coups (Adam 2012).

Holding the level of external threat constant, as the level of internal threat increases, the need to maintain loyalty increases and eventually trumps the need for fighting capability. Loyalty concerns induce state leaders to adopt recruitment practices that put greater social distance between the populace and the military. Because such practices preclude being able to raise a large military, and because they work contrary to merit-based promotion, they degrade military capability to win wars (Talmadge 2015).

This discussion about the effect of internal threat on recruitment produces the following testable prediction:

H2: As internal threat rises, the state is likely to trade war-winning capability for loyalty by adopting selective military recruitment practices that increase the degree of social distance between the military and the populace.

It is important to note that the threat environment that a state faces is not static. A country that had previously experienced very few protests may endure economic or political turmoil that makes domestic unrest more frequent and severe. Similarly, a country with no history of conflicts with its neighbors may begin to experience border disputes, which could eventually lead into a heated rivalry and into war. When the threat environment changes, we may see a lag before the regime is able to alter the military's social composition to adequately address the new situation. This lag should especially be evident in the officer corps. Because officership is a career, it could take years before new recruitment practices, whether more selective or less selective, result in an officer corps tailored to the state's new circumstances. Consequently, we need to examine the actual social composition of the military, not just current recruitment practices, to be able to predict what effect social distance between the military and society will have on protests and on virtuous shirking.

2.3.2 Social Distance and Opposition Movements (Stage 2)

Similar to the assumption I make about leaders rationally anticipating the behavior of the military based on social distance between the soldiers and the populace, we can also assume that those who could potentially join in a protest movement make a cost-benefit calculation in deciding whether to participate, based on their anticipation of the military's behavior. In cases where social distance between the military and society is large, protesters will usually opt to stay home instead of risking their lives, knowing that the likelihood of successfully cajoling the soldiers to come over to their side is very low.¹²

In addition to deciding whether to protest – and conditional on their decision to take mass action at all – the political opposition decides on a set of tactics to use. Again, this decision hinges on opposition members' anticipation of how the military will react, based on its social composition. One of these key decisions about tactics is whether to use violence or nonviolence. Nonviolent civil resistance can potentially be a very potent tool for inducing loyalty shifts in the regime, including the military. This is partly because nonviolent resistance is able to draw larger-scale civilian support due to the lower participation costs involved (Chenoweth and Stephan 2011; Nepstad 2011b), which gives the campaign a greater chance of success in representing itself publicly as “the people” as opposed to a collection of delinquents and crazies. In addition, soldiers whose self-conceived role is to fight against enemy armies and violent insurgent groups tend to recoil when asked to shoot unarmed civilians. In other words, civilians could expect to be safer in a decision to join an emerging protest if they live in a country where there are strong social ties between the soldiers and civilians than if they live in a country where the regime has built as much social distance as possible between soldiers and society.

Conversely, if greater social distance means less chance of inducing defection, as well as greater likelihood of getting targeted by the military, then the utility of nonviolent action is much lower in such cases. From the standpoint of the would-be protester, the strategic benefits of nonviolent action evaporate in this situation; joining an unarmed protest may be setting oneself up for massacre. Consequently, opposition leaders can expect much less success in attracting the degree of popular

¹²This is not to say that there would be no protests: there are always hardcore opponents of the regime, ideologues, and those who feel they have nothing to lose who would be willing to protest under any circumstance. However, any such protests that do arise will not swell to become a mass uprising.

participation necessary to really represent the movement to the regime as the voice and will of the people. Crucially, the strategic value of pre-emptive violence rises when regime opponents can expect the military to crack down forcefully no matter what tactics opposition groups use. “If the military is going to turn its guns on us regardless, then why not get the first strike?” this line of thinking goes.¹³ We can summarize this discussion as follows:

H3. Nonviolent uprisings are more likely to take place in states where there is low social distance between the military and the populace, while high social distance between the military and the populace should result in violent uprisings or the absence of uprisings.

2.3.3 Social Distance and the Response to Nonviolent Uprisings (Stage 3)

Mass uprisings are a significant source of threat to the political survival of leaders of non-democracies. Svobik’s (2009, 478) data for authoritarian regimes over 1945-2002 show that while coups are far and away the most frequent cause of nonconstitutional exit for state leaders, a decent number of leaders have been ousted from office by popular uprisings. That proportion doubles if we consider transitions to democracy as well, which are often made in response to the threat of revolution.¹⁴ In addition, if we also consider that protests may also be a triggering event for military coups (Hiroi and Omori 2013), it is clear that popular uprisings pose a serious threat to the leader’s political survival. Consequently, antigovernment movements in nondemocracies calling for political change are usually crushed before they are able gain traction.

While the previous stage of the theory took into account both violent and nonviolent uprisings, for this final stage, where the regime and the military have an opportunity to respond to an uprising, I examine only nonviolent uprisings. This subset of mass uprisings is the most crucial to examine for the theory, because nonviolent uprisings are the most direct real-world application of the experimental foundations of the theory that I summarize below. Soldiers deployed against

¹³Let me make it clear that I am not advocating violent insurgency. Besides the inevitable moral pitfalls of such an approach, in most cases this approach tends to be self-defeating in the long run, not the least because the civilians in whose name the warring parties fight are always the ones who suffer the most, including long after the war is over (Ghobarah et al. 2003; Humphreys and Weinstein 2006; Plümper and Neumayer 2006). From an empirical perspective, though, the theory described so far leads us to form testable expectations, one of which can cover the likelihood that political actors will resort to violence under varying conditions of social distance between the military and society.

¹⁴The exact percentages that Svobik gives, for leaders in office at least one year, are as follows: 63.71% leaving office due to a coup, 12.66% leaving office due to a popular uprising, and another 12.66% leaving office due to transition to democracy (n = 237).

nonviolent mass protests are being asked to kill unarmed civilians in order to defend the regime. As opposed to the arguably understandable need to meet force with force to end armed rebellions and defend the state against those who have taken up arms against their own country and people, the disproportionality of the ordered response in the nonviolent-uprising setting is jarringly clear. If social distance has any effect in producing unwillingness to carry out immoral orders, that effect should be most visible in this context. While it is entirely possible that the effect of social distance may also apply when soldiers are deployed against a violent insurgency, this is a question that is appropriately left for follow-up studies.¹⁵

The Regime's Response: Deploy or Not?

When faced with a growing antigovernment movement, state leaders can respond in a variety of ways. First, they can simply ignore it and hope that it burns out quickly. Second, they can offer minor concessions like increased economic subsidies or reshuffling the cabinet, hoping that these small steps will placate the opposition and dissuade uninvolved civilians from joining in. However, these first two options are not likely to succeed against protests explicitly calling for the removal of the state leadership, especially when the movement has already swelled to include great hordes of angry citizens. Such scenarios require leaders to choose between giving in, which means initiating a transition out of office, or fighting back with repressive tactics.

Among other factors,¹⁶ the choice between bowing out and fighting back depends crucially on the available tools to the regime to employ against the opposition movement. The military's expected loyalty is the crucial thing I examine in this study in determining what the leader's response to antigovernment movements will be, especially if the movement swells beyond the capabilities of the internal security forces to handle. Most nondemocracies employ sizeable internal security forces (ISFs), including state police, paramilitaries, militias, and intelligence agencies. ISFs are useful for quickly crushing dissent and keeping the population cowed, and in some cases, these forces may

¹⁵However, it is plausible to think that this pattern might travel to violent movements as well, such as how conscript soldiers might be more hesitant to shoot at insurgents than career soldiers would be. For instance, during the Bolivian Revolution of 1952, which was a violent insurgency, "[d]espite the superiority of their firepower many conscripts willingly surrendered to people who had been urging them not to shoot at their own families and friends or destroy the city . . . Whole groups of rank and file soldiers reversed their forage caps – the traditional sign of having changed sides – under the insults, pleading and even angry blows of the *cholas*, working women whose familiar authority frequently overcame the [conscripts'] residual fear of the officer class" (Dunkerley 1984, 38-39).

¹⁶One such factor is the personal health and safety that the state leader can expect after leaving office (Goemans 2008).

even be better funded than the military. Almost universally, ISFs are the primary responders when the state faces a politically threatening opposition movement that, in the state leader’s eyes, needs some roughing up.¹⁷

The military in a nondemocratic state may is usually viewed as a supplementary force for domestic policing. Partly, this is because of the specific capabilities that the military brings to the task. Even when ISFs enjoy greater funding from the state, the type of weaponry and training provided to the military is categorically different and makes it more useful as a force of last resort. Instead of capabilities suited for protest policing, the military is equipped and trained for large-scale combat. While, theoretically, overwhelming firepower might have the advantageous effect of being able to crush the opposition quickly, employing this response against mass protests – especially since large-scale protests usually take place in urban areas – brings the possibility of collateral damage, such as damage to urban infrastructure (not good for the state’s economy) and deaths of innocent bystanders (not good for containment). Similarly, training suited for interstate warfare as opposed to protest policing means that sending the military against protests may result in a security response that is too heavy-handed (Pilster, Bhmelt, and Tago 2016), with destruction far beyond the optimal level to scare protesters into disbanding.

Beside the question of firepower and training, two other factors weigh against using the military as a first-line responder. First, soldiers and officers who see themselves as defenders of the nation, hired to fight against foreign enemies, may thoroughly resent domestic-policing duty (Janowitz 1964, 37; Pion-Berlin, Esparza, and Grisham 2014). This resentment may itself pose a real threat to the military’s loyalty in the event that it is employed against a civilian uprising. Second, the regime may face backlash from domestic and/or foreign audiences for using the military against civilian protesters. Many nondemocracies are also recipients of foreign aid from democracies, and actual or even anticipated threats from donors to pull out aid incentivizes state leaders to avoid generating negative publicity, such as by hiding the regime’s culpability for repression of dissidents that takes place (Carey, Colaresi, and Mitchell 2015) – a feat that is impossible when the military is the one doing the repressing. Therefore, regimes have real domestic and international incentives to prefer

¹⁷Other than in the context of civil war or insurgency, most accounts of state violence against civilians in response to dissent focus on the behavior of the ISFs. Stories of police brutality are commonplace in nondemocracies, and the important point to take note of here is that what we are hearing about in these stories is *police* brutality, since state police are nearly always the firstline force against organized opposition and street demonstrations.

using ISFs over the military for protest policing.¹⁸

Where, due to strong social ties between the military and society, state leaders' anticipation of military loyalty is low, we can expect them to refrain from sending in the military. Even if this implies a sudden, unwanted career change for the leader, that outcome may be preferable to the alternative. If the military were deployed against the antigovernment movement and then it defected, this would prove the regime to be defenseless and at the mercy of the opposition. We can summarize this discussion in the following hypothesis:

H4. Given a nonviolent mass uprising, the regime is more likely to deploy the military to quell that uprising when social distance between the military and the populace is high.

The Military Response: Obey or Not?

Finally, once the opposition has mobilized to form a mass protest, and once the regime has decided to deploy the military against the protest, the soldiers and officers have a chance to decide whether they will be loyal to the regime or whether they will side with the protesters against the regime.

I draw from social-identity theory, which deals with how individuals see themselves vis-a-vis groups in society (e.g., Tajfel and Turner 1986), to provide the theoretical foundation undergirding this study's predictions about the effect of social distance on willingness to shoot into the crowd. This perspective expects individuals to act favorably toward groups to which they perceive themselves as belonging ("in-groups") compared with groups to which they do not see themselves as belonging ("out-groups"). In most settings, this effect depends on the salience of identity in the given context. However, the specific setting that I will be analyzing, popular revolution, essentially requires each member of the armed forces to choose between an identity as a supporter of the regime and an identity as citizen. In the context of mass protests that are manifestations of inter-group ethnic conflict, identity is all but guaranteed to be salient.

Psychologists have long studied what makes individuals willing to obey orders that would normally be considered immoral or unjust (e.g., Kelman and Hamilton 1989; Milgram 1974).

¹⁸There are some exceptions to the rule. In states facing long-running violent insurgencies, the military over time often assumes a domestic-policing role as an outgrowth of its counterinsurgency duties, such as the role the Israeli Defense Force bears in policing the Palestinian occupied territories. This role for the military is not as common as the pattern of using ISFs first and sending in the military only after the dissent overwhelms ISF capabilities.

Milgram's (1974) famous obedience experiments studied subject willingness to administer what they perceived as harmful levels of electric shocks to a victim when ordered to do so by an experimenter. His main argument was that the setting matters most; that normal, psychologically healthy people could be led by degree to perform atrocities as a result of believing that he or she was taking orders from a legitimate authority. Other researchers (e.g., Haney, Banks, and Zimbardo 1973; Mastroianni and Reed 2011) have also emphasized the importance of setting in predicting human rights violations. In addition to highlighting the importance of contextual factors demanding obedience, other factors that Milgram used in various experimental conditions were shown to make the subject *less* likely to obey orders. One of those factors was the physical distance between subject and victim. Subjects who were physically closer to the victim apparently could not help but empathize with the victim's suffering.

In the decades after Milgram, experimental follow-ups (Batson et al. 1981; Brant 1980; Dambrun and Vatiné 2010; Youssef 1968) adopted variants of the same lab design and extended this same line of reasoning to *social* distance – meaning, how similar to oneself another individual is perceived to be. Together, these laboratory experiments provide strong evidence that as social distance between the victim and the subject increases, the subject is able to more easily overcome internal restraints against harming the victim. In other words, people appear to be more willing to harm other people whom they see as “not like me.” To illustrate this pattern, I summarize the design and the conclusions of several obedience experiments that each incorporated social distance as an explanatory variable.

In the first, using an all-white sample of subjects, Youssef (1968) tested the effect of victim race on the level of painful shocks that subjects would be willing to administer. As with Milgram's (1974) design, the experiment was portrayed to the subject as a test of the effect of electric shocks in helping the victim with learning exercises. In one of Youssef's experimental conditions, the factor that divided the treatment group from the control group was victim race, measuring the willingness of white subjects to harm white victims versus black victims. As predicted, black victims received a large and significantly higher mean shock level.

In the next, also using an all-white subject pool, and somewhat further removed from the racial tensions of the Civil Rights era, Brant (1980) showed that the effect of social distance could apply along a continuum from very similar to very dissimilar. He employed four different idealized types

for the victim to model, as designed to fit four different group-based stereotypes: a white victim appearing as a normal undergraduate student, a black victim appearing as a normal undergraduate student, a white victim fitting a particular social-extreme stereotype, and a black victim fitting a social-extreme stereotype. The four victims were, in reality, just two individuals, a white and a black graduate-student confederate. In the black social-extreme case, the victim “spoke in a black English dialect, shuffled when he walked, wore ‘loud’ clothing, and spoke slowly, attempting to appear low in intelligence” (231). In the white social-extreme case, the victim also acted according to a certain stereotype: he was “attired in wrinkled dirty clothing, wore a torn leather hat, spoke in a ‘hippie’ vernacular and appeared to be in a ‘spaced out’ condition, and also spoke in a slow manner” (231). Subject willingness to administer increasingly painful electric shocks increased as the social distance between the (white) subject and the victim increased. Compared to the mean highest shock score that the normal white victim received,¹⁹ the hippie-stereotyped white victim’s mean punishment was 21% higher, the normal black victim’s mean punishment was 80% higher, and the stereotyped black victim’s was an alarming 221% higher.

In the last direct replication attempt to draw on social distance, which this time took place at a French university, Dambrun and Vatiné (2010) used the closest experimental setup to Milgram’s study that could pass a modern institutional review board. They employed an “immersive video environment” where subjects were asked to administer electric shocks that they knew were not real to a victim whom they knew was an actor on a pre-recorded interactive video. The intent was not to deceive the subject but rather to make the experience sufficiently life-like that the subjects would experience similar sorts of stress as if the experience were real.²⁰ All subjects were French nationals, and one of the experimental conditions the authors varied was victim ethnicity, with the cue for this condition indicating either a French or North African name for the victim, representing the prevailing ethnic cleavage in France. Although variation in actual *punishment levels* in this condition did not reach statistical significance, what the authors found speaks directly to the psychological mechanism theorized to be at work here: subjects’ self-reported *anxiety levels* differed strongly and significantly between those assigned to shock the French victim (the one like

¹⁹88.96 V/sec, measured as voltage times duration.

²⁰The simulation was realistic enough that under some experimental conditions, the authors observed a large, significant difference in both the number of fully obedient subjects and in the mean maximum voltage level that subjects employed, indicating that for many students the experience was too much to handle, even though they knew it was only a simulation.

themselves) and those assigned to shock the North African victim (the dissimilar “other”). Anxiety was significantly lower for subjects ordered to shock the victim who was ethnically dissimilar from themselves. In other words, it was physiologically easier to go through the motions of causing harm to an out-group member than to an in-group member. Given the weakness of the ethnicity cue – only the name differed, not the subject’s appearance – and given the complete lack of deception in this experimental setup, that they obtained this result was remarkable. This experiment provides direct evidence that the human physiological response differs when being asked to harm someone like oneself versus someone perceived as very different.

One more study is worth summarizing here, even though it differs from the others in that it did not rely on ethnicity. In a follow-up to Milgram’s obedience study, Batson et al. (1981) analyzed the causes of pro-social, rather than anti-social, behavior by exploring the conditions under which subjects – from an all-female subject pool – would be willing to volunteer to trade places and receive painful shocks on behalf of the original victim, a female confederate dubbed “Elaine.” They also operationalized social distance using a much more shallow basis and still obtained the same result as the other studies summarized above. Using a cue reminiscent of the minimal group paradigm (Tajfel 1970), social distance was operationalized as similarity of preferences on a “personal values and interest” questionnaire,²¹ where Elaine’s answers were forged so as to be nearly identical to, or not at all close to, the subject’s answers, respectively, in the two groups. After a rigged drawing to determine who would take the place as victim, which the confederate always won, the subject was allowed to privately compare her own survey answers with Elaine’s answers, then stationed to watch Elaine go through 10 stages of increasingly painful electric-shock treatments. The subject could volunteer at any time to switch places with Elaine after observing that the treatment was turning out to be exceptionally painful in this victim’s case – due to, she was told, existing health problems. The authors of this study found that subjects who perceived low social distance between themselves and Elaine were much more likely to volunteer to take Elaine’s place than those who perceived higher social distance, despite anticipating that the shocks really would hurt, and even when they were provided with the option of exiting instead of having to watch the remaining shock treatments. That the study found this effect, and on the basis of such a shallow measure of social distance,

²¹The authors provide examples of questions such as, “If you had a choice, would you prefer living in a rural or an urban setting?” and “What is your favorite magazine?” (294).

provides strong evidence that pro-social behavior, even behavior that may be personally costly – such as, perhaps, soldiers physically interposing between unarmed civilian protesters and the state police – can be motivated by low levels of social distance (see also Stürmer and Snyder 2010).

We also have two real-world studies that corroborate these laboratory results, indicating the importance of social distance in influencing state repression levels in response to mobilization by the opposition. The first, Davenport 2005, concerns covert – as well as invasive and illegal – forms of repression employed in neighborhoods in Detroit in the 1960s and 70s. Government intelligence and police organizations, including the FBI, used electronic and physical surveillance in a campaign against a Black Nationalist organization, the Republic of New Africa (RNA). Using the geographic neighborhood as unit of analysis, the study found that when the neighborhood was predominantly white and higher-income, the state used a targeted approach, only surveilling known RNA members residing there. In majority-black and lower-income neighborhoods, however, the state employed indiscriminate surveillance. The study found that the magnitude of covert repression used responded much more to neighborhood demographics in terms of race and income than to actual RNA activity, the behavior meant to be targeted, within the neighborhood.

In the second study, Davenport, Soule, and Armstrong (2011) analyzed a dataset of 15,000 protest events in the US, culled from the *New York Times*, to explore the causes of police presence and activity at protests. The authors found that black protests were much more likely to draw a police presence than white protests, and once on the scene, police were much more likely to make arrests at black protests than at white protests.²²

Applying these experimental and empirical studies on social distance to the context of mass uprisings, we can make our final prediction. This final hypothesis is the main prediction of this study, as it provides the theoretical foundation for the prior hypotheses I have listed that take into account the possibility and anticipation of virtuous shirking.

H5: Given orders to deploy against a nonviolent mass uprising, virtuous shirking by the military is more likely when there is low social distance between the military and the populace.

²²The authors also find a temporal effect: their result is largely limited to years coinciding with the Civil Rights era, which reinforces general expectations from social psychologists about salience of identity as a necessary precondition for in-group favoring and out-group targeting behavior.

The following chapters test these hypotheses using a combination of quantitative and qualitative approaches.

Chapter 3

Testing the Design Stage of the Theory

3.1 Introduction

As explained in Chapter 2, the key independent variables that this simple model takes into account are external and internal threat, and the dependent variable is whether military recruitment is exclusive or inclusive. I operationalize the dependent variable in two ways, reflecting different but also possibly complementary approaches to recruitment: first, whether or not the state uses conscription, and second, whether the state uses ethnic stacking¹ in the military. Conscript militaries generally draw recruits more widely from society than all-volunteer recruitment, making conscription a more inclusive approach to recruitment. Ethnic stacking, as defined previously, involves deliberately over-recruiting from specific ethnic groups expected to be politically loyal. This over-recruitment of trusted groups takes place at the expense of ethnic groups whose loyalty the regime doubts.

As hypothesized earlier, rising external threat should result in the use of more inclusive recruitment practices, as achieving war-winning capability becomes relatively more important for the regime than maintaining a politically loyal military. On the other hand, rising internal threat should result in a less inclusive military, in order to prepare to deal with revolutionary mass uprisings by force if necessary.

¹I use this term interchangeably with “ethnic exclusion.”

3.2 Research Design

3.2.1 Unit of Analysis

The theory that I outlined previously considers state leaders and the citizenry as each going through a regular cycle to assess the situation they face and optimize their actions in response. State leaders formulate military policy in response to the security situation, including to try to prevent uprisings from emerging (this chapter). The citizens' cycle involves assessing the military's composition in determining whether to rebel and, if so, whether to use violent or nonviolent tactics (chapter 4). Both the state leader and citizenry are theorized to repeat this cycle regularly. Should the leader decide to deploy the military in response to an uprising, the military then enters the model as an actor and decides whether or not to repress the uprising as ordered (chapter 5).

By and large, the data I use for my key variables are at the state-year level, so the unit of analysis in this chapter is the state-year.²

3.2.2 Sample

In its current formulation, the theory that I have set forth in the previous chapter is only meant to apply to interaction between society, regime, and military in autocratic countries. Therefore, my sample that I analyze only includes autocratic state-years, based on Polity IV data (Marshall 2016), v. 2015, updated 19 May 2016. I define autocracies as countries with a score of 0 or lower on the Polity2 scale, which includes both full autocracies (-10 to -6) and "closed anocracies" (-5 to 0).³ I also exclude cases of foreign interruption and cases of interregnum or anarchy, as marked in the Polity IV dataset.

Because regime type is not constant, there are several cases of countries in my sample that cross the regime type threshold. Some countries make a single transition, while other countries

²One alternative option that I opt not to use for the unit of analysis is the period between the leader's accession to power and the start of an uprising. Using this alternative would require me to take measures of the threat variables – which induce the state leader to change or maintain recruitment policy – in ways that discard useful information, either measuring the threat configuration only at the start of the leader's tenure but not thereafter, or taking an average over the period. Measuring variables only at the start of tenure would make the implicit assumption that new security-related developments have no bearing on military design, no matter how severe those developments. Similarly, taking an average for the period would not allow me to rule out backwards causation: the leader may institute conscription in one year, early on in his or her rule, and then experience a sudden rise in external threat a few years later, but using averages for the whole period would make it look like the higher average level of external threat caused the change in military policy.

³See the Center for Systemic Peace 2014 Global Report, 22.

occasionally pass back and forth. Although I take the lag of the independent variables before dropping non-autocratic state-years, this process still excludes some of the action that would otherwise be visible if this study included all regime types. Similarly, because I exclude state-years marked by interregnum / anarchy and cases of foreign occupation, some countries enter and exit my sample on this basis as well.⁴

I use Gleditsch and Ward's (1999) list of independent states and their dates of independence, v. 5.0, updated 14 March 2013.⁵ I limit the analysis to the post-World War II era. For models with conscription use as the dependent variable, this temporal limitation is to address the changing nature of military technology, where the dominant model of militarization has shifted from mass mobilization to a much greater potential role for technologically intensive militaries during and after the Cold War. This temporal limitation is also necessary because data coverage for several of my variables, including the military ethnicity data in particular, begins on or about 1946.

3.2.3 State Leaders and Their Policies

I assume that leaders, provided that they are still in office, take into account at the start of each new year the level of threat that the state faces as they formulate military recruitment policy to be put into effect afterward. For each state, I code the identity of the state's leader in a given year as the individual who occupied that office for more than half of the year.

For the analysis in this chapter, it is important to point out that military composition does not change instantly – even though that may be useful to the state leader if the security situation suddenly changed. Whether instituting conscription or changing ethnic recruitment policies, these state-directed policies require some time to implement. This creates the possibility that while the leader's policies may be well-tailored to the current security situation, by the time they reach fruition they may not be the ideal policy for regime maintenance anymore in the face of changing constellations of internal and external threat, a point that becomes important for Chapter 4 in explaining the rise of revolutionary movements, which under the theory are considered off-equilibrium behavior. I lag the independent variables by a period of one year to provide time for the implementation of new

⁴For instance, Polity codes Cambodia as being in anarchy in 1975, the year the Khmer Rouge captured Phnom Penh, so that year would not be included in my sample. Polity marks Cambodia as being under foreign occupation starting in 1979 with the Vietnamese invasion, ending in 1987, so the years 1979-1987 would also be excluded.

⁵This set of independent states exclude microstates, defined as countries with a total population of less than 250,000.

recruitment policies.

3.2.4 Omission of Paramilitary Forces

As discussed earlier in this dissertation, this study’s focus on the military should not be taken to imply that the military is the only force standing between the regime and the threat of overthrow during popular unrest. Local and national police, militia, and paramilitary forces – which I refer to collectively as internal security forces (ISFs) – tend to occupy that role across regime types.⁶

However, the manifest importance of ISFs does not detract from the need to understand the military’s institutional role in the maintenance of autocratic regimes. In most regimes, the military serves as a kind of backup force to protect against popular unrest, available to be deployed in the case that protests swell beyond the capacity of ISFs to quell, or the regime’s opponents turn to guerrilla warfare requiring military capabilities to combat. The metaphor of “institutional trenches” that Gandhi and Przeworski (2007, 1289) use to describe the political-survival logic behind setting up legislatures in autocracies applies well here. In their words,

Consultative councils, juntas, and political bureaus are the first institutional trench for autocrats. These narrower institutions neutralize threats from within the ruling elite. But their efforts to neutralize their closest competitors are not always successful, as demonstrated by the fates of Park, Kabila, and many others. Therefore, we should not expect rulers to retain power forever even if they are able to perfectly co-opt outsiders through their second institutional trench: partisan legislatures.

In the same way, in protecting the regime from domestic unrest, ISFs comprise the first institutional trench, and the military the second trench. We can therefore expect autocratic regimes to invest in the military in ways that demonstrate its important role as that second institutional trench. For instance, previous studies have tested the expectation that foreign aid would be diverted toward funding repression by the military for the sake of preventing revolution and found evidence that aid granted to autocratic states facing high internal threat leads to decreased civil liberties (Bueno de Mesquita and Smith 2009), higher military spending (Kono and Montinola 2013), and

⁶Paramilitary forces are also frequently formed as an institutional bulwark against military coups (Belkin and Schofer 2005; DeBruin 2014; Pilster and Böhmelt 2012, 2015).

even decreased opposition activity in anticipation of repression (Kono, Montinola, and Verbon 2015). In the present study, I extend this theme of treating the military as a potentially important agent of domestic repression by analyzing military recruitment policies.

3.2.5 Key Variables

DV: Conscription Use

For data on conscription use, I draw from Toronto’s Military Recruitment Dataset, v. 2005.1, updated 12 September 2007 (Toronto 2005; see also Asal, Conrad, and Toronto n.d.). This dataset measures conscription use in binary terms, based on whether or not a non-trivial number of recruits are enlisted through force (Toronto 2005, 3). It is drawn mainly from encyclopedic sources, including, among others, from War Resister’s International publications and the *Military Balance* annual periodical from the International Institute for Strategic Studies. The temporal coverage extends up to 2005, although most countries are only coded through 2004, and conscription use is measured annually by country.

I reverse the coding of the variable measuring recruitment policy so that 1 indicates conscription use and 0 indicates no conscription, or an all-volunteer force. I also extend the coverage through 2013 manually by coding each country annually using the same coding rules outlined in Toronto’s codebook, using volumes 105-114 of the *Military Balance*.

DV: Ethnic Underrepresentation

For the other dependent variable, military ethnicity, I draw from an original co-authored dataset (Johnson and Thurber 2016b) that provides time-series cross-sectional measures for the representation of ethnic groups in each state’s military. This Security Force Ethnicity (SFE) dataset covers one region, the Middle East, from 1946-2013. Coding in the version of this dataset⁷ that I drew from is focused on groups comprising at least 10% of the population, based on the list of groups and their population figures contained in the Ethnic Power Relations (EPR) Core dataset, v. 2014 update 2, updated on 16 March 2015, (Vogt *et al.* 2015), although smaller groups were also coded in some cases. Following EPR, the SFE dataset defines ethnicity broadly as “a subjectively experienced sense

⁷Some additional changes were made to SFE during its publication process, which took place well after the analyses that were performed for this chapter.

of commonality based on a belief in common ancestry and shared culture” (Wimmer, Cederman, and Min 2009, 325), including ethno-linguistic, ethno-religious, and ethno-racial groups. SFE also includes other identity-based groupings in cases where these were salient for military recruitment, such as tribe, nationality, and region. Because EPR group identities were constructed with no overlap, such that the sum of the group sizes would necessarily add up to the country’s population, these additional non-EPR groups consist of cross-cutting or nested identities. However, for the present analysis I only use the set of groups for each country that overlaps with EPR.

The military ethnicity data are collected at the group-level,⁸ with codes provided for how disproportionate, if at all, the group’s share in the military is in comparison to its share in the population, using a five-level ordinal scale with the following levels: excluded, under-represented, proportionately represented, over-represented, and monopoly. The SFE dataset provides separate measures for the rank and file and for the officer corps.⁹ I take into account the rank and file, not limiting the analysis to just the officer corps, since widespread refusal by enlisted soldiers to act can inhibit any orders officers may give, although I expect the role of officers to be more strongly determinative of the military’s behavior.

Since I am measuring threat at the state level, as described below, and there are not valid measures available for the threat that individual ethnic groups pose to the ruling ethnic group or groups, I use the group-level measures to calculate state-level measures for the degree of under-representation in the rank and file and the officer corps. This measure is constructed as the sum of the population shares for all groups covered in the SFE dataset that are under-represented in that portion of the military.¹⁰ In theory and in reality, this state-level variable ranges from 0 to

⁸In the EPR dataset, data on Sudan are provided for identity groups at a lower level of aggregation than in the dataset on military ethnicity. Borrowing from an approach taken in my co-authored work elsewhere (Johnson and Thurber 2016a), I deal with this problem by aggregating the EPR data for Sudan into three groups: “Northerners,” “Southerners,” and “Westerners.” For the group-size variable, this is straightforward. For the exclusion ordinal variable, I use a coding rule that the higher-level identity group is coded as having the political status of the most included subgroup; in other words, the lowest degree of political exclusion, if any, experienced by any subgroup within the parent grouping.

⁹As explained in Chapter 1, the theory does not focus on the behavior of senior officers, since the social-psychological mechanism that drives the story is expected to operate in the direct confrontation between street-level bureaucrats and protesters, not the behavior of generals who are more removed from such encounters. However, the data for the officer corps may include some irrelevant information, since the measures for the officer corps do not distinguish between junior- and senior-level officers.

¹⁰Note that an ethnic group being under-represented may be the result of factors beside state-level manipulation, such as the group’s members being more inclined to private-sector employment or not being able to meet (not intentionally discriminatory) educational standards. The military ethnicity dataset only measures actual representation rates, not state policies per se, although information about exclusionary state policies was often taken into account for producing the group-level representation measures.

1. A score of 0 implies that all groups of significant size are represented proportionately in the military, such as in the Iraqi officer corps in the first few years after the 2003 US invasion and the disbanding and reconstructing of the military. A score of 1 implies that all groups of significant size are under-represented in the military, which is accomplished through a policy of employing foreign mercenaries at the expense of all native groups, such as in Morocco in the first few years of independence when the military relied heavily on French and Spanish officers at the expense of both Berbers and Arabs.

Following EPR's format, the military ethnicity dataset's original unit of analysis is group-period, meaning that for each group in each country, that group's level of representation is assessed by period of time. The dataset mainly follows EPR's periods but subdivides them in some cases when there was clear evidence that ethnic representation in the military changed within a period. Having converted the group-period data into country-period data in generating the state-level measures for underrepresentation described above, I next transform the measures into state-year measures, in the same way that EPR measures, which were originally coded through expert surveys in country-period form, are transformed into country-year data for all of their empirical applications (e.g., Cederman, Weidmann, and Gleditsch 2011; Cederman, Wimmer, and Min 2010; Wimmer, Cederman, and Min 2009), by repeating the period-level codings at each year within the period.

IVs: External Threat

To address the theory's prediction about how states should react to rising external threat by putting greater weight on war-winning capabilities, I use multiple measures for external threat. Any threat assessment must reasonably be based on a perception of a potential enemy possessing both the willingness and opportunity to make war with the home state. If we assume that state leaders can anticipate the need for a large manpower pool, even for conflict that has not yet materialized, then, in addition to measures of actual conflict, we should also draw on measures of latent external threat. Measuring latent threat is more difficult, since policymakers' assessments of the likelihood of a threat materializing or the severity of the potential problem are not directly observable.¹¹ I therefore use multiple alternative measures for both latent and present threat. These measures

¹¹Indeed, it is entirely possible that threat perception may be mainly based on the national historical memory, formed in response to conflicts from generations ago, although this possibility is not directly testable.

differentially weight the importance of the objective-threat component and the subjective-threat component.

The objective-threat component is certainly present when the state is currently participating in an interstate war, and indeed, previous quantitative studies of the determinants of conscription use (Asal, Conrad, and Toronto [n.d.](#); Anderson and Tollison [1996](#); although see Ross [1994](#)) have found that current participation in interstate war is strongly and positively associated with conscription. Following these authors, I use a binary variable for **interstate war** as my first measure for external threat, using data from the Correlates of War Inter-State War dataset, v. 4.0, updated 1 March 2011 (Sarkees and Wayman [2010](#)), which I recode to participant-year format.

I also use participation in militarized interstate disputes (MIDs) as a measure for external threat. The use of this measure follows the same reasoning as using interstate war, but it allows me to show whether the effect of conflict participation (if any) applies at a lower threshold than war. I use the Correlates of War MID dataset, v. 4.01, updated 5 February 2014 (Jones, Bremer, and Singer [1996](#); Palmer, D’Orazio, Kenwick, and Lane [2015](#)), to generate a binary measure coded 1 if the state participated in any **MID** that year.

Following Asal *et al.* ([n.d.](#)), I also measure latent external threat as whether the state is involved in a **strategic rivalry**. I obtain start- / end-dates for rival dyads from Thompson and Dreyer ([2011](#)), who expand on Thompson’s ([2001](#)) initial data-collection project that attempted to directly tap into perceptions of enmity by coding dates of dyadic rivalries based on coding leader’s statements using primary sources, rather than conflict behavior. I convert this dyad-level data to the monadic level, producing a binary variable for whether the state is involved in a strategic rivalry with any other state in the given year

In addition, I use measures for external threat that have not previously been employed by previous studies of the determinants of military recruitment policy, most of which are based on the state’s strategic reference group (SRG). The concept of the SRG comes from Maoz ([2011](#), ch. 4). It represents an improvement on earlier measures of likely conflict partners, such as the politically relevant international environment (PRIE), because the SRG coding rules do not cast the net as widely as the PRIE measure, resulting in fewer false-positive predictions for conflict propensity. While PRIE measures conflict opportunity, SRG attempts to measure both opportunity and willingness. A state’s SRG is comprised of the following (Maoz [2011](#), 118): 1. states in its PRIE

during the first five years after the home state’s independence, on the assumption that relationships of friendship and enmity are still being worked out during that time; 2. recent conflict partners, including in wars and in militarized interstate disputes; 3. strategic rivals, incorporating Thompson’s (2001) measure for rivalry; and 4. allies of any of the above. The inclusion of that fourth component broadens the measure to capture the wider set of potential foes who might be expected to join in some way in a fight against the home state.

To address the possible concern over the fact that the SRG construct creates too many false positives for new states within 5 years of independence by coding their SRG as comprising all states in their PRIE, I compared the SRG size of states in their 5th year of independence with its size in the same states’ 6th year of independence (i.e., when the PRIE rule no longer applies, and the SRG is entirely composed of states with willingness, not just opportunity), in terms of their number of SRG members. The two correlate at a fairly high level of 0.72, suggesting that the use of the PRIE for new states does not create too many false positives, and the SRG measure maintains a good degree of continuity when crossing that coding-rule threshold from the 5th year to the 6th year from independence.

I use three SRG-based measures to capture this aspect of external threat. The first measure is the capability imbalance between the home state and its SRG. I use data on military spending and military personnel from the Correlates of War National Material Capabilities (NMC) dataset v. 4.0, updated June 2010 (Singer, Bremer, and Stuckey 1972; Singer 1988) and data on population and real GDP from the Expanded Trade and GDP dataset v. 6.0, updated 9 September 2014 (Gleditsch 2002). I convert the data on each of these kinds of capabilities to units of 1. I use directed-dyad data on annual SRG composition,¹² creating two separate measures for capability balance between the state and its SRG. The first measure I refer to as **present capability balance**, which is calculated as shown in Equation 1 below. I add 1 to the denominator in each ratio to prevent the possibility of dividing by 0 for countries with no military spending or with no military personnel.

¹²The author would like to thank Zeev Maoz for sharing these data by e-mail on 30 March 2016.

$$\text{Present capability balance}_{i,t} = \left[\frac{1}{2} \right] \left[\frac{\text{military expenditures}_{SRG,t}}{\text{military expenditures}_{i,t} + 1} + \frac{\text{military personnel}_{SRG,t}}{\text{military personnel}_{i,t} + 1} \right] \quad (3.1)$$

This first SRG-based measure combines two forms of capabilities that can be used immediately against state i , the home state: military expenditures and personnel. With the capabilities of the SRG in the numerator, an increase in this measure represents increasing threat – in other words, the imbalance between the SRG and home state is worsening – while a decrease in this measure represents an improving security situation for state i .

The second measure for capability balance is **latent capability imbalance**, and it refers to the balance of non-military capabilities that can be converted into military capabilities for future conflict. The construction of this measure is given in Equation 2 below:

$$\text{Latent capability balance}_{i,t} = \left[\frac{1}{2} \right] \left[\frac{\text{population}_{SRG,t}}{\text{population}_{i,t}} + \frac{\text{GDP}_{SRG,t}}{\text{GDP}_{i,t}} \right] \quad (3.2)$$

As with the first balance measure, an increase in this one represents a worsening security situation for state i .

The third SRG-based measure I employ is the raw count of SRG members. As opposed to the capability-balance measure, this third measure captures a slightly different security threat: as the count of SRG members rises, the possibility that the home state may have to fight on multiple fronts increases.

As with the SRG-measures, the final measure that I employ for external threat, **TEK rival**, is a network-based one.¹³ This is a binary measure that captures the intersection of two networks, the first level being strategic rivalries, and the second, transnational ethnic kinships. The reasoning behind including this variable in the analysis is that when state leaders face the prospect of rebellion by an excluded group in their own country, their perception of threat should be greatly increased

¹³I only use this variable as a predictor in the ethnic-stacking analyses, since there were no instances of conscription onset when this variable was equal to 1, making it impossible to generate estimates for the effect on adoption of conscription.

when that group has transnational ethnic kin (TEK) in neighboring enemy states who might intervene on behalf of the rebels.¹⁴ To code this variable, I start with the set of ethnic groups in the home state, using EPR data, that comprise at least 10% of the population and are politically excluded (having “self-exclusion,” “powerless,” or “discriminated” status). For state-years in which the home state was involved in a strategic rivalry, using Thompson’s data, I retain the set of ethnic groups in the rival state(s) that can be considered as leading the regime (“monopoly,” “dominant,” or “senior partner” status), also using EPR data. EPR’s Transnational Ethnic Kin dataset, v. 2014, updated 8 June 2015 (Vogt *et al.* 2015) codes ethnic ties across borders based on connections between the underlying ethnic identities in the EPR core dataset, as manually identified by coders. While this approach does not directly capture the full set of possible connections between groups in different countries based on more objective indicators of identity like language or religion, EPR TEK currently provides the only available large-*n* dataset on the subject, so the alternative to using it would be risking omitted variable bias by leaving out this important concept entirely. I code this variable 1 for state-years where the home country has an excluded group with ethnic ties to an ethnic group controlling a neighboring country that is also a rival, and 0 otherwise.

IVs: Internal Threat

I use a variety of measures for internal threat. This list includes, first, those that tap into present or imminent threat: civil war, domestic unrest, and economic growth rate. Second, it includes measures for latent threat: leader irregular entry, leader tenure, and degree of ethnic exclusion in the regime.

For **civil war**, I draw from the UCDP / PRIO Armed Conflict Dataset (ACD), v. 4-2015, updated 18 June 2015 (Gleditsch *et al.* 2002; Pettersson and Wallensteen 2015). It codes civil-war years as years witnessing a conflict between a state and at least one non-state group with an annual battle-death threshold of 1,000 casualties. For countries coded as having multiple ongoing civil wars, such as Ethiopia for most of the 1975-1991 period, I treat these observations the same as country-years facing only one civil war.

The data I use for domestic unrest come from Banks and Wilson (2013), version 2014 (accessed

¹⁴Cederman *et al.* (2013) find evidence for this kind of threat perception, showing that the presence of large TEK groups in neighboring states, particularly TEK groups in control of the state, can deter the regime in the home state from warring against excluded groups with TEK connections due to the looming threat of intervention.

24 September 2014). I use variables representing four different types of unrest: **strikes**, acts of guerrilla warfare (**guerrillas**), **riots**, and **protests**.

A declining **economic growth rate**, measured in terms of the year-to-year percentage change in real GDP, can be thought of as a triggering cause for challenges to the regime. Economic recession increases the level of grievances that the populace faces generally, giving the populace a short-term reason for pessimism about the direction in which the country and its economy are headed. The data for GDP per capita used to construct this measure came from the Expanded Trade and GDP dataset, cited above.

Leaders who came to office via irregular means face a more precarious situation trying to remain in power than leaders who came to power by regular means. Data for **irregular entry** come from the Archigos dataset v. 4.1, updated 1 March 2016 (Goemans, Gleditsch, and Chiozza 2009), whose authors elaborate on this indicator as follows: “A loss of office is considered irregular when the leader was removed in contravention of explicit rules and established conventions. Most irregular removals from office are the result of the threat or use of force as exemplified in coups, (popular) revolts, and assassinations . . . and occur at the hands of domestic opponents” (273). They further demonstrate that “a leader who came to power irregularly is over three times more likely to lose power in an irregular manner” (274), which means, for the present study, that such leaders should be expected to be especially worried about securing the loyalty of their military. Since leader entry and exit dates do not usually conform to the calendar year, I count the state’s leader for a given year as the individual occupying the office for more than six months of that year.¹⁵

Leaders who came to power by irregular means only manage to stay in power longer by consolidating their control over the state. Consequently, the higher threat perception that attends irregular-entry status should decrease over time (Goemans, Gleditsch, and Chiozza 2009, 274). To capture this expected decline over time, I use Archigos data on leader entry and exit dates to generate the variable **tenure**, a count of the number of years that the individual leader has been in office (starting with 1 for the first year), and interact **tenure** with **irregular entry** in the analyses below. The expectation is that **irregular entry** will initially induce the leader to adopt more exclusive military recruitment policies, but this effect will decrease over time.

The latent threat of uprising can be expected to be higher for regimes ruled by ethnic groups

¹⁵Irregular entry is coded as missing for state-years where no leader served longer than six months.

comprising a smaller proportion of the population than for more ethnically inclusive regimes. I create two measures to represent this threat. The first version, **minority regime**, is a binary variable coded 1 if EPR codes an ethnic group in the country that year as having “dominant” or “monopoly” status, and if EPR’s estimate for the group’s population proportion is less than 50%. Note that because of the sensitive nature of military leadership positions, we should expect the regimes facing high internal threat to treat access to the officership differently than access to the rank and file, with the former being more ethnically exclusive. Recognizing this allows us to see different possible combinations of military recruitment policies: for instance, prior to its civil war, Syria’s ethnic-minority regime employed conscription that drew in politically excluded Sunnis into the rank and file along with Alawites, but Sunnis were under-represented in the officer corps (Makara 2013, 348).

The second version, **ethnic threat**, is calculated as the ratio of politically excluded groups’ population share to politically included groups’ population share. I count included groups as those with “monopoly,” “dominant,” “senior partner,” or “junior partner” status, and I count excluded groups as those with “self-exclusion,” “powerless,” or discriminated” status. The data source for group population share and status is EPR, cited above. State-years with a score of 0 on this variable have no excluded ethnic groups, and higher values on this variable imply an increasing threat to the regime due to a larger share of the population being excluded from power.¹⁶

Regime instability is meant to capture periods where governing institutions are being reconfigured. During times of political instability, the regime’s vulnerability is expected to be higher, and hence the state leadership’s sense of internal threat should be higher as well. I follow Fearon and Laitin (2003, 81) in how I code this variable:¹⁷ “whether the country had a three-or-greater change on the Polity IV regime index in any of the three years prior to the country-year in question,” as well as any state-years that the Polity dataset marks as transition periods.

I also employ two dummy variables to capture the internal-threat perception of various types of authoritarian regimes, specifically **personalist** and **military**. Data for these variables come from Geddes, Wright, and Frantz (2014, v. 1.2, updated 23 June 2012). Perception of domestic threat should be highest for personalist dictators, as these regimes are the most precarious, followed by

¹⁶State-years where all ethnicity is considered irrelevant for all groups are coded as missing values on this variable.

¹⁷Given that the sample is limited to autocratic state-years, I do not include, as do those authors, state-years marked by anarchy.

military regimes. The excluded category for type of authoritarian regime is a pool of all other types of authoritarian regimes, such as monarchies and one-party regimes.

Finally, I employ **revolutionary uprising** as a measure for present internal threat. Chapter 4 provides a very detailed description of the data sources and construction of this variable, which for brevity I do not repeat here. Essentially, this binary measure represents whether or not the regime faced a violent or nonviolent uprising that year with the goal of overthrowing the regime.

3.2.6 Control Variables

Since previous quantitative work modeling the determinants of conscription use is scarce, and previous quantitative work using ethnic stacking as a dependent variable is, to my knowledge, nonexistent, there is nothing approaching a standard set of control variables to draw on for any of the present analyses. Many of the explanations that have emerged from qualitative work on either of these subjects are not amenable to quantification or else would require data collection that goes beyond the scope of this study to be able to control for them statistically; for instance, the perceived fairness of conscription laws (Levi 1997) or national myths and norms regarding military service and citizenship (Burk 1992; Krebs 2005; Leander 2004).

Fortunately, one recent study by Asal, Conrad, and Toronto (n.d.) provides a starting foundation for the present study here in this area. While their study does not delve as deeply into the subject of internal and external threat as this work does, their extensive literature review and quantitative analysis provide a useful starting set of control variables. Excluding their three threat-related variables (interstate war, civil war, and interstate rivalry), which I deal with separately, and democracy, which does not apply for the present study since my scope is limited to autocracies, I adopt the remaining variables in their models to form a set of potential control variables in the analyses below. These variables include development, former British colonial status, and militarization. I also add population to this list. I discuss each below.

Economic development is often thought to increase pressures for military professionalization. Development leads to societal attitudes becoming more individualistic and hedonistic, with a declining willingness to fight for one's country (Inglehart, Puranen, and Welzel 2015), as well as a mentality that views military service through a purely economic lens (Asal *et al.* n.d., 4-5; van Doorn 1975, 151-153). Economic development should also correlate negatively with internal conflict

(Collier 2003) and external conflict (Gartzke and Hewitt 2010), reducing the need for military manpower. While Asal *et al.* (n.d.) operationalize this variable in terms of energy use per capita in order to extend their analyses back to the early 1800s, since the present study's temporal domain is limited to the post-WWII period, I use the higher-quality measure of **GDP per capita**, with data taken from the Expanded Trade and GDP dataset v. 6.0, updated 9 September 2014 (Gleditsch 2002).

Within the small set of quantitative studies on the causes of conscription use, British colonial origin has consistently been shown to have a negative relationship with conscription (Asal *et al.* n.d.; Hadass 2004; Mulligan and Shleifer 2005). This finding has so far remained robust to the inclusion of variables controlling for threat levels, helping to rule out purely geographical explanations for the relationship. Asal *et al.* (n.d., 8-9) contend that this pattern is due to a long-standing cultural aversion to conscription set during the English Civil War (c.f. Schwoerer 1974), which Britain later exported to its colonies. My data source for British origin is Teorell (2010, 162).

The idea that conscription may have a primarily offensive purpose is partly corroborated by studies that show a positive relationship between conscription use and various forms of interstate conflict (Choi and James 2003; Pickering 2011). Asal *et al.* (n.d., 6-7) distinguish between the defensive logic suggested by threat-based explanations for conscription and the possibility that conscription may be implemented to promote an expansionary and revisionist foreign policy. They examine the relationship between **militarization**, operationalized as military spending divided by GDP, and conscription use. Although a far from perfect measure, this variable seeks to capture revisionist or expansive intentions by the regime. I use data on military expenditures from the COW NMC dataset and data on real GDP from the Expanded Trade and GDP dataset (both cited above).

In addition to the three control variables from Asal *et al.* n.d. described above, I also add one more control variable, country **population**, to this list. Previous work on conscription suggests population influences conscription use, although there are competing theoretical reasons as to the direction of this variable's effect. On the one hand, a larger population means a larger supply of available labor, which should make it easier for the regime to fill manpower needs using volunteers (Cohen 1985, 25), particularly from politically reliable ethnic groups. On the other hand, a larger population means that the financial fixed cost of setting up the bureaucratic structure necessary to

implement a military draft can be spread more widely across society, making conscription use more likely (Mulligan and Shleifer 2005). Larger countries are also more likely to experience internal threat in the form of insurgency (Fearon and Laitin 2003), and with more opportunities for other forms of turmoil to break out, it would also seem likely that other forms of internal threat would also be more common in higher-population countries. Data for population come from the Expanded Trade and GDP dataset (cited above).

3.2.7 Summary Statistics

Table 3.1 shows summary statistics for the variables used in the analyses.

Since ethnic stacking data are available for only one region, the Middle East and North Africa (MENA), the potential concern arises that this region famous for being a hotspot for interstate conflict might not have enough variation on the external-threat measures to see a meaningful effect of external threat on levels of ethnic stacking, even if one exists. To address this concern, I calculate summary statistics for this region separately, displayed in Table 3.2.

Two main points are apparent from Table 3.2. First, comparing the mean for each of these variables to the mean of the same variable in Table 3.1, we see that this region collectively is indeed higher on every measure of external threat. On the other hand, there is still plenty of variation in this region on these measures: for the binary variables, none of the means are worryingly close to 1 (the highest is strategic rivalry, at 0.69). Similarly, the standard deviations for most of these variables are very close to that of these variables' counterparts in Table 3.1. Only two variables have notably lower standard deviations, present capabilities balance and latent capabilities balance, but there is still a good deal of variation in this sub-sample even for those two variables. Hence, limiting the analyses of ethnic stacking to the MENA region should not pose problems for examining the effect of external threat on this form of military design.

Note that several of the variables as originally constructed exhibit heavy right-skewness. To reduce the influence of far-right outliers and obtain a better idea of the average effect of these variables, I transform their distributions using either a natural-log transformation or fractional exponents. In the case of **militarization**, **present capability balance**, and **latent capability balance**, I also use linear interpolation to fill in gaps in data coverage after exponentiating, although I do not extrapolate beyond the data's current range.

Table 3.1: Summary statistics

	count	mean	sd	min	max
DVs					
conscription	4063	.591681	.4915832	0	1
excl: officers	789	.3524664	.2831493	0	1
excl: enlisted	812	.2821749	.2730391	0	.75
IVs: ext. threat					
strategic rivalry	4480	.5205357	.4996339	0	1
interstate war	4480	.0334821	.179912	0	1
MID	4480	.3366071	.472602	0	1
SRG count ^(1/4)	4480	1.115242	.849994	0	3.288868
pres. cap. bal ^(1/10)	4008	1.129177	.7106447	0	10.81784
lat. cap. bal ^(1/10)	4009	1.05518	.6294352	0	2.669815
TEK rival	4480	.0372768	.1894604	0	1
IVs: int. threat (unrest)					
rev. uprising	4480	.0841518	.2776463	0	1
civil war	4480	.0529018	.2238624	0	1
strikes ^(1/4)	4289	.0463927	.2221055	0	1.898829
guerrillas ^(1/4)	4289	.1433481	.3650681	0	2.414737
riots ^(1/4)	4289	.1762511	.42393	0	2.378414
protests ^(1/4)	4289	.1772928	.4400309	0	2.932972
econ. growth rate	4162	.0510652	.3935987	-.8295917	22.96208
IVs: int. threat (regime)					
irreg. entry	4319	.3878213	.4873099	0	1
ln tenure	4427	1.91509	1.017892	0	3.89182
regime instab.	4480	.1292411	.335504	0	1
minority reg.	4283	.1935559	.395131	0	1
ln eth. threat	4283	5.860281	3.870512	0	13.10216
personalist reg.	3926	.2572593	.4371792	0	1
military reg.	3926	.115894	.3201386	0	1
Controls					
ln GDP pc	4247	7.820841	1.084428	4.888995	13.35702
British colony	4480	.2651786	.4414775	0	1
(milex/GDP) ^(1/4)	3887	.3053619	.1055767	0	.9706932
ln pop.	4247	15.80663	1.517486	11.68022	21.00419

Table 3.2: Variation of external-threat variables in MENA region

	count	mean	sd	min	max
strategic rivalry	970	.6907216	.4624345	0	1
interstate war	970	.0484536	.2148335	0	1
MID	970	.4484536	.4975924	0	1
SRG count ^(1/4)	970	1.369784	.8691717	0	3.288868
pres. cap. bal ^(1/10)	844	1.296727	.5121591	0	5.090398
lat. cap. bal ^(1/10)	846	1.265722	.5087937	0	2.135277
TEK rival	970	.057732	.2333562	0	1

3.3 Methods

For models with conscription use as the dependent variable, I conduct duration analyses using Cox proportional hazards models.¹⁸ Previous studies of the determinants of conscription have universally examined the incidence, rather than the onset of conscription. Given the high degree of continuity of this type of military recruitment policy from year to year – most countries never change their policy, and those that do change do not change more than a few times during the post-WWII period – examining time until onset of conscription use is a more appropriate way to go. Most of the variation in the sample is cross-sectional, and treating all years in all countries in the sample as independent observations artificially multiplies the number of observations. Conscription is not completely time-invariant, though, or else it would make more sense to conduct a purely cross-sectional analysis. Duration analysis takes an approach that strikes a balance between the desire to take advantage of within-country variation and the reality that many countries never change their policy (countries that never adopt conscription use are right-censored but still contribute information to the estimator).

I treat countries as at risk of onset of conscription use during all autocratic years in which the country is not already using conscription. In these Cox models, I use a conditional risk set approach with the hazard stratified by event number – i.e., the first time adopting conscription, the second time, etc. I also cluster standard errors by country and use the Efron method for ties.

For each of the Cox models, I run the *estat phtest* command, which checks the proportional hazards assumption using Schoenfeld residuals. In cases where individual variables are found to

¹⁸All analyses in this and the following chapters are run in Stata 14, using the **esttab** package (Jann 2005; 2007) to generate tables.

violate this assumption, based on a significant p -value for this test,¹⁹ I interact the offending variable with some function of time, trying various functions until one is found that results in a non-significant test when re-checking the proportional hazards assumption. In each of the statistical tables below that employ Cox models, I display the specific function of time used by variable in these cases (given in italics in the tables).

In predicting ethnic underrepresentation as the dependent variable, I use a fractional logistic regression model, since the variable ranges continuously in the range $[0, 1]$ and does not extend outside of that range. I include a lagged dependent variable, as discussed below in Section 4.3, to help account for the strong persistence of this variable from year to year. As with the conscription models, I cluster standard errors by country to account for non-independence of observations from the same country and deal with heterogeneity of errors that may arise from that fact.

My objective is not only to see whether, but which aspects, of external and internal threat matter for recruitment policy. Consequently, I employ a wide variety of measures for each. Since the various measures of each type of threat are closely related conceptually, I avoid placing them all in the same model simultaneously.²⁰ Instead, I examine the effect of individual measures for external threat and internal threat separately, with only that measure and any controls included. Afterward, I retain significant predictors from previous models to produce a full model.

3.4 Results

3.4.1 Relationships among the Dependent Variables

I first check the correlations between the dependent variables, conscription use and the two forms of ethnic stacking (officer corps vs. in rank and file). High negative correlations between these measures would suggest that conscription use does not coexist in practice with ethnic stacking, such as may be the case if implementing a policy of conscription in the military prevented the regime from manipulating the military's ethnic composition. Alternatively, zero-correlations would imply that conscription use and ethnic stacking have no relationship, that they capture entirely different dimensions of military recruitment. Although hypothetically it is possible to have a positive

¹⁹I set a threshold of $p < 0.1$ here.

²⁰This problem also precludes using stepwise regression.

correlation between conscription use and ethnic stacking – which might imply coerced military service that is only targeted toward one ethnic group in order to to bolster that group’s control over the military ²¹ – this would go against the theory outlined in this study, which views conscription as a policy that generates greater social inclusivity in the military and ethnic stacking as a policy of exclusivity.

The pairwise correlations between conscription and ethnic stacking in the officer corps and conscription and ethnic stacking in the rank and file, respectively, are -0.16 ($p < 0.001$) and -0.26 ($p < 0.001$). These low, negative correlations imply that my measures for ethnic stacking and conscription use do, in fact, capture different dimensions of military recruitment, and that these policies are not necessarily incompatible. It also makes sense that the negative correlation is slightly stronger for the rank and file, since conscription applies mainly to this portion of the military, suggesting that conscription use helps broaden the ethnic base at least a little among enlisted soldiers, and less among the officers.²² Note that since I only have data on ethnic stacking for the MENA region, it is not possible at present to tell whether these relationships among the dependent variables used in this chapter are generalizable to other regions.

Having demonstrated that conscription and ethnic stacking are not merely opposite sides of the same coin, I proceed to analyze the effects of external and internal threat on each dependent variable.

3.4.2 Determinants of Conscription Use

Before examining the effects of each type of threat-based variable, I first prepare the set of controls to include in the models of conscription onset by running Cox models (not shown) with only one covariate, rotating through the set of four controls borrowed from previous research: ln GDP per capita, British colony, militarization, and ln population. The only one that is significant in any of these one-predictor models is British colony, so in the remainder of the models of conscription onset I only include British colony as a control variable.

Table 3.3 shows the results from the Cox models for conscription onset with external threat

²¹This kind of policy was recently implemented in Qatar, for instance. See Victoria Scott, "Qatar’s National Service Program Puts New Emphasis on Military Training," Doha News, 9 August, 2015. <http://dohanews.co/qatars-national-service-program-puts-new-emphasis-on-military-training>

²²The pairwise correlation between the two forms of ethnic stacking is a moderately high 0.74 ($p < 0.001$)

measures as the predictors. **British colony** continues to be negative and highly significant in all models in this table. We see from models C1-C6 that most of the measures of external threat correlate positively and significantly with the onset of conscription use, as predicted; the only one that is not statistically significant is **latent capabilities balance**.

I take all of the independent variables from C1-C6 that were significant and enter them into combined models in C7 and C8 to see which remain significant after controlling for the others. Because SRG count correlates fairly highly with MID and present capabilities balance, I do not include SRG count in the same model in C7; rather, I enter it separately, without MID or present capabilities balance, in model C8. We see from models C7 and C8 that only two of the external-threat variables remain statistically significant in the combined models, **strategic rivalry** and **interstate war**, both of which also remain positive. The significance level of each varies between the two models, though: strategic rivalry is only significant (and only at the 0.1 level) in model C8, and interstate war becomes much more significant in C8 as well. The other variables, **SRG count**, **MID**, and **present capabilities balance** lose all significance in these two models.

The results in this table confirm the findings of Asal et al. (n.d.) regarding external threat: the two variables that they had used to capture external threat, interstate war and strategic rivalry, and which they had found to be positively related to conscription incidence were also shown to be positively related to conscription onset, here. Although the other measures of external threat tested here also generally correlated positively with conscription onset, none of those others were significant when controlling for war and rivalry. Hence, we can tentatively say that the two forms of external threat that matter for conscription onset are interstate war and strategic rivalry, both of which increase the hazard of adopting conscription as a military recruitment tool.

Table 3.3: Onset of conscription use: external threat

	(C1)	(C2)	(C3)	(C4)	(C5)	(C6)	(C7)	(C8)
strategic rivalry	0.56* (0.23)						0.41 (0.25)	0.43 ⁺ (0.25)
interstate war		0.69* (0.29)					0.58 ⁺ (0.35)	
$\ln(t)$ * <i>int. war</i>								0.60*** (0.15)
MID			0.54* (0.22)				0.29 (0.27)	
SRG count ^(1/4)				0.31* (0.15)				0.16 (0.17)
pres. cap. bal ^(1/10)					0.11* (0.04)		0.04 (0.06)	
lat. cap. bal ^(1/10)						0.24 (0.22)		
British colony	-1.07*** (0.30)	-1.03*** (0.30)	-1.07*** (0.30)	-1.03*** (0.30)	-1.44*** (0.34)	-1.12*** (0.33)	-1.44*** (0.35)	-1.05*** (0.31)
Observations	1752	1752	1752	1752	1559	1563	1559	1752
Events per variable	46.50	46.50	46.50	46.50	36.50	31.50	14.60	23.25
Pseudo R^2	0.035	0.030	0.034	0.032	0.055	0.038	0.069	0.044
<i>AIC</i>	678.02	681.15	678.83	679.79	485.78	414.84	484.55	675.55

Cox conditional risk set models stratified by event no., DV = onset of conscription use.

Country-clustered standard errors in parentheses

⁺ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 3.4 examines unrest-based measures of internal threat. Following the same pattern used in Table 3.3, I examine the effect of each measure for internal threat separately, controlling for British colony, and then enter the initially significant predictors together in a combined model. **British colony** continues to be negatively and significantly related to conscription onset in all models in the table.

We see from models C9-C15 that **strikes**, **riots**, and the **economic growth rate** are not significantly associated with conscription onset. Surprisingly, the four variables that are significant, **revolutionary uprising**, **civil war**, **guerrillas**, and **protests** are all positively correlated with conscription onset, rather than negatively as expected.

After entering the four initially significant internal-threat measures into the combined model, C16, we see that the only one that remains significant is **revolutionary uprising**, which continues to be positively related. This odd finding, in terms of the direction of the relationship, may indicate that state leaders view conscription as a population-control mechanism rather than something that would weaken the state's ability to deal with internal threat, although this only appears to apply to revolutionary uprisings.²³

²³For a real-world example of reacting to revolutionary protest by promoting conscription, see Ellen Barry and Sophia Kishkovsky, "Putin Takes Helm As Police Punish Moscow Dissent," *New York Times*, 8 May, 2012.

Table 3.4: Onset of conscription use: internal threat - unrest

	(C9)	(C10)	(C11)	(C12)	(C13)	(C14)	(C15)	(C16)
rev. uprising	1.09** (0.36)							0.74 ⁺ (0.41)
civil war								0.36 (0.46)
$\ln(t)$ * <i>civil war</i>		0.57*** (0.13)						
strikes ^(1/4)			-0.04 (0.29)					
$\ln(t)$ * <i>guerrillas</i> ^(1/4)				0.25* (0.13)				0.18 (0.13)
riots ^(1/4)					-0.04 (0.26)			
protests ^(1/4)						0.38 ⁺ (0.22)		0.33 (0.23)
econ. growth rate							-0.24 (0.56)	
British colony	-1.06*** (0.30)	-1.12*** (0.29)		-1.30*** (0.38)			-1.16** (0.37)	-1.25*** (0.38)
$(1/t)$ * <i>Brit. colony</i>			-1.66* (0.67)		-1.67* (0.66)	-1.61* (0.67)		
Observations	1752	1752	1659	1659	1659	1659	1613	1659
Events per variable	46.50	46.50	30.00	30.00	30.00	30.00	25.00	12.00
Pseudo R^2	0.030	0.037	0.029	0.053	0.029	0.033	0.043	0.064
AIC	681.34	676.34	369.42	360.40	369.40	367.81	296.79	362.23

Cox conditional risk set models stratified by event no., DV = onset of conscription use.

Country-clustered standard errors in parentheses

⁺ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 3.5 shows the results of regime-related measures for internal threat. Model C17 interacts leader tenure with whether the leader came to power by irregular means. The interaction term **irregular entry** \times **tenure** in that model is not significant, but both tenure and irregular entry are. **Tenure** is positive, as expected – leader should feel more secure in their position as time goes on, and hence be more willing to risk making the army socially inclusive by adopting conscription. On the other hand, **irregular entry** is also positive, against expectations. Similarly, the only predictor in models C18-C22 that is statistically significant, **regime instability**, is also positively related to conscription use, rather than negatively related as predicted. Both of these associations go contrary to the prediction that irregular-entry leaders should feel less secure and hence be less willing to make the military socially inclusive.

The combined model C23, which includes all three internal-threat measures from the previous models in this table that were initially significant, shows that the effect of regime instability is no longer distinguishable from zero after controlling for leader tenure and irregular entry, but the latter two variables remain significant and positive. The persistent positive effect for irregular entry adds some weight to the population-control interpretation, although the fact that tenure remains positive does not fit this interpretation. I return to this discussion below.

Table 3.5: Onset of conscription use: internal threat - regime

	(C17)	(C18)	(C19)	(C20)	(C21)	(C22)	(C23)
irreg. entry	0.89** (0.34)						0.55** (0.21)
ln tenure	0.32* (0.15)						0.26* (0.12)
irreg. entry \times tenure	-0.24 (0.21)						
regime instab.							0.31 (0.24)
$\ln(t)$ * regime instab.		0.32** (0.11)					
minority reg.			0.00 (0.37)				
ln eth. threat				0.01 (0.03)			
personalist reg.					0.25 (0.40)		
military reg.						0.60 (0.57)	
British colony	-1.10*** (0.32)	-1.09*** (0.30)	-1.11*** (0.33)	-1.10*** (0.33)	-1.03* (0.44)	-1.22** (0.44)	-1.12*** (0.32)
Observations	1685	1752	1637	1637	1440	1440	1685
Events per variable	20.75	46.50	35.5	35.5	21.00	21.00	20.75
Pseudo R^2	0.037	0.031	0.032	0.033	0.040	0.043	0.037
AIC	574.37	680.56	473.47	473.38	236.30	235.63	574.18

Cox conditional risk set models stratified by event no., DV = onset of conscription use.

Country-clustered standard errors in parentheses

+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 3.6 shows the result of entering all of the threat-related predictors that were significant in the preliminary combination-models C7-C8, C16, and C23, in order to see the effects of internal threat while controlling for external threat and vice versa. The two indicators of external threat, **strategic rivalry** and **interstate war**, continue to increase the hazard rate of adopting conscription, consistent with the expectation that state leaders react to foreign threat by making the military socially inclusive in order to raise needed manpower. **Revolutionary uprising** is no longer significant after controlling for the other forms of internal threat and controlling for external threat. The two regime-based measures of internal threat, **irregular entry** and **tenure** continue to be positive and significant, indicating an increase in the hazard of adopting conscription use. **British colony** remains negative and significant, as before.

Based on these results in Table 3.6, we can say that the prediction about higher levels of external threat inducing state leaders to adopt conscription use is shown to be correct. The forms of threat shown to matter for conscription adoption, interstate war and strategic rivalry, are the same forms of external threat shown by previous research (Asal et al. [n.d.](#)) to be related to the incidence of conscription use. The fact that the present analysis employs onset of use as the dependent variable instead of incidence of use, as they do, strengthens our confidence in the robustness of these findings across studies.

The prediction about higher levels of internal threat deterring conscription use is partly verified and partly rejected. As the state leader's tenure increases, indicating greater regime security and consolidation (and therefore, lower levels of threat), the state's military is more likely to begin using conscription for recruitment. On the other hand, leaders who come to power by irregular means, who should perceive higher levels of threat due to not having established a secure power base, tend to adopt a policy of conscription sooner than their autocratic counterparts who come to office by regular means.

3.4.3 The Determinants of Ethnic Exclusion in the Officer Corps

Table 3.7 explores the effect of external threat on ethnic exclusion in the officer corps. Since the dependent variable is highly persistent, meaning that the year-to-year values of the variable tend to be largely unchanging, I control for the previous year's level of ethnic stacking in all models. Since the decision to include a lagged dependent variable tends to raise the bar especially high for all other

Table 3.6: Onset of conscription use: combined model

	(C21)
strategic rivalry	0.43 ⁺ (0.24)
$\ln(t)$ * <i>int. war</i>	0.64** (0.24)
rev. uprising	0.59 (0.66)
irreg. entry	0.50* (0.21)
ln tenure	0.25* (0.12)
British colony	-1.06** (0.33)
Observations	1685
Events per variable	13.8
Pseudo R^2	0.054
<i>AIC</i>	568.06

Cox conditional risk set models stratified by event no., DV = onset of conscription use.

Country-clustered standard errors in parentheses

⁺ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

covariates included in the model to demonstrate any effect (Achen [2001](#)), I partially compensate by making note of and retaining variables whose p -value falls within the 0.10-0.15 range, rather than treating $p = 0.1$ as a strict threshold; I do this for all stacking models in Sections 3.4.3 and 3.4.4.

Models O1-O7 show that measures of external threat are generally not related to the level of stacking in the officer corps. The only variable that comes close to significance is **strategic rivalry**, at $p = 0.15$. However, the coefficient for strategic rivalry is positive, which goes against the prediction that states facing higher levels of threat from other states can be expected to make the military more socially inclusive in order to raise the military's ability to win wars. Since none of the other external-threat variables came close to significance, I do not include a preliminary combined model in this table, but I do retain strategic rivalry for the final combined model in Table 3.10.

Table 3.7: External threat and ethnic exclusion in the officer corps

	(O1)	(O2)	(O3)	(O4)	(O5)	(O6)	(O7)
strategic rivalry	0.24 (0.16) ¹						
int. war		0.00 (0.06)					
MID			0.00 (0.05)				
SRG count ^(1/4)				0.02 (0.04)			
pres. cap. bal ^(1/10)					0.05 (0.11)		
lat. cap. bal ^(1/10)						0.06 (0.11)	
TEK rival							-0.03 (0.05)
excl: officers _{t-1}	5.30*** (0.25)	5.28*** (0.26)	5.28*** (0.26)	5.27*** (0.26)	5.28*** (0.26)	5.29*** (0.25)	5.28*** (0.27)
Constant	-2.96*** (0.26)	-2.75*** (0.16)	-2.75*** (0.18)	-2.78*** (0.20)	-2.82*** (0.28)	-2.83*** (0.27)	-2.75*** (0.16)
Observations	764	764	764	764	692	700	764
Pseudo R^2	0.289	0.288	0.288	0.288	0.288	0.288	0.288
AIC	712.56	713.45	713.45	713.43	645.01	651.96	713.44

Fractional logit regression, DV = ethnic exclusion in officer corps

Country-clustered standard errors in parentheses

+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

1: $p = 0.15$

Table 3.8 displays the results for the regressions modeling the effects of unrest-based measures of internal threat on the level of ethnic exclusion in the officer corps. **Civil war** and **economic growth rate** are the only predictors that come anywhere close to significance, and growth rate falls slightly above the 0.1 level at $p = 0.14$. Civil war is positive, as expected, since states faced with an armed rebellion should be more likely to exclude politically unreliable groups from the military, in order to prevent defections if the military should have to be deployed against the rebellion. The similar effect that was hypothesized for nonviolent forms of internal threat does not appear in these results; apparently, armed rebellion is the only form of uprising that leaders feel the need to respond to by limiting the recruitment in the officer corps to politically loyal groups.

Unexpectedly, the coefficient on economic growth rate is positive, implying that better economic times lead to higher levels of ethnic stacking, rather than the reverse. The effects of civil war and economic growth rate persist in model O15, when both variables are entered together.

Table 3.8: Internal threat (unrest) and ethnic exclusion in the officer corps

	(O8)	(O9)	(O10)	(O11)	(O12)	(O13)	(O14)	(O15)
rev. uprising	0.09 (0.09)							
civil war		0.16* (0.08)						0.18* (0.09)
strikes ^(1/4)			-0.12 (0.09)					
guerrillas ^(1/4)				0.03 (0.05)				
riots ^(1/4)					0.00 (0.04)			
protests ^(1/4)						0.01 (0.04)		
econ. growth rate							0.01 (0.01) ¹	0.01 (0.01) ²
excl: officers _{t-1}	5.28*** (0.27)	5.27*** (0.26)	5.27*** (0.27)	5.27*** (0.27)	5.27*** (0.27)	5.27*** (0.27)	5.31*** (0.27)	5.31*** (0.27)
Constant	-2.76*** (0.17)	-2.76*** (0.16)	-2.75*** (0.17)	-2.76*** (0.17)	-2.75*** (0.17)	-2.76*** (0.17)	-2.76*** (0.17)	-2.78*** (0.17)
Observations	764	764	752	752	752	752	737	737
Pseudo R^2	0.288	0.288	0.286	0.286	0.286	0.286	0.289	0.289
AIC	713.36	713.24	703.78	703.81	703.82	703.82	685.13	686.89

Fractional logit regression, DV = ethnic exclusion in officer corps

Country-clustered standard errors in parentheses

+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$ 1: $p = 0.14$ 2: $p = 0.11$

Table 3.9 displays the regime-based measures of threat and their effect on ethnic exclusion in the officer corps. We see that the only internal-threat variables here that are statistically significant are **regime instability** and **ln ethnic threat**, both of which are positive, as predicted. These effects endure when these two variables are included together in model O22 – in fact, the effect of regime instability becomes more statistically significant in that model, with the p -value going from just above 0.1 to below 0.05.

The results from this table suggest that, as expected, political regimes going through times of institutional instability and states where the population-share ratio of politically excluded groups to included groups is higher tend to result in an officer corps that is recruited from a narrower ethnic base. However, other types of internal threat focused on the regime, its structure, and its leadership do not make any difference for this form of ethnic recruitment.

Table 3.9: Internal threat (regime) and ethnic exclusion in the officer corps

	(O16)	(O17)	(O18)	(O19)	(O20)	(O21)	(O22)
irreg. entry	0.06 (0.12)						
ln tenure	-0.01 (0.02)						
irreg. entry \times tenure	-0.11 (0.08)						
regime instab.		0.08 (0.05) ¹					0.09* (0.04)
minority reg.			-0.02 (0.08)				
ln eth. threat				0.07 ⁺ (0.04)			0.07 ⁺ (0.04)
personalist reg.					0.04 (0.10)		
military reg.						0.15 (0.11)	
excl: officerst-1	5.22*** (0.21)	5.28*** (0.26)	5.29*** (0.28)	5.04*** (0.24)	5.30*** (0.27)	5.31*** (0.28)	5.04*** (0.24)
Constant	-2.67*** (0.14)	-2.76*** (0.17)	-2.75*** (0.16)	-3.18*** (0.32)	-2.76*** (0.17)	-2.77*** (0.17)	-3.19*** (0.32)
Observations	741	764	764	764	707	707	764
Pseudo R^2	0.290	0.288	0.288	0.291	0.288	0.288	0.291
AIC	695.33	713.38	713.44	710.94	648.61	648.51	712.85

Fractional logit regression, DV = ethnic exclusion in officer corps

Country-clustered standard errors in parentheses

⁺ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$ 1: $p = 0.10$

Table 3.10 displays the results from the full model with all variables included that were found to be statistically significant in previous models of ethnic stacking in the officer corps. After running these variables together in model O23, I also examined the effect of each of the four original control variables listed in Table 3.1 in the same manner as done in the section on conscription onset, by running them one at a time in a one-predictor model. The only one of these variables that came close to statistical significance was \ln GDP per capita, with a p -value just above 0.1, so I include that variable in a second combined model in this table, O24, to address the possibility of omitted variable bias, examining whether any of the effects of the other variables in O23 may be shown to be spurious once this control is included.

We see from the two models in this table that **strategic rivalry**, the only form of external threat that had appeared to matter back in Table 3.7 for ethnic stacking in the officer corps, has no effect once we control for the various forms of internal threat. **Civil war** and **\ln ethnic threat** retain their significance and positively-signed coefficients. **Regime instability** remains in the $0.10 < p < 0.15$ range in both models, and the coefficient remains positive as before. While **economic growth rate** is statistically significant in model O23, the effect disappears when we control for **\ln GDP per capita** (which is itself not significant), suggesting that this odd result really was the effect of omitted variable bias.

Based on the results from this section and in particular from Table 3.10, we can fairly confidently say that external threat has no generalizable impact on ethnic stacking in the officer corps. Rather, this form of recruitment responds mainly to internal threat, specifically in the form of civil war, institutional instability, and the latent threat of ethnic rebellion due to a higher ratio of excluded-to-included size of ethnic groups in the population.

3.4.4 Determinants of Ethnic Exclusion in the Rank and File

I now move to the third set of analyses, focusing on ethnic stacking in the enlisted portion of the military. In Table 3.11, we see that none of the external-threat measures are statistically significant in their relationship with ethnic stacking. Even **strategic rivalry**, which had been significant initially when examining its effect on stacking in the officer corps, is nowhere close to significance here. I discuss this complete non-finding further below.

Table 3.10: Ethnic exclusion in the officer corps: combined model

	(O23)	(O24)
strategic rivalry	0.11 (0.10)	0.11 (0.11)
civil war	0.16** (0.06)	0.16** (0.06)
econ. growth rate	0.01 ⁺ (0.01)	0.01 (0.01)
regime instab.	0.06 (0.04) ¹	0.06 (0.04) ²
ln eth. threat	0.06 ⁺ (0.03)	0.06 ⁺ (0.03)
ln GDP pc		-0.00 (0.04)
excl: officers _{t-1}	5.09*** (0.24)	5.08*** (0.23)
Constant	-3.27*** (0.35)	-3.23*** (0.49)
Observations	737	737
Pseudo R^2	0.292	0.292
AIC	690.24	692.24

Fractional logit regression, DV = ethnic exclusion in officer corps

Country-clustered standard errors in parentheses

⁺ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

1: $p = 0.12$.

2: $p = 0.13$.

Table 3.11: External threat and ethnic exclusion in the rank and file

	(E1)	(E2)	(E3)	(E4)	(E5)	(E6)	(E7)
strategic rivalry	0.19 (0.17)						
int. war		0.13 (0.19)					
MID			0.09 (0.08)				
SRG count ^(1/4)				0.05 (0.07)			
pres. cap. bal ^(1/10)					0.05 (0.13)		
lat. cap. bal ^(1/10)						0.03 (0.14)	
TEK rival							-0.10 (0.12)
excl: enlisted _{t-1}	5.95*** (0.42)	5.94*** (0.44)	5.94*** (0.43)	5.94*** (0.43)	5.97*** (0.44)	5.96*** (0.44)	5.96*** (0.45)
Constant	-3.26*** (0.31)	-3.11*** (0.26)	-3.15*** (0.26)	-3.17*** (0.29)	-3.18*** (0.32)	-3.16*** (0.31)	-3.10*** (0.25)
Observations	786	786	786	786	714	722	786
Pseudo R^2	0.325	0.325	0.325	0.325	0.324	0.325	0.325
AIC	638.82	639.29	639.14	639.23	572.09	584.05	639.27

Fractional logit regression, DV = ethnic exclusion in rank and file

Country-clustered standard errors in parentheses

+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 3.12 shows the effect of unrest-centered measures of internal threat on ethnic exclusion in the rank and file. As with stacking in the officer corps, **civil war** is positive, although here its significance level is just above 0.1. The only other statistically significant internal-threat variable in this table is **economic growth rate**, which as before is unexpectedly positive. Both of these variables retain their positive coefficient and their significance when entered together in model E15; civil war even increases in significance.

As with stacking in the officer corps, nonviolent forms of unrest do not seem to matter for stacking in the rank and file.

Table 3.12: Internal threat (unrest) and ethnic exclusion in the rank and file

	(E8)	(E9)	(E10)	(E11)	(E12)	(E13)	(E14)	(E15)
rev. uprising	0.11 (0.18)							
civil war		0.25 (0.16) ¹						0.28 ⁺ (0.16)
strikes ^(1/4)			-0.12 (0.09)					
guerrillas ^(1/4)				0.10 (0.13)				
riots ^(1/4)					-0.10 (0.08)			
protests ^(1/4)						-0.07 (0.05)		
econ. growth rate							0.02** (0.01)	0.02** (0.01)
excl: enlisted _{t-1}	5.95*** (0.44)	5.95*** (0.44)	5.93*** (0.43)	5.93*** (0.43)	5.92*** (0.43)	5.92*** (0.43)	5.96*** (0.44)	5.99*** (0.45)
Constant	-3.11*** (0.26)	-3.12*** (0.26)	-3.09*** (0.25)	-3.11*** (0.26)	-3.08*** (0.25)	-3.09*** (0.25)	-3.12*** (0.26)	-3.15*** (0.27)
Observations	786	786	774	774	774	774	759	759
Pseudo R^2	0.325	0.325	0.326	0.326	0.326	0.326	0.326	0.326
AIC	639.25	638.99	629.05	628.97	628.94	629.01	616.67	618.21

Fractional logit regression, DV = ethnic exclusion in rank and file

Country-clustered standard errors in parentheses

⁺ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$ 1: $p = 0.12$

Table 3.13 shows the effect of the set of regime-based internal-threat measures on ethnic stacking in the rank and file. The only variable with an effect distinguishable from zero in this table is the interaction term **irreg. entry** \times **tenure**, which is negatively signed. The constituent terms in this model, **irregular entry** and **ln tenure**, are both in the direction expected, but neither is statistically significant.

Table 3.13: Internal threat (regime) and ethnic exclusion in the rank and file

	(E16)	(E17)	(E18)	(E19)	(E20)	(E21)
irreg. entry	0.18 (0.19)					
ln tenure	-0.01 (0.03)					
irreg. entry \times tenure	-0.27 ⁺ (0.14)					
regime instab.		-0.05 (0.10)				
minority reg.			0.08 (0.16)			
ln eth. threat				0.03 (0.04)		
personalist reg.					0.07 (0.24)	
military reg.						-0.16 (0.20)
excl: enlisted _{<i>t</i>-1}	5.82*** (0.39)	5.94*** (0.43)	5.88*** (0.42)	5.83*** (0.40)	6.02*** (0.48)	6.04*** (0.47)
Constant	-3.00*** (0.24)	-3.09*** (0.25)	-3.10*** (0.26)	-3.31*** (0.37)	-3.13*** (0.27)	-3.12*** (0.26)
Observations	763	786	786	786	729	729
Pseudo R^2	0.336	0.325	0.325	0.325	0.319	0.319
<i>AIC</i>	613.44	639.33	639.22	638.63	575.00	574.96

Fractional logit regression, DV = ethnic exclusion in rank and file

Country-clustered standard errors in parentheses

⁺ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

The models in Table 3.14 combine the predictors that were shown in previous tables to be statistically significant. As with Table 3.10, I include an additional model, E22, that inserts the only control variable that came close to significance when run on its own, **ln GDP per capita**. This control variable is once again not significant when included together with the combined measures of threat, but as in Table 3.10, the seemingly positive effect of **economic growth rate** is once again shown to be spurious once we control for GDP per capita.

Civil war remains positively and significantly related to stacking in the enlisted portion, as predicted. **Irregular entry**, **ln tenure**, and their interaction, **irregular entry** \times **tenure** continue the pattern shown in the previous table in model E21. In E22, the constituent term **irregular entry** also becomes significant; its effect is positive, as predicted, suggesting that leaders who come to power by irregular means tend to create an more ethnically exclusive rank and file, but the effect wears off over time, as shown by the negative interaction term.

Table 3.14: Ethnic exclusion in the rank and file: combined model

	(E21)	(E22)
civil war	0.43** (0.16)	0.48** (0.16)
econ. growth rate	0.02* (0.01)	-0.00 (0.01)
irreg. entry	0.07 (0.16)	0.20 ⁺ (0.11)
ln tenure	-0.01 (0.03)	0.01 (0.03)
irreg. entry \times tenure	-0.25 ⁺ (0.13)	-0.27* (0.13)
ln GDP pc		0.09 (0.07)
excl: enlisted _{t-1}	5.86*** (0.40)	5.99*** (0.46)
Constant	-3.02*** (0.25)	-3.93*** (0.76)
Observations	740	740
Pseudo R^2	0.336	0.337
AIC	598.60	599.63

Fractional logit regression, DV = ethnic exclusion in rank and file

Country-clustered standard errors in parentheses

⁺ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

3.5 Discussion and Conclusion

This chapter showed that different types of military-recruitment policies in authoritarian regimes tend to respond to different kinds of threat-based factors. External threat tends to influence the onset of conscription. This is true of both present threat, in the form of interstate war, as well as one type of latent threat, strategic rivalry. External threat has no effect on ethnic stacking though, at least that could be shown in the sample for which data on stacking are available.

Internal threat also matters in different ways for different recruitment policies. Nonviolent forms of domestic unrest had no effect on conscription onset or the levels of stacking in either portion of the military. Revolutionary uprising, which forms the dependent variable for the following chapter, also had no effect on any of these recruitment policies. Although the lack of any relationship between revolutionary uprisings and military composition goes against the predictions of the theory, at the same time it relieves this study of the burden of disentangling what would otherwise be an endogenous relationship. Having shown that, at least under the assumptions of the research design in this chapter, revolutionary uprisings do not affect military design, this study is able to move directly to the next phase and see whether the military design affects uprisings, without having to worry about a concern of reciprocal causation.

With regard to non-revolutionary forms of unrest, civil war²⁴ is positively and robustly related to ethnic stacking in both the officer corps and the rank and file. This was the only type of domestic-unrest measure that had any effect on any of the recruitment policies analyzed, and it had no effect on conscription onset.

Several regime-based measures of internal threat made a difference, some of them even across multiple recruitment policies. The manner in which the state leader came to power, whether by regular or irregular entry, had a direct and observable effect on conscription use and ethnic stacking in the rank and file. Irregular-entry leaders, who tend to face a higher threat of ouster from the start of their tenure, tended to make the rank and file more heavily stacked than leaders who came to power by regular means, as predicted. On the other hand, they also tended to adopt military conscription sooner than their regular-entry counterparts, which goes against the theory. It is not clear why irregular-entry leaders would narrow the recruitment base in the enlisted portion of the

²⁴The measure takes into account all types of insurgencies, which frequently involve secessionist attempts or movements trying to obtain greater autonomy.

military in one way (ethnically) but simultaneously broaden it in another way (conscription use); this seeming contradiction merits further examination by future work.

In addition, the longer a given state leader in an autocracy had been in office, implying a lower threat of overthrow due to regime consolidation, the more likely he or she was to adopt conscription, as predicted. Tenure had no direct effect on either type of stacking, but a longer tenure tended to reduce the stacking-promoting impact of irregular entry on the rank and file, in an interactive manner. Two other features of the regime, regime instability and ethnic threat, also mattered but only for stacking in the officer corps. Higher levels of institutional instability in the regime tended to promote higher levels of stacking, most likely in the attempt to avert coups d'état. Higher levels of ethnic threat, measured as the ratio of the excluded ethnic population proportion to the included proportion, also induced the state leadership to engage in greater levels of stacking, as predicted.

Finally, status as a former British colony was robustly and negatively related to conscription use, following previous research. There was also a very strong carryover effect from ethnic stacking in previous years to the present, which showed up in every model.

The possible concern arises that the lack of a relationship between external threat and ethnic stacking may result from selection bias due to using a sample that is truncated on the range of the independent variable – especially since these analyses were limited to the MENA region, which accounts for a disproportionately large share of the interstate conflict in the world. I directly explored this possibility in Table 3.2 above. While the MENA region was indeed higher on average on every measure of external threat than the global sample as a whole, there was still a good deal of variation within the Middle East on all of these measures. In other words, despite this geographic limitation to a high-threat region, external threat still varies enough within the region that if there were a real-world relationship between ethnic stacking and external threat, it should have shown up in these results. It is still possible, though, that even if external threat made no difference within the MENA region, state leaders in other regions might actually respond to higher threat from foreign enemies by varying the ethnic composition of the military. Until data is available for ethnic stacking on a global basis, this possibility and the generalizability of this result remains an open question. Future extensions of this study should therefore expand the sample beyond the Middle East to see if the patterns found here for the determinants of ethnic stacking hold once autocratic regimes from all regions are included in the sample.

Additionally, it would also be useful to vary the lag period used. This study used a one-year lag for all independent variables on the assumption that this would be just enough time to give the regime an opportunity to retool national military recruitment policies and make a change in the military's social composition. However, since military policies and composition tend to be slow-moving outcomes, it is interesting to consider the possibility that events like wars may have effects that last for years or even perhaps decades. For instance, European countries used conscription during the Cold War to maintain mass armies to protect themselves from possible Soviet invasion, but the wave of transitions to volunteer militaries did not take place for the most part until well over a decade after the fall of the Soviet Union.

Chapter 4

Testing the Uprising Stage of the Theory

4.1 Introduction

This chapter's theoretical coverage begins at the point where the autocratic regime has already established the military recruitment policy, and the populace considers whether to rebel or not. Potential violent or nonviolent rebels are expected to take the military's social composition into account, calculating whether the military could be swayed to join their cause or to stand by if they were to attempt a revolution. Hence, whereas conscription use and ethnic stacking in the military were dependent variables in Chapter 3, in the present chapter they enter as independent variables as key predictors of violent and nonviolent mass uprisings.

Before beginning, I briefly restate the hypothesis that I am testing in this portion of the study. If the theory is correct, we should expect that states where the military is more socially inclusive in its composition would be more likely to experience nonviolent mass uprisings, while states where the military is more socially exclusive would be more likely to experience violent mass uprisings (or, alternatively, no uprisings). In considering that statement, it is important to more clearly conceptualize "mass uprising," which I define as an anti-government movement of significant size that adopts revolutionary goals.

Very little previous quantitative research has taken into account the strategic choice between

nonviolence and violence for anti-government movements, focusing either on nonviolent campaigns exclusively or violent campaigns exclusively (although see Cunningham 2013). In contrast, this chapter does examine that strategic choice of movement tactics, and by focusing on the repressive apparatus that provides the context of potential uprisings, this portion of the study allows us to examine one potentially important determinant of the choice of opposition tactics.

4.2 Sample

4.2.1 Revolutionary Campaigns

My starting point for the analysis in this chapter is to obtain a sample of anti-government movements, both violent and nonviolent, that could reasonably be considered as proto-revolutions. I turn to the NAVCO dataset (Chenoweth and Lewis 2013b; Chenoweth and Lewis 2013a) as my starting point in this task, which provides a sample of anti-government campaigns that meet the following conditions: 1. The campaign has at least 1,000 active participants. 2. The campaign at some point makes “maximalist” demands: secession, self-determination, or regime change.

Whether violent or nonviolent, revolution requires, in the first place, a hard-core group of revolutionaries willing to take initiative and pay the personal start-up costs necessary to overcome the collective action problem and to get the movement going. However, for a popular movement to succeed against a state with its military, internal-security forces, and the ponderous weight of sovereignty behind it, those revolutionaries need to attract widespread participation if they want to establish themselves as an alternative government in embryo with the ability and legitimacy to be able to displace the ruling regime. Hence, I do not consider uprisings prior to having reached a significant size, as smaller protests and insurgencies may be easily quashed by the internal security forces. My use of the NAVCO dataset essentially meets this criterion through its floor of 1,000 participants for inclusion in the dataset.¹

Similarly, I only consider uprisings after the point where they have adopted revolutionary goals – essentially, those that would require that the leader step down from power. To that end, I only keep campaigns that sought regime change or significant institutional reform, as coded by NAVCO.

¹This coding rule is followed fairly strictly in the dataset, although there are a few isolated instances of violent campaign-years coded as having less than 1,000 participants.

The latter category, significant institutional reform, represents demands just short of regime change, such as extensive political liberalization.² I code onset of a revolutionary campaign as the first year in which that campaign had revolutionary goals.³ I acknowledge that protests about economic issues or insurgent movements fighting for the right to secede from the state may adopt revolutionary goals well after the movement has started. These campaigns would enter my sample only after transitioning to making revolutionary demands. In doing so, I follow a pattern similar to that of NAVCO here for when to begin coverage of a movement: as NAVCO focuses on campaigns with maximalist demands, it essentially disregards campaign-years where demands came short of that standard.

In addition, since the scope of the theory being tested is limited to autocratic settings, I only retain campaigns that break out in independent autocracies.⁴ For independent state-years, I use Gleditsch and Ward's (1999) list, which makes minor modifications to the Correlates of War set. I operationalize autocracy the same as in Chapter 3, using Polity IV data (Marshall, Gurr, and Jaggers 2016). As part of this criterion, I only keep campaigns that target their own government, since the theory is not meant to apply to the political leaders and military forces of foreign countries. This condition excludes independence movements like the 1952-1954 movement in Tunisia that won Tunisia's independence from France, or Lebanon's Cedar Revolution in 2005 against the occupying Syrian military.

In making the determination of the country's regime type, I lag Polity's regime coding by one year. Polity tends to code regime late in the calendar year, and using data from the same year as the uprising would cause me to wrongly exclude campaign-years that succeeded in overthrowing a dictatorship and establishing something more democratic in its place by the end of the year. I make adjustments to the regime-type coding for a few campaigns to accommodate the reality of the

²Examples from NAVCO include Uruguay 1984-1985, Egypt 2000-2005, and Poland 1968, 1970, and 1976.

³I recoded goals for a few campaign-years based on evidence I encountered while carrying out this study. These instances include:

- Anti-Banda campaign in Malawi 1992: goal recoded as regime-change.
- KDPI campaign in Iran 1982-1983: goal recoded as autonomy.
- Karens campaign Myanmar 1988-2006: goal recoded as autonomy.

⁴A corollary to this criterion is that the state must have sovereign control over its own military (and have a military of its own to begin with), a requirement that excludes two NAVCO uprisings: East Germany 1953, where the military was not established until three years later (Herspring 1998, 21), and South Korea 1960, which had a military that was legally subordinate to the United Nations command whenever combat-activated, including for martial law (Kim 1983, 6).

situation on the ground in each, including using the non-lagged regime type coding for the first year of independence.⁵ In applying the regime-type criterion, I drop campaigns that break out during non-autocratic years. Revolutionary movements that emerge during non-autocratic periods and endure after the regime transitions to autocracy do not fall under the scope of the theory, and while possibly useful to examine in future extensions of this study, the theory as presently formulated only deals with regime responses to revolutionary outbreaks in autocracy. I also drop campaign continuation years that take place in years where the regime is not autocratic.

NAVCO provides data at the campaign-year level, and one of the variables it codes annually for each campaign is whether the campaign was primarily violent or nonviolent.⁶ NAVCO’s authors gathered the set of nonviolent campaigns primarily through reviewing literature on nonviolent conflict and social movements and then corroborating the list using other secondary sources and a panel of experts on nonviolent conflict, while the violent campaigns came primarily from datasets on civil wars and insurgencies (Chenoweth and Lewis 2013a, 3).

4.2.2 Corroborating Campaign Onsets and Active Years

Having correct years of onset and activity for all campaigns is crucially important for this project – not only for this chapter where I use this information for my dependent variables, but even more so for the following chapter, where I will be examining military deployment (and if deployed, whether the military defects) in response to campaign activity. In order to obtain correct start dates and years of activity, I systematically assess NAVCO campaign-years against two events-based datasets. The

⁵Other changes include:

- The Nepalese Anti-Government campaign that broke out in 2002 (based on MEC; see below) was in reaction to autocratization that had just barely taken place that year, so I code 2002 as autocratic.
- In Cambodia, the Second Khmer Rouge insurgency is omitted on the grounds that it broke out after the Khmer Rouge were ousted by occupying Vietnamese forces, so this campaign is excluded on the grounds that it was taking place under foreign occupation.
- For the Taliban in Afghanistan in 1992, the lagged regime variable does not capture the reality that this insurgency began simultaneously with a period of anarchy following the end of the Najibullah regime, so I recode that year as anarchy instead of autocracy, which serves to exclude that campaign.
- For the North Vietnamese-sponsored NLF insurgency in South Vietnam, 1973 is coded as autocratic instead of foreign-occupied, since the US pulled early that year, and the insurgency continued throughout the year.
- I code Uruguay 1985 as non-autocratic, thereby excluding that campaign, since the free elections that the protesters had been demanding took place in 1984.

⁶Based on evidence found (Way 2012, 118-119), I recoded the primary method in one case, changing 1962 for the Marxist rebels campaign in Guatemala from violent to nonviolent.

first is UCDP/PRIO’s Armed Conflict Dataset (ACD), v. 4-2015, updated 18 June 2015 (Gleditsch *et al.* 2002; Pettersson and Wallensteen 2015), which I use to corroborate violent campaigns and verify onset and continuation dates for those campaigns. The second is the Major Episodes of Contention (MEC) dataset,⁷ version 1.0 (Chenoweth 2015), used to corroborate information about nonviolent campaigns.

Both ACD and MEC provide data at the state-year level based on campaign-level activity. As opposed to NAVCO, which bases its set of campaigns and their dates of activity on expert reports and consensus, MEC and ACD are based on somewhat more concrete evidence: event reports. ACD codes an insurgency as active in a given year if armed anti-state activity by that organization produced at least 25 combat-related fatalities (Gleditsch *et al.* 2002, 618-619). MEC codes a nonviolent campaign as starting only after news reports showed at least two direct-action events of at least 1,000 visible participants within a week’s time, and the campaign lasted longer than a week (Chenoweth and Ulfelder *n.d.*, 13-14); the same maximalist-goals requirement from NAVCO apply to MEC as well. These two datasets, ACD and MEC, therefore provide an ideal aid in the attempt to nail down the active years of revolutionary campaigns.

MEC also provides information on the existence of a number of campaigns that are not in NAVCO. I do not include these in my sample, though, since only NAVCO currently provides information on campaign goals, necessary for my purposes to know if the campaign qualifies as revolutionary in a given year. In an ideal world, I could investigate each non-NAVCO campaign provided by ACD and MEC to code these variables, but doing so would go beyond what is possible for this project.

A systematic comparison between MEC / ACD and the set of NAVCO revolutionary campaigns that I identified using the criteria outlined above shows that while the presence of nearly all, although not all, of these campaigns is corroborated, the start and end dates differ frequently. I therefore run through the entire set of NAVCO campaigns that meet the regime-type, target, and goals criteria, removing the few campaigns that are not corroborated at all by MEC / ACD, and adjusting the start dates and active years accordingly.⁸

⁷Made publicly available by Chenoweth and Ulfelder (*n.d.*) as part of their replication data at <https://raw.githubusercontent.com/ulfelder/nonviolent-uprisings-replication/master/data/nvc.transformed.csv>; accessed 9 May 2016.

⁸A few examples serve to highlight the differences between NAVCO coverage and MEC / ACD coverage. For instance, while NAVCO lists the FSLN (Sandinistas) rebel campaign as starting in 1978, ACD lists 1977 as the first

For each campaign, I also establish the start month, meaning the first month of qualifying nonviolent direct-action or insurgent activity, using a variety of secondary sources: Google Books, news reports (mainly found through Factiva), Swarthmore College’s Global Nonviolent Action Database,⁹ the Civil Unrest Event Dataset from the SPEED Project (updated 14 December 2012),¹⁰ the Non-State Actors dataset (v. 3.4, updated 23 November 2013; Cunningham, Gleditsch, and Salehyan 2009, 2013), and other sources.¹¹ This step allows me to confirm the campaign’s onset and is mainly taken in preparation for Chapter 5, where I will be coding the regime’s response to campaign activity. For new campaign-years added, I used the same kinds of sources to code the campaign’s size and goals.¹²

The resulting sample of revolutionary campaigns in autocratic countries, which is listed in its entirety in Table 4.1, includes 34 violent campaign onsets and 53 nonviolent campaign onsets.

year with 25 battle deaths attributed to the Sandinistas. In this case, we would need to extend NAVCO’s coverage for this campaign backward by one year to adequately capture the start of the movement. In a second example, Mali’s anti-military campaign in the early 1990s is listed in NAVCO as taking place from 1990-1992, while MEC codes the campaign as ending in 1991. In this second case, the NAVCO coverage would need to be curtailed by one year. Third, the Sacred Union campaign in the DRC that NAVCO lists as taking place 1991-1995 is not contained within either ACD or MEC, so this campaign is entirely omitted.

⁹ Available from <http://nvdatabase.swarthmore.edu/>

¹⁰ Available from the Cline Center for Democracy at <http://www.clinecenter.illinois.edu/data/speed/event>.

¹¹ The spreadsheet containing the full set of adjustments made to NAVCO campaigns and the full list of sources used is available [here](#).

¹² In the process of using secondary sources to code start months for MEC- / ACD-corroborated start years for revolutionary campaigns, occasionally I encountered evidence of qualifying campaign activity earlier than ACD and MEC had indicated, necessitating moving the start date back, such as the Islamic Salvation Front in Algeria, where a source (Chalcraft 2016, 470) showed that the campaign was active in 1988, four years earlier than NAVCO had indicated. In another example, the Solidarity campaign in Poland is first listed in NAVCO as having qualifying goals starting in 1986, but I was not able to find evidence of qualifying campaign activity that year; the earliest month for which I found such evidence is May 1987 (Wrobel 2014, 2073), so 1987 is coded as the year of campaign outbreak. At least one campaign, Tanzania’s 1992-1995 Pro-Democracy Movement, had to be eliminated entirely after no qualifying events at all were found; the transition to democracy in that country appears to have been a top-down affair with no direct mass action spurring it on.

Table 4.1: Adjusted NAVCO Sample of Revolutionary Campaigns in Autocracy

G&W ID	Country	Outbreak year/month	Outbreak type ¹	Qualifying years	Name ²
40	Cuba	1953/07	1	1953-1958	Cuban Revolution
41	Haiti	1985/11	2	1985-1986	Anti-Duvalier
70	Mexico	1988/07	2	1988-1994	Anti-PRI
90	Guatemala	1962/01	2	1962-1966, 1975-1986	Marxist Rebels / URNG
92	El Salvador	1977/02	2	1977-1982	Civil Conflict
93	Nicaragua	1977/10	1	1977-1979	FSLN
93	Nicaragua	1982/03	1	1982-1990	Contras
95	Panama	1987/06	2	1987-1989	Anti-Noriega
101	Venezuela	1958/01	2	1958	Anti-Jimenez
110	Guyana	1990/01	2	1990-1992	Anti-Burnham/Hoyte
140	Brazil	1984/01	2	1984	Diretas Ja
145	Bolivia	1952/04	1	1952	Bolivian Revolution
145	Bolivia	1977/11	2	1977-1982	Anti-Junta
150	Paraguay	1947/03	1	1947	Leftists
155	Chile	1983/05	2	1983-1989	Anti-Pinochet
160	Argentina	1981/11	2	1981-1983	Pro-Democracy
165	Uruguay	1984/01	2	1983-1984	Anti-Military
265	East Germany	1989/09	2	1989	Pro-Democracy
290	Poland	1956/06	2	1956	Poznan Protests
290	Poland	1968/03	2	1968	Anti-Communist I
290	Poland	1970/12	2	1970	Anti-Communist II
290	Poland	1976/06	2	1976	Warsaw Worker Uprising
290	Poland	1987/05	2	1987-1989	Solidarity
310	Hungary	1956/10	2	1956	Anti-Communist
310	Hungary	1989/03	2	1989	Pro-Democracy
315	Czechoslovakia	1989/11	2	1989	Velvet Revolution
339	Albania	1990/12	2	1990	Anti-Communist
345	Yugoslavia	1996/11	2	1996-2000	Anti-Milosevic
350	Greece	1973/11	2	1973-1974	Anti-Military
355	Bulgaria	1989/11	2	1989-1990	Anti-Communist

360	Romania	1987/11	2	1987-1989	Anti-Ceaucescu
365	Russia	1991/01	2	1991	Pro-Democracy
370	Belarus	2006/03	2	2006	Regime Opposition
432	Mali	1990/12	2	1990-1991	Anti-Military
434	Benin	1989/12	2	1989-1990	Anti-Communist
450	Liberia	1989/12	1	1989-1990	Anti-Doe
450	Liberia	2000/07	1	2000-2003	LURD
451	Sierra Leone	1991/03	1	1991-1996	RUF
475	Nigeria	1993/07	2	1993-1999	Anti-Military
483	Chad	1966/07	1	1966-1968, 1971-1979, 1986-1990	FROLINAT
483	Chad	1991/12	1	1991-2006	Anti-Deby
490	DRC	1977/08	1	1977-1978	FLNC
500	Uganda	1988/02	1	1988-2006	LRA
501	Kenya	1990/07	2	1990-1991	Anti-Arap Moi
516	Burundi	1991/11	1	1991-1993, 1997-2002	Third Hutu Rebellion ³
517	Rwanda	1990/10	1	1990-1994	Tutsi Rebels
520	Somalia	1982/01	1	1982-1991	Clan Factions/SNM
522	Djibouti	1991/11	1	1991-1994	Afar Insurgency
530	Ethiopia	1981/02	1	1981-1991	TPLF
540	Angola	1975/11	1	1975-1992, 1994-2002	UNITA
541	Mozambique	1977/02	1	1977-1992	RENAMO
551	Zambia	1990/06	2	1990-1991	Anti-Single Party
553	Malawi	1992/03	2	1992-1993	Anti-Banda
580	Madagascar	1991/05	2	1991	Active Forces
615	Algeria	1988/10	2	1988-2004	FIS
625	Sudan	1983/05	1	1983-1986, 1990-1994, 1997-2005	SPLA
625	Sudan	1985/03	2	1985	Anti-Jaafar
625	Sudan	2003/03	1	2003-2006	JEM/SLA
630	Iran	1977/10	2	1977-1979	Iranian Revolution
630	Iran	1979/01	1	1979-1997	MEK
645	Iraq	1959/03	1	1959	Shammar Tribe
645	Iraq	1982/08	1	1982-1996	SCIRI
651	Egypt	2000/09	2	2000-2005	Kifaya
652	Syria	1979/06	1	1979-1982	Muslim Brotherhood

678	Yemen	1948/02	1	1948	Yahya Family Revolt
678	Yemen	1962/10	1	1962-1970	Royalists
680	South Yemen	1986/01	1	1986	Leftists
698	Oman	1969/01	1	1969-1975	PFLOAG
700	Afghanistan	1978/04	1	1978	Saur Revolution
703	Kyrgyzstan	2005/03	2	2005	Tulip Revolution
710	China	1979/01	2	1979	Pro-Democracy
710	China	1989/04	2	1989	Tiananmen
732	South Korea	1979/10	2	1979-1980	Anti-Junta
732	South Korea	1986/03	2	1986-1987	Anti-Military
770	Pakistan	1983/08	2	1983	Pro-Democracy
771	Bangladesh	1987/07	2	1987-1990	Anti-Ershad
775	Myanmar	1988/03	2	1988-1990	8888 Uprising
790	Nepal	1990/02	2	1990	The Stir
790	Nepal	2002/10	2	2002-2006	Anti-Government
800	Thailand	1973/10	2	1973	Student Protests
800	Thailand	1974/10	1	1974, 1977-1978	Communist Rebels
800	Thailand	1992/05	2	1992	Pro-Democracy
811	Cambodia	1967/05	1	1967-1975	Khmer Rouge
811	Cambodia	1978/09	1	1978-1979	Anti-Khmer Rouge
817	South Vietnam	1957/01	1	1957-1965, 1973-1975	NLF
840	Philippines	1985/09	2	1985-1986	People Power
850	Indonesia	1997/09	2	1997-1998	Anti-Suharto

¹Outbreak type: 1 = violent, 2 = nonviolent.

²Campaign names abbreviated or changed from NAVCO's names in several instances.

4.3 Methods

To test the prediction about the effect of military composition on violent versus nonviolent campaign onsets, I first employ Cox proportional hazards models, splitting the sample between violent and nonviolent campaigns to analyze the hazard of each type of campaign.¹³ My dependent variable for this first set of tests is the onset of the given type of campaign, and the statistical estimator used models the time until event occurrence. I treat countries as at risk of onset of revolutionary campaign during all autocratic years.¹⁴ I also cluster standard errors by country and use the Efron method for ties. All independent variables used in the analyses below are lagged by one year.

In these Cox models, I use a conditional risk set approach with the hazard stratified by event number, which may be seen as a useful alternative to controlling for past violent or nonviolent conflict as a covariate in the models. Stratifying the hazard rate in this way allows me to take into account past uprisings in the modeling process, since the hazard of a country experiencing a second (or third, or fourth, etc.) uprising may be very different than the hazard of experiencing its first uprising, as non-state actors refine their toolbox for direct-action methods, and other potential contenders learn by observation.

As in the previous chapter, for each of the Cox models, I run the *estat phtest* command, which checks the proportional hazards assumption using Schoenfeld residuals. In cases where individual variables are found to violate this assumption, based on a significant p -value for this test,¹⁵ I interact the offending variable with some function of time, trying various functions until one is found that results in a non-significant test when re-checking the proportional hazards assumption. In each of the statistical tables below that employ Cox models, I display the specific function of time used by variable in these cases (given in italics in the tables).

Since both conscription and ethnic stacking are rather persistent variables – i.e., with little in-country change – their ability to predict something as highly variable as campaign onset may be quite limited. Therefore, in addition to the Cox models run below, I also employ a crude test that omits within-country variation and takes advantage only of cross-country variation. I run

¹³All analyses in this chapter are run in Stata 14.

¹⁴Maintaining this assumption and data set-up means that countries are at risk of a new campaign onset even while there is a campaign ongoing. This accommodates the reality of simultaneous campaigns in the NAVCO dataset, including overlapping revolutionary campaigns.

¹⁵I set a threshold of $p < 0.1$ here.

OLS analyses based on a cross-sectional version of the dataset, described below. Also, as described below, I attempt to account for potential endogeneity of military design to mass uprisings using an instrumental-variables approach as a further supplementary analysis.

4.4 Key Independent Variables

The construction and data sources for conscription and ethnic stacking were explained in Chapter 3. As explained there, the **excluded: officers** and **excluded: rank and file** variables are only available for Middle Eastern and North African (MENA) countries, with data on 15 countries (679 country-years) for **excluded: officers** and 16 countries (703 country-years) for **excluded: rank and file**.

Traditionally, the rule of thumb for logit and Cox models has been to require 10 events per variable (EPV), but Vittinghoff and McCulloch (2007) show that there is more of a gradient in terms of risking biased coefficients, with 1-4 EPV being the worrisome range, 5-9 being reasonably safe, and 10-16 at low but still present risk. Given the tradeoff between risking bias from overtaking a small number of events and risking omitted variable bias, they recommend relaxing the rule of 10 and treating the 5-9 EPV range as acceptable in order to include control variables necessary to prevent omitted variable bias. For the models with conscription as the independent variable, we have 34 violent revolutionary uprisings and 53 nonviolent revolutionary uprisings. To accommodate this low number of events, I limit the number of variables per model to no more than 7, in order to keep the EPV at least in the 5-9 range for the models of the effect of conscription on each type of uprising.

However, within the subsample for which data on stacking are available, there are only 4 nonviolent revolutionary uprising onsets and 11 violent revolutionary uprising onsets within NAVCO's temporal domain. This means that the EPV will unavoidably be low when using stacking as the independent variable. I do not include any control variables in these models to help alleviate this problem, which already means that I will be risking omitted variable bias and cannot show that any results from these models are not the spurious effect of a third variable. In addition to that problem, given that there are only 4 nonviolent uprisings in this region, any results for models of the effect of stacking on nonviolent uprisings should be treated with great caution, due to the additional risk of

bias from falling in the 1-4 EPV range.¹⁶

In order to help deal with this problem for ethnic stacking and nonviolent revolutions – and to relieve a little pressure in terms of EPV from the other models as well – I add a supplementary analysis that broadens the sample of uprising cases to include violent and nonviolent uprisings in general, rather than revolutionary uprisings only, and from all regime types. I take advantage here of the full NAVCO sample of uprisings, with only minor adjustments retained to correct a few coding errors in the dataset and similarly small fixes. Doing so gives me 134 violent uprisings and 96 nonviolent uprisings to work with for the conscription analyses and 28 and 7, respectively, for the stacking analyses. This supplementary analysis also allows me to see whether odd results I may obtain for any of the control variables might be due to the sample selection process, by making the procedures in this chapter more directly comparable to what previous authors have done in studies of nonviolent revolution and insurgency (which nearly always pool across types of uprisings and across regime types).

There is a tradeoff here, though. This robustness check comes at the cost of broadening the set of cases well beyond the scope of the theory, which is meant to apply only to revolutionary uprisings in autocracies. If the results change greatly, it is possible that those divergent results indicate that the revolutionary-uprising models were afflicted by bias from a low EPV, but it may also simply indicate that the theory does not apply outside of its stated scope condition – e.g., that it does not apply to secessionist movements, anti-occupation movements, or movements in non-autocratic regimes.

4.5 Control Variables

In examining the influence of conscription on nonviolent and violent revolution, I control for a variety of potential explanations for violent and nonviolent campaigns. Each of these variables can be expected to directly affect the likelihood of campaign onset as well as the feasibility or utility of conscription as a means for military recruitment.

First, I control for ongoing campaigns in neighboring states to capture the possibility of diffusion: specifically, diffusion of nonviolent campaigns, **NV in neighbor**, in the analyses that use nonviolent

¹⁶With 11 violent uprisings in this region, the EPV for models of this type of uprising is high enough to not be too concerning.

campaign onset as the dependent variable and diffusion of violent campaigns, **VIO in neighbor**, for analyses of violent campaign onset. Following Gleditsch and Rivera (n.d.), I create a binary indicator for whether any of the state’s neighbors experienced a nonviolent movement (any NAVCO movement, not just revolutionary ones) in the year in question, designating neighboring states as those within 500 km of each other’s outer boundaries, using CShapes data (Weidmann, Kuse, and Gleditsch 2010) and the *cshapes* R package, v. 0.5-1 (Weidmann and Gleditsch 2015) to help me generate this variable. I use the same coding rules to generate the variable to capture violent diffusion, based on the the presence of at least one violent campaign in a neighboring state.

I control for **ln GDP per capita**, **ln population**, and **economic growth rate** using the Expanded Trade and GDP dataset, v. 6.0, updated 9 September 2014 (Gleditsch 2002). I calculate economic growth rate as the year-to-year percentage change in GDP. I also control for **ln urban population** using United Nations (2014) data.¹⁷

Since high levels of external threat may dissuade the regime’s political opponents from taking direct action, based on rally-round-the-flag logic, I employ three measures of external threat: whether the state is involved in an **interstate war**, based on the COW war dataset v. 4.0, updated 1 March 2011 (Sarkees and Wayman 2010); whether the state is involved in a **MID**, from the COW MID dataset v. 4.01, updated 5 February 2014 (Jones, Bremer, and Singer 1996; Palmer et al. 2015); as well as the number of potential enemy countries in the state’s strategic reference group (Maoz 2011).¹⁸ As the SRG count variable is both heavily right-skewed and also has a sizeable number of zeros (for countries with no SRG members in the given year), I use a power transformation instead of a log transformation to create the variable **SRG count**^(1/4).

Finally, to capture the effect of the nonmilitary coercive forces under the state’s control, I control for the size of the state’s paramilitary forces in terms of the total personnel count, compiled by Pilster and Böhmelt (2012, 2015) from the IISS annual periodical *Military Balance*.¹⁹ I transform the variable to generate **paramilitaries**^(1/8) for reasons similar to SRG count, and I compensate for somewhat inconsistent year-to-year coverage by the *Military Balance* by taking a 3-year moving average of the variable. The temporal domain of this variable is rather limited compared to the

¹⁷Available from https://esa.un.org/unpd/wup/CD-ROM/WUP2014_XLS_CD_FILES/WUP2014-F19-Urban_Population_Annual.xls

¹⁸The author thanks Zeev Maoz for sharing the data on this variable on 30 March 2016.

¹⁹I use all paramilitary forces, not just ground combat-capable forces as those authors do.

other control variables, as 1970 was the first year that *Military Balance* started providing data on paramilitary numbers for a large sample of countries; this reduces further to 1972 onward after taking the 3-year moving average.

Tables 4.2-4.4 show summary statistics for the variables used in these analyses. Table 4.2 covers the sample that includes only revolutionary campaigns. Table 4.3 covers the full sample of NAVCO campaigns. Table 4.4 covers the cross-sectional sample to be used in the OLS analyses.

Table 4.2: Summary statistics: state-year version, revolutionary campaigns only

	count	mean	sd	min	max
DVs					
VIO onset	4195	.0083433	.0909705	0	1
NV onset	4195	.0126341	.1117025	0	1
IVs					
conscription	3714	.597469	.4904738	0	1
excl: officers	716	.3540084	.2851674	0	1
excl: enlisted	740	.2755946	.2704858	0	.75
Controls					
NV in neighbor	4146	.1874096	.390287	0	1
VIO in neighbor	4146	.6186686	.4857723	0	1
ln GDP pc	3957	7.783732	1.065876	4.888995	13.35702
ln pop.	3957	15.77753	1.517883	11.68022	20.97389
Econ. growth rate	3872	.051212	.4070175	-.8295917	22.96208
ln urban pop.	3739	14.54977	1.703566	8.294049	20.14437
interstate war	4146	.0364206	.187357	0	1
MID	4146	.3598649	.4800185	0	1
SRG count ^(1/4)	4146	1.174662	.8283858	0	3.253153
Paramilitaries ^(1/8)	2655	2.342106	1.835293	0	9.981126

4.6 Results

4.6.1 Revolutionary Campaigns

Tables 4.5 and 4.6 show the results from the set of models predicting nonviolent and violent revolutionary onset. We see from models NV1-NV5 that **conscription** has no statistically significant relationship with onset, and the sign is in the opposite direction from that predicted, in any case. Models NV6 and NV7 show the effect of ethnic stacking in the officer corps and the rank and file,

Table 4.3: Summary statistics: state-year version, full NAVCO set of campaigns

	count	mean	sd	min	max
DVs					
VIO onset	7956	.0168426	.1286897	0	1
NV onset	7956	.0120664	.1091891	0	1
IVs					
conscription	7197	.5935807	.4911987	0	1
excl: officers	932	.3525429	.2728609	0	1
excl: enlisted	956	.2637552	.254941	0	.75
Controls					
democracy	7956	.3141026	.4641866	0	1
autocracy	7937	.5285372	.4992164	0	1
NV in neighbor	7852	.2130667	.4095005	0	1
VIO in neighbor	7852	.5668619	.4955409	0	1
ln GDP pc	7490	8.191616	1.184703	4.888995	13.35702
ln pop.	7490	15.88813	1.556271	11.68022	20.97389
Econ. growth rate	7323	.0439213	.3010336	-.8295917	22.96208
ln urban pop.	7221	14.87641	1.758066	8.294049	20.14437
interstate war	7852	.0390983	.1938412	0	1
MID	7852	.3632196	.480958	0	1
SRG count ^(1/4)	7852	1.14737	.845206	0	3.260391
Paramilitaries ^(1/8)	5239	2.004972	1.763149	0	9.981126

Table 4.4: Summary statistics: state version (rev. campaigns only)

	count	mean	sd	min	max
DVs					
prop. yrs NV	122	.0341323	.0637246	0	.375
prop. yrs VIO	122	.0740099	.1814604	0	.8709677
IVs					
prop. years conscript	122	.543752	.427589	0	1
avg. stacking officers	16	.3167519	.2693571	0	.7244262
avg. stacking enlisted	17	.2508268	.2673844	0	.7

respectively, on time until nonviolent campaign onset. We see in these models that, as expected, higher levels of **exclusion in the officer corps** tend to dissuade opposition forces from engaging in mass protests. The sign on the variable **exclusion in the enlisted portion** is also negative as expected, although the relationship falls just short of conventional levels of statistical significance ($p = 0.14$).

With regard to the control variables in Table 4.5, we see that a larger **ln population** hastens the onset of nonviolent revolutions, while a higher **economic growth rate** lengthens the time until uprising onset. Because of its high correlation with population, I do not include **ln urban population** in the same model as population; in the one model it appears in, NV3, we see a similar effect and magnitude for this variable as for population. The effects of these three variables are as expected and are robust across all models in the table. These results suggest that revolutionary protest movements in autocracies are largely the result of economic grievances and mobilization opportunity based on large (and concentrated) masses of civilians.

NV in neighbor is positive and, in three of the five models, significant at the 0.1 level, showing some similarity to previous findings about the diffusion of nonviolent movements (Gleditsch and Rivera [n.d.](#)). None of the external-threat variables are significant in any of the models. Surprisingly, **paramilitaries** is positive and significant, implying that uprising onsets happen sooner in countries with larger paramilitary forces. In other words, instead of being deterred from attempting to topple the regime through protests and similar means, the prospect of facing state police, gendarmes, and other paramilitary forces appears to make the state's populace more willing to take to the streets. This odd finding may be due to a pattern of backlash against repression by those internal security forces: anger over repression by paramilitaries during the pre-revolutionary stages of nonviolent movements may cause those movements to transition into revolutionary campaigns.

Table 4.5: Onset of NV rev. camps in autocracy

	(NV1)	(NV2)	(NV3)	(NV4)	(NV5)	(NV6)	(NV7)
excl: officers						-3.03* (1.52)	
excl: enlisted							-4.04 (2.74) ¹
conscription	-0.06 (0.29)	-0.37 (0.33)	-0.08 (0.30)	-0.03 (0.30)	-0.02 (0.30)		
Paramilitaries ^(1/8)		0.22* (0.10)					
NV in neighbor	0.53 ⁺ (0.30)	0.50 (0.30)	0.46 (0.29)	0.52 ⁺ (0.31)	0.52 ⁺ (0.31)		
ln GDP pc	0.20 (0.13)	0.09 (0.13)	0.03 (0.13)	0.20 (0.13)	0.21 (0.13)		
ln pop.	0.37*** (0.08)	0.26* (0.10)		0.41*** (0.08)	0.41*** (0.09)		
ln urban pop.			0.41*** (0.08)				
Econ. growth rate	-2.29** (0.85)	-2.36* (0.92)	-3.07** (1.09)	-2.27** (0.87)	-2.32** (0.87)		
interstate war	-0.71 (1.04)	-0.69 (1.10)	-0.76 (1.05)				
MID				-0.46 (0.32)			
SRG count ^(1/4)					-0.22 (0.17)		
Observations	3496	2458	3288	3496	3496	716	740
Events per variable	8.83	6.71	8.50	8.83	8.83	4.00	4.00
Pseudo R^2	0.059	0.084	0.065	0.063	0.061	0.105	0.135
AIC	367.24	310.39	345.59	365.66	366.38	19.96	19.36

Cox conditional risk set models stratified by event no., DV = NV rev. campaign onset

Country-clustered standard errors in parentheses

⁺ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$ 1 : $p = 0.14$

Table 4.6 shows the results for violent revolutionary campaign onset. We see in models VIO1-VIO5 that the effect of **conscription** on campaign onset is again not distinguishable from zero. Models VIO6 and VIO7 show the effect of **exclusion in the officer corps** and **exclusion in the enlisted portion** on the emergence of violent revolutionary campaigns. Neither variable is significant, and the model fit statistics are especially poor for these two models, suggesting no relationship at all between ethnic stacking and violent campaign onset. In other words, potential rebels do not appear to take into account the possibility of shirking from the military based on social distance when considering whether to launch a revolutionary violent rebellion.

There appears to be a diffusion effect from **violent campaigns in neighboring states**, hastening the onset of revolutionary violent campaigns in the home state, and this result is robust across all models in which this variable appears. Similarly, higher levels of **ln GDP per capita** reduce the hazard of violent campaign onset, as expected, although the **economic growth rate** has no discernable impact. Contrary to previous studies, though, the effect of **ln population** on insurgency is negative here, rather than positive. This difference may be the result of examining revolutionary versus non-revolutionary insurgencies: secessionist movements (a very common type of non-revolutionary, violent uprising) would be easier to pull off, and there would be more groups motivated to try, in a large state, while on the other hand, a large population size would be a deterrent to attempts to supplant the central government (the definition of revolutionary violent uprising), due to the difficulty in conquering a populous state.

Since the data availability is more limited for paramilitaries and urban population, I enter these variables separately from the rest of the controls in models VIO4 and VIO5, only also including conscription as a covariate in these models, in order to accommodate the small number of events in the dependent variable. The coefficient for **urban population** is negative, implying that violent revolutionary uprisings are more common in largely rural societies. **Paramilitaries** has no apparent effect on uprising onset.

Finally, two of the three measures of external threat in Table 4.6 are significant, the occurrence of an **interstate war** and occurrence of a **MID**. However, the sign on these two external-threat variables is positive, suggesting that rather than a rally-round-the-flag effect that would deter rebellions, we instead see rebellions with revolutionary demands emerge when the state's attention is focused outward and the military is tied down and therefore less able to re-establish order internally.

Table 4.6: Onset of VIO rev. camps in autocracy

	(VIO1)	(VIO2)	(VIO3)	(VIO4)	(VIO5)	(VIO6)	(VIO7)
excl: officers						1.23 (1.09)	
excl: enlisted							0.52 (1.07)
conscription	0.55 (0.37)		0.57 (0.36)	0.41 (0.39)	0.15 (0.49)		
$\ln(t)$ * <i>conscription</i>		0.13 (0.12)					
Paramilitaries ^(1/8)					-0.03 (0.11)		
VIO in neighbor	1.08* (0.48)	0.96* (0.47)	1.06* (0.48)				
\ln GDP pc	-0.56** (0.17)	-0.58** (0.19)	-0.59** (0.19)				
\ln pop.	-0.27* (0.12)	-0.32** (0.12)	-0.30* (0.13)				
\ln urban pop.				-0.24* (0.12)			
Econ. growth rate	0.43 (0.84)	0.01 (1.03)	0.10 (1.00)				
interstate war	1.33* (0.55)						
$\ln(t)$ * <i>MID</i>		0.31* (0.14)					
SRG count ^(1/4)			0.31 (0.21)				
Observations	3496	3496	3496	3347	2458	716	740
Events per variable	5.17	5.17	5.17	14.00	12.00	9.00	9.00
Pseudo R^2	0.080	0.079	0.069	0.014	0.001	0.025	0.005
<i>AIC</i>	220.48	220.69	222.94	204.58	164.67	37.36	38.68

Cox conditional risk set models stratified by event no., DV = VIO rev. campaign onset

Country-clustered standard errors in parentheses

+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

4.6.2 Robustness Check 1: Broadening the Sample to Include All NAVCO Campaigns

In Tables 4.7-4.10, I broaden the sample to include the full set of NAVCO campaigns in all regime types instead of just revolutionary campaigns in autocracies. This allows me to check the results of the above analysis, to see if results that diverge from previous studies are due to selecting only a certain type of campaign taking place in a certain type of regime. For these tables, I introduce a new dummy variable for regime type based on Polity data: **democracy**, coded as 1 for countries with a Polity2 score of 7 or above.

In Table 4.7, we see that again, the effect of **conscription** on nonviolent campaigns is not statistically distinguishable from zero. This non-finding holds across all models with this variable.

Models NV13 and NV14 examine the effect of ethnic stacking on onset of nonviolent campaigns in general. The sign on both **exclusion in the officer corps** and **exclusion in the enlisted portion** is negative as expected, but neither is statistically significant at conventional levels, although the latter comes close ($p = 0.15$). Therefore, as opposed to revolutionary campaigns, ethnic stacking in the officer corps does not appear to matter for the hazard of nonviolent campaigns in general, while for both types of campaign onsets, we see a hint of the possibility that stacking in the rank and file reduces the hazard of campaign onset.

Similar to what we had seen in Table 4.5, **ln population** and **ln urban population** increase the hazard of uprising onset. Most of the remaining controls have different effects, though, now that we are analyzing all nonviolent campaigns instead of revolutionary campaigns only, and all regime types rather than autocracies only. A diffusion effect of **nonviolent uprising in neighbor** is shown by the positive and significant coefficient for this variable, which holds in all models. **Ln GDP per capita** is now significant (still positive) in some of the models, implying that more economically developed countries have a larger hazard of experiencing nonviolent uprisings, although the effect disappears when controlling for paramilitaries and urban population. As before, the coefficient for **economic growth rate** is still negative, but its statistical significance is now all but completely gone, implying that this variable tends to matter much more for revolutionary campaigns than for other types of campaigns. Similarly, the significance level of **paramilitaries** is reduced,

likely for the same reason.²⁰

Democracy, which was not a control variable in Table 4.5 (since that sample only included autocratic regimes) is negative and significant as expected based on the idea that having institutional avenues available for obtaining redress of grievances obviates the need for taking direct action, and the effect is robust across all models containing this variable. Finally, one of the external-threat variables is now significant, **SRG count**, with the negative sign on the coefficient indicating a lower hazard of nonviolent uprising when facing the threat of a larger number of SRG members.

²⁰That it is significant at all is probably due to the effect of revolutionary campaigns still being felt within this full NAVCO sample of nonviolent onsets, since if there is any heterogeneity in types of campaigns, these regressions using this pooled sample are averaging across all types of campaigns.

Table 4.7: Onset of general NV campaigns

	(NV8)	(NV9)	(NV10)	(NV11)	(NV12)	(NV13)	(NV14)
excl: officers						-1.65 (1.46)	
excl: enlisted							-2.01 (1.39) ¹
conscription	0.07 (0.23)	-0.01 (0.29)	-0.07 (0.25)	0.09 (0.24)	0.15 (0.24)		
democracy		-1.86*** (0.48)			-2.21*** (0.44)		
$(1/t) * democracy$	-71.01*** (20.15)		-70.45*** (20.84)	-72.67*** (20.31)			
Paramilitaries ^(1/8)		0.13 ⁺ (0.08)					
NV in neighbor	0.68** (0.25)	0.72** (0.27)	0.68** (0.26)	0.67* (0.26)	0.68** (0.26)		
ln GDP pc		-0.04 (0.12)					
$(1/t) * \ln GDP pc$	4.78* (2.00)		0.95 (2.48)	4.87* (1.98)	4.75* (1.97)		
ln pop.	0.42*** (0.09)	0.30*** (0.08)		0.47*** (0.09)	0.51*** (0.09)		
ln urban pop.			0.42*** (0.08)				
Econ. growth rate	-1.27 (0.81)	-1.03 (0.92)	-1.17 (0.85)	-1.29 (0.81)	-1.45 ⁺ (0.84)		
interstate war	-0.53 (0.70)	-0.93 (1.06)	-0.47 (0.70)				
MID				-0.39 (0.28)			
SRG count ^(1/4)					-0.39** (0.15)		
Observations	6762	4904	6508	6762	6762	932	956
Events per variable	12.14	8.88	11.29	12.14	12.14	6	6
Pseudo R^2	0.101	0.111	0.097	0.103	0.110	0.037	0.047
AIC	599.67	478.39	558.60	597.90	593.57	30.55	30.26

Cox conditional risk set models, DV = NV campaign onset

Country-clustered standard errors in parentheses

⁺ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$ 1 : $p = 0.15$

Table 4.8 shows the results from the Cox models estimating the hazard of violent campaigns in general in all regime types. Once again, **conscription** appears to have no effect, not being statistically significant in any of these models.

As for the control variables, unlike general nonviolent uprisings, when examining violent uprisings **democracy** is not statistically different from zero, implying that there is no measurable difference between regime types in time until violent uprising. The effect of **ln GDP per capita** has not changed from Table 4.6: still negative and statistically significant, implying that the hazard of experiencing violent uprisings is lower for more developed countries. **Violent uprising in neighbor** is still positive, suggesting a diffusion effect as shown in Table 4.6, although the significance level is reduced in some models. Whereas **population's** impact on revolutionary violent uprisings was negative, this variable (and **urban population** as well) loses all statistical significance when considering all violent uprisings; this non-finding goes against previous studies, where population is generally a strong correlate of civil war. The difference may be attributable to the use of the NAVCO dataset for the sample, as opposed to other, more-often used datasets of civil war. **Economic growth rate** is negative, suggesting that the hazard of insurgency is lower in good economic times, but this effect is only significant in three of the five models. As in Table 4.6, there is no detectable effect of **paramilitaries**.

Finally, all three of the external-threat variables increase the hazard of experiencing a violent uprising. This is true of both manifest external threat, in the form of **interstate wars** and **MIDs**, where the interpretation might be that would-be insurgents take advantage of a distracted and divided military to launch their rebellion, but also in terms of latent threat, **SRG count**.

Table 4.8: Onset of general VIO campaigns: conscription

	(VIO8)	(VIO9)	(VIO10)	(VIO11)	(VIO12)
conscription	-0.07 (0.22)			-0.08 (0.21)	-0.11 (0.21)
$(1/t)$ * <i>conscription</i>		-5.02 (5.32)	-4.45 (3.36)		
democracy		-0.64 (0.49)			
$(1/t)$ * <i>democracy</i>	0.24 (4.09)		2.04 (4.14)	0.39 (3.92)	0.96 (3.96)
Paramilitaries ^(1/8)		0.13 (0.09)			
VIO in neighbor	0.59* (0.28)	0.78 ⁺ (0.42)	0.73* (0.29)	0.52 ⁺ (0.29)	0.54 ⁺ (0.28)
ln GDP pc	-0.45*** (0.11)	-0.52*** (0.14)	-0.50*** (0.11)	-0.49*** (0.11)	-0.49*** (0.11)
ln pop.	0.12 (0.09)	0.04 (0.10)		0.09 (0.08)	0.10 (0.08)
ln urban pop.			0.09 (0.08)		
Econ. growth rate			-1.32 (1.01)	-2.26* (1.15)	-2.17 ⁺ (1.20)
\sqrt{t} * <i>Econ. growth rate</i>	-0.46* (0.23)	-0.41 (0.25)			
interstate war		1.05* (0.44)	1.03** (0.34)		
$\ln(t)$ * <i>interstate war</i>	0.24 ⁺ (0.13)				
$\ln(t)$ * <i>MID</i>				0.21** (0.08)	
SRG count ^(1/4)					0.32* (0.16)
Observations	6762	4904	6508	6762	6762
Events per variable	15	8.75	14	15	15
Pseudo R^2	0.068	0.117	0.072	0.072	0.066
<i>AIC</i>	674.61	394.53	624.15	671.68	676.02

Cox conditional risk set models, DV = VIO campaign onset

Country-clustered standard errors in parentheses

⁺ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Models VIO13-VIO22 in Table 4.9 show the effect of ethnic **exclusion in the officer corps** on violent campaigns in general. As opposed to revolutionary violent campaigns, where a stacked officer corps made no difference on the timing of uprising onset, when considering all types of violent campaigns, higher rates of ethnic exclusion appear to hasten the onset of uprisings. The positive effect of stacking in the officer corps is significant in all but two of the models in the table, only disappearing when controlling for paramilitaries and interstate war. Together, the findings for this variable in Tables 4.6 and 4.9 suggest that ethnic groups react to exclusion from the military by rebelling to obtain autonomy or secede from the state, rather than to try to wrest control of the central government from the ruling regime. In addition, the positive association between ethnic stacking and rebellion in general fits with prior research (Roessler 2011) that found the same relationship using rougher proxies for stacking.

There are some similarities in the behavior of the control variables in Table 4.9 compared to what we saw in Table 4.8: a lower hazard of rebellion due to higher levels of **ln GDP per capita** and **economic growth rate**, and no effect of **democracy**. There are some differences, though. The diffusion effect of **violent uprising in neighbor** is completely gone, and there is no effect of any of the measures of external threat. **Ln population** is now positive and significant, which differs from what we observed in Table 4.8 but fits with prior research on insurgencies; **urban population** is also positive and significant, although only at the 0.1 level. **Paramilitaries** is positive and significant at the 0.1 level, the first time that this variable has shown any effect on violent uprisings.

Table 4.9: Onset of general VIO campaigns: stacking in officer corps

	(VIO13)	(VIO14)	(VIO15)	(VIO16)	(VIO17)	(VIO18)	(VIO19)	(VIO20)	(VIO21)	(VIO22)
excl: officers	2.61* (1.15)	1.25 (0.89)	2.38* (1.08)	2.98* (1.25)	3.35** (1.04)	3.55** (1.12)	2.47* (1.05)		2.56* (1.11)	2.60* (1.08)
<i>t</i> * <i>excl: officers</i>								0.05 (0.03)		
democracy	0.25 (0.34)									
Paramilitaries ^(1/8)		0.39 ⁺ (0.23)								
VIO in neighbor			-0.95 (1.11)							
ln GDP pc				-0.52* (0.23)						
ln pop.					0.43* (0.22)					
ln urban pop.						0.51 ⁺ (0.26)				
Econ. growth rate							-4.92* (2.06)			
interstate war								0.73 (0.68)		
MID									0.59 (0.44)	
SRG count ^(1/4)										0.15 (0.37)
Observations	932	610	932	898	898	835	881	932	932	932
Events per variable	12.50	7.50	12.50	12.00	12.00	10.00	11.50	12.50	12.50	12.50
Pseudo R^2	0.090	0.067	0.098	0.132	0.136	0.161	0.125	0.034	0.104	0.091
<i>AIC</i>	78.50	40.76	77.78	69.13	68.81	58.37	66.24	83.03	77.36	78.42

Cox conditional risk set models, DV = VIO campaign onset

Country-clustered standard errors in parentheses

⁺ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 4.10 shows the corollary to Table 4.9: **ethnic exclusion in the enlisted portion** and general violent campaign onset. We see from this table that there is no discernable effect of this kind of ethnic exclusion on violent uprisings.

The significance of the control variables is somewhat reduced, compared to the previous table. The controls that remain significant at least at the 90% level are the usual suspects: **ln GDP per capita** (lowers the hazard rate), **ln population** (increases the hazard rate), and **economic growth rate** (lowers the hazard rate).

Table 4.10: Onset of general VIO campaigns: stacking in rank and file

	(VIO23)	(VIO24)	(VIO25)	(VIO26)	(VIO27)	(VIO28)	(VIO29)	(VIO30)	(VIO31)	(VIO32)
excl: enlisted		0.42 (1.11)		0.51 (0.90)	1.04 (0.90)	0.74 (1.14)	0.50 (0.94)			
<i>t</i> * <i>excl: enlisted</i>	-0.01 (0.03)		-0.01 (0.03)					-0.01 (0.03)	-0.01 (0.03)	-0.01 (0.03)
democracy	0.13 (0.38)									
Paramilitaries ^(1/8)		0.37 (0.24)								
VIO in neighbor			-1.51 (1.05)							
ln GDP pc				-0.36 ⁺ (0.21)						
ln pop.					0.31 ⁺ (0.17)					
ln urban pop.						0.37 (0.25)				
Econ. growth rate							-4.00 ⁺ (2.08)			
interstate war								0.70 (0.52)		
MID									0.65 (0.45)	
SRG count ^(1/4)										0.10 (0.32)
Observations	956	633	956	922	922	859	904	956	956	956
Events per variable	12.50	7.50	12.50	12.00	12.00	10.00	11.50	12.50	12.50	12.50
Pseudo R^2	0.002	0.053	0.031	0.034	0.030	0.027	0.056	0.009	0.021	0.003
<i>AIC</i>	86.77	41.74	84.42	77.60	77.84	68.08	71.94	86.18	85.19	86.71

Cox conditional risk set models, DV = VIO campaign onset

Country-clustered standard errors in parentheses

⁺ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

4.6.3 Robustness Check 2: Cross-Sectional Tests

For the next robustness check, I use an approach meant to help deal with the fact that the independent variables are all rather persistent, with little in-country variation. I use OLS models on a cross-sectional version of the dataset. While a crude test, this device should allow us approach the relationship between social distance and mass uprisings from another angle, to supplement what we have already observed from the duration models above.

I use country averages for the independent variables to predict the state-level averages for the dependent variables. Specifically, for the independent variables, I use **prop. years conscript**, or the proportion of its independent, post-1945 autocratic history spent using conscription as its method of military recruitment, and the average degree of stacking in the officer corps, **avg. stacking officers**, and in the rank and file, **avg. stacking enlisted**, over the same period. For the dependent variables in these models, I use the proportion of the country's time over the same period spent with at least one active nonviolent campaign.²¹

Table 4.11 provides the results of the cross-sectional tests estimating the proportion of years spent in a nonviolent revolutionary campaign. The coefficients are all in the expected direction, with a positive relationship for the **proportion of years spent using conscription** and a negative relationship for **average stacking in the officer corps** and **average stacking in the enlisted portion**. However, none of the independent variables is statistically significant at conventional levels, although stacking in the enlisted portion comes close in model NV17 at $p = 0.11$.

Table 4.12 shows the impact of social distance on the proportion of years spent in a violent revolutionary campaign. None of the results in any of the models come anywhere close to statistical significance, and **avg. stacking officers** even flips its sign compared to Table 4.11.

From a purely cross-sectional standpoint, there does not appear to be a systematic relationship between social distance and violent or nonviolent revolutionary uprisings. However, there is slight evidence from this supplementary analysis that higher levels of ethnic stacking at least in the rank and file may make nonviolent uprisings less likely.

²¹Because some of the most successful campaigns are the ones that are shortest in duration, the number of years spent in campaigns of a given type may not be the best way to measure the dependent variable in cross-sectional terms. However, this weakness of this supplementary test cannot be addressed here, since running logit models, such as with the outcome variable being whether the country ever experienced a revolutionary campaign, would not be possible for the ethnic stacking models, because the number of events per variable would be unacceptably low.

Table 4.11: Rev. NV campaigns: cross-sectional models

	(NV15)	(NV16)	(NV17)
prop. years conscript	0.01 (0.01)		
avg. stacking officers		-0.04 (0.04)	
avg. stacking enlisted			-0.06 (0.03) ¹
Constant	0.03** (0.01)	0.03 ⁺ (0.02)	0.03* (0.01)
Observations	122	16	17
R^2	0.004	0.078	0.164
AIC	-323.03	-56.21	-62.42

Standard errors in parentheses

OLS regression. DV = prop. autocratic years with NV campaign

⁺ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$ 1. $p = 0.11$

Table 4.12: Rev. VIO campaigns: cross-sectional models

	(VIO33)	(VIO34)	(VIO35)
prop. years conscript	0.02 (0.04)		
avg. stacking officers		0.06 (0.15)	
avg. stacking enlisted			-0.04 (0.15)
Constant	0.07* (0.03)	0.09 (0.06)	0.11 ⁺ (0.05)
Observations	122	16	17
R^2	0.001	0.010	0.006
AIC	-67.39	-11.48	-12.94

Standard errors in parentheses

OLS regression. DV = prop. autocratic years with VIO campaign

⁺ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

4.6.4 Robustness Check 3: Accounting for Possibility of Endogeneity

As theorized in Chapter 2, state leaders should take into account the possibility that the military may have to be deployed against a mass uprising in choosing recruitment policies for the military. Therefore, in this second supplementary analysis, I address the reality that military design may be endogenous to uprisings by taking an instrumental-variables approach. The task of finding variables that should influence conscription or ethnic stacking but not mass uprisings is a particularly difficult one. As explained previously, the quantitative study of military recruitment policy is still in its infancy, with only a small handful of existing studies of the determinants of conscription, as well as Chapter 3 in this study, to provide possible instruments to choose from.

Of the variables that have been shown to correlate robustly with conscription, only one plausible candidate emerges, British colonial origin. Countries with British (non-settler) colonial origins tended to avoid conscription use historically, based on the anti-conscription precedent set during the English Civil War (Asal, Conrad, and Toronto [n.d.](#)). The sample correlation between British colony and conscription use is not as high as would be desired ($r = -0.23$), but still high enough to justify using this variable as an instrument. In addition, this variable has no theoretical relationship with revolutionary uprisings, and the sample correlation is basically zero between British colony and nonviolent revolutionary onset ($r = -0.02$) and between British colony and violent revolutionary onset ($r = 0.01$), so the exclusion criterion is arguably met in this case.

On the other hand, in the case of ethnic stacking, no plausible candidate instruments present themselves. The present study is the first to employ the state-level measure of ethnic stacking being used here, and of the few predictors that were shown in chapter 3 to have any robust relationship with ethnic stacking, all of them have a theoretical relationship to uprisings. Short of a massive data-mining fishing expedition to find variables correlated with this measure of ethnic stacking – which would pose the threat of harm to future studies that may use this new dataset on military ethnicity – it will not be possible to come up with plausible instruments for stacking at present. Therefore, as was stated above in the context of EPV problems for models using ethnic stacking, the results from this chapter for the relationship between ethnic stacking and uprising onset should be treated as rather preliminary – here, because I am not able to overcome the endogeneity problem understood to possibly be present. However, the fact that there was no detectable relationship in

Chapter 3 between revolutionary uprising and subsequent use of ethnic stacking should alleviate this concern somewhat.

Table 4.13 shows the results from the instrumental-variable models for conscription and revolutionary uprisings of both types. The specific estimator that I use here is the bivariate probit threshold model, which is the most appropriate type of instrumental-variables model available given that my dependent and endogenous independent variables are both binary (Nichols 2011). Besides British colonial origin, I glean from Tables 4.5 and 4.6, respectively, the covariates used in these models of nonviolent and violent uprisings, choosing variables that were shown there to have a significant relationship with the type of uprising in question. Owing to the limited data availability for the paramilitaries variable, I show the results both with and without this variable, in models NV18 and NV19, respectively.²²

The results in Table 4.13 show that after instrumenting for conscription using British colonial origin, the effect of **conscription** on nonviolent mass uprisings (models NV18 and NV19) and violent mass uprisings (model VIO36) is not statistically significant, as before. The effects observed from the control variables in the second stage are also essentially the same as what we observed in Tables 4.5 and 4.6.

4.7 Conclusion

The statistical analyses in this chapter have revealed some limited evidence that social distance between the military and society influences whether and when revolutionary mass uprisings take place, although this effect is limited to certain forms of social distance.

Conscription had no effect on any form of uprising onset, whether revolutionary or not, in any of the analyses. The results from the analyses above are very consistent in this null finding, which contradicts our expectations based on the theory set forth in Chapter 2, that potential revolutionaries would be more willing to take the risk of challenging the regime when they anticipated shirking by conscripts in the military. Instead, protesters and insurgents appear to not take into account this form of social distance at all when deciding whether to initiate a campaign.

On the other hand, ethnic stacking in the officer corps and possibly in the rank and file as

²²The first-stage pseudo R^2 statistic displayed at the bottom of the table was calculated separately by running a normal probit model using that set of covariates as predictors of conscription.

Table 4.13: Onset of rev. NV, VIO campaigns: instrumenting for conscription

	(NV18)	(NV19)	(VIO36)
DV = rev. uprising			
conscription	0.78 (1.07)	0.25 (0.46)	0.11 (0.40)
ln urban pop.	0.09 (0.13)	0.09 ⁺ (0.05)	
Econ. growth rate	-0.90 ⁺ (0.50)	-0.97* (0.48)	
Paramilitaries ^(1/8)		0.10* (0.04)	
VIO in neighbor			0.36 ⁺ (0.20)
ln GDP pc			-0.23** (0.07)
ln pop.			-0.13* (0.06)
interstate war			0.34 (0.22)
MID			0.33* (0.16)
Constant	-3.88* (1.79)	-3.80*** (0.68)	0.91 (1.15)
DV = conscription			
british colony	-0.74* (0.29)	-0.69* (0.28)	-0.63* (0.28)
ln urban pop.	0.25** (0.09)	0.18 ⁺ (0.09)	
Econ. growth rate	0.06 (0.04)	-0.08 (0.25)	
Paramilitaries ^(1/8)		0.11** (0.04)	
VIO in neighbor			-0.49*** (0.15)
ln GDP pc			0.16 (0.12)
ln pop.			0.20* (0.09)
interstate war			0.44* (0.21)
MID			0.22 ⁺ (0.12)
Constant	-3.28* (1.29)	-2.59 ⁺ (1.35)	-3.87* (1.87)
First-stage pseudo R^2	0.108	0.120	0.107
Observations	3288	2355	3561
AIC	4522.74	3283.50	4658.04

Bivariate probit threshold model

Country-clustered standard errors in parentheses

⁺ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

well does appear to be negatively related to the onset of nonviolent revolutionary uprising, as expected. However, the effect for stacking in the officer corps was not present when analyzing this variable's effect on nonviolent uprisings in general (including other types of campaigns like secessionist movements) in all regime types, nor when examining cross-sectional versions of the dependent variable. On the other hand, the hint of a negative effect of stacking in the rank and file persisted through both of these robustness checks.

There was also a difference observed between types of violent uprising, as stacking in the officer corps appears to be positively related to nonrevolutionary violent uprisings but has no relationship with revolutionary violent uprisings. Stacking in the enlisted portion, though, had no relationship with either type of violent uprising.

Although interesting and certainly meriting further exploration, the results for ethnic stacking should be seen as rather preliminary, for three reasons: 1. the MENA sample for which these data are available contained a very low number of uprisings, which created problems with the EPV rate and also made it difficult to employ control variables to rule out the possibility that the estimated effect is the result of excluded third variables; 2. no candidate instrumental variables for stacking are available at this time, preventing ruling out the possibility that the observed effect is due to reverse causation; 3. the limited geographical domain means that even if #1 and #2 were not a problem, it is possible that the observed relationship may not apply in other regions.

Besides showing the particular ways in which social distance might matter, this chapter also added to what we know about violent and nonviolent uprisings by showing that the effects of standard explanations differ when analyzing revolutionary uprisings versus uprisings in general. While some variables maintained their effect across types of nonviolent uprisings, like population (+) and urban population (+), or across types of violent uprising, like violent uprisings in neighboring states (+) and GDP per capita (-), other variables applied only to type or another. For nonviolent uprisings, economic growth rate (-) and manpower employed in paramilitaries (+) only made a difference for revolutionary onset, while nonviolent uprisings in neighboring states (+), GDP per capita (+), and latent external threat in the form of a larger SRG (-) only appear to be related to non-revolutionary onsets. For violent uprisings, population (-), urban population (-), and external threat in the form of interstate wars and MIDs (both +) only affected revolutionary uprisings, while SRG size (+), population (+), and urban population (+) only appear to affect non-revolutionary

uprisings.

The differential impact of many of these variables shows the need for studies of both insurgency and nonviolent uprising to separate out types of uprising based on the opposition movement's goals, as the relationships for many of these standard predictors appears to be heterogeneous across types of movements. In other words, all insurgencies are not interchangeable, nor are all nonviolent movements, and policymakers interested in hindering or in helping these movements succeed need to understand that many of the factors widely thought to make a difference may only apply to a certain subset of that kind of movement.

Chapter 5

Testing the Response Stage of the Theory

5.1 Introduction

Chapter 5 tests the final stage of the theory, examining the response of the autocratic regime and of the military to nonviolent revolutionary uprisings. This stage, therefore, begins at the point where the onset of a nonviolent uprising has already occurred.

On such occasions, the state leaders are faced with the choice of whether to attempt to crush the uprising using the military or not. The threat that mass uprisings pose to the regime and to the leader's ability to remain in office is reflected in two aspects that comprise their revolutionary nature: their significant size and their serious demands. The criteria for the sample of uprisings generated for the previous chapter were constructed to reflect this threat, as described therein.

It is important at this point to outline exactly how and why the outbreak of mass uprisings threatens the leader's tenure, which takes place in one of three main ways. The first and most apparent threat is the prospect of angry masses directly ejecting the state leader from office. This is obvious in the case of insurgencies, but it is also frequently true of mass demonstrations, even ones that initially began as primarily nonviolent. Gualberto Villarroel, president of Bolivia from 1943-1946, experienced this threat firsthand when, in response to his regime's bloody repression of its opponents, a nonviolent mass demonstration in the capital formed in July 1946. The protests soon

escalated to riots, and the rioters proceeded to lay siege to the presidential palace. After smashing their way inside, they murdered Villarroel and strung up his body from a lamppost (Loveman 1999, 110). Surely every state leader who has ever witnessed a mass demonstration calling for his or her removal has at least briefly imagined personally encountering the same fate.¹

The second way that mass uprisings can threaten the leader's tenure is by inciting the military to attempt a coup (Albrecht and Koehler n.d.; Hiroi and Omori 2013), whether out of dissatisfaction with the regime's handling of an ongoing insurgency or because of taking mass protests as a sign that the regime has lost its popular mandate – and often, more cynically, as a sign that they can overthrow the government without retribution from the public. This pattern describes the events in Egypt that led to the downfall of two of that country's leaders in the last few years. In response to the mass uprising of 2011, where millions of civilians demonstrated in the streets of Cairo against the longtime dictator Hosni Mubarak, the same senior officers who formed his support base responded to the nonviolent protests by quietly forcing him from office by privately informing him that he no longer had their support. After democratic elections, the first in Egypt's history, were held to choose a new leader, Mohammad Morsi rode into office on the support of the Muslim Brotherhood, the only civil-society group in the country that was well-enough organized to win an election. The Egyptian military viewed the Brotherhood as its traditional enemy, having been tasked for decades with repressing the Islamist movement, and the officers were spoiling for a fight ever since he took power. When a second mass uprising that nearly rivaled the 2011 revolutionary demonstrations in size formed in 2013 to protest Morsi and his policies, the military gladly took the initiative again to resolve the conflict by removing the leader from office.

The third way that mass uprisings can threaten the state leader, in an explanation favored for obvious reasons by advocates of nonviolence, is that mass uprisings can simply make the state ungovernable. Strikes and protests can shut down the center of government, including transportation, business, and the bureaucracy that keeps the state running. Even stereotypically self-serving dictators have political agendas, and if the protesters can credibly threaten to keep going until the leader resigns from office, the leader may opt to resign or to initiate a pacted transition from office in

¹Faced with angry masses, some state leaders are lucky enough to get away first, such as Akayev of Kyrgyzstan in 2005 and Yanukovic of Ukraine in 2013, both of whom escaped to a safe haven in their patron state of Russia in time to hear or watch reports of previously nonviolent protesters taking over the presidential mansion. For Kyrgyzstan, see Shishkin 2013, ch. 1. For Ukraine, see Steven Erlanger, "At Abandoned Ukrainian Palace, an Anxious Look Toward the Future," *New York Times*, 27 February, 2014.

order to preserve that agenda, at least in part.

If the regime does decide that an uprising is sufficiently threatening to justify sending the military to crush it before it gets out of hand, and if senior officers pass the orders along, the next decision node in the theoretical process belongs to the street-level bureaucrats in the military. The soldiers and their unit-level officers leading them are charged with using violence to put down the protests, and they now decide whether to follow those orders or not. I re-emphasize here that the focus of the present study is on the behavior of these soldiers and officers, not the senior officers, the military as an institution, or the success or failure of the campaign. The dependent variable of interest in this final stage of the theory, following deployment, is simply whether a significant number of soldiers and junior officers sent to confront the protesters refuse orders to violently repress the protests. This is the most appropriate way to conceptualize the dependent variable, given the theory of social distance that I have outlined in earlier chapters, as well as the experimental foundation outlined in Chapter 2.

This is a major difference between the present study and previous studies of military responses to mass uprisings: those studies tend to focus overwhelmingly on the senior officers and count as shirking phenomena rather different from what I have just outlined, such as responding to the uprising by attempting a coup (Albrecht and Koehler [n.d.](#)) or by ordering the soldiers under their command to remain barracked (Pion-Berlin, Esparza, and Grisham [2014](#)). These phenomena are conceptually distinct from virtuous shirking as I have conceptualized it. Indeed, they may even result from shirking by their underlings or take place in anticipation of it.

5.2 Sample

For this chapter, I only examine campaigns that start as nonviolent mass demonstrations. I exclude violent insurgencies, although following NAVCO I still count the campaign as nonviolent even if mass protests took place alongside rioting. Limiting the sample by omitting violent, organized insurgencies does not present a problem with selection bias because of the limited nature of the inferences I seek to make. If this study *as a whole* were to only examine nonviolent campaigns, we would miss some of the potential effect that social distance might have, due to its influence on preventing campaigns from emerging under conditions where participants anticipate that the

military cannot be persuaded to shirk. However, I have already addressed the possibility of selection bias for the present chapter by analyzing in Chapter 4 the effect of social distance on the outbreak of campaigns and their use of violent or nonviolent tactics. The subset of possible campaigns that I examine in the present chapter is therefore the set where potential revolutionaries made the calculated decision to launch a movement to overthrow the regime through mass demonstrations, hoping that they might be able to get away with doing so and not be crushed by the state’s coercive forces.

In other words, the sample that I use in this chapter is the set of campaigns where shirking is most likely to take place – otherwise, the participants would not have made the attempt in the way that they did. This is not a problem for inference, though, because my primary research question in this stage is whether social distance between the military and society matters *in the context of nonviolent revolutionary uprisings*. Examining the effect of social distance in the context of fighting violent insurgencies may be a useful topic of study, but one that can appropriately be addressed in future research, as with various other extensions.²

Therefore, the sample used in this chapter includes the set of campaigns with nonviolent onsets that was analyzed in Chapter 4. My total sample size is 53 nonviolent mass uprisings.

5.3 Research Design, Dependent Variables, and Methods

For the dependent variables of deployment and shirking, to my knowledge, no previous study has ever directly measured military deployment against mass uprisings on a cross-national basis. NAVCO does provide a measure for defection of security forces for its set of campaigns, but the coding rules for this measure are broad enough that it includes behavior that is conceptually very distinct from the phenomenon that I seek to analyze, such as coups by the senior officers and responses by non-military security forces. Hence, since no data are available measuring deployment and defection as I have conceptualized them, I use original data collected from secondary sources. The full data for these two variables and the sources used to code them are available [here](#). The precise definitions and coding rules for each are explained below.

For the analyses in this chapter, the unit of analysis is the campaign. The dependent variables

²For example, one interesting possible extension is whether social distance can induce even foreign military forces to shirk orders to violently quell mass protests in an occupied territory.

are coded at the campaign level: was the military deployed at some point during that campaign with orders to use lethal violence?; if so, was there a significant degree of shirking among the deployed soldiers? The key independent variables change slowly enough over time that there was no variation in conscription use or ethnic stacking during the course of any of the nonviolent uprisings. Regarding the control variables, which are measured at the campaign-year level, for interval-level variables I take the variable's average over the course of the campaign, and for binary or ordinal variables, I record the maximum of each over the course of the campaign, as described below. Nonviolent campaigns tend to be brief enough that this approach to coding the control variables is a safe course: 44 of the 53 campaigns in the sample last three years or less, and the longest-lasting campaigns are only seven years.

Owing to the small sample size, I use exact logistic regression, which relies on conditional maximum likelihood instead of regular maximum likelihood estimation, since the former is better able to handle small-sample problems such as sparse data and separation. I also sharply limit the number of covariates in each model.

5.3.1 Deployment

I define deployment as whether *soldiers* were *sent* to the scene of *mass demonstrations* with *orders to use lethal violence* to put down the demonstrations. I code this binary variable as 1 if all these conditions are met and 0 if any of these conditions are not met. Further explanation follows:

- Soldiers: Riot police or paramilitary members do not count. The source used must be explicit, using terms like “army,” “soldiers,” or “military.” Terms like “security forces” or “troops” do not count, as these may signify ISFs instead.
- Sent: Orders to go to the scene of the demonstration must reach the soldiers. Decisions by senior officers to remain barracked or to attempt a coup instead of passing on the orders count as non-deployment.
- Mass demonstrations: Deployment only counts if in response to mass demonstrations. E.g., voter intimidation during elections (Mexico 1988) does not count, even if taking place in the context of a nonviolent revolutionary campaign.

- Orders to use lethal violence: crowd-control duty (such as restraining civilian excesses during rioting or showing up just to look intimidating) does not count. Orders can come from the state leader or from the senior officers. Note: if soldiers arrived and started shooting civilians en masse, I count this as sufficient to establish deployment, unless the source is explicit that the shooting was in contravention of orders.

Of the 53 uprisings in my sample, only 25 of them witnessed the regime’s deployment of the military with orders to crush the campaign violently. In other words, over half of them did not witness military deployment.³ For those that did witness deployment, I code the shirking variable.

5.3.2 Shirking

I define shirking in binary terms as whether a *significant proportion* of deployed soldiers *refrained from using lethal violence* against the protesters *in contradiction of orders*. I code this variable as 1 if all these conditions are met and 0 otherwise. Further explanation follows:

- Significant proportion: Isolated actions by individual soldiers do not count. On the other hand, reports that widespread shirking did occur are sufficient to establish a coding for shirking even if segments of the military did use lethal violence (e.g., China 1989, Iran 1978, and Romania 1989) – which may simply reflect aspects of military composition not accounted for in this study. Refraining from using lethal violence is coded as shirking whether taking place on the initiative of the soldiers or of their unit-level officers, since both categories count as street-level bureaucrats.
- Refrained from using lethal violence: This covers a range of potential actions, from defection (declaring their support for the protesters), to protection (shielding protesters from attacks by ISFs), to passivity (standing by while ISFs do the dirty work). If the source provides evidence that soldiers attempted to put down the protests using lethal violence and does not provide any evidence of shirking, I code shirking as 0.

³While I did not systematically collect information on the reason for non-deployment for every one of the cases falling into this category, the sources I used sometimes provided clues on this score. Some common reasons included that the uprising did not exceed the capacity of the internal security forces to contain (e.g., Argentina 1981-1983, Madagascar 1981, and China 1979); the mass demonstration ended due to factors unrelated to coercion, such as the leader stepping down or making concessions, or the protesters giving up (e.g., South Korea 1986-1987, Poland 1987-1989, and Guyana 1990-1991); the senior officers attempted to remove the leader through a coup (Russia 1991 and Zambia 1990-1991); or the senior officers publicly or privately refused to defend the regime (e.g., Serbia 1996-2000, Bangladesh 1987-1990, and Indonesia 1997-1998).

- In contradiction of orders: The source must at least convey the impression that the soldiers' actions violated orders from political or military commanders.

It is essential to again distinguish this measure from related but still-distinct measures used by previous studies. First, my measure for shirking addresses the behavior of the street-level bureaucrats in the military, not the military as a whole, the way that nearly every previous study of military responses to mass uprisings has measured the dependent variable. Second, this measure focuses on refusal to follow orders to inflict violence on civilians, not on whether (or how much) violence was inflicted (Koren 2014; Saha 2014).

Shirking, measured in this manner, took place in 32% of the campaigns in which the military was deployed (8 of 25 cases). In other words, for autocrats who made the gamble of sending the military to crush a mass uprising by force, this risk usually paid off as the soldiers followed orders and remained loyal. On the other hand, the fact that shirking still took place 1/3 of the time serves as a reminder of the risks associated with this strategy.

5.4 Key Independent Variables

The construction and data sources for **conscription** and **ethnic stacking** were explained in Chapter 3. For the quantitative analysis in this chapter, I am only able to draw on the data for conscription, because as noted in Chapter 4, there are simply not enough instances of nonviolent revolutionary uprisings in the Middle East in my sample – four uprisings total⁴ – to be able to examine the effect of ethnic stacking statistically. This may very well be due to ethnic stacking proving to be a very successful deterrent against nonviolent uprisings, but as discussed in that chapter, that is not possible to show with any degree of conclusiveness until future work can take advantage of data on ethnic stacking extending beyond the MENA region.

⁴In these four uprisings, two of them witnessed deployment; in one of these (Iran) significant numbers of soldiers shirked, while in the other (Algeria), there was no shirking. In the two cases that did not witness military deployment, Egypt's Kifaya movement was small enough for the ISFs to contain, and Sudan's anti-Nimeiri protests led to senior officers launching a coup instead of passing on orders to deploy.

5.5 Control Variables

I control for a variety of potentially confounding variables in analyzing the determinants of military deployment and shirking.

5.5.1 New Variables

Campaign size: I use NAVCO's (cited in Chapter 4) ordinal variable ranging from 0 (less than 10,000 participants) to 5 (more than 1 million). Smaller campaigns should be more easily contained by ISFs, while larger revolutionary campaigns should be more threatening and require additional manpower to put down, making deployment more likely for larger campaigns. Shirking, on the other hand, should correlate positively with this variable, with the large protest size conveying the impression that the people collectively are rising up against the regime, as opposed to a collection of antisocials and thugs.⁵

Campaign diversity: I take the sum of four NAVCO dummy variables measuring whether the campaign is diverse in terms of age, class, ethnicity, and religion, respectively. This new variable ranges from 0 to 4 by construction. Campaigns that are more diverse by definition draw more broadly from society, meaning that social distance should be lowest for such uprisings, making shirking more likely and the regime more hesitant to deploy the military in anticipation of shirking.

Radical flank: I use NAVCO's measure for whether the nonviolent campaign has a faction in it that uses extremist rhetoric and advocates violent strategies to pursue their goals. Movements exhibiting a radical flank should be easier to repress, as the regime can paint the movement as a whole as gangsters, thugs, terrorists, etc., making deployment more likely and shirking less likely. Since I am only examining nonviolent movements in this chapter, I recode this variable as 0 representing no radical flank and 1 representing the presence of a radical flank.

Personalist regime: I include two dummy variables for type of autocratic regime, including this

⁵There is a countervailing relationship we might expect to see for deployment: the larger the campaign, the less likely deployment, due to fears of shirking or the fear that soldiers would be hopelessly outnumbered.

one, using data from Geddes, Wright, and Frantz (2014, v. 1.2, updated 23 June 2012).⁶ Personalist dictators have the most to lose, as their personal fate is tied intimately to the fate of the regime, so deployment should be more likely. On the other hand, these regimes tend to be brittle during revolutions, making shirking more likely.

Military regime: This measure uses the same data source as the measure for personalist regimes. Military regimes tend to rely on coercive repression out of familiarity, so deployment should be more likely (Davenport 1995). Soldiers should be less likely to shirk here, due to institutional interests.

5.5.2 Variables Previously Introduced

The data source and construction for each of these were given in previous chapters:

Minority regimes: Elites in these regimes have a greater expectation of being purged, likely with violence, if the uprising succeeds, giving them a greater willingness to fight to hold on to power at all costs (McLauchlin 2014), making deployment more likely. In the analyses with shirking as the dependent variable, minority regime is a very loose proxy for ethnic stacking, used to help compensate for the lack of data on stacking outside of the MENA region, under the assumption that minority regimes are more likely to recruit soldiers and junior officers from the ethnic group in power, so shirking should be less likely here.

Civil conflict: The same logic as for radical flank applies to campaigns taking place during episodes of civil conflict – the regime can claim that the protesters are part of the same group of insurgents, or at least on the side of the insurgents.

Paramilitaries: A larger number of paramilitary forces, or ISFs, should make deployment less likely, since these forces should be better able to contain the uprising. Greater paramilitary forces should also make shirking more likely, due to bitterness among the soldiers over having to compete with these ISFs for funding and manpower (e.g., Tófalvi 2012, 18-19).

⁶The excluded category for type of authoritarian regime is a pool of all other types of authoritarian regimes, such as monarchies and one-party regimes.

Economic growth rate: The objective grievances of the populace can be expected to be higher as the state of the economy declines, making relatively medium-sized protests more likely to turn rapidly into very large ones. Elites should perceive it as necessary to crack down rapidly if the growth rate is low or negative, making deployment more likely. In addition, even though officers are usually well-paid, soldiers are often affected by economic trends as much as the average citizen, so lower economic growth should make shirking more likely.

Military spending: I control for military spending per soldier as a loose proxy for the regime's attempts to buy the loyalty of officers and soldiers through its spending on the military as an institution, including pay, benefits, and access to weaponry. Higher spending per soldier should result in a lower likelihood of shirking, as well as greater confidence in the military's reliability and capability, hence greater likelihood of deployment. I use COW NMC data (cited previously) for total military spending and for total military personnel. I divide spending by personnel, correct for heavy right-skewness by taking the natural log,⁷ and interpolate to cover missing years.

Rivalry: Involvement in a strategic rivalry should make deployment more likely and shirking less likely, as the regime can portray the dissidents as foreign or foreign-sponsored agents.

SRG count: This variable follows the same logic as rivalry.

When taking the average of campaign-year-level variables to generate campaign-level measures, all control variables are lagged one year before taking the campaign average (for interval-level variables) or maximum (for binary variables). For interval-level variables, after taking the campaign average, I round to the nearest 0.1 in order to reduce the computational expense in running the exact logit regressions.

⁷All countries with nonviolent revolutions in my sample had non-zero personnel and spending totals, so neither dividing by 0 nor taking the log of 0 is a problem here.

5.6 Results

Table 5.1 shows summary statistics for the variables used in these analyses. As noted above, there are only four nonviolent campaigns for which I have data available for ethnic stacking, so I use neither the officer-level or enlisted-level versions of this variable in the analyses below.

Before moving to the statistical analysis, I first examine the bivariate relationship between conscription use and each dependent variable.

Deployment took place in 56% of the uprisings in my sample (10 of 18 cases) with volunteer militaries and 43% of the time (15 of 35 cases) with conscript militaries. The direction of the relationship fits my theoretical expectation: conscript militaries were less likely to be deployed against nonviolent revolutions in my sample. However, this association is not statistically significant ($\chi^2 = 0.77, p = 0.38$).

Shirking took place 20% of the time (2 of 10 cases) in volunteer militaries and 33% of the time (5 of 15 cases) in conscript militaries. Again, the higher rate of shirking for conscript militaries fits my theoretical expectation, but the effect is not statistically significant here, either ($\chi^2 = 0.53, p = 0.47$).

The lack of significance for conscription's relationship with deployment and shirking may be due to the small sample size I am working with in this chapter – with only 53 and 25 cases, respectively, the difference in percentages between the two groups would have to be greater in order to have confidence that the effect was happening in the larger (hypothetical) population of nonviolent revolutionary cases. However, this is an open question to be resolved by future work.

5.6.1 Deployment

In all the regression results displayed below, owing to the small sample size, I choose to display p -values in parentheses next to each coefficient, rather than standard errors in parentheses as hitherto done in this study, and omit reporting stars. I do this to avoid using conventional but arbitrary cutoffs for judging the statistical significance of each effect. Even if we cannot establish a very high level of confidence for the relationships found, there may be something worth further exploration if we see a p -value above the 0.1 level but that is still relatively low.

Table 5.2 shows the first set of multiple-regression results for conscription use and deployment, with control variables included for various campaign-level and regime-level characteristics. The

Table 5.1: Summary statistics

	count	mean	sd	min	max	Expected Sign for DV	
						deployment	shirking
DVs							
deployment	53	.4716981	.5039755	0	1		
shirking	25	.28	.4582576	0	1		
IV							
conscription	53	.6603774	.4781131	0	1	-	+
Controls							
camp. size	50	3.22	1.282377	1	5	+	+
camp. diversity	52	3.365385	.7147973	2	4	-	+
radical flank	53	.5849057	.4974536	0	1	+	-
personalist reg.	53	.2075472	.4094316	0	1	+	+
military reg.	53	.2830189	.4547763	0	1	+	-
minority reg.	53	.1320755	.3418128	0	1	+	-
civ. conflict	53	.2075472	.4094316	0	1	+	-
paramilitaries ^(1/8)	47	3.625532	1.773779	0	10	-	+
econ. growth rate	53	.0226415	.0775627	-.2	.2	-	-
ln mil. spending	53	8.616981	.9101428	6.8	10.9	+	-
rivalry	53	.4528302	.5025335	0	1	+	-
SRG count ^(1/4)	53	1.211321	.7026489	0	3	+	-

coefficient for **conscription** is consistently negative, as expected. However, the p -value for this effect in each model is consistently high, never dropping below 0.5. This does not provide us with enough evidence to be even a little confident in asserting that low social distance in the form of military conscription might deter deployment.

Of the other variables in Table 5.2, **military regime** in model D5 has the lowest p -value, at $p = 0.14$, with a positive coefficient as expected. **Radical flank** in model D3 comes in at a very distant second at $p = 0.43$, the effect being positive as predicted. The model scores for these two models are also higher than the other models, and notably so in the case of model D5, with military regime, suggesting a better fit for the data than the other models. I therefore return to these two variables below for further consideration.

Table 5.2: Deployment: Conscription, Campaign and Regime Characteristics

	(D1)	(D2)	(D3)	(D4)	(D5)	(D6)
conscription	-0.38 (0.73)	-0.44 (0.62)	-0.26 (0.90)	-0.50 (0.55)	-0.53 (0.54)	-0.43 (0.67)
camp. size	-0.08 (0.80)					
camp. diversity		-0.03 (1.00)				
radical flank			0.63 (0.43)			
personalist reg.				-0.16 (1.00)		
military reg.					1.09 (0.14)	
minority reg.						0.25 (1.00)
Observations	50	52	53	53	53	53
Model score	0.63 (0.72)	0.61 (0.78)	1.91 (0.44)	0.81 (0.66)	3.91 (0.15)	0.85 (0.69)

Exact logistic regression. DV = Military deployed.

Coefficients are median unbiased estimates. **P-values in parentheses.**

Table 5.3 displays the results from the next set of models using deployment as the dependent variable. The sign on **conscription** is still consistently negative. The lowest p -value for conscription is in model D8, at $p = 0.19$, when controlling for paramilitaries – which also has the strongest correlation with conscription of any of the control variables, with a correlation coefficient of 0.38. However, since this possibility of an effect for conscription only shows up in one model out of 12, we still cannot be confident that there is any relationship here between conscription and deployment.

Of the control variables in Table 5.3, one is highly significant: **military spending**, at $p = 0.005$. Contrary to predictions for this variable, though, the sign is negative, implying that higher rates of

spending per soldier make the regime *less* likely to deploy the military. This curious effect does not have any theoretical reasons behind it but is certainly worth exploring further. **Paramilitaries** comes in a distant second in terms of statistical significance, at $p = 0.38$; the model score for that model, D8, is also notably higher than the other models (excluding D10, which contains the military-spending variable). Consequently, I explore the effects of military spending and paramilitaries further below.

Table 5.3: Deployment: Conscription, Other Factors

	(D7)	(D8)	(D9)	(D10)	(D11)	(D12)
conscription	-0.44 (0.64)	-1.07 (0.19)	-0.57 (0.49)	-0.25 (0.92)	-0.49 (0.56)	-0.48 (0.58)
civ. conflict	0.24 (0.98)					
Paramilitaries ^(1/8)		0.16 (0.38)				
Econ. growth rate			2.41 (0.63)			
ln mil. spending				-0.97 (0.00)		
rivalry					0.21 (0.91)	
SRG count ^(1/4)						-0.09 (0.83)
Observations	53	47	53	53	53	53
Model score	0.89 (0.61)	2.76 (0.25)	1.21 (0.54)	8.42 (0.01)	0.90 (0.65)	0.81 (0.68)

Exact logistic regression. DV = Military deployed.

Coefficients are median unbiased estimates. **P-values in parentheses.**

In Table 5.4, I omit conscription completely and engage in a model-building exercise using the highest-performing variables from the previous models of deployment. Each of the models in Table 5.4 contains **military spending**, which remains consistently negative and highly significant. Adding **military regime** in model D14 does not hurt the model fit, and military regime has a decently low p -value at $p = 0.18$. Adding **paramilitaries** in model D16 worsens the model fit, so I do not consider this variable further. Although the significance level of **radical flank** is rather low when adding it in model D15, at $p = 0.44$, this variable does not worsen the model fit. I create a final, combined model in D17.⁸ In this final model, radical flank is nowhere close to significant, at $p = 0.73$, but the model score is nearly equal to that of model D14, suggesting that even if it is not helping the model fit, it is not hurting it, either. Hence, based on these results from Table 5.4, we can say that we are confident that military spending lessens the chances of deployment; that there

⁸With three covariates, the ratio of events per variable is 8.3, low enough to warrant caution but still high enough that we can be reasonably confident that the effects are not biased (Vittinghoff and McCulloch 2007).

is suggestive evidence that military regime makes deployment more likely; that there is the slightest hint that radical flank makes deployment more likely, although we cannot say this with any degree of confidence; and no evidence at all of any effect for paramilitaries.

Table 5.4: Deployment: Combined Models

	(D13)	(D14)	(D15)	(D16)	(D17)
ln mil. spending	-1.01 (0.00)	-1.01 (0.00)	-0.98 (0.00)	-0.93 (0.01)	-0.98 (0.01)
military reg.		1.10 (0.18)			0.96 (0.27)
radical flank			0.64 (0.44)		0.42 (0.73)
Paramilitaries ^(1/8)				0.04 (0.81)	
Observations	53	53	53	47	53
Model score	8.29 (0.00)	10.62 (0.00)	9.18 (0.01)	6.54 (0.03)	10.93 (0.01)

Exact logistic regression. DV = Military deployed.

Coefficients are median unbiased estimates. **P-values in parentheses.**

5.6.2 Shirking

I next examine the effect of conscription and the other covariates on virtuous shirking. Since there are only 8 total instances of shirking in the entire sample, I include only one covariate per model in all of these models in order to keep the ratio of events per variable high enough to be fairly confident of avoiding rare-event bias, although this rate is still low enough to warrant caution (Vittinghoff and McCulloch 2007). This limitation makes it so that it is not possible to see whether the sign and significance on conscription change from model to model.

We see in model S1 of Table 5.5 that although its sign is positive as expected, conscription is not anywhere close to significant, at $p = 0.80$. This is as far as we can go with the sample in analyzing the relationship between conscription and shirking, so for the time being, we cannot say that there is any detectable relationship between conscription use and virtuous shirking across cases of nonviolent revolution in this time period.

Of the remaining covariates in Table 5.5, **radical flank**, **military regime**, and **campaign size** have the highest levels of significance – although nothing spectacular in any – at $p = 0.23, 0.24$, and 0.36 , respectively. The model scores are decently high for S4 (with radical flank) and S6 (with military regime), and less so but still higher than the others for model S2 (with campaign size).

To follow up, since each of these variables is either ordinal (campaign size) or categorical (radical flank, military regime) I supplement the regression models here with tabular analysis. The resulting statistics are as follows:

- Campaign size: $\chi^2_{(4)} = 4.32$, $p = 0.37$.
- Radical flank: $\chi^2_{(1)} = 2.82$, $p = 0.09$. Shirking took place 18% of the time (3 of 17 cases) when soldiers were deployed against a campaign with a radical flank, as opposed to 50% of the time without a radical flank (4 of 8 cases).
- Military regime: $\chi^2_{(1)} = 2.68$, $p = 0.10$. Shirking took place 10% of the time (1 of 10 cases) when soldiers were deployed by a military regime, as opposed to 40% of the time for non-military regimes (6 of 15 cases).

Based on these results from Table 5.5 and the χ^2 tests, we can say with a fair degree of confidence that soldiers are less likely to shirk when deployed by a military regime and when deployed against a campaign with a radical flank. There is a slight hint that larger campaigns are better able to induce shirking.

Table 5.5: Shirking: Conscription, Campaign and Regime Characteristics

	(S1)	(S2)	(S3)	(S4)	(S5)	(S6)	(S7)
conscription	0.65 (0.80)						
camp. size		0.40 (0.36)					
camp. diversity			0.18 (1.00)				
radical flank				-1.46 (0.23)			
personalist reg.					0.67 (0.87)		
military reg.						-1.70 (0.24)	
minority reg.							-0.16 (1.00)
Observations	25	23	25	25	25	25	25
Model score	0.51 (0.66)	1.24 (0.29)	0.09 (1.00)	2.71 (0.16)	0.43 (0.60)	2.57 (0.18)	0.02 (1.00)

Exact logistic regression. DV = Virtuous shirking.

Coefficients are median unbiased estimates. **P-values in parentheses.**

Table 5.6 shows the effect of the remaining covariates on virtuous shirking. **Paramilitaries** comes very close to conventional significance levels, at $p = 0.11$, with a decently high model score as well, and the sign on this coefficient is positive, as expected. The next-lowest p -value for any variable in these models, although not very close to conventional levels at $p = 0.34$, belongs to

economic growth rate, which is negative, as expected. No other variables come anywhere close to significance. Since both paramilitaries and economic growth rate are interval-level variables, and the dependent variable is binary, no further tests are possible.

Therefore, at this point, we can say fairly confidently that soldiers having to compete with paramilitary forces for funding and manpower from the regime makes shirking more likely. There is a slight hint that soldiers are less likely to defect when the economy is doing better.

Table 5.6: Shirking: Other Factors

	(S8)	(S9)	(S10)	(S11)	(S12)	(S13)
civ. conflict	-0.78 (0.89)					
Paramilitaries ^(1/8)		0.59 (0.11)				
Econ. growth rate			-8.37 (0.34)			
ln mil. spending				-0.01 (1.00)		
rivalry					0.48 (0.90)	
SRG count ^(1/4)						-0.00 (1.00)
Observations	25	20	25	25	25	25
Model score	0.48 (0.64)	2.90 (0.09)	1.66 (0.33)	0.00 (0.98)	0.31 (0.67)	0.00 (1.00)

Exact logistic regression. DV = Virtuous shirking.

Coefficients are median unbiased estimates. P-values in parentheses.

5.7 Conclusions

Based on the statistical results from this chapter, we cannot say with any degree of confidence that social distance, in the form of conscription, has any relationship with deployment and shirking. The effect almost never came close to statistical significance in any test. This lack of detectable relationship may very well be due to the very small sample size used in these tests. Similarly, the effect of ethnic stacking could not even be examined at all, since only four nonviolent campaigns took place in the one region for which data on stacking are available (which, as mentioned previously, may be evidence for deterrence of uprisings at work).

If future work on the subject is able to employ a larger sample of nonviolent revolutionary uprisings, the fact that the signs for conscription were at least in the directions expected (negative for deployment, and positive for shirking), combined with the wealth of anecdotal evidence in its favor, suggests that there may still be something worth pursuing here. In this study, at least, I

do examine more closely why conscript militaries are in many cases willing to fire on unarmed protesters in order to dig into why I got the results I did in this chapter. In the case studies employed in Chapter 6, I include one case (South Korea 1980) where there was no shirking by the conscript military that was deployed against the nonviolent uprising, as well as another case (Romania 1989) where the military inflicted a significant number of casualties against the (mostly) nonviolent protests before eventually shirking.

Social distance aside, there were some interesting findings from some of the control variables. Deployment appears to be less likely with higher military spending per soldier and more likely under military regimes. There is also a slight hint that deployment might be more likely if the campaign has a radical flank. With regard to virtuous shirking, we can be fairly confident that soldiers employed by a military regime are less likely to shirk, and shirking is less likely in response to a campaign with a radical flank. There is also a slight hint that larger campaigns are more likely to induce shirking. All of these findings should be considered preliminary, due to the small sample size at work, but certainly worth investigating further.

Chapter 6

Exploring the Causal Mechanisms at Work

6.1 Introduction

In this final empirical chapter, I examine four countries in depth using a process-tracing approach in order to see whether the proposed causal mechanisms were at work in each stage of the theory. Specifically, I look for:

1. Whether countries facing high internal threat that created militaries with high social distance did so in anticipation of needing to use the military to put down mass uprisings; conversely, whether countries facing high external threat that created low-social distance militaries did so because of the constraining influence of having to prepare to meet the external threat.

2. For countries with narrowly recruited militaries that experienced no uprisings or only violent uprisings, whether the lack of nonviolent uprisings was due to fears of repression by the military; similarly, for countries that had broadly recruited militaries and faced nonviolent uprisings, whether the participants were willing to rise up against the regime because they anticipated shirking by the military.

- 3A. For countries experiencing a nonviolent uprising, whether the regime's decision about whether to deploy the military (and the senior officers' decision of whether to pass on that deployment order) was influenced by anticipations of whether the soldiers would shirk or repress when put to the test.

3B. For cases where soldiers were deployed against a nonviolent uprising, the extent to which social distance played a role in determining the soldiers' willingness to follow orders to repress.

These questions at each stage of the theory are kept in mind and addressed as I examine each of the cases studied in this chapter. Note that since the stages build on each other, some of the cases will end early: if a given country does not experience nonviolent uprisings (stage 2), then the analysis stops there for that country; similarly for a country experiencing a nonviolent uprising but where the military is not deployed (stage 3A), the analysis would stop there and not move on to examine the behavior of the soldiers once deployed, since there was no deployment.

Conducting a qualitative analysis along these lines also provides the opportunity to answer other important questions that emerge from this study. One of these questions is what role may have been played by other variables for which it was not possible to control in the statistical analyses in the previous chapters due to lack of large- n data on these variables – for instance, international pressure on the regime about how to respond to the mass uprisings (see Barany 2016, 37-38). In many of the cases where the predicted outcome did not match the actual outcome, it is entirely possible that this divergent result may have been due to omitted variables, which should become apparent when examining such cases.

Another important question that adding this comparative case study allows me to address is variable measurement. It is possible that there are alternative, valid ways of measuring the key variables that only become apparent when examining these cases. For instance, we already have some preliminary evidence from Saha (2014) that rural-versus-urban identity was a critically important component of social distance in Iran, which would not be captured by most measures of ethnicity. It is possible that external threat, internal threat, and social distance may be at work in ways that were not previously apparent due to the specific measurements used for these variables.

A third and especially important topic that I address in this chapter is why we would ever see off-equilibrium behavior. If we assume that all actors are rational and that they tailor their responses to the circumstances they face in ways that maximize their utility, then we would not expect to see cases of virtuous shirking, because the state leader should be able to anticipate such behavior and therefore avoid deploying the military. As shown in Chapter 5, the fact that there are so few cases of deployment of broadly recruited militaries suggests that such anticipations may very well be at work here. On the other hand, the fact that broadly recruited militaries are still

deployed sometimes, and that shirking takes place in some of those instances, begs the question of why we see this off-equilibrium-path behavior. Is this outcome simply the result of a failed gamble, or could there be something else systematic at work here? Similarly, why do we see protest movements get crushed by the military, as opposed to the protests not materializing in the first place in anticipation of that outcome? I address such questions through the case selection process used and devote attention to them in the narratives below.

6.2 Case Selection

I follow case selection advice from Goertz (2016) for multimethod designs combining statistical and qualitative analyses, for which the purpose of the qualitative portion is to show whether the proposed causal mechanism is at work. I focus first on providing “1, 1” cases, where the key concept represented by the independent variable was present and the phenomenon captured by the dependent variable also took place as expected, and secondarily on providing “1, 0” cases, where the IV concept was present, but the result was that the DV did not take place as expected. “1, 1” cases are the most crucial tests of the theory, because they allow us to see if the expected mechanism really was the one driving the results. “1, 0” cases are of secondary importance: they allow us to see why the theory was incorrect in its prediction, an outcome that may result from the key IV having no causal effect, or alternatively, from other causal factors outweighing the influence of the key IV. For instance, in stage 3B, a “1, 1” case (IV present, outcome as expected) would be one where a broadly recruited military shirked as expected. Upon examining such a case where the theorized mechanism is most likely to be at work, if it turned out that social distance had nothing to do with the observed shirking, this would constitute strong evidence against the theory. Similarly, for stage 3B, a “1, 0” case (IV present, unexpected outcome) would be one where a broadly recruited military did not shirk, which could also help disprove the theory if it is shown that social distance played no role at all.

Given that the most important stage of the theory to test is stage 3B, where deployed soldiers are faced with the choice of shirking or not, two of the cases chosen must therefore arrive at this stage with a broadly recruited military, with one ending in shirking (1, 1) and the other not (1, 0). To fit this need, I choose Romania 1989 and South Korea 1980. Romania 1989 was part of the

wave of Eastern European revolutions against communist regimes propped up by the Soviet Union. This case is famous for how the military turned against the regime after it was deployed against the widespread popular protests in December of that year.¹ South Korea 1980, also known as the Kwangju massacre, after the city in which the nonviolent uprising and subsequent mass shootings by the military took place, is an iconic example of a massacre of nonviolent protesters by the military.² Despite having a broadly recruited military, there was no shirking in this case. Both the Romanian and the South Korean case are also useful for showing why a regime would ever choose to take the risk of deploying a broadly recruited military against nonviolent revolutionary protests. The outcome was favorable to the regime in the South Korean case, but not the Romanian case, so we can ask why Chun Doo Hwan apparently got it right while Ceaușescu got it (fatally) wrong.

The next aspect of the theory to be tested is this study's contention that a large part of the effect of social distance works through deterrence. This includes deterrence of deployment, due to the state leader's fear of the military shirking, and also deterrence of nonviolent uprisings, due to potential revolutionaries' fear of the military cracking down violently. For the case of deterrence of nonviolent uprising, I choose Jordan, which maintained a narrowly recruited military. The state constructed a military in which Palestinians were under-represented and Transjordanians, especially Bedouins, were over-represented. Although it experienced a civil war in 1970, the Hashemite Kingdom has never experienced a nonviolent revolutionary uprising. This case allows us to address whether this absence of nonviolent uprising, particularly by the Palestinians, was due to expectations that the military would crack down violently. Jordan is also the only one of these four cases that is on the equilibrium path the whole way. For the case of deterrence of deployment, I choose East Germany 1989, where the regime had threatened to deploy the broadly recruited military but ultimately did not give orders to use lethal violence to quash the protests. This case allows us to see both whether protesters acted in anticipation of military shirking and whether the lack of deployment was also due to fears of shirking.

The final aspect of the theory being tested is whether the military design that the regime chose

¹In the Romanian case, soldiers initially followed orders and killed a number of protesters prior to defecting against the regime en masse several days later. Because my rules for coding the shirking variable focus on whether or not there was any widespread refusal to use violence, not whether or not there was widespread violence, this case was coded positively for shirking in the analysis in chapter 5.

²This case is more appropriate here than the better-known example of China's Tiananmen Square protest, which actually witnessed widespread shirking in the days leading up to the violent crackdown, and which is therefore coded 1 for shirking in my dataset.

to adopt was the result of the threat configuration that the country faced. The theory predicted that high internal threat induces state leaders to adopt a narrowly recruited military, while high external threat calls for a broadly recruited military. South Korea strongly fits the profile of a high-external / low-internal threat country, due to the existential threat from its northern rival on the one hand and the low levels of political instability and ethnic homogeneity of South Korea's population on the other, so this case allows us to see what role this threat configuration played in the regime's decision to recruit the military broadly using conscription. Jordan strongly fits the profile of a low-external / high-internal threat country, at least from the 1970s onward. Jordan's political system empowers the minority Transjordanian populace at the expense of the majority Palestinians, while the external threat from Israel has been low since the Six Day War (and arguably even before then). This case allows us to see whether the narrowly recruited military that Jordan maintains resulted from that threat configuration.

Part of the reason for choosing East Germany 1989 and Romania 1989 out of the larger pool of candidate cases is that they allow us to address the difficulties of defining a threat environment in a world that is often marked more by hierarchy than by anarchy (Lake 2009; Wendt and Friedheim 1995). Both ended up with a broadly recruited, conscript military for most or all of the Cold War, but the threat situation that they faced was complex. The regimes of Eastern European countries were built upon support from the USSR at the expense of domestic support, with Soviet willingness to act as force of last resort to prop up the local communist regime against internal uprisings. While subject to the threat of NATO invasion, the regime in both countries was also at risk of overthrow by their patron state if they did not toe the line well enough, as demonstrated in Czechoslovakia in 1968 and as formalized that year with the Brezhnev Doctrine. The sudden about-face by the Soviet Union in the 1980s with its renunciation of the Brezhnev Doctrine that meant that "the rug was pulled from under the feet of [these] dictators" (Barany 2016, 102) may be precisely the kind of external shock that could explain the emergence of mass uprisings taking place against the backdrop of a military ill-suited to the task of quelling them.

6.3 Military-Design Stage

6.3.1 East Germany

The purpose of Eastern European militaries as re-established by Stalin following WWII was to serve as direct appendages to the Red Army in case of a renewed European war, with Soviet advisers and commanders in the national militaries, although local militaries were given more autonomy after Stalin's death. After re-establishing the militaries of the other satellite states, the Soviets initially held back in their support for the East German military (NPA). The NPA was not created until 1956, and its loyalty was initially doubted after the then-recent anti-regime uprising in 1953, but the Soviets reversed course and came to support it in the early 1960s (Johnson, Dean, and Alexiev 1980, 7-8, 78).

The Soviet perception of external threat from NATO was high, and the militaries of satellite states in Eastern Europe were intended to help counter that threat, together with Soviet forces. The GDR military was more subordinate than other Eastern European militaries to the USSR. It had no control over the number and locations of Soviet troops stationed in the country. The Soviets maintained the right to intervene at will in response to anything they could define as an emergency due to internal or external conditions, and as well as the right to take control of NPA forces during war. The USSR also maintained staff working at every level in the NPA, and nearly all high-level officers attended Soviet military academies (Johnson et al. 1980, 82-84, 101).

A law for universal conscription passed in 1962 (after the Berlin Wall was constructed, in order to prevent further increasing the exodus of citizens; see Johnson et al. 1980, 78-79). Alternative service was introduced in 1964, but harassment of those who used it kept it rare (Leonhard 2008, 157-8). As a result of the strictness of the conscription law and its widespread adherence, participation in the military in East Germany was higher than any other Eastern European country and was supplemented by premilitary education³ and civil defense programs (Johnson et al. 1980, 76). As a result, “regular military service [became] an inherent part of a ‘normal’ male East German biography” (Leonhard 2008, 157).

³According to a 1980 estimate, “[a]pproximately 90 percent of the population between the ages of 16 and 19 . . . undergoes rigorous compulsory premilitary training” (Johnson et al. 1980, 101). These authors also commented on the universality of the military program, that “[v]irtually all males between the ages of 18 and 26 pass through the NPA's ranks, and the process continues for the majority of them in the reserves or in associated paramilitary organizations” (76).

The first purpose of the military from the regime's perspective was to serve as the primary setting for political socialization of society, especially to give everyone a personal stake in national defense: the "party school of the nation." This was the main method of compensating for the lack of a nationalist basis of legitimacy in the country: the fact that that East Germany was an artificial construct that had no sense of national identity separate from West Germany, and so the regime's legitimacy was entirely founded on political ideology. The next-most important purpose to the regime was deterring assault from NATO forces across the FRG-GDR border. GDR military doctrine presupposed that NATO forces would try to make a quick offensive to reunify Germany, so the doctrine highlighted the need to hold down invading forces long enough for a Warsaw Pact response that would generalize the conflict into a continental war, as well as the need to be able to take the offensive into the FRG. The third, lesser objective in building up such a big and strong military was to try to gain equal treatment as a military and political partner in the Warsaw Pact (Johnson et al. [1980](#), 74-81).

With regard to the officer corps, initially, the regime focused on recruiting based on political reliability, which meant from the working class, but over time the demands of modernization forced the main qualification for recruitment to be technical skill. Still, while this modernization of the military was taking place, the regime made special efforts to target the most politically loyal segments of society, such as the sons of party workers. The regime supplemented this targeted-recruitment program with a variety of controls to ensure that it would maintain the loyalty of the officer corps. These included sizeable economic benefits, making officering a socially prestigious profession, monitoring the organization at every level through informers who worked for Stasi (the regime's intelligence organization), and constant efforts at indoctrination and oversight by political officers (Johnson et al. [1980](#) 76, 93-107, 118).

6.3.2 Romania

From the beginning of its inclusion under the Soviet umbrella, Romania asserted its national prerogatives more than other Eastern European states. This included successfully opposing Soviet plans for the Romanian economy to remain based on oil extraction and agriculture, embarking instead on industrialization and trade with western countries. Romania signed the Warsaw Pact but insisted on nationalization of defense instead of military subordination to the USSR. In 1962

Romanian leaders refused to allow Warsaw Pact maneuvers on Romanian territory, and in 1964 the regime reduced the size of its military and the length of conscription terms as a rebuke to Warsaw Pact military centralization. Ceaușescu called for Soviet troop withdrawal from Eastern Europe in 1966, resisted participation in Warsaw Pact exercises (although he insisted on an equal voice for Eastern European states in Warsaw Pact governance), and angrily and publicly denounced the invasion of Czechoslovakia in 1968, refusing to send troops for the effort (Burke 1980, 32-35; Johnson et al. 1980, 14).

As with East Germany, Romania's threat orientation had been directed outward toward NATO when the military was first rebuilt after WWII. "On questions of doctrine, military organization, and the threat its forces were intended to meet, the Romanian military was essentially indistinguishable from the other Eastern European forces that had been re-created in the image of the Soviet Army in the early Cold War" – until the 1968 invasion of Czechoslovakia (Burke 1980, 34-35). That invasion caused a fundamental shift in threat perception in the Romanian regime, where elites were shocked and horrified that the USSR had overthrown a "fraternal socialist state." Nationalist sentiments, which had always been high among the Romanian public, rose even higher as the regime enlisted the populace in a massive territorial defense program modeled after the Yugoslav concept of Total Defense (see Johnson 1973). A law passed in 1972 involved mobilizing the civilian population through expanded conscription that extended even to women, with mandatory time afterward in either the military reserves or a civic militia called the Patriotic Guards. Premilitary training was instituted for boys and girls starting in the 5th grade and continuing through age 20. The 1972 law declared that national defense was the only purpose of the military and made surrender illegal, thereby guaranteeing prolonged guerrilla resistance by the entire populace mobilized in Patriotic Guard units throughout the country, all intended to make invasion too costly to consider. The Soviet Union was the unspoken but obvious target of this defense program – a fact reinforced by Romanian refusal to permit passage to Soviet troops in transit or even share intelligence with the USSR (Burke 1980; Johnson et al. 1980, 203-205).

6.3.3 South Korea

For its entire post-WWII history, South Korea has faced a constant existential threat from its northern neighbor, in an enmity constituting one of the world's most severe interstate rivalries. The

nature of the threat is worsened by the reunification imperative, the idea that the Korean nation had been artificially split and that one of the two Korean states must eventually subsume the other, so by its very existence each country was an ideological threat to the other (Buzan 1983, 47-48). The founding ideology of South Korea was anti-communism, and its string of autocratic leaders frequently used the threat from North Korea to justify repressive measures. Prior to the Korean War, both Kim Il-Sung and Syngman Rhee talked openly about conquering the other half of the peninsula. The perceived severity of the threat from North Korea was deeply reinforced by the Korean War, including the North's invasion, the 4 million civilian and military casualties sustained, and the hundreds of thousands of refugees fleeing ideological persecution in the North (Moon 2005, 25-26).

This perception of high external threat continued after the Korean War. Both Koreas participated in great measure, in terms of both manpower and resources, on their respective ideological sides in the Vietnam War. In addition, North Korea made an assassination attempt on South Korean leader Park Chung Hee in 1968 and also sent armed guerrillas to infiltrate South Korean towns that year. Tensions diminished a little during the 1970s, due to US detente with the USSR and overtures toward China, but there still continued stiff competition between North and South for military dominance, which on the South Korean side included mass mobilization of society for civil defense training and high taxes to pay for the military buildup (Moon 2005, 26-29, 36).

The South Korean regime traditionally used the (both real and alleged) threat of communist takeover to portray political dissidents as agents of the North. This was the case immediately after WWII, where those in power were mainly wealthy landowners and individuals who had been rulers under Japanese colonialism. They harnessed the US anti-communist effort for their own purposes, labeling political dissidents and leaders of social movements as communists. This tendency on the regime's part continued throughout the Cold War (Moon 2005, 24-25). The ideological division between the communist North and anti-communist South became the most salient identity cleavage within the country: "Whereas North Korean communists were external others, political dissidents and workers organizing democratic unions, often accused of being communist sympathizers, were internal others, who also needed to be punished" (Moon 2005, 37-38). In other words, the us-versus-them psychological process that underlies the concept of social distance was manifest within South Korean society in terms of communist versus anti-communist identity, rather than, as

with many other countries, in terms of ethnicity.

Soon after becoming the first president of South Korea, Syngman Rhee instituted military conscription with the 1949 conscription law, which created the principle of universal service duty for all males. Initially, this law provided for exemptions for children of the elite, and draft evasion was widespread during the 1950s, with a yearly average of 100,000 evaders. Park Chung-Hee, who succeeded Rhee after a brief interlude, doubled down on draft enforcement, including eliminating exemptions for the elite and establishing an onerous citizen identification and registration system that helped crack down on military service evasion (Jung 2014, 126-133; Moon 2005, 28-29).

6.3.4 Jordan

Since Transjordan was created as an artificial construct, carved out of Greater Syria following WWI, and the Hashemite dynasty installed by the British as a gesture of gratitude for Sharif Hussein's help during the war, the people of the country lacked a common national identity or a secure political base for the new regime. The post-WWII Jordanian military, which had been forged by British military adviser John Glubb, was largely composed of Bedouin Transjordanians, due to the martial-race theory that motivated British recruitment efforts in all its colonies at the time and Glubb's image of Bedouins as a martial-race group (Layne 1994, 10). Hence, the military's initial formulation was not due to threat-based considerations on the regime's part, but rather due to foreign tutelage and sponsorship.

During the decades after independence, the recruitment base of the military expanded to include Hadari (settled, or non-Bedouin) Transjordanians as well as – although to a much lesser extent – Palestinians, due to the threat from Israel that required a sizeable and sufficiently modernized military to protect against. However, internal threat was even greater for the regime. From the very beginning, the Hashemite monarchy faced a long string of assassination attempts⁴ as well as numerous coup attempts and plots, and mass demonstrations requiring the military to put down by force. The political danger that the regime faced in its first two decades was mainly from anti-western nationalist and republican nationalist Transjordanian townsmen, and recruitment in the military was based on careful assessment of political loyalty (Massad 2001, 188-204).

Starting with the formation of the PLO in the 1960s, which challenged Jordanian sovereignty

⁴Most notably, Hussein's grandfather Abdullah was successfully assassinated in 1951.

and administered its own state entity in Palestinian territories it claimed to have liberated from Jordanian control, the Palestinians became the source of the primary threat to the regime, leading up to the 1970-71 civil war that ended with the expulsion of the PLO from Jordan. That war helped to unite the various non-Palestinian identities in the country into a single Transjordanian identity, which became the new basis of the regime instead of its previous, more limited Bedouin identity. Bedouins, who had since the beginning played a critical regime-maintenance role (Axelrod 1978; Brand 1988, 155; Vatikiotis 1967, 133-134), still retained a sizeable share of the military, but their importance diminished. The war also established the Transjordanian-Palestinian division as the primary identity cleavage in the country (Massad 2001, 208, 218-19).

In addition to the Palestinians who defected during the civil war, the remaining Palestinians in the military were largely purged, reducing their share from approximately 45% in the 1960s out of a population share of about two-thirds (figure from Yazid Sayigh, cited in Massad 2001, 240) to, by one estimate, less than 10% in the late 1970s (Keegan 1983, 393). The regime had experimented with military conscription in 1966 in response to Palestinian demands for it in order to protect themselves against Israeli incursions into the West Bank, and again in 1968 in an effort to occupy Palestinian youth to prevent them from joining the PLO guerrillas (Abu-Odeh 1999, 131; Massad 2001, 206, 241-242). The conscription policy was rescinded in 1970 because of the belief that “the military training of the entire population (Palestinian and Transjordanian) might prove fatal to the regime itself. [Instead], the government opted for a different military alternative that would implicitly select only Transjordanians for such service . . .” (Massad 2001, 242).

After the war, the regime against adopted conscription in 1976, but the measure was highly selective. The military already had sufficient manpower, especially for its combat units, due to good pay, high prestige, and martial traditions among the Bedouins, but the kind of technical education and skill that Palestinians tended to possess was desperately needed for the army’s continuing modernization. Palestinians had always been used primarily for non-combat positions in the Jordanian military, and this conscription drive was no different (Brand 1988, 155; Keegan 1983, 393-397; Massad 2001, 215). The selectiveness of the conscription law is manifest in the fact that in the mid-1980s the Palestinian share only rose to approximately 25% (figure from Yazid Sayigh, cited in Massad 2001, 241).

Due to the strong British and, later, American influence on Jordan, its relations with Israel

had never been as hostile as Israel's other Arab neighbors. Jordan participated in the 1948 War, but King Abdullah's main objective there was in capturing and annexing the West Bank, rather than defeating the Israeli forces. Israel continued its raids and attacks on Jordan, but largely to pressure Jordan to control the Palestinian guerrillas. These tensions eased gradually after the 1967 war and the 1970-71 civil war, eventually leading to a peace treaty in the 1990s. The regime's perception of external threat dropped, leaving only the internal threat from the still politically excluded Palestinian population. Conscription was ended in 1992, two years before the peace treaty, in an effort to ensure Transjordanian dominance of the military in order to prevent any Palestinian takeover of the country (Ryan 2002, 127-128; 2011, 567). Since that time, rather than being under-represented in the military, Palestinians have been completely excluded from the military, barred even from serving in the auxiliary and technical forces (Zahran 2012).⁵

6.4 Uprising Stage

6.4.1 East Germany

After the mass uprising in the GDR in 1953 was crushed by Soviet forces, the East German populace remained quiescent until the late 1980s. Gorbachev's leadership in the USSR and the reforms that he embraced and urged on Eastern European leaders became a rallying cry for East Germans and signaled to the populace that, for the first time, the Red Army would not intervene to oppose reform efforts. As the regime vehemently resisted this reform pressure from its patron state, the populace took the cue.

In addition to signals from the USSR, the emmigration crisis in 1989 provided the other impetus for the mass uprising. The GDR maintained an open border with Hungary as a fellow Warsaw Pact state. When Hungary unilaterally opened up its border with Austria in September 1989 in contravention of its treaties with the GDR to allow East Germans to flee into Austria, where they could gain free passage to West Germany, this caused a massive and sudden outflow of citizens from the country. This outflow served as exactly the kind of signal necessary for citizens to gauge the prevailing level of discontent with the regime – otherwise hidden information due to institutional

⁵See also "Jordan's King Abdullah Purges Regime and Army of Palestinians," *DEBKAfile*, 5 September, 2009. <http://www.debka.com/article/8335/Jordan-s-King-Abdullah-purges-regime-and-army-of-Palestinians> (March 18, 2014).

incentives to hide anti-regime preferences. Whereas prior to late 1989, the regime and its coercive apparatus appeared solidly intact (Terrill 1994, 366), this mass-emigration movement convinced many to join the demonstrations in the belief that a critical mass of their fellow countrymen and -women would participate, insulating each individual from having to pay the participation costs of facing down the regime in small numbers (Pfaff 2006).

The demonstrations in East Germany began as a weekly “pray for peace” event that took place every Monday, organized by pastors from the Evangelical Lutheran Church. These gatherings soon took on a life of their own, with the regular meeting times and places providing focal points that allowed for coordination for thousands and then tens of thousands of uninvolved citizens who determined on their own to join in. Despite the initial organizing efforts by the Church, the demonstrations that by early October had swelled to massive proportions were best characterized as spontaneous and uncoordinated in nature (Opp, Voss, and Gern 1995). The influence of the church on their choice of nonviolent tactics was still strong, though, as pastors did everything in their power to plead for the protesters to abstain from violence, and the demonstrators followed this counsel diligently (Nepstad 2011b, ch. 3).

There is mixed evidence as to how the participants of this spontaneous revolution took the anticipated actions of the military into account. On the one hand, prior to the actual demonstration on October 9, there were widespread expectations of a violent crackdown. The regime had publicly and frequently praised the Chinese government’s response to the mass protests in Tiananmen earlier that year, making it very clear that political elites in the GDR were willing to follow the same solution to any mass uprising (Stokes 1993, 138). As they later expressed in surveys, the protesters who participated in the demonstrations leading up to October 9th expected “drastic measures” from the regime, which left a majority of the protesters “full of dread,” although they were willing to participate anyway (Pfaff 2006, 124-125). This expectation was not limited to the protesters: hospitals were reported to be “stocking blood supplies, clearing space in emergency rooms, and calling in doctors with expertise in gunshot wounds” (Nepstad 2011b, 47), and soldiers were even calling their wives and girlfriends to warn them against joining the protest for fear of their being harmed (Opp et al. 1995, 139).

On the other hand, Barany (2016, 117) asserts based on interview evidence that because “virtually all the male protesters ages 25-60 served as NVA conscripts themselves and understood the soldiers’

predicament[,] [f]ew of them thought that the troops would use live ammunition against them.” The most likely solution to this apparently contradictory evidence is to separate protesters’ expectations about the actions of the internal security forces and the military. As is common with autocratic regimes, the GDR maintained an array of internal security forces to deploy against mass uprisings such as this one, including riot police, party militias, and the especially-feared Ministry of State Security (“Stasi”) forces. Although the protesters may have expected the conscript soldiers to refrain from firing, they could very well anticipate a brutal crackdown from these ISFs. Despite the fact that the militias and riot police, many of whom were actually conscripts themselves, were unhappy with the task of protest policing and turned out to be unwilling to crack down violently (Pfaff 2006, 175-178), the main factor that stopped the ISFs, especially the Stasi, from opening fire was the sheer size of the demonstrations (Bruce 2010, 177).

6.4.2 Romania

For the two decades after the Soviet invasion of Czechoslovakia, Ceășescu’s main source of domestic legitimacy was Romanian nationalism, directed against threatening external others that included at least the USSR and Hungary. However, with the threat of invasion from the Soviet Union gone after the ascension of Gorbachev, the populace was free for the first time to vent its frustrations with the tyrannical neo-Stalinist policies of their regime, as well as free to oppose the corruption, nepotism, and bizarre cult of personality that the Ceășescus built up (Siani-Davies 2005, 18-26; Stokes 1993, 158-161).

The triggering event for the Romanian Revolution came on December 10, when the regime sent an eviction notice to a popular minister for the Hungarian Reformed Church in the city of Timișoara. His supporters began holding nightly vigils outside of his church, which swelled into large protests by December 16th. Over the course of the next week, the protests spread from Timișoara to other cities, including the capital, Bucharest.

During the December 1989 demonstrations, protesters employed a mix of violent and nonviolent tactics. The events in Timișoara on December 16 that led to the deployment of the military against the protesters the following day are best characterized a riot, which resulted in damage to a large number of stores in the downtown area and the ransacking of the ground floor of the local party headquarters building (Siani-Davies 2005, 60-62). There were also direct attacks on the soldiers,

including the throwing of rocks and petrol-bombs, in various instances (Hentea 2007, 173-175; Siani-Davies 2005, 72), although much of this may have been the work of agents provocateurs (Watts 1992, 107 and fn. 59-60).

However, there were also numerous instances of citizens deliberately approaching soldiers and fraternizing with them in the expectation that the conscript soldiers would not fire on them. For instance, after having been fired on by soldiers on the 17th, the protesters in Timișoara won over those army detachments a few days later on December 20th: when a large column of protesters confronted armed soldiers, “after a few moments of hesitation, the crowd surged forward and, while shouting ‘the army is with us’ and ‘without violence,’ overwhelmed the soldiers. Clambering over the armored personnel carriers they placed flags in the barrels of the machine guns and handed cigarettes, meat, and bread to the young conscripts” (Siani-Davies 2005, ch. 2).

Barany (2016, 126) reports that “there was little disconnect between society and the armed forces in Romania. Most male demonstrators served as conscripts in their youth and understood the pressures the draftees were under . . . Few of them would have expected the soldiers to use their guns against them to save Ceaușescu’s regime, and in the rare occasions when they did, the protesters were incredulous.” Similarly, Siani-Davies (2005, 38), states that

the members of the crowd, well before there was any discernible evidence that it really was the case, do seem to have been driven by the idea that the army was about to defect to the side of ‘the people.’ Fired by this belief, demonstrators unhesitatingly moved forward to fraternize with the troops . . . At a purely emotional level, few seem to have felt that the soldiers were capable of firing in cold blood at a crowd of innocent civilians. Many of the demonstrators had once been conscripts themselves and often still had friends or family who were serving within the army. Indeed, stories abound of conscripts coming face to face with relatives within ranks of the demonstrators during the revolution. . . . Following the embracing of nationalism, the Romanian army, as the chief forum for patriotic socialization, had taken on a renewed importance as the ‘schoolroom’ of the nation. This led to a widespread tendency both within the ranks of the military and society as a whole to identify the army with the nation – that is,

‘the people’ – at their widest extent . . .

Hence, to the extent that protesters used nonviolent tactics, this was done so in full expectation that the military would not fire on the people.

6.4.3 South Korea

South Korea had experienced a nonviolent revolution in 1960 that began with student protests and, after expanding to include a wide swath of society, had succeeded in toppling the dictatorship of Syngman Rhee. The 1960 uprising had featured support from the South Korean military for the protesters and pressure from the United States to avoid human rights abuses (Kim 1983). Since the Korean military was subordinate at that time to the United Nations Command in Korea whenever called into action, including for martial law, US pressure at that time for the military to not put down the demonstrations by force was likely a determinative factor in that earlier uprising (Kim 1983, 6). Control over the South Korean military became a joint US-South Korean responsibility in 1978,⁶ which meant in practical terms that the South Korean government could choose whether to deploy the military against domestic uprisings if the United States did not oppose its doing so. US influence over the regime also continued to be strong throughout the Cold War, with South Korea playing the role of an important anti-communist bulwark and US client state in the region. As with many other authoritarian client states of the US during the Cold War, American perception of client indispensability allowed the South Korean regime to use the fight against communism as a pretext for putting off democratization and engaging in repressive measures of all stripes.

Student protests opposing the military dictatorship had become commonplace by the end of the 1970s. Student demonstrations took place on October 16-18, 1979, and swelled to the tens of thousands before the protests were repressed by riot police and martial law was declared (Shin and Hwang 2003, xiii). Less than two weeks later, on October 26, Park was assassinated by the head of his own intelligence agency, the Korean CIA. Choi Kyu-Ha, the weak leader who succeeded him, was soon afterward ousted in a military coup on December 12, 1979 by Chun Doo Hwan, who established himself as dictator.

Pro-democracy demonstrations spread to Seoul, the capital city, in the spring of 1980, with 150,000 students demonstrating on May 15. The Chun regime responded by declaring martial law

⁶See <http://www.usfk.mil/About/Combined-Forces-Command>.

and making widespread arrests, and the student protest organizers backed off for a few days (Lee 2000, 27; Shin and Hwang 2003, xiv). On May 17, Chun ordered the arrest of his political opponent Kim Dae Jung and extended martial law to all of South Korea. Kwangju, the largest city in Honam and 5th largest city in South Korea, with a population of 730,000 in 1980 (Shin and Hwang 2003, xiv), became the next site of protests the following day, on May 18. Student demonstrations against martial law started at the gates of Chonnam National University, which had been ordered to close.

The anticipations by the Kwangju protesters of a possible response by the military and the regime appears to have been mixed. On the one hand, the protesters expected Chun's military regime to crack down in some way, which it had already done using internal security forces against the earlier protests. One student activist who participated in organizing the Kwangju protests asserts that the protesters were taking into account possible reactions of both the military and the United States:

One of our main concerns, naturally, was to try to understand in advance how the United States and the military – we thought of them in one breath – would react to the protest movement. . . . The United States and the (South Korean) military were seen as the only players that could bring about change . . . Seoul's student leadership suspended their demonstrations, believing that the military was subordinate to the United States but was still independent of the domestic government. Everybody expected a military reaction as the movement peaked. The question was, if the military used force to end the pro-democracy movement, what would the United States do? (Lee 2000, 26, internal quotes omitted).

Hence, it appears that the possibility of a military crackdown, not just a crackdown by the military regime, was present on protest organizers' minds.

On the other hand, once the army paratroopers did start killing students and civilians in Kwangju, there was a widespread sense of astonishment among the protesters: "The reaction was: 'What the hell is the military up to?' 'How could a (Korean) national army do this to fellow Koreans?' Rank incomprehension was overtaken by a sense of outrage" (Kim 2000, 8). This astonishment suggests that many participants had not expected the military to be willing to use violence to put down protests.

6.4.4 Jordan

Following the 1970-71 civil war, there have been four rather limited waves of political demonstrations in Jordan. The first of these was associated with the 1987 Palestinian Intifada that took place in Israel. Although Israel had captured the West Bank from Jordan in the Six Day War, Jordan continued to maintain political administration over the territory in ways that included Jordanian citizenship and representation in Jordan's parliament for the Palestinian residents of the area, as well as payment of salaries to Jordanian-employed bureaucrats in the area. King Hussein feared that because of the heavy Palestinian presence in the East Bank, the uprisings in Israel and the West Bank would spread to Jordan proper and end in the overthrow of the regime. This fear was exacerbated by Israel's attempts to promote a "Jordanian solution" for the problem of Palestinian displacement, which would have made Jordan the Palestinian homeland, also at the expense of the Hashemite monarchy. In response, Hussein suddenly and unilaterally severed Jordan's ties to the West Bank, claiming that doing so would promote the Palestinian cause by recognizing the sovereignty of the PLO over Palestinians in the West Bank. This move rendered the previously Jordanian Palestinians there stateless, but it successfully contained the Intifada to the West Bank, keeping it directed against Israel, and it put a decisive end to attempts to promote the Jordanian solution (Gandolfo 2012, 59-69; Lukacs 1999, 175-177; Ovendale 2015, 261-262).

The second, third, and fourth waves of demonstrations in Jordan advocated political reform and gradual democratization; they did not challenge the monarchy. Each of these waves took place mainly with Transjordanian participation, rather than Palestinian. The second and third waves took place in response to economic austerity measures imposed in 1989 and 1996, respectively, and were mainly riots. The fourth wave took place in 2010-2011 as part of the regional protest movement. All three of these waves were successfully contained with a combination of ISF deployment⁷ and limited policy concessions by the regime (Ryan 2002, ch. 3; 2011).

There were no real Palestinian uprisings in Jordan to speak of, especially the kind of nonviolent revolutionary uprisings we are interested in here. I have not been able to find any evidence that this lack of uprising was because of fear that the (increasingly Transjordanian-dominated) military would be deployed against it. Rather, there was a strong attempt by the regime at nation-building, with a

⁷The military was deployed in 1996, but only for crowd-control duty (Ryan 2002, 57-58).

constant stream of rhetoric regarding the unity of the Jordanian identity, that Transjordanians and Palestinians are one people. For their part, although many remained loyal to the PLO first, most Palestinians in Jordan viewed themselves as loyal Jordanians and diligently sought to assimilate. Even laws and practices regarding state employment and state funding that discriminated in favor of Transjordanians, as well as virulently nativist rhetoric on the part of many Transjordanians against Palestinians, was not enough to shake this general desire among Jordanian Palestinians to be viewed as loyal citizens of Jordan (Massad [2001](#), 246-275).

6.5 Deployment and Military-Response Stage

6.5.1 East Germany

In East Germany in 1989, the military was never deployed with orders to fire on the protesters (Opp et al. [1995](#), 179). Soldiers did confront protesters, especially in the dramatic showdown in Leipzig on October 9th that marked the turning point of the movement, but they were ordered to engage in crowd control only, ensuring that the protests did not turn violent. Despite the regime's public applause of Tiananmen and its support for a violent crackdown, there was a good deal of internal division about whether such a course would actually work. Consequently, when the central Party authorities placed the military at the disposal of the local leaders in cities where demonstrations were taking place, they only authorized the use of force, without mandating it. In turn, the local Party leaders and police commanders who took charge of the military and ISF units were eager to avoid responsibility for the twin dangers of massacre and mass defections, and they ordered the troops to quell the demonstrations without lethal violence. A few days after the October 9th events, the regime explicitly forbid the security forces to use violence against the protests throughout the country, which led to the fall of the regime a few months later (Barany [2016](#), 115-117; Dale [2006](#), 155-156; Nepstad [2011b](#), 48-50).

The unwillingness of the military to crack down had initially been revealed in incidents leading up from September to the October 9th protests, such as when soldiers in Dresden refused orders to clear (using non-lethal methods) a train station of protesters on October 3-4 (Opp et al. [1995](#), 178; Pfaff [2006](#), 178):

NVA officers openly admitted their frustrations with and resentment of the

situation they found themselves in: compelled to rough up peaceful protesters, leading soldiers who in many cases would rather risk being court-martialed and imprisoned rather than obey their superiors, and, in general, being forced ‘to do the party’s dirty work.’ Many soldiers publicly expressed sympathy with the demonstrators and, not surprisingly, those who spotted their parents, siblings, and acquaintances in the crowds were especially troubled (Barany 2016, 117).

Further, the regime had reports predating the protest movement about the loyalty of the military should it be deployed against then-hypothetical mass protests, which suggested that it would not stand by the regime: “As early as August 1989, the Stasi cautioned the state against using the military against civilians because of troop unreliability . . . Even Military State Attorney Köcher admitted that security force leaders expected that rank-and-file troops would disobey orders” (Nepstad 2011b, 48). Given this intelligence and warning signs from the early, limited deployment attempts, it is understandable why the regime would not order the military to crack down using lethal violence.

6.5.2 Romania

Why did Ceășescu gamble on the loyalty of his broadly recruited military? The best answer appears to be that he thought he could still succeed in mustering up support for the regime by playing on Romanian nationalism, painting the protesters in an us-versus-them fashion as agents of foreign powers like the US, USSR, and Romania’s traditional rival and neighbor, Hungary. In a meeting with his staff on December 17th in response to the initial events in Timișoara from the day before, he asserted emphatically that “foreign circles are involved, foreign spy agencies, beginning with Budapest [Hungary] . . . Both East and West have decided to change things and they are using any means possible” (quoted in Siani-Davies 2005, 64-65). Interestingly, Ceășescu may have even been sincere in these statements. Increasingly paranoid toward the possibility of overthrow by foreign powers or domestic subversives as the years went on, he also seems to have bought into his own cult of personality and the legitimacy of his rule to the point where he could not fathom that his own people might collectively oppose him (DeNevers 2003, 259-262; Hampton and Burnham 1990, 130-138).

Whether due to Ceășescu's own paranoid beliefs, or out of political expedience, the soldiers deployed were manipulated in such a way as to maximize the social distance between them and the people in the crowd they were ordered to shoot at:

The units were isolated in their barracks where only official information channels were open to them, all of which confirmed the impression of an imminent Hungarian aggression coordinated with internal terrorist actions designed to tear Transylvania from Romania. Lieutenant General Ilie Ceășescu personally visited the units, presenting this scenario and supplying convincing 'proof' that over 2,000 Hungarian-sponsored terrorists had destroyed Timișoara and that all nonviolent efforts to calm the situation had failed, necessitating the use of the military (Watts 1992, 106).

In addition, the soldiers to be deployed were told that the "terrorists" had already killed Romanian soldiers and captured military weaponry; that there had been attacks on the families of some soldiers; that they had plotted to kill senior officers; and that "psycho-chemical drugs were being used by the attackers/terrorists to manipulate the population" (Watts 1992, 106-107, internal quotes omitted).

In addition to this psychological manipulation, Ceășescu also made sure to deploy the more loyal internal security forces alongside the military. ISFs deployed ranged from citizen forces like the Patriotic Guard to the regime's mainstay, the Securitate (Romanian secret police) (Siani-Davies 2005, 67).

The response by the military forces deployed against protests taking place in various locations throughout Romania was mixed. There were several instances, most notably in Timișoara on the night of December 17th, when soldiers opened fire on the civilians (Barany 2016, 121, 126). However, over time, especially in response to deliberate efforts by the protesters to fraternize with the soldiers, widespread defections took place, necessitating the withdrawal of entire units by their officers back to the barracks (Siani-Davies 2005, 92-93).

Besides the role that social distance appears to have played due to the psychological-manipulation tactics described above, the other reason for the military's initial willingness to shoot at the civilians instead of shirking is the use of violence by the protesters. Although the analyses previously employed

in this study coded this uprising as primarily nonviolent, in reality, it falls somewhere in the middle of the spectrum. The damage done by rioting in Timișoara prior to the evening of December 17th was extensive, which did not help to dispel the soldiers' initial perceptions (Siani-Davies 2005, 67-68). In addition, in many of the instances when soldiers confronted protesters over the next few days throughout Romania, although the bulk of the crowd avoided violence, there were attacks on soldiers by small groups of civilians, which tended to provoke the poorly trained conscripts into firing wildly, often hitting other soldiers. Eyewitness accounts by members of the military hint that these instigators were not part of the demonstrations and were probably agents provocateurs (Barany 2016, 125; Hentea 2007, 173-178; Watts 1992, 107 and fn. 59-60).

6.5.3 South Korea

The military had not been deployed against protests in Pusan and Masan in 1979, which were managed entirely by the ISFs, nor against the student demonstrations in Seoul earlier in May 1980, which ended of their own accord, but Chun chose to deploy a force of 3,000 army paratroopers against the demonstrators in Kwangju. This force consisted of the 7th and the 11th Brigades, and they were given orders to put down the protest movement there by force (Lee 2000; Shin and Hwang 2003, xv, xix, 4-5). These soldiers followed through on the orders, resulting in civilian casualties estimated at 500 dead and over 3,000 injured, by the end of May (Shin and Hwang 2003, xvii).

Chun's decision to deploy the military in addition to police forces against the uprising in Kwangju was quite premeditated. During the period of May 3-17, in preparation for the declaration of martial law, paratroop forces had been ordered to prepare for possible deployment to "every major city throughout the nation except Pusan," and advanced forces had been sent to these locations "in secret, to familiarize themselves with the terrain" (Lee 2000, 28). This evidence suggests that Seoul might have been targeted for deployment as well had the demonstrations there continued a little longer.

Some information about the type of military units sent may help resolve the puzzle about why Chun would gamble on the loyalty of a military that was broadly recruited. Paratroopers were trained as the front-line military forces in any conflict with North Korea, but especially in guerrilla warfare. Also known as Black Berets, paratroopers in the South Korean army constituted the most politically loyal and the most hardline anti-communist units within the military.

As in Romania, the South Korean regime attempted to manufacture social distance through propaganda fed to the soldiers at the time of their deployment. The justification provided for the deployment was the need to prepare against threatening maneuvers by North Korea. The paratroopers deployed to Kwangju were told that they were putting down an insurrection by communist agents (Lee 2000, 28, 31; Shin and Hwang 2003, xv-xx).⁸

Upon deployment, soldiers arrived in Kwangju without visible identification on their uniforms. Neither their names nor their regimental insignia were displayed, contrary to standard practice, perhaps in the expectation that anonymity would facilitate brutality. The first day of the crackdown, the paratroopers charged into the crowd of students with batons and bayonets, injuring 78. Their attacks against female students were just as vicious, if not more so. In response to this brutality, the citizens of Kwangju began joining in with the students the following day, May 19, and the soldiers attacked these combined demonstrations as they had the previous day. As the protest size swelled to the tens of thousands on the third day, the soldiers opted for firearms and flamethrowers instead of bayonets, murdering dozens and injuring hundreds, including women and the elderly, over May 20-21. In response to this onslaught, protesters began adopting violent tactics and fighting back, including using guns captured from police armories. The demonstrations on May 21 had swelled to 300,000 residents of Kwangju – over 40% of the entire population of the city, an enormous proportion for any protest movement. In response, the soldiers withdrew to the suburbs, while the citizens formed an ad hoc militia and took control of the city for the next week. Finally, on May 27, the paratroopers re-entered the city with 30 tanks and crushed the movement by force. Crucially, the move to re-deploy the soldiers to put down the uprising took place with the endorsement of the US commanders in the region, who appear to have bought the regime's propaganda that the uprising was communist-inspired (Choi 2003, 4-8; Kim 2000, 9, 16; Lee 2000, 32; Shin and Hwang 2003, xv-xvii, xxiii).

Despite the overriding influence of ideological indoctrination and regime propaganda, there is still evidence that Chun doubted the loyalty of the rank and file soldiers in the paratroop brigades deployed. One observer of the soldiers' charge into the crowd on May 18th described an

⁸Another interesting similarity with the Romanian case: one author notes that this politically convenient assertion may have actually been sincere. Based on evidence that emerged from his trial following democratization a decade later, Chun appears to honestly have believed that the restive Honam province was under the sway of communist sympathizers (Lee 2000, 38-39).

important preventative measure at work to prevent shirking: “As the men marched in column into the demonstration, they were led by lieutenants at the front. Behind the officers came the privates. Behind them came MPs with armbands, carrying rifles. The MPs pointed their barrels at the columns ahead. To recap: The officers led the way, at the rear came those MPs. A private who broke ranks without receiving a direct order to do so was to be shot by one of the MPs” (Kim 2000, 16). This critical piece of evidence suggests that the regime was hedging its bets in the decision to deploy conscripts against the civilian uprising, that without this procedure shirking may have taken place.

6.6 Conclusions

For the most part, the causal mechanisms that this study has proposed were found to be at work in driving the behavior of the actors involved, although the case studies revealed several nuances and useful points to take into account in future work on the subject.

6.6.1 Military-Design Stage

Of all three stages of the theory, the behavior of actors in the first stage most closely fit the causal mechanisms proposed, that of responding to internal and external threat. In the GDR, Romania, and South Korea, the broad recruitment base for the military established by the regime in each country was a direct response to external threat that each faced: for South Korea, from its communist neighbor to the north; for the GDR and Romania prior to 1968, from NATO forces; and for Romania after 1968, from the Soviet Union. The regime in each of these countries promoted universal or near-universal conscription as a means to involve the populace in national defense; beyond providing necessary manpower, this also served as a means of political socialization to bolster the regime’s legitimacy, especially in the case of the GDR, which as an artificial creation of Cold War politics lacked the benefit of the coinciding of nation and state.

In Jordan, the narrowly recruited military initially provided by the British broadened in its recruitment base somewhat in the years leading up to the 1970-71 civil war, due to the moderately severe threat from Israel and the need to bring in Palestinian technical expertise to meet the demands of military modernization. After the civil war, in response to a diminishing threat from

Israel and the increased perception of threat from the Palestinian population, the military became steadily more exclusive in order to serve as a reliable force of last resort for the monarchy.

6.6.2 Uprising Stage

As was the case in the quantitative analysis in chapter 4, the second stage of the theory received the least support of the three stages here. The timing of uprisings appears to have been only somewhat connected to the military design that each country maintained. External influence and other factors were generally of greater importance. When uprisings broke out in the GDR and Romania, it was mainly due to the external shock of the USSR renouncing the Brezhnev Doctrine, which meant that uprisings would not face the threat of being crushed by the Red Army. Once the regime had to rely on its own coercive apparatus, and after protest movements had been ignited by fairly exogenous and idiosyncratic causes, at that point the protesters were generally unrestrained by a fear of military crackdown, even expected and attempting to induce defection by the military.

In South Korea, the Kwangju uprising took place as part of a long train of student demonstrations that started with the successful 1960 revolt. That revolt had witnessed military and international support for the student cause, which might have provided hope of a similar outcome in the case of future uprisings. As students tend to be one of the most radical demographics in any society and the least likely to be deterred by the prospect of repression, the continuation of the student protest movement in itself is weak evidence of anticipations of military behavior. The Kwangju student protest leaders actually did contemplate the possibility of a military crackdown but hoped to be able to induce foreign pressure on the regime from the United States to avoid killing civilians. The citizens of Kwangju who participated in the uprising, however, seem to have been genuinely astonished at the army's willingness to employ lethal force against the nonviolent uprising, based on its public perception as a people's army, and only adopted violent tactics in response to the paratroopers' brutality.

For Jordan, despite the difficulty of demonstrating the counterfactual – that a revolutionary nonviolent uprising would have taken place if the military had been broadly instead of narrowly recruited – the available evidence for the lack of such an uprising seems to indicate a variety of politically savvy maneuvers by the regime, rather than military composition, as the main culprit. Such maneuvers included combining limited policy concessions with limited repression

by ISFs (in response to the 1989, 1996, and 2010-11 protests); promoting national unity through propaganda and rhetoric aimed at transcending ethnic divisions; and one instance of outright, wholesale disenfranchisement of the politically unreliable populace (in response to the 1987 Intifada). At the same time, it is not possible to rule out the possibility that the narrowly recruited military serves as a deterrent to potential uprisings, and the regime itself apparently perceives the military as playing this role.

6.6.3 Deployment and Military-Response Stage

In all three of the cases that made it to this stage, the case studies uncovered strong evidence that social distance was at work in one form or another. In the GDR, although it came close, the regime never deployed the military with orders to use lethal violence, precisely because of fears of widespread shirking by conscript soldiers with close ties to the populace. These fears were based on studies conducted prior to the uprising that arrived at that conclusion, as well as initial acts of shirking and unhappy sentiments by the soldiers sent to conduct crowd-control operations using non-lethal violence in order to clear out protesters in some of the initial confrontations. However, this effect was strongly conditioned on protest tactics. Had the protesters rioted or attacked the soldiers, as took place in Romania, it is highly likely that the October 9th confrontation would have ended in bloodshed, even without orders to open fire ever having been given by the political authorities. This is especially likely to have been the case since, as happened in Romania, the military was reinforced with ISF forces, including the secret police that formed the regime's mainstay in each country.

In Romania and South Korea, the regime attempted to compensate for low social distance that was due to recruitment through propaganda and indoctrination aimed at artificially generating social distance, making the soldiers perceive the protesters as threatening others. In South Korea, political ideology in the form of pro- versus anti-communism formed the strongest identity cleavage within society, and special forces like the paratroopers chosen for deployment in Kwangju would have been especially susceptible to this line of propaganda, given that the *raison d'être* behind their force was to fight against communist-led guerilla movements. The South Korean paratroopers were told that they were being sent to fight against communist subversives. As a backup, in case the rank and file soldiers had second thoughts about firing into the crowd, the process of deployment included

a strict enforcement mechanism of having the military police train their guns on the other soldiers to guard against possible disloyalty. In Romania, regular military forces, rather than special forces, were deployed. The Romanian soldiers were fed a set of outright lies, including that they were fighting against Hungarian-sponsored terrorists who had drugged the population, captured military weaponry, and killed soldiers and even the soldiers' families back home. However, in contrast with the South Korean case, the Romanian soldiers were eventually persuaded otherwise through efforts by the Romanian protesters to fraternize with the soldiers.

Chapter 7

Conclusions

This study has explored the impact of social distance on the reaction by the military to nonviolent revolutionary mass protests in autocracies. It presented and tested a multi-stage theory starting with the setting of military recruitment policy and proceeding to the formation of a revolutionary protest movement, the regime's decision of whether to deploy the military in response to the mass protest, and finally to the behavior of the soldiers tasked with putting down the protest. Chapters 3-5 subjected the theory to statistical tests to explore the generalizability of the patterns highlighted by the anecdotes and case studies used in previous research and journalistic reports on the subject, while Chapter 6 examined whether the causal mechanisms were actually at work in four historical cases, representing varying outcomes on different branches of the proposed theoretical pathway.

This study represents one of the very first quantitative approaches to the topic of military response to revolution. As a result, a great deal of original data collection was required in order to generate the specific measures needed to test the theory, including measures of military composition (ethnic stacking), military deployment, and virtuous shirking, which were created from scratch using hundreds of secondary sources. Since publicly available data on mass protests was ill-suited to examining the specific category of protests at issue here, empirical tests of the theory also required making extensive modifications to those data to generate a set of revolutionary mass protests. On the one hand, this strategy has the benefit of ensuring a higher validity for my measures used in this study, compared to trying to employ awkward proxies that only indirectly capture each essential concept. On the other, one downside to this approach is that the measures I have used herein have

not been thoroughly vetted through application by other authors in other quantitative studies.¹

This study is also unique in terms of the comprehensive approach it takes to the topic. Most research on military behavior during revolution takes the emergence of mass protest as the starting point, but this leaves out the possibility that factors determining the military's response to revolution may also influence whether those protests emerge in the first place, and their form, violent or nonviolent. I take it one step further back and also consider the military's institutional design in the first place, in terms of recruitment. If we assume that state leaders and protesters are rational actors who can anticipate the behavior of the military during confrontations and act strategically to pursue their interests – maintaining the regime and overthrowing the regime, respectively – then to observe the full effect of a factor such as social distance due to recruitment patterns, it is essential to examine these prior stages.

As an examination of social distance as the key factor proposed to be at work here, it was essential for this study to incorporate the decades' worth of social-psychology experiments on the subject (summarized in Chapter 2). No prior research on the effect of social distance on military response to revolution has taken advantage of this low-hanging fruit. This set of experiments, which examined how variation in social distance affected individuals' willingness to disobey authoritative orders to cause harm to others, or to intervene to protect them from harm at cost to self, and the physiological basis underlying that willingness, provide the microfoundations for the theory by showing how the social-distance mechanism operates at the individual level.

While prior work on the subject of military response to revolution either treats the military as a collective actor or focuses on the behavior of the senior officers (although see Barany 2016; Bou Nassif 2015b), a theory of social distance must focus specifically on the street-level bureaucrats, the soldiers and junior officers, who are the ones that would directly confront the protesters. Indeed, outcomes like coups that have received comparatively greater attention often take place in response to behavior by the street-level bureaucrats. Disaggregating the military into its vertical components is a critical step for the literature on this subject to make, and this study shows both the feasibility and the utility of doing so.

Last, as summarized in Chapter 1, this study adds to existing work on the subjects of military

¹The military ethnicity data used in this paper are, as of this writing, under review for publication as a stand-alone dataset (Johnson and Thurber 2016b), so this should change in the near future.

design and contentious politics. Large- n studies of the causes of conscription are few, and on the causes of ethnic stacking, basically nonexistent. I examined a wide range of threat-related factors, both domestic and foreign, to demonstrate which ones influence military recruitment patterns. Regarding protests, this study is also one of the first to examine both violent and nonviolent movements and the tactical choice between them.

7.1 Theory: Recap

The main proposition being tested was that confrontations between militaries and protesters marked by low social distance between the two sides would be more likely to witness virtuous shirking, the refusal to carry out orders to use lethal repression against the civilians protesting. This study operationalized social distance in terms of conscription use and ethnic stacking in the military, with conscription representing a form of broad-based recruitment, implying lower social distance, and ethnic stacking representing a narrow form of recruitment, and thus higher social distance. As demonstrated in Section 3.4.1 above, the correlation between these two variables is low, indicating that they represent quite different dimensions of social distance.

The main hypothesis, stated above, concerns behavior in the final stage of the theory, and the remaining hypotheses being tested were generated on the basis of backward induction (using the term loosely, since the theory being tested is qualitative, not formal, in nature). If a military that is recruited in a way that creates low social distance between soldiers and protesting masses is more likely to shirk, then we should expect state leaders, in anticipation of that shirking, to avoid deploying such a force. Similarly, if would-be protesters can observe the military's social composition and likewise anticipate shirking in the event of deployment, they should be more likely to launch nonviolent mass protests in hopes of inducing shirking, as opposed to engaging in violent insurgency or simply staying home. Finally, going back to the initial stage of the tree, state leaders anticipating shirking from a low-social-distance military and civilians who would take advantage of that weakness in the regime's coercive forces to launch a nonviolent revolution should be expected to narrow the recruitment basis of the military to build in high social distance, out of regime self-preservation.

The theory presented in this study also took account of the potentially constraining international environment that state leaders face. Since the military is the force of last resort to protect the

state and its rulers from external threats, high levels of external threat were hypothesized to result in broad-based recruitment practices: specifically, adopting conscription, which enables recruiting large forces to deal with foreign military threats, and avoiding ethnic stacking, since that practice degrades war-winning capability. However, since military composition changes slowly by nature, regimes may be left vulnerable to shocks in terms of the kinds and levels of threat that the state faces, potentially resulting in a military that is no longer ideally suited to preserving the regime from its enemies. Hence, while in equilibrium nonviolent revolutionary protests should not arise, because the protesters would be deterred by the presence of a military designed to counter mass protests, we would still expect to see off-equilibrium behavior due to sudden, exogenous changes in the threat environment.

7.2 Summary of Findings

7.2.1 Military Design

Regarding military design, as expected, the adoption of military conscription is due to both external and internal threat. Both the statistical and the case-study tests were in agreement here. In terms of the quantitative results, interstate war and strategic rivalry appear to make conscription use more likely, while new dictators, who are presumed to feel less secure in office, are less willing to use conscription than long-serving autocrats. Contrary to initial expectations, the effect of irregular entry on conscription use was positive, which may represent an additional role of conscription as a mechanism for population control, an interpretation partly bolstered by the case-study evidence.

External threat had no measurable impact on ethnic stacking in the quantitative tests. Rather, internal-threat factors, particularly civil war, made the difference there. Jordan was the only one of the four cases in Chapter 6 that involved the use of ethnic stacking. In Jordan's case, external threat may have played a small role, as Palestinians went from being under-represented prior to the *de facto* and then *de jure* peace with Israel to being completely excluded from the military. However, this largely coincided with a rise in perception of the internal threat posed by the Palestinian population in Jordan, especially due to the civil war and also fear of spillover from the intifadas, so it was not possible to tease out the relative weight of internal versus external threat in this case.

Interestingly, revolutionary uprisings – a separate phenomenon from protests and civil wars

broadly considered, due to having revolutionary goals – had no apparent effect on military design in the quantitative tests. The case studies shed some light on this odd finding, by showing insecure regimes sometimes using conscription as a means of political socialization. This was certainly the case with East Germany’s leaders, for whom the 1953 uprising loomed large, and who used conscription to attempt to manufacture a sense of nation in the hopes of generating legitimacy for the regime.

7.2.2 Mass Protests

Conscription had no measurable effect on the onset of revolutionary mass protests, either violent or nonviolent, in the quantitative tests employed herein. The case studies helped explain why, showing that while protesters, once mobilized, expected to be able to win over the conscript military, conscription played no role in the *timing* of the mass protests. Instead, other factors, particularly the influence of foreign patron states, mattered more for explaining revolutionary onset.

The statistical tests found that while neither ethnic stacking in the officer corps nor in the rank and file has any apparent effect on the onset of violent revolutionary movements, each form of stacking appears to deter nonviolent revolutions. These results should be considered quite tentative due to the limitations posed by the sample for which data on ethnic stacking is available, and particularly the fact that there were so few nonviolent revolutionary movements that emerged in the MENA region. Additionally, the study of the Jordanian case showed that other factors such as regime tactics of concession and police repression, as well as the steady use of rhetoric emphasizing national unity, appear to have played a much larger role than military ethnicity in explaining the lack of nonviolent revolutions in this country.

7.2.3 Deployment and Shirking

Although the sign on the coefficient was consistently in the direction expected, conscription did not come anywhere close to significance in any of the statistical models of deployment or shirking, a result that cannot be entirely blamed on the small sample size available. The case-study evidence uncovered was invaluable in explaining this non-finding, in two ways.

First, while conscription use did play an important role in explaining deployment and shirking in these cases, that role was highly contingent on protest tactics: the East German case, where

expectations of shirking due to social distance deterred the regime from deploying the military, would have very likely ended in widespread bloodshed, as in Romania in the first few days of confrontation, had the protesters not remained strictly nonviolent.

Second, the Romanian and South Korean regimes demonstrated that social distance could play an important role that this study had not initially anticipated. Shirking eventually took place in the Romanian case, but only after repeated attempts by the protesters to fraternize with the soldiers, while the South Korean case, where nonviolent protests escalated into street warfare and where fraternization did not take place, witnessed no shirking.² The regime worked hard in both of these cases to prevent anticipated shirking by artificially generating social distance through propaganda. The South Korean protesters were portrayed as communist subversives, playing on the most salient identity cleavage in the country, communist vs. anti-communist. The Romanian protesters were portrayed as terrorists and foreign agents, while the moderate degree to which they used violence slowed down rate at which the realization emerged that these were really Romanian civilians protesting for a just cause. These initial perceptions of the protesters that the regimes created through propaganda entirely overcame the effect of conscription use in the South Korean case, although that result was probably also due to the use of elite forces within the military specially trained for domestic warfare and enforcement mechanisms designed to immediately detect and punish shirking, factors not initially considered but which will be very useful for future work to take into account.

There were too few cases of nonviolent revolutionary campaigns in the MENA region to examine ethnic stacking's effect on deployment and shirking statistically. As the Jordanian case did not witness the emergence of a nonviolent revolutionary protest, for this case I did not examine the effect of ethnic stacking on soldiers' responses when deployed, or the regime's decision of whether to deploy the military. The other three cases examined only countries that were relatively homogenous ethnically, so they only go so far in showing the impact of lack of ethnic stacking. Space and time constraints prevented including a fifth case that could do a better job of showing the process and degree to which ethnic stacking makes a difference in determining these outcomes, but a useful case for follow-on work here would be one that is not ethnically homogenous and that experienced

²For an earlier study highlighting the importance of fraternization as an opposition tactic to affect military loyalty, see Binnendijk and Marovic [2006](#).

variation over time in the degree of ethnic stacking.

7.3 Recommendations for Future Work

Future work might do well to broaden the theoretical scope in various ways to answer questions such as: Does social distance affect military responses to *violent* revolutionary protests? What about non-revolutionary protests? And, do the effects (and non-effects) found here travel to non-autocratic regimes?

One of the greatest difficulties encountered in conducting this study was dealing with the lack of readily available protest data. The admittedly awkward solution resorted to here was to use NAVCO campaign-level data, taking the subset of campaigns meeting the “revolutionary” criteria and then fixing the start and end dates for the revolutionary period based on corroborating datasets (MEC, ACD) and secondary sources. In reality, the proper unit of analysis for testing this theory of social distance is the protest event, or even the protest-security force dyad (for the two sides in each event confrontation), rather than the protest campaign. A single antigovernment campaign may last for years (or decades, in the case of some violent insurgencies) but be marked by only periodic episodes of actual street protests, each usually lasting no more than a few days. Saha’s (2014) single-country study of protest events in Iran provides an excellent template here for this kind of approach.

Better measures for some of the key variables would be very helpful as well. For instance, the deployment measure does not disaggregate the actions of state leaders and of senior officers, a distinction that would require the addition of a separate stage of the theory to account for. The conscription measure from Toronto (2005), while helpful, is also wanting, as it classifies countries as using conscription if a “non-trivial number of recruits are enlisted through force” (p. 3). In practice, this results in rather disparate militaries being lumped together, including massive armies recruited through universal conscription, smaller forces drawn from selective-service lotteries, militaries that conscript only one segment of the populace, and forces comprised mainly of volunteers who serve alongside a smaller (but still non-trivial) number of conscripts. The cases of South Korea, Romania, and East Germany all fit the universal-conscription model, so it is possible that the divergence in findings between the case study and the statistical tests for the effect of conscription on deployment

and shirking is due to the latter averaging across heterogeneous effects.

Similarly, the military ethnicity data presented significant challenges here. The fact that the data are only available for the MENA region limited confidence in the results from testing the protest stage of the theory and entirely precluded being able to test statistically the deployment and shirking stage. Additionally, although breaking out separate measures for the officers and the rank and file was very useful here, the measures are still too aggregated to capture a lot of the action we know takes place on the ground, such as the regime only calling up specific crack units to quash the protests, which are often drawn from especially loyal ethnic groups.

Last taking internal security forces into greater account would also be essential for future work. Generally, even less information is available about ISFs than about state militaries, especially when we consider more ad-hoc kinds of forces such as militias, making this task difficult, but recent efforts by authors like Belkin and Schofer (2005), Pilster and Böhmelt (2011, 2012, 2015), De Bruin (2014), and Carey and co-authors (Carey, Mitchell, and Lowe 2013; Carey, Colaresi, and Mitchell 2015) show that ISFs are amenable to quantitative study.

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