When Only a Genius Could Keep the Peace

The effect of repressive instability on internal conflict

Tonje Jelstad Sandanger

Master Thesis in Political Science,
Department of Political Science

UNIVERSITY OF OSLO

October 2015

When Only a Genius Could Keep the Peace

The effect of repressive instability on internal conflict

Master Thesis in Political Science,
Department of Political Science
University of Oslo

Tonje Jelstad Sandanger

Word Count: 32,723

2015

When Only a Genius Could Keep the Peace:

The effect of repressive instability on internal conflict

Tonje Jelstad Sandanger

Word Count: 32,723

http://www.duo.uio.no/

Print: Reprosentralen, Universitetet i Oslo

Abstract

In recent years, nonviolent conflict has increasingly captured both popular and academic attention. One established feature of nonviolent conflict is intriguing; it often arises in highly repressive circumstances commonly thought to hinder collective action. It is this incongruence between theory and empirics I seek to illuminate. Thus, it is the aim of the thesis to contribute both to the repression literature and the nascent quantitative research on nonviolent campaign onset.

I propose an integrated theoretical framework based on two of the major theories of conflict onset, grievance theory and political opportunity structure approaches. Although these are often depicted as opposing, I contend that they are in fact complementary. I further suggest that conflict arises based on a grievance-opportunity function, in which it is the changes in either grievances or opportunities that induce contentious action, of which repression can be both. Thus, the research question is *'does repressive instability increase the likelihood of nonviolent conflict?'*

I put forth seven hypotheses, four of which are tested on annual data for 149 states between 1972 and 2006. The analysis finds empirical evidence that liberalization of civil liberties repression increases the likelihood of nonviolent conflict onset, though it does not have a significant effect upon violent conflict. Thus, the results conform to other quantitative studies of nonviolent conflict in emphasizing that the causes of violent and nonviolent conflicts are fundamentally different. Furthermore, the findings in this thesis suggest that the present inconclusive efforts to establish a unitary effect of repression upon dissent may be aided by accounting for the many facets and possible effects of the concept of repression.

Acknowledgements

To write a master thesis is no trifle. Though it has been a challenge, it has also been a privilege. To be allowed free reign and time to work on something I find intensely interesting is an opportunity I have treasured. I have been fortunate, both to have been allowed to do this, as well as in guides and friends along the way. I would like to express my sincere gratitude to everyone who has contributed advice, solace, commiseration, and elements of fun throughout the year.

For my advisor, Håvard Strand. First and foremost, thank you for your invaluable guidance, and statistical and academic support. Thank you for all the metaphors, analogies, and coffee breaks. Thank you for your insight when I felt lost. Thank you for keeping my spirits up, and for reprimanding me when they were low. Finally, thank you for the jokes.

For my fellow students on the 9th floor: You made the past two years wonderful, and your proffered help, comradery, and feedback during the crux of my thesis work were beacons of light when confusion overtook understanding. For Stian – thank you for always being there when I needed a hug or a beer. For Alexander – thank you for reminding me I am capable when everything seemed difficult.

For all the wonderful professors and PhD students who gave me their time and advice through this process, as well as the previous year – thank you.

For Oda, my roommate across town. Thank you for calling me, and allowing me to call you, at least twice a day. Thank you for being you.

For Øyvind, and Thomas. The conversations and the enduring patience are much appreciated. Thank you for being the best of friends.

Finally, for my parents and sisters. I could not have finished this thesis without you. Your support means the world to me, and I would not be the person that I am if not for you. I love you.

The work, including any errors or inaccuracies, is my own.

Tonje Jelstad Sandanger

October 30th

Table of Contents

List of Tables

List of Figures

List of Appendix Tables

1 Introduction

"(...) the most perilous moment for a bad government is one when it seeks to mend its ways."

Alexis de Tocqueville (1955: 177)

.

The self-immolation of Mohamed Bouazizi on the 17th of December 2010 initiated the nonviolent revolution in Tunisia, and sparked an unexpected wave of popular contention throughout the Middle East which challenged some of the most entrenched and repressive dictatorships in the region. Conflict scholarship and political analysts were wholly unprepared for this monumental event – there were no predictions of a sweep of dissent through the repressive Arab states (Weyland, 2012: 917). Rather, established theories of dissent postulate repression as a deterrent to popular uprisings (e.g.,Goldstone & Tilly, 2001; Gurr, 1968, 1970; Tarrow, 1998; Tilly, 1978).

The phenomenon, though unexpected, is not unique. Countless despots have fallen to the sheer power of their aggrieved subjects in common uprising. Sharp (1973) called the mechanism behind it the 'Theory of Power', and emphasized the dependence of the ruler on the quiescence and cooperation from the ruled. Withdrawal of public consent, Sharp theorized, will eliminate the pillars of power the dictator relies on and produce revolution without the necessity of armed force.

In the years following the so-called Arab Spring, nonviolent conflict has received greatly increased attention, both in scholarship and media. With their seminal book, *Why Civil Resistance Works: The Strategic Logic of Nonviolent Conflict*, Chenoweth and Stephan (2011) pioneered the global quantitative research on nonviolent conflict – a field thus far dominated by qualitative analyses. In their analysis of major maximalist campaigns, the authors emphasized the notion derived from observing the Arab Spring – nonviolent conflict is not only possible in highly repressive conditions, it is in fact most prevalent in autocracies (Chenoweth & Stephan, 2011: 66).

While the nascent quantitative research field on nonviolent conflict following Chenoweth and Stephan (2011) have re-affirmed this relationship between repressive circumstances and nonviolent conflict (Butcher & Svensson, 2014; Chenoweth & Lewis, 2013d; Chenoweth & Ulfelder, 2015; Cunningham, 2013; Sutton, Butcher, & Svensson, 2014), no study has sought to explain the discrepancy between theories claiming repression as a deterrent and the apparent predisposition of autocracies to nonviolent conflict. The overarching puzzle I seek to unravel in this thesis is thus why repressive regimes experience high levels of civil resistance, despite the postulation that repression deters conflict.

Specifically, two theories of contention argue that repressive regimes will be able to deter revolts. Grievance theory, as presented by Gurr (1968; 1970: 15), argues that politicized grievances produced by relative deprivation will produce civil conflict, but that governmental monopoly of coercive power – or highly repressive circumstances – will stifle dissent.

Political opportunity structure (POS) scholars, such as Tarrow (1998), view repression as a constraining feature of the political opportunity structure in which the dissidents operate – in other words, repression increases the costs of rebellion. Highly repressive states should therefore, according to Tarrow (1998: 19-20), be less likely to experience conflict than less repressive states, because the cost-benefit analysis of the rational dissidents is less favorable in such regimes. Yet the paradox remains – nonviolent conflict occurs, in absolute numbers, more in autocracies than in less challenging circumstances (Chenoweth & Stephan, 2011: 66).

Scholarship has suggested that nonviolent dissenters are more adept than their armed counterparts at affecting defections from the repressive apparatus of the regime (Nepstad, 2013), and that repressive measures against nonviolent campaigns are likely to produce a backlash-effect of increased mobilization rather than the intended quiescence (Francisco, 1995, 1996, 2004; Rasler, 1996; Sutton et al., 2014). However, none have sought to reconcile the established conflict theories and the empirical evidence presented above, and some have even suggested that as these theories are not able to provide satisfactory explanations for nonviolent conflict we should refocus our efforts towards actor-agency instead (Chenoweth & Ulfelder, 2015).

While I do not deny the relevance of actor-agency in nonviolent conflict research, it is my contention that the established conflict theories may still provide important insights into the occurrence of nonviolent dissent. Rather than dismissing the theoretical contributions of scholars such as Gurr and Tarrow, I argue that their theoretical propositions must be scrutinized more thoroughly. Though grievance theory and POS-approaches are often posed

as contradictory and irreconcilable contenders for one singular explanation of civil conflict, I propose a unification of the two theories. I do this because they are *not* in fact opposing, nor irreconcilable, though their emphases on conflict-inducing factors differ.

Grievance theory emphasizes discontent arising from a disadvantageous change in individuals' value calculus – i.e., what they feel entitled to relative to what they believe they are capable of obtaining – as the basis for all civil conflict (Gurr, 1970: 13). POS-approaches posit changes in the political opportunity structure as the instigating factor (Tarrow, 1998: 20). However, Tarrow (1998: 6) concedes that grievances are an underlying factor – no rational actor would pay the cost of conflict unless he had pressing reason to do so. Likewise, Gurr (1970: 15) recognizes the possible constraint of the political system, citing governmental monopoly of coercive power as an explanation for unaddressed, long-standing grievances. Notably, both scholars highlight change as relevant in explaining conflict onset, and neither fundamentally denies the other's explanatory factors. Rather, it seems to be an argument of precedence. I propose, based on these theories, that conflict is a function of both grievances and political opportunity, where changes in either may produce conflict, given the existence of the other.

While this may seem a modest modification to the theories – and in fact, it is intended to be – it has important implications, not only for research on internal conflict. The literature on repression has not been able, despite intense and admirable efforts, to pinpoint the effect of repression upon dissent. The only consistency in analyses of the Repression-Dissent Nexus is that of controversy – with disparate findings suggesting negative, positive, non-linear, and non-existent effects of repression upon dissent (Davenport, 2007a: 7-8; Earl, 2006: 134; 2011: 264). Davenport (2007a: 8) dubs this inconsistency in view of the relatively consistent findings that dissent increases repression 'the Punishment Puzzle'.

Based on a unified grievance-opportunity framework, I propose an explanation of change to the 'Punishment Puzzle'. Because repression is viewed both as a source of grievance and a constraining factor in the literature, and can credibly be construed as both, it is no wonder that inconsistent effects have been produced. Instead of focusing on the strength, timing, or consistency of repression, I propose that repression is both a source of grievance and a political constraint, and that it is changes in the repressive levels of a state that produce conflict. Thus my research question is,

> **Research question:** *Does repressive instability increase the likelihood of nonviolent conflict?*

Based on this research question and the grievance-opportunity function of conflict from the theoretical framework, I posit seven hypotheses. The first four relate to the liberalization of repression, and the final three to the autocratization of repressive policies. In the first four hypotheses, the proposed relationship between liberalization and conflict onset is positive – when liberalization occurs, conflict onset is more likely. This is derived from the grievance-opportunity function, and presumes some initial level of repression. Furthermore, I posit that when civil liberties repression is reduced, the probability of nonviolent conflict onset is increased. This hypothesis is derived from a combination of features of nonviolent campaigns relative to violent campaigns, and the grievance-opportunity function.

For the final three hypotheses, autocratization is posited to have opposite effects given the outset – in repressive states, increased repression will close the opportunity structure and hamper dissidence, but in liberal states, increased repression will be a source of grievance and increase the likelihood of conflict onset.

As can often be the case with theory-driven research, I am not able to test all seven hypotheses. Due to lack of available and suitable data, the number of hypotheses I am able to test is restricted to four of the seven. Therefore, I test these on a dataset of major maximalist campaigns, containing state-year data for 149 non-free independent states between 1972 and 2006. I find support for the hypothesis that liberalization increases the likelihood of nonviolent conflict, while the other three hypotheses do not receive sufficient empirical support to reject their null hypotheses. Thus, the conclusion is that repressive instability, in the form of liberalization, does increase the likelihood of nonviolent conflict in states that are not fully liberal – or non-free – between 1972 and 2006.

With this thesis, I aim to contribute both to the repression literature, and to the nascent quantitative quest to understand the origins of nonviolent conflict (e.g., Butcher & Svensson, 2014; Chenoweth & Lewis, 2013d; Chenoweth & Ulfelder, 2015; Cunningham, 2013). There are several reasons why studying nonviolent campaigns is relevant and important. First, as well as being prevalent in autocracies, they have been found to be more successful than their armed counterparts in overturning established dictatorships, and establishing subsequent democracy and sustained peace (Celestino & Gleditsch, 2013; Chenoweth & Stephan, 2011).

Additionally, in recent years, the importance of understanding the origins and causes of nonviolent campaigns has increased due to the phenomenon's increasing prevalence in the international system, and that they have been found to be the most common cause of dictator

exit (Kendall-Taylor & Frantz, 2014). Thus, studying nonviolent conflict is justified by its potential to affect lasting change, as well as its prominence in the system, and its potency for success relative to violent conflict (Celestino & Gleditsch, 2013; Chenoweth & Lewis, 2013d; Chenoweth & Stephan, 2011; Kendall-Taylor & Frantz, 2014).

Likewise, I seek to propose yet another solution to 'the Punishment Puzzle', as well as amending the lack of quantitative research on liberalization of repression, as lamented by Christian Davenport (2007a: 12). As shown earlier, the hypothesis of a relationship between repression liberalization and conflict is not only no novelty, it is 159 years old – and to my knowledge thus far untested quantitatively. Finally, combining the repression research and nonviolent conflict research is not only desirable, but also quite necessary, as theory has linked the two for decades.

1.1 Thesis Disposition

Chapter 2 – Literature Review– sketches out the literature on the subject of nonviolence, and repression to date, respectively, as well as the existing scholarship on the relationship between repression and nonviolence. Chapter 3 – Definitions and Concepts – give detailed definitions of the central concepts applied in this thesis, before Chapter 4 – Theoretical Framework – outlines the two theories combined for the theoretical background for the present analysis. Chapter 5 – Research Design – describes the operationalizations of the variables, the dataset, and the statistical model of the thesis, as well as the methodological concerns related to it. Chapter 6 – Analysis – contains the multinomial regression analysis, as well as simulated quantities of interest, an evaluation of the hypotheses, robustness checks, and goodness of fit assessments of the models in the analysis. In Chapter 7 – Final Thoughts – I recapitulate the findings in Chapter 6, address the limitations of this analysis, and remark on possible future avenues of research opened up by this thesis.

2 Literature Review

This chapter will review central contributions to the nonviolent conflict literature and repression literature, respectively, before outlining the remaining gaps in the research field as it stands currently.

2.1 The Study of Nonviolent Dissent

The study of nonviolent conflict has, until recently, primarily been qualitative. There are, however, several important historical and descriptive works that have heavily influenced today's research, both through analytical tools and through descriptive efforts. This paragraph will outline a few.

With his seminal three-volume epos, Sharp (1973: 8) established the theoretical foundation for the study of nonviolent civil unrest (Chenoweth & Stephan, 2011:21; Schock, 2013). In his three volume opus, *The Politics of Nonviolent Action,* he first outlined the influential theory of power. He argued that all state leaders rule at the mercy of their citizens – they depend upon the cooperation or acquiescence of the ruled. If the citizenry withdraw their consent or cooperation, the ruler's power and ability to rule begins to lessen (Sharp, 1973: 8). The following two volumes describe the various methods of nonviolent action, as well as the process through which a successful nonviolent campaign is waged.

Discussing the ramifications of nonviolent insurrections, Zunes (1994) systematically investigates why nonviolent campaigns have increased in numbers throughout the Third World. He argues that certain characteristics of nonviolent resistance make it strategically favorable over armed struggle, given the context in which contention occurs. He notes the relative efficacy of nonviolence over violence in achieving the stated goals, and argues that this is explained by the fact that unlike its armed counterpart, nonviolent dissidence delegitimizes government repression; that unarmed movements allow for larger and broader participation; and that nonviolent resistance creates alternative institutions to those of the government which further undermine the status quo, and form the basis for a new order (Zunes, 1994: 411-418).

A Force More Powerful (Ackerman & DuVall, 2000) is an empirical description of how nonviolent tactics have been employed in conflicts between state and citizens throughout the past century. While it provides important insights and descriptions, as well as accessibility of the topic, it does not attempt to provide any analytic tools nor generalize across cases. The bibliography of nonviolent action provided by A. Carter, Clark, and Randle (2006) also adds to the descriptive works of nonviolent conflict throughout the 20th and 21st centuries.

Schock's (2005) *Unarmed Insurrections: People Power Movements in Nondemocracies* is one of the earliest cross-national comparative studies of political contention (Chenoweth & Stephan, 2011: 22; Schock, 2005: xviii-xix). In his analysis of six nonviolent campaigns in non-democracies, Schock merges the strategic trajectory literature on nonviolent campaigns with the structural focus of political process approaches, in order to advance both research fields and bridge the structure-agency divide (Chenoweth & Stephan, 2011; Schock, 2005: xviii-xix). Schock also highlights the importance of popular quiescence in maintaining dictatorships; without tacit or overt consent from its people, even the most powerful must eventually crumble (Schock, 2005: 37-38).

Additionally, several other qualitative works have built upon these foundations, to provide key insights to what distinguishes and drives nonviolent campaigns. The Freedom House report by Karatnycky and Ackerman (2005) established a link between civil resistance and democratic transitions, which has influenced the research field in later years. Nepstad (2013) suggests that security defections are somewhat dependent on the strategy choice of the dissidents, with nonviolent actors being more adept at eliciting them than their violent counterparts. Others have suggested that electoral fraud makes nondemocratic regimes more vulnerable to nonviolent action (Beissinger, 2013:261), and that election years provide politicized focal points in which the populace may be increasingly inclined to rise up against the regime (Tucker, 2007). Furthermore, participation in nonviolent activism has been found to increase awareness of the governmental oppressive behavior being resisted, suggesting a reinforcing effect of activism (Davenport & Trivedi, 2013), and nonviolent tactics has been shown to be the most prevalent way of ousting incumbent dictatorships (Kendall-Taylor & Frantz, 2014: 40).

While these works all provide invaluable insights to and theoretical foundations of the nature of nonviolent conflict, they provide no generalizable explanations for the origins and outcomes of nonviolent campaigns. The following paragraphs will outline the quantitative efforts made in recent years to amend this fact.

2.2 The Quantitative Study of Nonviolent Campaigns

Within social sciences research, quantitative analyses of civil dissent against government have been dominated by a focus on civil war. This neglect of comparative quantitative work on nonviolence has been explained by the death tolls of civil war, as well as an errant assumption that nonviolence occurs when dissident resources do not permit civil war (Chenoweth & Stephan, 2011: 7).

With Chenoweth and Stephan's (2011) seminal work, a new era of quantitative research of nonviolent conflict ensued. The nonviolent campaign dataset they created, NAVCO 1.0, provided the opportunity for empirical testing of several theoretical claims, and the authors themselves disproved the oft-stated 'truth' that violence equals efficacy. Their analysis proved that the success rate of nonviolent campaigns is markedly higher than that of violent campaigns (Chenoweth & Stephan, 2011: 7).

In the aftermath of this study, nonviolent campaigns have received a much-needed increase in attention in conflict studies. With the release of NAVCO 2.0, in which the unit of analysis is campaign-year, analyses comparable to those previously done on civil war are possible – though this nascent area of study is still fairly limited (Chenoweth & Cunningham, 2013: 274; Chenoweth & Lewis, 2013d: 421).

The first focal point of the quantitative literature on nonviolent conflict was on the outcome of nonviolent campaigns relative to violent campaigns. Chenoweth and Stephan (2011: 60-61) established that nonviolent resistance is not only more effective than violent resistance in effecting regime transitions, but also more likely to produce stable and viable democracies, as was suggested by Karatnycky and Ackerman (2005). Celestino and Gleditsch (2013) subsequent study confirmed these results, finding that nonviolent campaigns are more likely to result in regime transition, and that this transition is more likely to lead to democracy, than in the cases of violent campaigns or no campaigns.

Svensson and Lindgren (2011) argue that the success of a violent campaign is dependent on what aspect of the state's legitimacy it is challenging. If the campaign is against the vertical legitimacy of the state – i.e. the political and institutional apparatus and the regime's right to govern – it is likely to be more successful than a campaign seeking to challenge the horizontal state legitimacy – i.e. the perception of the state as representative of the larger community (Svensson & Lindgren, 2011: 98). Challenging the horizontal

legitimacy of the state is likely to sow discord within and between groups in the community, and if the community is divided, withdrawing consent will be difficult if possible at all (Svensson & Lindgren, 2011: 98).

2.2.1 The Causes of Nonviolent Conflict

Thus far we know far less about the origins of nonviolent conflict than we do about its outcome, especially when we consider the attention afforded civil war onset (e.g.,Collier & Hoeffler, 2004; Collier, Hoeffler, & Rohner, 2009; Fearon, Kasara, & Laitin, 2007; Fearon & Laitin, 2003; Hegre, 2014; Hegre, Ellingsen, Gates, & Gleditsch, 2001; Hegre & Sambanis, 2006). However expansion of the NAVCO dataset has spurred interest in the questions of why and when nonviolent campaigns emerge (Butcher & Svensson, 2014; Chenoweth & Cunningham, 2013; Chenoweth & Lewis, 2013d: 416; Chenoweth & Ulfelder, 2015; Cunningham, 2013; Sutton et al., 2014).

In a preliminary analysis of the NAVCO 2.0 dataset Chenoweth and Lewis (2013d) found that, using the model specified by Fearon and Laitin (2003), there is a clear divergence in the causes of violent and nonviolent campaigns. The only variable predicting conflict of both types was population – in other words, countries with a large population is more prone to conflict of either character than those with small populations. Otherwise, the predictors from the original model were either insignificant or reversed for nonviolent conflict (Chenoweth & Lewis, 2013d: 420). The results suggest that the existing statistical models of conflict onset between government and citizenry – i.e. models of causes of civil war – cannot necessarily be applied to nonviolent conflict because they are essentially different.

In her analysis of self-determination disputes, Cunningham (2013), fuses the hitherto separate fields of research of violent and nonviolent resistance in an effort to discover the determinants of civil war and nonviolent campaigns relative to conventional politics, respectively. Her results confirm the insights from Chenoweth and Lewis (2013d); nonviolent resistance and civil war do have divergent determinants. While larger self-determination groups operating in states at lower levels of economic development that have kin in adjoining states and are internally fragmented are more likely to engage in civil war relative to conventional politics, smaller groups operating in non-democracies that are less geographically concentrated are more likely to employ nonviolent resistance (Cunningham, 2013:299-301). However, she also finds that both nonviolent resistance and civil war are more likely relative to conventional politics if the self-determination group is excluded from

political power, face economic discrimination and makes independence demands (Cunningham, 2013: 300).

Butcher and Svensson (2014) draw upon resource mobilization theory, and argue that the determinants of violent and nonviolent campaigns diverge because the tactics have different resource mobilization demands and thus draw upon different social networks for mobilization. Nonviolent campaigns require mass mobilization as well as leverage over the regime. Thus, the authors argue that extensive social networks with economic interdependence with the regime – labor organizations – increase the feasibility of nonviolent conflict especially. Their argument is empirically supported; while a high proportion of manufacturing goods to GDP increases the likelihood of nonviolent conflict, the relationship between the labor organization-proxy and violent conflict is both negative and insignificant (Butcher & Svensson, 2014: 15). Butcher and Svensson thus conclude that industrialization creates structural conditions that favor nonviolent conflict – corroborating theories of modernization and conflict hitherto unsupported by empirics (2014: 21-22).

In the most recent contribution within the field, Chenoweth and Ulfelder (2015) seek to discover whether structural conditions can in fact predict the onset of major maximalist nonviolent campaigns. They specify models based on the four most prevalent theories within civil unrest scholarship – grievance theory, resource mobilization theory, modernization theory, and political opportunity approaches – and assess their relative explanatory power by comparing their predictive ability (Chenoweth & Ulfelder, 2015: 4). Their findings suggest that of the four theories, the model specified based on the political opportunity approaches performs the best – followed by grievance theory and resource mobilization. However, according to Chenoweth and Ulfelder, a culled model of the strongest variables is still not performing well enough to conclude that structural conditions trump agency-based approaches in explaining the occurrence of nonviolent campaigns (Chenoweth & Ulfelder, 2015: 22).

Of these studies, four are global and inclusive. Cunningham (2013) confines her analysis to self-determination disputes, which limits the degree to which her results could be generalized across all nonviolent campaigns. The remaining four analyses (Butcher & Svensson, 2014; Chenoweth & Lewis, 2013d; Chenoweth & Stephan, 2011; Chenoweth &

Ulfelder, 2015) utilize global data without restrictions to grievances or actors[1], and it is in this company this thesis seeks to expand the literature.

Although Chenoweth and Ulfelder apply both grievance-based and political opportunity theories to create their models, the models do not perform as well as one might expect given their standing within civil unrest literature (Chenoweth & Ulfelder, 2015: 22). The authors conclude that this is because agency is more relevant than structure, though they concede that there is a possibility that the models are misspecified (Chenoweth & Ulfelder, 2015: 21). It is my contention that the models aren't necessarily misspecified nor are structural conditions irrelevant. However, I do propose an alternative theoretical approach, in which their two best performing theories – grievance and political opportunity approaches – are combined (see 4.3 A Grievance-Opportunity Approach).

Most analyses of nonviolent campaigns include a measure of repression – either as an explanatory variable or as a control for spurious effects. Repression research and the nascent quantitative nonviolent conflict scholarship are intrinsically linked – though both could benefit from a greater interaction of the two fields. This thesis will attempt to use quantitative analysis to further illuminate the relationship between governmental repression and nonviolent conflict.

2.3 Repression and Dissent

The connection between repression and dissent is essential within repression scholarship (for reviews, see Davenport, 2007a; Earl, 2011). The scholarship on repression and dissent can roughly be divided into two strands; studies on the origins of government repression, and analyses seeking to explain the relationship between repression and dissent. This section will outline the major findings within both strands.

2.3.1 When Governments Repress

The effect of dissent upon repression has consistently been established as positive, both formally and empirically (Carey, 2006; Davenport, 2007a; Ginkel & Smith, 1999; Shadmehr, 2014) – regimes tend to respond to civil unrest with repression. State repression is commonly

[1] Note that these three studies utilize either the NAVCO 1.0, NAVCO 2.0, or Major Episodes of Contention (MEC) datasets, and therefore the coding rules for these datasets do act as a certain limitation with regards to what campaigns, as well as which grievances or stated goals (i.e. 'maximalist'), are included in the analyses.

understood as the result of a cost-benefit analysis, in which the costs of repression is weighed against the benefits of the continuation of the status quo (Nordås & Davenport, 2013: 928), and when faced with popular contention, governments react with repressive measures. This phenomenon has been dubbed the 'Law of Coercive Responsiveness' (Davenport, 2007a: 7).

Further scholarship seeking to answer the question of why governments repress their citizens has thus focused on establishing the contextual factors that increase the likelihood of government repression. This section outlines the trends in this research so far.

In an early quantitative study, Henderson (1991: 132) found that the extent of societal inequality, democracy, and the economic growth rate all provide explanatory power with regards to the use of repression by the government. Subsequently, the connection between polity and repressive government policies has received copious amounts of attention from scholars.

Stable institutional democracy has consistently been associated with low levels of repression (Carey, 2006, 2010; Davenport, 1995, 2004; Davenport & Armstrong, 2004; Fein, 1995; Henderson, 1991; Mesquita, Downs, Smith, & Cherif, 2005; Poe & Tate, 1994; Regan & Henderson, 2002; Zanger, 2000). The difference in levels of repressiveness between democracies and autocracies has been explained by the divergent threat perceptions of the two regime types; democracies are both less likely to experience anti-government threats and less likely to perceive dissent as threatening to the regime's survival (Davenport, 1995: 703).

Further analyses have established that both complete democracies and autocracies are less repressive than mixed regimes – or anocracies. This phenomenon was dubbed the 'Murder in the Middle Hypothesis' by Fein (1995: 184). Regan and Henderson (2002) conclude that there is an inverted U-relationship between regime type and political repression, and attribute this to the level of threats the regime is facing. In complete autocracies, fear of retribution discourages threats to the regime, while in democracies institutional channels relieve discontent without threatening the state as such. In semi-democracies, demands are great yet no such channels exist, thus repression is greater (Regan & Henderson, 2002: 133).

The relationship between threats and governmental repressive behavior has been further explored, and Carey (2010: 182-183) concludes that while fully institutionalized democracies have a lower risk of repression onset than other regimes, democracies under severe threat are not immune to applying repressive behavior. Davenport (2007b: 499-500) finds significant variations within autocracies – single-party regimes are less likely to engage

in repression, while military regimes are less restrictive of civil liberties, relative to other autocratic regimes.

Regime transitions have also been found to increase repression (Davenport, 2004; Davenport & Armstrong, 2004; Zanger, 2000). Both democratization and autocratization have been associated with higher levels of repression, though Davenport and Armstrong (2004: 551) suggest the existence of a threshold of democracy above which democratic institutions reduce repressive behavior. This threshold of democratic pacification has subsequently found additional empirical support (Mesquita et al., 2005).

Scholarship has also been focused on what qualities in dictators and heads of state affect repressive behavior (Frantz & Kendall-Taylor, 2014; Ritter, 2014; Young, 2009). Positional security of state leaders has been found to reduce the costs of implementing favored policies and increase bargaining power in relation to dissidents, and therefore reduce repression violating personal integrity (Young, 2009: 296). Similarly, increases in executive job security have been found to decrease the likelihood that repression will occur in the first place, but increase the severity of observed violations (Ritter, 2014:158). Dictatorial reliance on cooptation through the use of legislature and political parties increases incentives to use political terror, while decreasing the need for empowerment rights restrictions, like censorship. Cooptation allows the dictator to draw the opposition out, which makes it easier to identify, gauge and monitor, but increases the risk of rivals will use their position to usurp the dictator, which generates incentives to increase physical integrity violations (Frantz & Kendall-Taylor, 2014: 9-11)

Other scholars have focused on environmental aspects in their analyses of repression. Nordås and Davenport (2013: 933) argue that because 'youth bulges' make states more susceptible to dissent and especially political violence, regimes are forewarned about potential civil unrest. In an effort to deter dissent in such circumstances, the government increases repression. The authors find that this relationship is supported empirically, even when controlled for actual protest behavior; governments in states experiencing youth bulges are more repressive than others. Danneman and Ritter (2014: 268) argue that conflict in neighboring countries will give autocrats incentives to increase domestic repression – not in an effort to emulate their neighboring state leaders but rather to avoid their fate. Their analysis provides empirical evidence that states with neighbors engaged in civil war repress more. Both these studies find support that dictators apply preemptive repression when they face threats of future dissent.

2.3.2 The Effect of Government Repression on Dissent

In contrast to the relative agreement on the effect of dissent upon repression, the only consistency in research on the effect of repression upon dissent is that it is controversial (Davenport, 2007a: 7; Earl, 2006: 134; 2011: 267). The question of the effect of repressive policies upon dissent is at the center of repression research, and yet both theoretical approaches and empirical analyses have yielded widely diverging answers.

Scholars who follow the political opportunity structure (POS) approach argue that coercive government policy reduces dissent and movement mobilization by increasing the cost of participation and collective action (DeNardo, 1985; McAdam & Tarrow, 2000; Muller & Weede, 1990; Tarrow, 1998; Tilly, 1978). When costs are imposed – or opportunity structures closed – individuals and groups are less willing to participate in collective action, and dissent stifled (Koopmans, 1997; Opp & Roehl, 1990; Tarrow, 1998; Tilly, 1978).

Grievance-based theories and analyses propose an opposite effect; repressive measures by government officials radicalize and aggrieve the population, and thereby increase mobilization and civil strife (Gurr, 1970; Hirsch, 1990; Opp & Roehl, 1990). In this strand of research, repression is seen as a motivational factor; when the regime applies physical sanctions and restriction of liberty, the population is outraged, and therefore increases their commitment to the cause. The proponents of grievance-based analyses expect increased repression to be met by increased dissent.

Yet other theorists and analysts argue that the connection between repression and dissent is more complex than the monotonic positive or negative relationships proposed above. Some argue that the relationship is U-curved (Lichbach & Gurr, 1981; Shadmehr, 2014), others suggest that it is an inverted U-shape (DeNardo, 1985; Francisco, 1996; Hibbs, 1973). Some scholars suggest that timing is important: in the short-run, repression deters dissent, while in the long-run, repression increases dissent (Rasler, 1996: 148), and a reciprocal relationship has also been proposed (Carey, 2006).

Some scholars claim that the effect of repression upon dissent is best approached through interaction models (Cunningham & Beaulieu, 2010; Francisco, 1995; Lichbach, 1987; Moore, 1998, 2000). Dissidents adapt and alter their strategy of either violent or nonviolent protest, depending on the response to either from the state – also known as the Substitution hypothesis (Lichbach, 1987:285; Moore, 1998: 870). Dissidents apply the strategy met with the most accommodative responses from the government, and therefore repressive inconsistency by the government will lead to an escalation in dissident activity, as

the dissidents will achieve less policy reform when they substitute to their less effective tactic, and therefore they increase their efforts (Lichbach, 1987: 286). In other words, consistent repressive or accommodative policies reduce dissent, while inconsistency increases dissent (Lichbach, 1987:287). Empirical analysis has found that inconsistent state behavior encourages more violent dissent (Cunningham & Beaulieu, 2010: 194).

Several analyses have found empirical support for Sharp's theoretical proposition of political jiu-jitsu – that governmental repression of dissent is followed by a backlash of mobilization rather than deterring the opposition (Francisco, 1995, 2004; Rasler, 1996; Sharp, 1973: 109-110; Sutton et al., 2014). Dissidents respond to coercion both by increasing their efforts and by adapting their methods to avoid being targeted by repression (Francisco, 1995:277), and backlash mobilization occurs in the event that dissidents consider the repression important and are able to communicate the government response to other potential participants (Francisco, 2004: 118-121).

Others have emphasized that the pre-existing campaign infrastructure influences the effect of repression upon dissent (McLauchlin & Pearlman, 2012; Sutton et al., 2014). McLauchlin and Pearlman (2012: 60) argue that repression amplifies trends in cooperation or conflict existent in a movement before the onset of repression, while others argue that a preexisting infrastructure increases the likelihood of both increased domestic mobilization and security defections in the aftermath of repression, while international repercussions – and domestic mobilization – are affected by the existence of parallel media institutions (Sutton et al., 2014: 9-10).

Formal game theoretic models have provided several theoretic insights to the relationship between repressive governmental policy and dissent (Ginkel & Smith, 1999; Pierskalla, 2009; Shadmehr, 2014). Some claim that highly repressive conditions favor large, but few, protests because in the event that early dissidents find the status quo sufficiently unbearable, they demonstrate a greater resolve to the general public by mobilizing despite high costs of exposure, which increases the likelihood of mass mobilization (Ginkel & Smith, 1999:301-302).

Others argue that the perceived willingness and capacity of the state determine whether the dissidents will mobilize. Governments that are capable of repressing protest will be able to deter dissident protest in the first place. Thus, protest and the repression thereof stem from lack of information and skewed perceptions. In transitional or failed regimes there is greater uncertainty and less reliable information about the capabilities and resolve of the

actors, which increases the likelihood of protest (Pierskalla, 2009: 19). In periods of liberalization, which can lower the cost of protest through increased freedom of assembly and speech, a mismatch between costs of protest and the willingness of the regime to yield may occur, which would lead to the repression of protest (Pierskalla, 2009: 20).

Shadmehr (2014) argues that the mixed empirical support for the grievance-based and political opportunity theories within the study of repression is because these theories – rather than being contradictory – are complementary, and should be unified in a single framework. He argues that grievances are the instigator for protest, but that dissidents also consider the costs and benefits of protest before mobilizing (Shadmehr, 2014: 622). While increased grievances increase the motivation for protest, they also raise the costs of accommodation for the state and thereby make repression a more likely response. Dissidents expect the state to repress when grievances are high, and thus will not mobilize unless the grievances are sufficiently high that the benefits from altering the status quo surpass the cost of being repressed. In other words – dissidents will protest at either extreme level of grievances, but will refrain from protest at intermediate levels of grievances (Shadmehr, 2014: 622).

2.4 Remaining Gaps in the Literature

The quantitative field of research on nonviolent campaigns is widely uncharted territory, and thus far we know very little of its origins. The research done to date suggests that there are fundamental differences in the causes of violent and nonviolent conflict, and that we cannot with certainty apply the models of civil war on nonviolent conflict.

The connection between repression and dissent – including nonviolent dissent – is, as previously shown, one of some controversy. Currently no consensus exists on the effect of repression on dissent. However, we do know that maximalist nonviolent campaigns tend to arise in highly repressive circumstances, though seldom in full autocracies (Chenoweth & Stephan, 2011: 67). Several scholars have argued that liberalization of repressive regimes will lead to conflict (Gurr, 1968, 1970; Hegre et al., 2001; Pierskalla, 2009; Tarrow, 1998; Tocqueville, 1955), as grievances held by an oppressed population are allowed to surface in the less restrictive political climate or because the dissidents believe the liberalized regime to be weak (Gurr, 1970; Pierskalla, 2009; Tarrow, 1998). However, the analyses of this

phenomenon have been qualitative and historical in nature (Davenport, 2007a: 12), or predictions derived from formal models (Pierskalla, 2009).

Equally, predictions have been made on increased repressiveness and conflict. According to Gurr (1968: 1104), we should expect dissent if restrictions of political rights are imposed on a population, as this constitutes a grievance held by a majority of the population. Along the same lines, Tucker (2007) argues that electoral fraud will provide a rallying point for nonviolent dissent. In other words, there are theoretical arguments suggesting that both increased repression and liberalization should produce conflict between the state and dissidents – but no quantitative study has been done to support these widely held presumptions.

There are, however, studies that approximate these presumptions within the civil war research field. Hegre et al. (2001) established a relationship between regime transitions and civil war, in which both autocratization and democratization increased the probability of conflict. This supported the purported inverted U-shape relationship between regime type and conflict, suggesting that civil war is more likely in semi-democracies than in both autocracies and democracies (Hegre et al., 2001: 33-34). The theoretical argument behind this relationship is that because semi-democracies are partly open, partly repressive, this invites dissent through grievances created by repression, and opportunities to organize and rebel through openness. According to Hegre et al., it is this political incoherence that is linked to civil conflict, and thus drives the inverted U-relationship (Hegre et al., 2001: 33).

While there are notable differences between Hegre et al. (2001) and the original arguments of repressive instability leading to conflict, this study does at least suggest that there may be some merit to the hypotheses. However, the study is exclusively done on violent conflict. No analysis has been done on whether changes in repressiveness increase the likelihood of nonviolent campaigns, despite the prevalent link between repressive conditions and nonviolence. Furthermore, as nonviolent campaigns require mass mobilization, the grievances that source the revolt should presumably be inclusive to major parts of the population. Governmental repression is such a grievance – especially when it encompasses all of society. Electoral fraud, disappearances, or lack of civil liberties affect all citizens and should therefore provide a potential for wide-spread mobilization.

Thus, the research question for this thesis is ***does repressive instability increase the likelihood of nonviolent conflict?***

2.5 Summary

This chapter has highlighted the scholarship on nonviolent dissent, and the repression-dissent research to date. While extensive work has been done on the repression-dissent nexus, no conclusive effect of repression upon dissent has been established. The quantitative field of nonviolent conflict research is widely uncharted territory, and only recently have global data on nonviolent campaigns been available. This thesis seeks to expand this literature, as well as provide a contribution to the repression-dissent literature by establishing whether repressive instability increases the likelihood of nonviolent conflict.

The next chapter contains the theoretical definitions of the key concepts of this analysis.

3 Definitions and Concepts

Making precise definitions is important, because if the theoretical concept is not sufficiently clear, it is not obvious what we are in fact studying. Additionally, without a clearly defined concept, how can we hope to assess whether our operationalizations cover the entirety – and nothing more – of our theoretical concept? In short, a poorly specified concept will inevitably lead to poor measurement validity, which in turn pulls the entire study into question (Adcock & Collier, 2001: 531-532).

The research question – *does repressive instability increase the likelihood of conflict* - calls for a definition of two central concepts – **repression** and **dissent**. This chapter outlines the different theoretical definitions of central concepts applied in repression-dissent and nonviolent conflict scholarship, as well as a discussion about their applicability, before the definitions used in this thesis is specified.

3.1 Dissent, Violent and Nonviolent Campaigns

3.1.1 Defining Dissent, Social Movements and Campaigns

Unsurprisingly, there is a plethora of definitions of civil dissent. The theoretical precision varies, and some are more easily applicable in quantitative analysis. This section outlines a few variations, and then presents the definition used by Chenoweth and Lewis (2013a: 2; 2013d: 416), which is used in this thesis.

Some scholars have used typologies of dissent. Ted R. Gurr argues that political violence varies across three factors; scope, intensity, and duration of conflict. He proposes a three-category typology that includes *turmoil, conspiracy,* and *internal war* (Gurr, 1970: 11).

While *turmoil* – i.e. relatively spontaneous and unorganized political violence – does not correspond well to the definition applied in this thesis, *conspiracy* – highly organized political violence with limited participation –, and *internal war* – highly organized political violence with widespread popular participation – might both correspond to the concept of major maximalist campaigns. Importantly, Gurr emphasizes maximalist tactics in *internal war* – they are intended to overthrow the regime (Gurr, 1970: 11). However, this typology does

not include nonviolent campaigns, nor does it offer any threshold for categorization of conflicts.

Charles Tilly (1978: 7, 40) defines social movements as groups of people who share a common belief system and actively promote change based on their views. His definition is close to Paul Wilkinson's (1971: 27) more elaborate definition:

> "… a deliberate collective endeavor to promote change in any direction and by any means, not excluding violence, illegality, revolution or withdrawal into 'utopian' community… A social movement must evince a minimal degree of organization, though this may range from a loose, informal or partial level of organization to the highly institutionalized and bureaucratized movement and the corporate group… A social movement's commitment to change and the raison d'être of its organization are founded upon the conscious volition, normative commitment to the movement's aims or beliefs, and active participation on the part of the followers or members."

These definitions add significant important aspects to the theoretical concept of social dissent. First, the group must have common interests; second, they must act through nonconventional means to promote change; and third, some aspect of organization must be present.

Sidney Tarrow (1998: 4) view social movements as collective challenges – or in a collective action perspective. The actors involved make cost-benefit analyses to evaluate their own participation, as does the group of actors when choosing tactics. He reiterates the importance of common purposes and social solidarities, but includes a third important aspect: *sustained interaction* with opponents. Tarrow distinguishes between contentious politics, which are essentially sporadic and unorganized, and social movements, which draw on social networks – i.e. are organized – and can sustain conflictual interaction with powerful opponents over time (Tarrow, 1998: 10, 19).

This definition of social movements allows us to distinguish between contentious events and social movements that are sustained over time, as well as a distinction between concurrent contentious events and social movements. The social movement must be organized, consensual action for a common purpose over time.

The definition used by Erica Chenoweth and Orion A. Lewis (2013a: 2) in the Nonviolent and Violent Campaigns and Outcomes (NAVCO) Data Project v2.0 is more stringent than the previously presented suggestions: "We define a campaign[2] as a series of

[2] Note that Chenoweth and Lewis (2013a) uses the term 'campaign' rather than 'social movement'. (Chenoweth & Lewis, 2013d: 417)

observable, continuous, purposive mass tactics or events in pursuit of a political objective". In other words, a campaign's tactics must be documentable and overt, sustained over an unspecified amount of time – though distinct from one-off events or revolts, be organized and directed towards achieving a common goal (Chenoweth & Lewis, 2013a: 2; 2013d: 416).

3.1.2 Distinguishing Nonviolent and Violent Campaigns

According to Gene Sharp, nonviolent action is characterized by nonconventional political action, including noncooperation, protest, and intervention, in which the dissidents do not threaten or cause physical harm to their opponent (Sharp, 1973: 68).

Nonviolence does not imply inactive or passive – Sharp identified approximately 200 nonviolent tactics, all of which are either persuasive – i.e., attempting to sway their opponent through demonstrations, parades and speeches; non-cooperative – i.e., refusal to aid the government in upholding the status quo, such as boycotts –; or disruptive – i.e., increasing the government's cost of maintaining the status quo, such as strikes, sit-ins and the like (Sharp, 1973: 68-69). In other words, nonviolent tactics can be acts of omission, acts of commission, and combinations of both (Chenoweth & Stephan, 2011: 12).

To summarize, **nonviolent campaigns**[3] are sustained interactions between government and an organized opposition over some political contention, in which the dissidents purposively apply active nonconventional tactics that do not threaten or cause physical harm to their opponents to achieve their stated goal (Chenoweth & Lewis, 2013a: 418; 2013d-13; Chenoweth & Stephan, 2011: 3; Sharp, 1973: 68-69).

Violent campaigns differ from nonviolent campaigns in tactics. Both types of campaigns are the concerted efforts of civilians to change the *status quo* which the government attempts to maintain, over time. Violent campaigns, however, are waged by armed dissidents that can draw from a wide range of violent actions and tactics – e.g., bombings, shootings, physical sabotage and so on (Chenoweth & Stephan, 2011: 13). In other words, a violent campaign is characterized by the use of physical force – either through threats or through concrete physical action – to coerce the opponent by physical means (Chenoweth & Lewis, 2013a: 3).

[3] The term 'campaign' is used rather than 'conflict' to annotate the unilateral choice of strategy implied. While 'nonviolent conflict' could be construed as nonviolent on parts of both dissidents and government, this is frequently not the case. Therefore, campaign is used to underline that the tactics of nonviolence or violence is on the part of the dissidents.

Note that the campaign types outlined here are ideals – in practice dissidents may apply a combination of tactics, change their tactical approach over the course of the campaign, and nonviolent and violent campaigns may occur simultaneously, or predominately nonviolent campaigns may experience radical flanks of violence – or vice versa. However, we can usually distinguish the primary modus operandi of a campaign, and as such the ideal types can be recognized in a more confounded reality. Nevertheless, it is important to recognize that these ideals are a simplification of a complex reality (Chenoweth & Lewis, 2013a: 3; 2013d:418-419).

3.2 Repression – Coercion and Deterrence

Although the field of repression studies is not exactly barren, few scholars spend a sufficient amount of time clearly defining the concept of political repression. This section will present some of the definitions available before presenting the definition used in this thesis.

Charles Tilly describes repression as "…any action by another group which raises the contender's cost of collective action" (Tilly, 1978: 100). Similarly, Alan Wolfe (1973: 6) defines repression as "a process by which those in power try to keep themselves in power by consciously attempting to destroy or render harmless organizations and ideologies that threaten their power". Though both definitions are admirably concise and parsimonious, the concept of repression is infinitely more complex and convoluted (Earl, 2011: 263; Goldstein, 1978: xvi).

First, neither definition differentiates between government repression and private actor repression. Second, both descriptions remains agnostic with regards to chronology – does repression predate collective action, or is it merely reactive? Third, no restriction is put upon the types of acts that are considered repression. If all acts that raise the cost of collective action – or render harmless organizations and ideologies – are considered repressive acts, several governmental actions we consider to be 'normal' governmental policy could be included – such as policing, propagating the governmental politics, etcetera. Fourth, there are no distinctions between types of repression and their purposes – are all repressive acts the same?

Goldstein (1978: xvi) argues that the definition provided by Wolfe (1973: 6) is underspecified, specifically with regards to the powerholder as the government, and which acts are deemed to be repression, and presents his own:

> "Political repression consists of government action which grossly discriminates against persons or organizations viewed as presenting a fundamental challenge to existing power relationships or key policies, because of their perceived political beliefs."

This conceptualization specifies that the government is the perpetrator of repression, and that acts of repression constitute gross discrimination on the basis of perceived beliefs of individuals or organizations. Goldstein further emphasizes that government action which applies to all persons and that are done in the presence of a 'clear and present danger' does not constitute repression (1978: xx). Davenport (2007a: 2) includes threats of physical sanctions as repression, as well as specifying the territorial boundaries of repression:

> "… repression involves the actual or threatened use of physical sanctions against an individual or organization, within the territorial jurisdiction of the state, for the purpose of imposing a cost on the target as well as deterring specific activities and/or beliefs perceived to be challenging to government personnel, practices or institutions."

Additionally, Davenport (2007a: 2) also notes that though repression is a form of coercion, it is not concerned with all coercive behaviors – such as the punishment and deterrence of theft or murder. In other words, repression is coercive behavior extraneous to the 'conventional' coercive properties of government.

Furthermore, this definition does not specify that repression as a direct response to dissidents, nor that any law or norm must be violated – both of which have been common in the literature (e.g., *protest policing* in Earl 2003 and *human rights violations* in Poe & Tate 1994). This makes for a broader and more inclusive definition (Davenport, 2007a: 3).

This definition also includes the duality of repression – the purpose of imposing costs on the target in the event of certain behavior, and the purpose of deterring specific behavior and/or beliefs. Others have also suggested similar distinctions to the concept of repression.

Snyder (1976: 285-287) proposes a useful distinction between 'preemptive' and 'responsive' repression, wherein preemptive repression signifies repressive and oppressive policies by government that are designed to prevent dissent in the absence of dissident action, while responsive repression is a reaction to the event of dissent. Karen Rasler (1996: 138) defines repression as "… the actions taken to coercively mobilize the opposition." This is clearly within the bounds of what Snyder dubbed responsive repression.

Sutton et al. (2014: 4) classifies repression as governmental communicative action directed at three separate audiences – the dissidents, the inactive population, and the regime's

supporters. Their definition implies both reactive and proactive properties at the same time; repression is intended to convince dissidents that the cost of continued opposition is higher than that of compliance, and at the same time communicate to the inactive population that the cost of joining the dissidents is higher than compliance with the status quo. This is fairly similar to the one proposed by Davenport (2007a).

The definition applied by Sutton et al. (2014) includes both deterrence and coercion as mechanisms of repression. Correspondingly, Oliver (2008: 14) identifies three separate – though linked – mechanisms of repression: *deterrence*, *incapacitation*, and *surveillance*. Deterrent repression works through threat of punishment for a certain class of acts – and is successful when the threat persuades the populace to avoid that sort of actions. Incapacitation is preventive, and occurs when people are removed from the system before they have committed the undesirable acts. Coercive surveillance works by gaining information about the populace to identify possible protesters.

According to Oliver, only deterrence is target-specific – the threat is directed at certain actions, and the threat is executed when the action is performed. Incapacitation and surveillance are less precise, and in certain respects more random and society-wide – the identification of possible dissenters and the incapacitation of said would-be dissenters predate the crime, and some pretext is often posed (Oliver, 2008: 14).

This thesis will rely heavily on the definition proposed by (Davenport, 2007a), but with an important distinction. Because the primary goal of this analysis is to investigate the link between changes in repressive levels of a government and civil conflict, the concept of deterrent or preemptive repression is most interesting. However, because governments are thought to repress more in response to perceived threats – meaning that there may or may not be any real threat – we cannot necessarily isolate the two theoretical concepts. Nevertheless, it is still useful to distinguish between them for the purpose of highlighting the causal direction.

Thus, the definition of repression for this analysis is **the actual or threatened use of force and physical sanctions by government against an individual or organization, within the territorial jurisdiction of the state, to ensure quiescence, to coerce the target by imposing costs on specific undesired behavior, or to deter undesired behavior and/or beliefs perceived to be challenging to government personnel, practices or institutions** (Davenport, 2007a; Oliver, 2008; Snyder, 1976; Sutton et al., 2014; Tilly, 1978).

3.2.1 Willingness, Ability, and Behavior

Coercion and deterrence as governmental behaviors are not exclusive to intrastate conditions. In International Relations scholarship has produced frameworks for the efficiency of interstate coercive diplomacy directly relevant for repression research. In IR theory, deterrence is the proactive use of threats to influence the adversary to avoid certain undesired actions in the first place. Coercion is the reactive threats, and use, of limited force in the face of undesired actions. Both in both cases, the objective is to avoid the use of force, but rather elicit compliance based on the threat of force (Jakobsen, 2013: 241). Thomas C. Schelling argues that in order to successfully deter or coerce, the threat must be sufficient and credible – in other words, the adversary must believe that the cost of noncompliance is higher than that of compliance, and that the coercer is both willing and able to impose those costs should he not yield (Jakobsen, 2013: 245; Schelling, 2008 [1966]: 339).

This insight is relevant to the definition of repression because it highlights two essential elements of repressive behavior. First, the regime must be able to impose costs that are sufficient, and second, it must be willing to do so. The regime's attitude and policy towards limiting the liberties and infringing on the integrities of their citizens will directly influence the extent of repressive behavior. A government that is held accountable by its citizenry will, presumably, be less inclined to adopt repressive policies of either type – which, regardless of their capabilities, will limit their repressive behavior. Likewise, a government's lack of capabilities may restrict its repressive behavior, in spite of willingness to repress. The institutional and organizational power must be present in order to execute the extensive surveillance, and physical policing, required to implement repressive policies. Thus, the actual behavior of the regime is a function of the willingness and capabilities of repression it possesses.

3.2.2 Civil Liberties and Personal Integrity

The concept of political repression proposed here encompasses a wide variety of tactics, such as domestic surveillance, physical and verbal harassment, as well as political bans and prohibitions. The literature has divided these tactics into two types of repression; *civil liberties repression* and *personal integrity repression* (Davenport, 2007a: 2; 2007b: 487).

Civil liberties repression corresponds to what Davenport (2007a: 2) and Goldstein (1978: xviii-xxi) call state infringement of First Amendment-type rights, such as freedom of

assembly, expression, association, and beliefs (Davenport, 2007b: 487). In other words, civil liberties repression constitutes state activity that limits or restricts, for instance, the civilian population's political participation, expression and beliefs, as well as the freedom of press, the freedom of travel, and the freedom to boycott, peacefully picket, or strike (Davenport, 2007a: 2).

Personal integrity violations involve governmental activities that directly interfere with the physical integrity, survival and safety of the civilian population, including (but not limited to) torture, disappearances, extrajudicial execution, mass killings, *etcetera* (Davenport, 2007a: 2; 2007b: 487). In other words, personal integrity repression corresponds well to the idea of political terror; the basic right of physical security is being infringed upon by the government.

Figure 1 depicts the relationships between the different definitions of repression outlined in this chapter. Repressive actions are categorized by their *strategic aim*, *specificity with regards to target, timing and method*, and the *mechanism* at play. Note that the categories proposed by Goldstein (1978), Davenport (2007a, 2007b), and Oliver (2008) span across the categories defined by Snyder (1976). This highlights the communicative effect proposed by Sutton et al. (2014) – coercive and targeted repression has a deterrent effect as well. While we can make theoretical distinctions between coercive, target-specific, responsive repression and deterrent, diffused, preemptive repression, the successful application of the former will produce the latter.

Nevertheless, the distinction between preemptive and responsive repression is theoretically salient and significant. For the purposes of this analysis, we are most concerned with the governmental preemptive attempts to dissuade dissident behavior, while acknowledging that perceived threats may at times be real, but repressed efficiently and therefore not distinguishable in the widespread repressive state of the regime.

3.2.3 Repressive Change

For the purposes of this thesis, I will use the term *liberalization* to denote decreased repression, and *autocratization* to denote increased repression. I remark on this to avoid confusion with the democratization literature – I do not refer to changes in institutional polity, but to the increase or decrease in repression.

Table 1 Typologies of Repression

Strategy	**Deterrent**		**Coercive**	
Specificity	*Diffused*		*Target-specific*	
Timing and method (Snyder, 1976: 287)	Preemptive nonviolence	Preemptive violence	Responsive violence	Responsive nonviolence
Action (Davenport, 2007a: 2-3; Goldstein, 1978: xviii-xxi) *(Davenport, 2007b: 487)*	• Violations of First Amendment-type rights • Due process transgressions *Civil liberty restrictions*	• Personal integrity violations • Due process transgressions *Personal integrity violations*	• Personal integrity violations • Due process transgressions *Personal integrity violations*	• Violations of First Amendment-type rights • Due process transgressions *Civil liberty restriction*
Mechanism *(Oliver, 2008:14)*	*Incapacitation, surveillance*	*Incapacitation*	*Deterrence*	*Deterrence*

Note: Coercive strategies will contain an element of deterrence, if the cost associated with the action is sufficient. Repressive responses by government are communication to all audiences, not only the dissidents it reacts to – and thus both coercive and deterrent (Sutton et al., 2014:4).

3.3 Summary

This chapter has defined the most important concepts employed in this thesis. **Nonviolent campaigns** are sustained interactions between government and an organized opposition over some political contention, in which the dissidents purposively apply active nonconventional tactics that do not threaten or cause physical harm to their opponents to achieve their stated goal. **Violent campaigns** are sustained interactions between government and an armed organized opposition over some political contention, in which the dissidents purposively threaten or cause physical harm to their opponents to achieve their stated goal.

Repression is the actual or threatened use of force and physical sanctions by government agents against an individual or organization, within the territorial jurisdiction of the state, to ensure quiescence, to coerce the target by imposing costs on specific undesired

behavior, or to deter undesired behavior and/or beliefs perceived to be challenging to government personnel, practices or institutions. **Preemptive repression** signifies governmental transgressions against personal integrity or restrictions of civil liberties that occur in the absence of open or prevalent dissent, are diffused across the civilian population, and intended to deter dissidence.

The next chapter will outline the theoretical foundation for this analysis, and the proposed relationship between repression and nonviolent and violent campaigns outlined in the research question - *does repressive instability increase the likelihood of nonviolent conflict?*

4 Theoretical Framework

This chapter outlines the theoretical foundation of the analysis. Two conflict theories are presented; grievance theory and the political opportunity structure approach. Theoretical issues related to repression research and these theories are sketched out, before the integrated approach used in this thesis is described. Finally, the theoretical arguments for the hypotheses are presented before each hypothesis.

4.1 Internal Conflict, Grievance Theory, and Political Opportunity Structures

Internal conflict is conflict between the government and its population, in which opponents of the state use collective action to seek redress for their grievances through nonconventional means. The main focus of this thesis is maximalist violent and nonviolent campaigns. Both violent and nonviolent campaigns work through collective action, which means that the dissidents must have a common purpose and be aware of this fact, they must be able to mobilize others to their cause, and finally they must be able to act against their opponent – the regime.

Two existing theories of civil conflict propose to explain how dissent occurs within a polity; grievance theory argues that prevalent and intense grievances cause the deprived population to rise up against the regime (Gurr, 1968, 1970); political opportunity structure (POS) approaches argue that openings within the political structure of a polity create opportunities that the aggrieved dissidents exploit to mount their protest (McAdam & Tarrow, 2000; McAdam, Tarrow, & Tilly, 2004; Tarrow, 1998). In the following paragraphs, both theoretical approaches will be presented, before the integrated approach applied in this thesis is sketched out.

4.1.1 Grievance Theory

Grievance theory scholars argue that the origin of all conflict lie in the grievances held by the citizenry against the state (Davies, 1962; Gurr, 1968, 1970). Ted R. Gurr suggests that the grievances arise through the psychological mechanism of relative deprivation – i.e. the

discrepancy between the value an actor believes he should be able to attain, and the value he is in fact capable of attaining. Disequilibrium in this value calculus produces discontent and frustration, which – if it exceeds constraining social conditions – produces participation in social strife (Gurr, 1968: 1104; 1970: 13).

Societal conditions that increase value expectancies without increasing value capabilities can produce an intensification of the perception of relative deprivation – as can societal conditions which decrease the value capabilities without addressing value expectancies (Gurr, 1970: 13). Such changes in the value calculus cause disequilibrium, which intensify the perceived relative deprivation (Gurr, 1970: 46).

The discontent produced by relative deprivation is politically salient only if it is directed at the government, shared among a relatively large part of the population, and fairly intense. The potential for political conflict then, according to Gurr, lies in the intensity and extent of this shared discontent within the population of a society, for which the citizens blame their government (Gurr, 1970: 8). Thus, the general argument is that the greater the grievance, the greater the likelihood of conflict (Gurr, 1970: 9).

Importantly, relative deprivation differs from absolute deprivation in its comparison with an ideal – the importance is placed on a perception of deprivation relative to the expected value to which the individual believes he is entitled. Thus the mechanism may be in play even if an actor is not living in conditions of absolute deprivation – and equally, may not occur in actors despite their deprived state (Gurr, 1970: 24).

4.1.2 Political Opportunity Structure

The political opportunity literature (Collier & Hoeffler, 2004; McAdam, 1999; McAdam, McCarthy, & Zald, 1996; McAdam & Tarrow, 2000; McAdam et al., 2004; Meyer, 2004; Tarrow, 1998; Tilly, 1978) holds that grievances, though not insignificant, are a constant factor of society and therefore not able to explain the solution of the collective action problem of civil unrest. Rather, the political opportunities in the context in which movements emerge determine whether or not the grievances will result in conflict. In other words, because grievances often exist and persist without the eruption of open conflict, these cannot be the *explanans* of dissidence. According to these scholars, the precursory condition to political conflict is an opening in the political opportunity structure of the polity (Collier & Hoeffler, 2004: 563; Tarrow, 1998: 71).

Heavily influenced by collective action-scholarship (Olson, 1965) and rational choice research, this strand of the literature argues that dissidents are rational actors who, when aggrieved by government, gauges the contextual opportunities in order to choose the best strategy. If the political opportunities are not conducive mobilization is unlikely, and the collective action problem of civil unrest cannot be resolved. Conflict arises, not because grievances have changed, but because the structure of opportunities and constraints of the polity has changed (Tarrow, 1998: 19).

Political opportunities are changes in the polity that enable dissent such as institutional access, elite discord, new allies, or reduced state capacity (Tarrow, 1998: 20, 71). Conversely, political constraints are factors that discourage dissent by increasing costs, such as governmental repressive capacity and will, institutional control, as well as the capacity to present a unified front to the dissidents (Tarrow, 1998: 20). The removal of constraints can also be viewed as an opportunity.

In their analysis of the feasibility of civil war, Collier et al. (2009: 23-25) imply a dual-axis to the political opportunity structure – the feasibility of rebellion is determined both by the capabilities of the state, as well as its willingness to militarily deter dissidence. This corresponds well to Tarrow's description of repression as political constraints, and to the definition of repression in section 3.2.1. The description of repression as a constraining feature of the political system determined by both the willingness and capability of the state calls to mind the importance of credibility for successful coercive strategies, highlighted by IR theory. Likewise, the feasibility of rebellion – or the opportunity for dissidence – is determined by a function of the state's willingness and capability of repression.

4.2 The Problem of Repression

This thesis seeks to expand the literature on the effects of repression upon dissent. However, pinpointing the direction of the effect of repression upon conflict has proven difficult (Earl, 2011: 267) (see Chapter 2, paragraph 2.4.2). Several answers have been proposed, countless analyses have been done – and yet the repression-dissent nexus remains unexplained. An inspection of the two theories presented previously indicates why; repression is predicted to have at least two contradictory effects; as a grievance which incites conflict [grievance theory] and as a constraint of the political system which discourages collective action [POS].

Both civil liberty repression and personal integrity repression could credibly be viewed as a grievance for an individual. Gurr (1970: 25) defines three types of values especially relevant to a theory of political conflict; welfare values, interpersonal values, and power values.

Power values are related to an individual's ability to influence, and avoid unwanted interference from, others. Both types of repression affect power values. Civil liberty repression limits the freedom of speech, right of assembly, and suffrage – among other things – which corresponds well to Gurr's specification: "(…) the desire to participate in collective decision-making – to vote, to take part in political competition, to become a member of the political elite (…)". The second part of his definition of power values – desires for self-determination and security – corresponds well to the goods and conditions personal integrity repression targets through such tactics as terror, disappearances, and torture (Gurr, 1970: 26).

Thus, one simplified implication of governmental repression is a discrepancy between an ideal power value expectancy of participation and security and the reality of the value capabilities in the repressive regime in which one lives. The discontent is quite obviously politicized, as the repression is a feature of the polity – an aspect of government. Because repressive acts most often are performed by government officials, the regime is likely held responsible by the population. Furthermore, it is likely, in highly repressive regimes, that most of the population feels the effect of repression – either through coercion or deterrence. This makes the grievance widely dispersed throughout the population.

However, repression can also credibly be considered a constraining feature of the political structure – Tarrow specifically mentions repression as a typical political constraint (Tarrow, 1998: 20). Repression discourages collective action, which presumably is the rationale of the government employing the tactic. According to Tarrow, repression can work in two ways; either through violent suppression of dissidents – i.e. through infringement on their personal integrity, or through increasing the costs of mobilization and organization for the dissidents – i.e. through limiting the civil liberties afforded them (Tarrow, 1998: 83).

The difficulty in pinpointing the exact effect of repression within repression literature is unsurprising when the two theoretical approaches propose widely different and opposing effects of repression. Gurr (1970: 15) concedes that a governmental monopoly of coercive capability could limit the effects of grievances for an extended period of time, yet the relative deprivation felt as a result of the repression should produce conflict at some point.

Conversely, Tarrow views repression solely as constraints which deters or increases costs of collective action (Tarrow, 1998: 20).

4.3 A Grievance-Opportunity Approach

Both grievance theory and POS approaches have had conflicting evidence in empirical studies, and been the subject of harsh criticism (Alimi, 2009; Chenoweth & Ulfelder, 2015; Collier & Hoeffler, 2004; Collier et al., 2009; Earl, 2011; Fearon & Laitin, 2003; Meyer, 2004). In a review of the political opportunity structure literature, Meyer (2004: 131) concludes that the premises derived from the political process approach "(…) generally do not perform well". Grievance theory has fared no better – a plethora of analyses has discredited the hypothesis that increased grievances lead to more dissent and conflict (Collier & Hoeffler, 2004; Collier et al., 2009; Fearon & Laitin, 2003). The two approaches to conflict research are, however, still applied within current scholarship, perhaps due to their intuitive premises.

Although they are often presented as conflicting theories, they are not fundamentally opposing. Grievances and political opportunities may not be as distinct as is implied by viewing these two theories as disparate. In fact, it seems that the features of a political system may be both the source of grievance and a constraint or opportunity, such as repression or the economic conditions of a state. The conditions of a society would necessarily inform the grievances of its populace, but they may also limit the possibility for rebellion. Following Shadmehr (2014), this thesis also calls for an integration of grievance theory and POS approaches. According to Shadmehr (2014: 625), the resulting theoretical argument is that:

> "(…) grievances provide the incentive to perform collective action; however, in their 'calculus of protest', actors also account for the costs and likelihood of success, which are determined by factors such as available resources and political opportunity structure."

This is fairly close to the theoretical arguments presented by both Gurr and Tarrow. While Gurr argues that grievances are the basic instigator to conflict, he concedes that politicized discontent can be prevalent and persistent without producing open conflict if the regime is strong and monopolizes coercive control (Gurr, 1970: 15). In other words, even if grievances are intense and widespread, the political structure may prevent conflict. Likewise, Tarrow does not deny the importance of grievances – in fact he emphasizes the importance of common purposes in the pursuit of collective action. In his own words; "People do not risk

their skins or sacrifice their time to social movement activity unless they have a good reason to do so" (Tarrow, 1998: 6).

Furthermore, both approaches argue that it is changes in the status quo that increases the likelihood of conflict. As described, Tarrow (1998: 20) proposes changes in the political opportunity structure as the instigation to conflict. Gurr (1970: 46) argues that changes in the value calculus cause disequilibrium and thus intensification of discontent, which can result in conflict.

Because collective action requires both motive and opportunity, and all internal conflicts are cases of collective action, it is my argument that it is not a case of either grievance or opportunity structure as the root cause, but rather changes in either set of variables. Conflict is based on a function of the two, but the instigating factor is change in either.

The argument is fairly intuitive. The population must have both a motive for revolt, as well as the opportunity to do so. Grievance theory faces the problem of conflicts that do not arise in situations where population is clearly deprived, and should by all premises of the theory revolt. The POS approach struggles more with defining clearly its premises, as the concept of political opportunity has become something of a 'catch-all' in which almost everything is construed as elements of the political structure (Meyer, 2004: 128). An integrated approach, in which changes in both the value calculus of individuals, and changes in the political opportunity structure are considered should allow more specified explanations, as well as ameliorate these problems.

The next section will discuss how an integrated approach may help illuminate the repression-dissent nexus, and possibly explain why repression has had both negative and positive effects on conflict in previous research.

4.4 Repression Revisited – the Hypotheses

The problem statement of this thesis – *does repressive instability increase the likelihood of conflict* – calls for a theoretical foundation that allows for both the negative and positive effect of repression upon dissent. This thesis proposes that repression, rather intuitively, can both be a grievance and a constraining factor of the political opportunity structure. Thus, the effect of repression upon dissent is controversial precisely because of this fact – sometimes it is the grievance that spurs protest, and in other situations it is the constraining feature of

society which discourages collective action. By considering the changes in repression rather than the concept as a static, the different characters of repression may better be illuminated.

Several scholars have proposed the thesis that liberalization of repressive regimes will result in popular revolt. The argument is, in essence, one relating to POS; when a regime has oppressed its population over a long period of time, there have surely been grievances but no opportunity to address them. When the despot releases some of his hold over the population, he allows them the opportunity to rise up against them – which they surely will.

Alexis de Tocqueville famously described the situation thus,

> "Only consummate statecraft can enable a King to save his throne when after a long spell of oppressive rule he sets to improve the lot of his subjects. Patiently endured for so long as it seemed beyond redress, a grievance comes to appear intolerable once the possibility of removing it crosses men's minds." [4]
>
> (Tocqueville, 1955: 177)

Essentially, the argument is that the opening of the political opportunity structure through lessened repression of the general public – for instance through increased suffrage, rights of assembly, or freedom of speech – allows the aggrieved population an occasion to discuss their discontent more freely[5]. This allows both the discovery of allies as well as ability to mobilize in larger groups with less consequences and costs. In other words, the cost-benefit analysis of each individual for participation in protest is changed favorably towards dissent.

Thus, the central hypothesis of the thesis is:

> ***H_1**: Liberalization of repression opens the opportunity structure, which allows the aggrieved populace to address longstanding grievances, and thus increases the likelihood of conflict onset.*

[4] This quote is the origin of the title of this thesis, albeit in another translation. The inspirational translation, beginning "Only a great genius can save a prince who undertakes to relieve his subjects after a long oppression (…)" can be found in *Why Men Rebel* by Ted R. Gurr (1970), p. 117.

[5] Gurr (1970: 15, 117) also notes the same relationship – intense and widespread grievances may be checked by expansive governmental repression, but if the regime's control is weakened, conflict will erupt. If the aggrieved and oppressed populace is promised improved conditions only to find that they are not, in fact, improved, the grievance is intensified and results in revolt against the regime (Gurr, 1970: 118). He argues that weakening regimes will be more susceptible to revolt because they are forced to impose intermediate sanctions, which will expose them to both the new grievances caused by the changed behavior, as well as accumulated hostility of the more severe previous repressive acts (Gurr, 1970: 244). Gurr dubs the mechanism at play 'aspirational deprivation', wherein the populace's value capabilities remain stable, while the value expectations increase (Gurr, 1970: 46).

Note that this hypothesis assumes some degree of repression at the outset. However, theory suggests that if the regime is extremely repressive, liberalization may have a stronger effect than if it is only moderately repressive. Tarrow argues that it is in non-democracies that newly opened access is most likely to lead to conflict – the disastrous consequences of the *glasnost* and *perestroika* policies of the USSR President Mikhail Gorbachev a stark example (Tarrow, 1998: 74-78). While the shifts in the repressive policy of the government may seem minor, any opening in severely repressive systems may signal an opportunity to dissidents, and convince them that their opponent is weakening (Tarrow, 1998: 78).

Finally, in repressive regimes, the aggrieved citizenry is more likely to find unity against an oppressive regime. The repression that affects large parts of the population yokes separate groups together. In addition, the absence of channels of expression leads even moderate dissidents toward maximalist claims, according to Tarrow (1998: 85). Together, these effects of authoritarian repressiveness turn trickles of contention into floods (Tarrow, 1998: 83).

Thus, a second, expanded hypothesis is proposed:

> ***H_2:*** *When highly repressive states experience a liberalization of their repressive policies the opened opportunity structure allows for longstanding grievances to be addressed through dissent and they experience a higher likelihood of major maximalist campaign onset than do less repressive states experiencing liberalization.*

These theoretical arguments are agnostic as to what form of conflict the liberalizing regime will face. Theoretically, an opening can be utilized both for nonviolent and violent campaigns. However, certain features of nonviolent conflict may guide theory towards a more specific hypothesis.

First, maximalist nonviolent campaigns require consistent and sustained mass mobilization {Chenoweth, 2013 #109;Chenoweth, 2013 #6;Chenoweth, 2011 #4;Dahl, 2014 #180}. If the society in which the grievance is held is so constrictive that the dissenters have no ability to mobilize the masses, it is unlikely that maximalist nonviolent campaigns can occur. This is not to say that nonviolent campaigns do not occur within highly repressive polities – in fact they are, as noted, most prevalent in non-democracies. The difference between impromptu protests and general unrest and nonviolent campaigns is organization and directed effort towards a goal. If the regime in which the grievance is held is extremely oppressive to the extent that it is impossible to effectively organize, or if no institutions or

organized communities exist for the dissidents to co-opt, mounting a large nonviolent campaign would be very difficult, indeed. This is reflected by the low relative number of maximalist nonviolent campaigns in full autocracies (Chenoweth & Stephan, 2011: 67-68).

Violent campaigns do not require mass mobilization to this extent. Political violent campaigns, like nonviolent campaigns, require a common grievance, the means to protest violently, and the ability to mobilize – but relatively small groups can disrupt polities through violence, through such tactics as acts of terror, guerrilla warfare, and so on {Dahl, 2014 #180}.

While the repression of personal integrity certainly raises the cost for each individual to participate in protest through fear for his own security, it is likely that repression of civil liberties is more effective in restricting mobilization (Tarrow, 1998: 83). Because mobilization is especially important for nonviolent campaigns, it is credible that these types of campaigns will be more likely when the decline in repression concerns civil liberties. Thus, it is probable that nonviolent conflict onset is affected positively by a liberalization of civil liberties:

> ***H_3:*** *Liberalization of civil liberties increases the likelihood of nonviolent conflict onset relative to no conflict onset.*

Given the theoretical arguments that highly repressive regimes are more likely to experience conflict after liberalization, this hypothesis too should be expanded to include this condition:

> ***H_4:*** *Highly repressive regimes that liberalize civil liberties are more likely to experience nonviolent conflict onset than less repressive regimes experiencing liberalization of civil liberties.*

While declining repression leading to conflict is essentially a POS-derived argument, increasing repression leading to conflict is not. Following the POS approach, autocratization should lead to a lower likelihood of conflict, because the opportunity structure is constricted. This is therefore the fifth hypothesis:

> ***H_5:*** *Autocratization reduces the likelihood of conflict onset.*

However, this hypothesis may not be sufficiently specific to capture the POS argument. While repression is viewed as a constraining factor of the political opportunity structure, no linear argument is made. Tarrow does not mention at what level repression should reduce the likelihood of conflict, nor does he posit the effects of autocratization directly. Therefore, I include an amended hypothesis thought to capture the 'repression as constraint'-argument better:

> ***H_6:*** *Autocratization toward extreme repression decreases the likelihood of conflict onset.*

If the 'repression as constraint'-argument is taken at face value, increased repression should decrease the likelihood of conflict because the room for mobilization is restricted, even in a relatively open polity where the opportunity to express concerns is present. However, a grievance-based view would argue that increasing governmental repressive activity would increase the likelihood of conflict. The value calculus of the populace is changed when repressive policies are introduced into a relatively open polity. Gurr describes 'decremental disequilibrium' as a situation where the citizenry's value expectations remain relatively stable while value capabilities are perceived to decline (Gurr, 1970: 46).

In a situation where the state is fairly open and the citizens enjoy a high degree of civil liberties and personal integrity, a decrease in these rights and conditions could easily trigger such disequilibrium, and thereby increase the perceived relative deprivation of the collective (Gurr, 1970: 46). Importantly, the comparison is made to their past condition. As Gurr puts it, "Men ordinarily expect to keep what they have; they also generally have a set of expectations and demands about what they should have in the future, which is usually as much or more than what they have at present" (Gurr, 1970: 27). If they suddenly find their value capabilities in terms of power values reduced, it is probable that they would react with frustration.

Finally, as previously stated, deprivation is relevant to political conflict insofar as many people experience the same discontent (Gurr, 1970: 29). While certain groups may experience deprivation, a large conflict requires a large part of the population to perceive the same deprivation. It is likely that imposed repressive policies by a government, such as disappearances, state violence or limitation of civil liberties, should provoke large parts of a populace. When this is combined with a relatively open polity, the following hypothesis is proposed:

H_7: *When repression increases in liberal regimes, this produces grievances which through the relatively open opportunity structure can be addressed and thus result in a higher likelihood of conflict onset.*

Thus, the integrated grievance-opportunity framework has yielded seven hypotheses relating repression and dissent to be tested. The next chapter will outline the research design for this endeavor.

4.5 Summary

This chapter has presented two of the most influential theories of social conflict – grievance theory and political opportunity structure (POS) approaches, and proposed a combined theoretical framework based on the two. Seven hypotheses were derived from the resultant theory.

The next chapter presents the research design constructed to test the hypotheses quantitatively, including the dataset, operationalizations of the variables, statistical model, and methodological concerns related to the decisions made in the research design.

5 Research Design

This chapter describes the research design of the thesis. First, the dataset is presented, including the operationalization of the dependent variable, and potential issues and limitation related to the data on nonviolent conflict onset. Third, the statistical model is outlined, including the motivation for the choice of model, the independent variables, and the control variables included in the analysis. In this section, a discussion of issues relating to data unavailability is included, before the specifications of the independent variables are described. Finally, the key methodological concerns pertinent to this analysis will be discussed, before the descriptive statistics are presented.

5.1 Dependent Variables and Dataset

The dataset for this analysis is based on the dataset produced by Charles Butcher and Isak Svensson (2014)[6]. It comprises yearly data for all independent states between 1976 and 2006, and the unit of analysis is country-year. The dependent variable – campaign onset as coded by Butcher and Svensson (2014: 9) – is based on the Nonviolent and Violent Campaign and Outcomes (NAVCO) Dataset 2.0 (Chenoweth & Lewis, 2013b).

The research question of this thesis asks whether repressive instability increases the likelihood of conflict. The hypotheses of Chapter 4 specify conflict as major nonviolent or violent maximalist campaigns, and thus the dependent variable of this analysis is the onset of major nonviolent or violent campaigns with maximalist goals.

In Chapter 3, nonviolent campaigns were defined as sustained interactions between government and an organized opposition over some political contention, in which the dissidents purposively apply active nonconventional tactics that do not threaten or cause physical harm to their opponents to achieve their stated goal. Violent campaigns were described as sustained interactions between government and an armed organized opposition over some political contention, in which the dissidents purposively threaten or cause physical harm to their opponents to achieve their stated goal.

[6] The codebook and replication data for "Manufacturing Dissent: Modernization and the Onset of Major Nonviolent Resistance Campaigns" (2014) by Charles Butcher and Isak Svensson are available at jcr.sagepub.com/.

To operationalize these concepts, the data from the Nonviolent and Violent Campaign and Outcomes (NAVCO) Dataset 2.0 (Chenoweth & Lewis, 2013b) is utilized, because it is the single most comprehensive, and global, dataset of major maximalist nonviolent and violent campaigns presently. It is the second iteration of the NAVCO 1.0 dataset used by Chenoweth and Stephan (2011), and extends the research possibilities by disaggregating the unit of analysis from 'campaign' to 'campaign-year', which opens up for more reliable tests of causal processes (Chenoweth & Lewis, 2013d: 416). Additionally, 'ideal-types' of conflict (i.e. nonviolent and violent) have been included to aid the identification of causal mechanisms associated with campaign strategy (Chenoweth & Lewis, 2013d: 416).

NAVCO 2.0 contains yearly data of 150 violent and 100 nonviolent campaigns between 1945 and 2006. These campaigns constitute the full population[7] of presently known major maximalist campaigns (Chenoweth & Lewis, 2013d: 416). All included campaigns are major, mature campaigns with stated maximalist goals, and a coherent organization over time (Chenoweth & Lewis, 2013a: 2). The campaigns, as well as the dates for onset and end, are based on consensus data from multiple sources, reviewed by leading authorities within the field of nonviolent conflict and social movements[8]. Campaigns have been included along two rules of inclusion – first, they must have at least 1,000[9] participants; and second, they must have a stated maximalist goal of regime change, expelling foreign occupation, or self-determination (Chenoweth & Lewis, 2013d: 417).

First, in order to qualify as a campaign in the NAVCO 2.0 dataset, a contentious event with at least 1,000 participants must be followed within a year by another contentious event of 1,000 or more participants that claim the same goals. In addition, there must be evidence of organization and coordination between the events (Chenoweth & Lewis, 2013d: 417).

Second, only campaigns that purport 'maximalist' goals of regime change, secession, or the removal of foreign occupation at some point in their duration are included. In other words, reformist or limited calls for policy change that do not challenge the established

[7] Note that the campaign 'Venezuelan anti-coup (2002)' was included in NAVCO v1.1, but was mistakenly omitted from the NAVCO v2.0 (Chenoweth & Lewis, 2013c: 8).

[8] The data were gathered from an extensive review of the literature, including encyclopedias, case studies and qualitative works on nonviolence and social movements. The resultant data were subjected to a corroboration method of review by a dozen experts, who were asked to assess their categorization as violent/nonviolent, whether their outcomes were correctly identified, and whether any cases were omitted. If any new cases were suggested, they were subjected to the same process (Chenoweth & Lewis, 2013d: 419).

[9] Note that in NAVCO v1.1, the inclusion rule for major violent maximalist campaigns followed the Correlates of War Project's inclusion rule of 1,000 battle deaths. NAVCO 2.0 continues this focus, but have for comparison and compatibility purposes revised the start and end dates of these campaigns to the time period when the campaigns had 1,000 'participants' – in order to correspond better to the inclusion criteria for nonviolent campaigns (Chenoweth & Lewis, 2013c: 1).

regime are not included. However, some campaigns begin as reformist, but turn toward maximalist goals during the course of the campaign. Such cases are included, while those that remain reformist throughout are excluded (Chenoweth & Lewis, 2013d: 417)

In the NAVCO 2.0 dataset, campaign onset is coded as the date of the first event associated with the campaign that reached the 1,000 participant threshold, as is the coding of the onset variable in this thesis (Chenoweth & Lewis, 2013d: 419).

The dependent variable in this thesis is the onset of major maximalist campaigns, as coded by Butcher and Svensson (2014: 9). In their analysis of manufacturing and the onset of nonviolent and violent campaigns, they use the NAVCO 2.0 data to code a nominal onset variable, in which 0 denotes no major campaign onset, a value of 1 is assigned to state-years in which a major nonviolent campaign onset occurred, and a value of 2 for state-years in which a major violent campaign onset occurred.

5.1.1 Notes on NAVCO 2.0 and the Dependent Variable

There are certain issues with the NAVCO 2.0 data that should be addressed before moving on to the independent variables and statistical model.

First, the inclusion threshold of 1,000 participants for the campaigns is inherently arbitrary, as is any threshold applied to a qualitative concept. Chenoweth and Lewis (2013d: 417) admit to this, but maintain that as one is forced to begin somewhere, this is a logical starting point. First, because setting such a high cutoff point decreases the issues of underreporting, and second because 1,000 participants emulates the standard of 1,000 battle deaths set and utilized by the Correlates of War Project. The authors argue that by setting the threshold at 1,000 participants, the comparison to similar datasets of violent conflict is more readily made without facing the severe underreporting issues one would if the cutoff point was set lower, at for instance 25 participants[10].

While it is important to note that this is a feature of the data, it is not necessarily a grave cause for concern, as long as we are aware that we are analyzing major campaigns and that our inferences based on the data should reflect this acknowledgement. Thus, the findings of this thesis will be limited to major nonviolent and violent campaigns with maximalist goals, and cannot be used to generalize beyond that.

[10] To correspond to the Uppsala/PRIO threshold of 25 battle-related deaths in civil wars.

Second, and far more perturbing, is the question of comparability of nonviolent and violent campaigns. The previous iteration of the NAVCO dataset – v1.0 – had separate inclusion rules for violent and nonviolent campaigns, in which nonviolent campaigns must meet a participation threshold of 1,000 participants, while violent campaigns were included if 1,000 battle-related deaths occurred. Arguably, both thresholds result in the inclusion of large campaigns, but to compare 1,000 deaths to 1,000 participants is not necessarily as straightforward as it may seem. In the NAVCO 2.0 data, this has been amended – the onset and end dates of both violent and nonviolent campaigns have the same participatory inclusion threshold of 1,000 participants, though the focus on 1,000 battle-related deaths remain for the violent campaigns (Chenoweth & Lewis, 2013c: 1). Though the authors stress this point in their codebook, the participatory threshold for violent campaigns is still gleaned from an implied participation based on the battle-death threshold of 1,000 in the datasets used for data on violent campaigns (Chenoweth & Lewis, 2013a: 4). Essentially, the question remains: can we compare nonviolent campaigns with 1,000 or more participants to violent campaigns of 1,000 or more participants which generate 1,000 or more battle-related deaths a year?

This is a complex problem, because it concerns the very conceptualizations, tactics and efficacies of the two different campaigns. When we know that nonviolent campaigns rely upon mass mobilization in order to be efficient and successful while violent campaigns can cause severe disruption despite low participation, is it sensible to apply the same threshold of participation? Also, as battle-related deaths could imply something about the intensity of the violent conflict, should there be some kind of related indicator for nonviolent campaign's intensity? While these are important and interesting concerns, there is no conclusive answer. The two concepts, though intrinsically related, have very different qualifications, and any threshold imposed for participation would incur some problem for comparison. How many battle-related deaths compare equally to nonviolent participation is difficult to assess, and therefore, this thesis relies on the expertise of the leading authorities on nonviolent conflict who have corroborated on the dataset (Chenoweth & Lewis, 2013d: 419-420).

Third, the NAVCO 2.0 data allow for mixed tactics within the campaigns. Acknowledging that campaigns are seldom strictly nonviolent or violent, and that several campaigns experience radical flanks, transform throughout their duration, or are comprised of mixed tactics throughout the campaign is important (Chenoweth & Lewis, 2013a: 3). However, as this thesis is concerned with the onset of campaigns, as well as ideal types, the analysis focuses on what Chenoweth and Lewis dub the 'primary method' of the campaigns.

In other words, when campaigns rely primarily on nonviolent tactics, they are defined as nonviolent, and when they rely primarily on violent tactics, they are defined as violent.

While this is a simplification of a very complex universe of resistance methods, the types are not irrelevant, and a distinction is theoretically salient (Chenoweth & Lewis, 2013a: 3). Because we know that there are discerning characteristics between the two types, it is necessary to categorize each campaign as nonviolent or violent, even if it is a simplification (Chenoweth & Lewis, 2013d: 422). Again, this thesis relies on the expertise and corroboration that went into the consensus method of generation of the NAVCO 2.0 dataset (Chenoweth & Lewis, 2013a, 2013d).

Fourth, the second inclusion rule of maximalist goals eliminates all reformist campaigns from the dataset. According to Chenoweth and Lewis, this rule is in place to "ensure conflict conditions and to generate a conservative test of the efficacy of nonviolence" (Chenoweth & Lewis, 2013d: 417). Though the theoretical foundation of this thesis does not specify maximalist goals, it is implied that the conflict ensuing repressive change often should be directed at regime change. However, in other cases it might also be probable that the dissidents would demand policy change – perhaps especially when the repressive level increases from a low level. If this is the case, this analysis will not be able to ascertain this, which may influence the results. Nevertheless, no viable alternative exists at present, and I am forced to accept that the findings in this thesis are limited to mature, maximalist campaigns, pending data which include civil rights movements and reformist campaigns.

Finally, and perhaps most importantly, there is the pressing concern of underreporting bias. While this issue is not unique to nonviolent conflict data, it is feasibly more pronounced than for other types of contentious politics, such as violent campaigns. The data may be biased towards success, because it is large and mature campaigns that are most commonly reported while campaigns that are crushed in their infancy and thus fail are not included in the dataset (Chenoweth & Lewis, 2013a: 4).

More importantly for this study, however, is the possibility that highly repressive conditions, both with regards to press freedom to report and to response to dissent, might effectively suppress initiated campaigns that fail in the face of repression. Because we lack information about such unknown campaigns, we are forced to assume their nonexistence.

Given the research question and hypotheses of this thesis this is a frustrating problem. While we cannot know if there was a failed campaign, or if no campaign were ever initiated, in the highly repressive state, both cases would be of interest to the question at hand. In the

resultant analyses, the examples are treated the same – as non-occurrence, which may bias the results negatively. Although ameliorating actions have been taken to neutralize this underreporting bias, this is a constant feature of all conflict research which deserves attention, and must be considered when drawing inferences from quantitative models[11].

Several measures have been taken to counteract the effects of underreporting bias – among which is the comparison to violent campaigns, which suffer from a similar issue, rather than viewing nonviolent campaigns in isolation. Furthermore the high threshold for inclusion ensures that we concern ourselves with large and mature campaigns of both kinds, which are more likely to be reported by media even if they are faced with repressive measures. Additionally, as the data are based on a consensus sample, more faith can be stored in that all known campaigns in the given time period is included. Unknown, failed campaigns of either denomination are necessarily omitted from the dataset (Chenoweth & Lewis, 2013a: 5; 2013d: 420).

To summarize, these concerns are essentially relevant with regards to external validity. The findings of this thesis are thus limited to major campaigns with maximalist goals and a high level of participation over time (Chenoweth & Lewis, 2013d: 420).

5.1.2 The Dataset

The dataset used for this analysis is based on the dataset created by Butcher and Svensson (2014). It has been expanded to include a measure of repression, as well as additional control variables. The unit of analysis is state-year for all independent states between 1972 and 2006.

The dataset is constricted as compared to the original by (Butcher & Svensson, 2014), because there is a lack of data on repression before 1972. This reduces the amount of conflicts in the dataset, but with no alternative measures of repression corresponding to the theoretical concept available, the restriction seems necessary. There are 74 major, maximalist nonviolent campaign onsets and 73 major, maximalist violent campaign onsets in the resultant dataset, with a total of 3,929 units of analysis.

In the original NAVCO 2.0 dataset, there are 150 violent and 100 nonviolent campaigns. In other words, I have lost more violent campaigns than nonviolent campaigns. In addition, I have restricted the time period to 30 years, which is only half of the original

[11] Other than the measures described below, we are forced to accept the underreporting bias as is. Any assumption of failed conflict based on repressive levels would be counterfactual, and indeed risky, as repression has both constraining and aggrieving features (as described in the theory chapter). Sometimes what we don't know would be of interest, but as we don't know we cannot be sure. Such is the life of a social scientist.

timespan in NAVCO 2.0. These concerns are not amendable, pending increased data collection on repression, but should be noted as a limitation to the study's external validity. The findings presented in this thesis are thus a result of analysis of all independent states between 1972 and 2006, and the implications should be interpreted as such.

5.2 Statistical Model

The dependent variable is nominal and has three outcomes; (i) no major maximalist campaign onset; (ii) onset of major maximalist nonviolent campaign; (iii) onset of major maximalist violent campaign. These outcomes are mutually exclusive – campaign onset either occurs or it does not, and in the event of campaign onset it can only be assigned one primary method, either violent or nonviolent. I therefore employ multinomial logit analysis to model the likelihood of the three outcomes of the dependent variable, setting 'no major maximalist campaign onset' as the reference category.

There are several reasons why I use multinomial logit analysis. Chief among them, we cannot assume a linear relationship between the independent and dependent variables, as the dependent variable is conceptually nonlinear. My intention is to discover the effect of the independent variables on the probability of nonviolent or violent conflict onset, relative to no conflict onset. These outcomes are mutually exclusive, and the probability can only vary between 0 and 1. Ordinary Least Squares regression (OLS) assumes a linear relationship, and thus employing it in analyses with nominal dependent variables can lead to nonsensical predictions, in which the estimated y can take on values that lie beyond reasonable probabilities[12] (Long, 1997: 39).

The potentially nonsensical predicted y-values can be driven by the assumption in OLS that each additional unit of x_1 produces a constant effect on y – due to the assumed linear relationship. However, in this case of probability estimation, it is more realistic to assume a nonlinear relationship between x_1 and y as additional units should have a diminishing effect on the probability of $y = 1$ as the probability approaches 0 or 1 (Long, 1997: 39-40).

Estimating probabilities of an outcome given a set of independent variables could also be achieved using multinomial probit analysis. Because these statistical methods frequently produce similar estimates, the choice between them is usually a matter of convention (Stock

[12] Such as a negative predicted probability – i.e. a probability less than 0 – or a predicted probability greater than 1. While famous athletes may consider themselves to be at "110 % effort", operating with probabilities beyond 1 (or 100 %) is unreasonable with regards to conflict onset relative to no conflict.

& Watson, 2012: 435-436). I therefore follow Butcher and Svensson (2014: 9, 12) and others in using multinomial logit.

5.2.1 Concerns with Time-Series Cross-Section Data

Autocorrelation, or the correlation between the a time series variable and its lagged value is a grave concern with the use of time-series cross-section (TSCS) data (Stock & Watson, 2012: 405-406). Autocorrelated omitted variables further exacerbate the issue, producing autocorrelated regression errors, and thus heteroskedacity. Autocorrelation may be both spatial and temporal, and in logit analyses utilizing TSCS data, failing to account for either type of autocorrelation produces inefficient estimation and incorrect standard errors (Beck, 2001: 288).

Temporal autocorrelation violates the assumption of temporal independence in both probit and logit analyses, and the produced standard errors will be incorrect, understating variance. Additionally, the autocorrelated error terms can also affect the parameter estimates. Thus, ignoring the temporal dependence of TSCS data with a categorical dependent variable may produce inefficient and overly optimistic estimates (Beck, Katz, & Tucker, 1998: 1263). To correct for temporal dependence, I follow Butcher and Svensson (2014: 11) in including a cubic polynomial of time since last nonviolent campaign and a cubic polynomial of time since last violent campaign (Beck et al., 1998; D. B. Carter & Signorino, 2010)

Spatial autocorrelation may also bias the estimates and standard errors of a multinomial logit model, and therefore standard errors are clustered on states, to allow for autocorrelation within each state, but assuming no autocorrelation across states (Stock & Watson, 2012: 404-405). Clustered standard errors[13] are have been shown to perform well, and to be robust against false assumptions of heteroskedacity (Beck & Katz, 1995: 641).

Finally, regional dummy variables are included in robustness checks, to control for additional spatial autocorrelation.

5.2.2 Measuring Repression

Recalling the conceptualization of repression in Chapter 3 (see 3.2 Repression), as well as the theoretical suppositions of Chapter 4, there are two separate types of repression. While I am

[13] Though Beck and Katz (1995) discuss panel-corrected standard errors, other scholars have equated the two (King & Roberts, 2015: 163).

mostly interested in the constraining factors of civil liberties repression, personal integrity repression may have a similar effect and it would be preferable to include it in the analysis.

Recalling Chapter 3, section 3.2.2, the theoretical concept of civil liberties repression is concerned with governmental infringement on First Amendment-type rights. Civil liberties repression constitutes state behavior and/or policy that limits or restricts, for instance, civil freedoms of participation, expression, association, travel, or assembly (Davenport, 2007a: 2). For this thesis, I use the Freedom House Civil Liberties scale from the annual *Freedom in the World* report to operationalize this concept (FreedomHouse, 2015a, 2015b).

The coding rules of the civil liberties scale from Freedom House correspond closely to the theoretical concept presented in Chapter 3. Regimes that score highly on the seven-point civil liberties scale are very restrictive of such liberties as expression and association, are marked by frequent political arrests, and are often expansive in their areas of control. A value of 1 on the CL scale indicates that the population enjoys a wide range of civil liberties[14], free economic activity, and that the country has an established and generally fair legal system that ensures the rule of law. It is important to note, however, that a 1 on the civil liberties scale does not necessarily equate liberal democracy, but that the two are highly correlated (FreedomHouse, 2015a, 2015b). The scale is built on 15 indicators of freedom of expression and belief, associational and organizational rights, the rule of law, and personal autonomy and individual rights (FreedomHouse, 2015b)[15].

Operationalizing *Personal integrity repression* proves more of a challenge, if not impossible. Because the goal of this thesis is to establish an effect of changes in the repressive levels of a regime, a scale that considers both state behavior and state policy is necessary. No such scale exists for personal integrity violations. The most widely used scales measuring something close to my theoretical concept – the Political Terror Scale (PTS) data project (Wood & Gibney, 2010) and the Cingranelli and Richards (CIRI) data project (Cingranelli & Richards, 2010) both fall short of the requirements. Both scales are indexes based on several indicators, and utilizing the same empirical sources. In addition, both scales give scores based on governmental behavior within a single year, disregarding the governmental policies behind the behavior.

[14] Including freedoms of expression, assembly, association, education, and religion.
[15] A detailed description of the indicators, the question included in them, and the scoring practices can be found in Freedom House's online methodology (FreedomHouse, 2015b). For the purposes of this thesis the importance is to note that the indicators correspond closely to the content of the theoretical concept.

This last point is what renders both scales useless for the purposes of this analysis. Because the PTS measures actual violations occurring within country-years, the score would not necessarily reflect the repressiveness of the state, or their attitude towards personal integrity repression. As a consequence, regimes that are successful in their deterrent repression could receive a low score on the PTS – not because the regime is not willing or capable of imposing physical sanctions on its citizens, but because it does not have to. History has countless examples of less frequent acts of physical repression, and studies have established a U-curve of repression – democracies repress less than autocracies, but autocracies repress less than transitional regimes or anocracies (Carey, 2010; Davenport, 2007b; Davenport & Armstrong, 2004; Fein, 1995; Regan & Henderson, 2002). In other words, frequent application of personal integrity repression is perhaps most likely in regimes that are establishing their authority, and less so in established dictatorships, because there is a temporal dependency that the scale does not take into account. The previous actions of a regime will help deter future challenges, and therefore the necessity of repression of personal integrity (Wood & Gibney, 2010: 370).

The problem then, is that since the score does not in fact measure the extent to which the regime is willing and able to impose such sanctions on its populace, but rather to what extent it actually does, we cannot know whether a state with a score of 2 is a really successful repressive state, or a reasonably open state with a few blemishes on its track sheet. Also, the concept of repressive instability, or change, is less theoretically precise if we apply this scale. The result would presumably be several instances of change in level of repression on the scale that in fact were not change in the level of repression over all, and thus would not be an appropriate operationalization of our theoretical concept. In other words, the measurement validity would be compromised, which would draw the validity of the analysis into question (Adcock & Collier, 2001). Therefore, the concept of *personal integrity repression* has been excluded from this analysis pending more suitable data material, and concepts of repressive change are operationalized by the Civil Liberties scale from Freedom House exclusively (FreedomHouse, 2015a).

Utilizing the Civil Liberties scale is not without potential for criticism. First, as it is an aggregated scale measuring a diffuse and wide concept, some degree of subjectivity is necessarily involved. Freedom House is open about its normative approach, stating that "Freedom in the World operates from the assumption that freedom for all peoples is best achieved in liberal democratic societies" (FreedomHouse, 2015b). The report's methodology

is derived from the Universal Declaration of Human Rights, and an assumption that these are indeed global in their validity (FreedomHouse, 2015b). Second, the methodology is reviewed periodically, to keep up with evolving ideas about civil liberties (FreedomHouse, 2015b). While this is desirable from a theoretical view, it may reduce the comparability of the values across time.

The idealistic leanings are somewhat amended by an easily accessible methodology, as well as the consensus approach to scoring the cases. As the methodology is easily accessed by the public, scholars can review the indicators and questions used to score countries to evaluate the scale's applicability. The rigorous consensus approach, as well as the relatively high number of analysts, expert advisors, and scholars involved in the discussions and scoring ensures some certainty that the values assigned are not the product of individual bias or mistakes, which increases the reliability (FreedomHouse, 2015b). Nevertheless, this is – as it always is in social sciences – one noteworthy objection to the use of this scale. Still, if the goal is to measure repressiveness and the change thereof without resorting to counting occasions of overt repression, some subjectivity is necessary[16]. As such, the credibility of Freedom House and its panel of experts, as well as their coding practices, seem sufficient to defend the use of the scale.

Second, according to the Freedom House *Methodology* (2015b), the issue of comparability over time is solved by incrementally instituting changes, as well as not revising the time-series data retroactively[17]. This may not be a perfect solution, and the problem of comparing any subjective score across time is a pervasive threat to measurement validity, and thus the internal validity, of cross-sectional time-series analyses in social sciences (Adcock & Collier, 2001: 535). Still, it does not appear to be any greater for the CL scale than other aggregated scales, such as those of institutional polity. It is necessary nevertheless worth mentioning, to note the possible weakness of any analysis including such scales.

There are especially two aspects of the Civil Liberties scale and Freedom House's methodology that are attractive for the purposes of this analysis. First, it does not equate legal guarantees of rights with the real-world practices and fulfillments. Both are factored into the

[16] After all, repression must always be relational to some ideal of 'liberal' – a state is repressive to the degree that it is not permissive of certain defined and ideal liberties. The fact that Freedom House openly addresses the ideal on which cases are judged only increases the researcher's possibility to make an informed choice.

[17] Retroactively revising the data would not necessarily increase the comparability. The issue of context-specificity is enlightened in Adcock and Collier (2001: 535) – 'complete' freedom (1 on CL) may not have had the same standards thirty years ago as it does today. However, since the time span in this analysis is relatively short, 34 years, compared to other analyses in social sciences, I do not judge this to be a fundamental objection to the use of the CL scale.

scoring, but implementation is given precedence (FreedomHouse, 2015b). This allows the score to reflect the effective repressive level in the country, rather than the less trustworthy constitutional situation. Furthermore, each country's score is based on conditions and events in their territorial jurisdiction annually, and are influenced by previous scores. Thus, the temporal dependency lacking in the PTS scale is accounted for in CL, with changes denoting real-world development – such as hard restrictions on press freedom, or a country's first free and fair election – or occasionally gradual changes (FreedomHouse, 2015b). This corresponds well to the idea of liberalization or autocratization employed in this thesis – change is signaled by events or radical changes, or occasionally a gradual evolution, rather than some count of overt behavior.

The next section reviews the coding of the independent variables, and the limitations lack of data imposes on the analysis.

5.2.3 Repression as Independent

The lack of data has proven to be a grave challenge for this analysis. With fairly limited data availability on repression combined with nonviolent conflicts being relatively rare events, coupled with a high threshold of inclusion for conflicts in NAVCO 2.0, the options for testing the hypotheses presented in Chapter 4 are restricted. The original intention was to create dummy variables denoting movement between the three repressive categories – *Not Free*, *Partly Free*, and *Free*. However, this solution proved to be too demanding for the dataset containing only 76 nonviolent and 74 violent campaigns, and with several missing values on key variables.

Table 2 shows the frequency of major maximalist campaign onsets by repressive type (*Free, Partly Free, Not Free*) between 1972 and 2006, which highlights the issue. There are only two nonviolent campaign onsets and one violent campaign onset in state-years in the *Free* category. The resultant model of the approach described above, which can be perused in Appendix Table 3, suffers from overdetermination and questionable standard errors. Thus, this solution is untenable for testing my hypotheses, and I have chosen another approach pending expanded data on conflict and repression[18].

[18] That is not to say I wish for more conflict, only larger datasets.

Table 2 Frequency of Major Maximalist Campaign Onset by Regime Repressive Type, 1972 to 2006

		Onset of NAVCO Major Maximalist Campaign, 1= Nonviolent, 2 = Violent			
		0	1	2	Total
Free	1	1,404	2	1	1,407
	0	3,782	74	73	3,929
Total		*5,186*	*76*	*74*	*5,336*
Partly Free	1	2,360	52	38	2,450
	0	2,826	24	36	2,886
Total		*5,186*	*76*	*74*	*5,336*
Not Free	1	1,422	22	35	1,479
	0	3,764	54	39	3,857
Total		*5,186*	*76*	*74*	*5,336*

Notes: There are 4 units with missing values. All of these units have a 0 value on the onset variable, i.e. none of them experienced conflict onset.

This problem also reduces the number of hypotheses I am able to test using this dataset. The three hypotheses specifying the level of repression before liberalization or autocratization are therefore not tested in this thesis, which leaves me with four hypotheses:

> ***H_1**: Liberalization of repression opens the opportunity structure, which allows the aggrieved populace to address longstanding grievances, and thus increases the likelihood of conflict onset.*
>
> ***H_3**: Liberalization of civil liberties increases the likelihood of nonviolent conflict onset relative to no conflict onset.*
>
> ***H_5**: Autocratization reduces the likelihood of conflict onset.*
>
> -and-
>
> ***H_6**: Autocratization toward extreme repression decreases the likelihood of conflict onset.*

While restricting the number of hypotheses is a disappointment, it is an eventuality one must accept when research is theory-driven. The real world does not always provide the necessary

data to test all hypotheses. That does not make them any less substantially interesting, and these theoretical propositions deserve academic attention at some future point in research, when data is more readily available.

The independent variables used to test the three remaining hypotheses are based on data on civil liberties from the *Freedom in the World* reports from Freedom House (FreedomHouse, 2015a, 2015b, 2015c). The Civil Liberties scale from Freedom House ranges from 1 to 7, where 1 denotes completely free – or least repressive – and 7 represents completely repressive (FreedomHouse, 2015b). The Freedom House methodology operates with a tripartite denotation of states – *Free*, *Partly Free*, and *Not Free*, which corresponds to the respective values of 1, 2 (*Free*), 3, 4, and 5 (*Partly Free*), and 6, 7 (*Not Free*). A dummy variable is created for states with values 1 or 2 (*Free*) and for states with a value of 6 or 7 (*NotFree*). The units with a 1 on the *Free* dummy variable are dropped from the analysis because there are only two occurrences of conflict, which confounds the estimates and standard errors in the analysis. The resulting model is then restricted to information on *Not Free* states and *Partly Free* states, and contains 3,929 state-years based on data from 149 countries, with 74 nonviolent and 73 violent campaign onsets.

To capture liberalization and autocratization, a variable indicating a change on the civil liberties scale from *t-1* to *t* is created. The resultant measure ranges from -5 to 4. Based on this variable, two variables denoting liberal change (*change_pos*) and autocratic change (*change_neg*) are created[19]. Using the Binary Time-Series Cross-Section (BTSCS) software for STATA created by Beck et al. (1998), two variables measuring time since liberalization (*ts_pos*) and autocratization (*ts_neg*) were created.

Finally, proximity to either type of change in the level of civil liberties repression is measured by two decay variables (*ProximitytoLiberalization* and *ProximitytoAutocratization*). The decay variables use the variables measuring time since change and a decay rate denoting at what rate the effect of repressive change reduces over time – i.e., how long it takes before the effect of the repressive change is reduced to 50 %. The formula for the decay function is thus $2^{(-t\ /\ \alpha)}$, where *t* is time since repressive change, and α is the decay rate. For the base model, α = 8 is chosen because this is the value that produced the best log likelihood[20]. This means that for the countries in the sample it takes, in general, 8 years before the effects of autocratization or liberalization of repressive policies have been reduced by 50 %. Both decay

[19] Liberalization (change_pos) = CLchange<0, autocratization (change_neg) = CLchange> 0.
[20] The full formula for each decay variable is then decay_pos_8 = $2^{(-ts\_pos\ /\ 8)}$, and decay_neg_8 = $2^{(-ts\_neg\ /\ 8)}$

variables are continuous measures varying between 1 and 0, with 1 denoting maximal proximity to change. The *Proximity*-variables do not assign effect of liberalization or autocratization until $change_{t+1}$, to ensure that the repressive change and conflict are not measured in the same year.

Finally, an interaction term between the dummy variable *NotFree* and the decay variable *Proximity to Autocratization* is included to capture the proposed relationship between autocratization and extreme levels of repression described in $\boldsymbol{H_6}$ – that autocratization leading into extreme levels of repression will decrease the likelihood of conflict.

5.2.4 Control Variables

In logistic regression, omitted variable bias is a graver concern than in OLS regression (Mood, 2010: 67). Exclusion of variables that affect either the dependent or independent variables will bias estimates upwards or downwards by a factor determined by the correlation between the excluded variable and the independent variable, and the correlation between the excluded variable and the dependent variable when controlling for the independent variable. In addition, excluding variables that affect either the dependent or independent variables will bias our estimates downwards by a factor determined by the difference in residual variance between the models including and excluding the variable (Mood, 2010: 67-69).

In other words, specifying an inclusive model with factors thought to influence the dependent and independent variables is especially important in logistic regression. That is not to say that we abandon the ideal of parsimony, but rather that we should pay close attention when we attempt to identify potential spurious effects. Including irrelevant variables will bias estimates upwards because the scale they are based on is changed (Mood, 2010; Stock & Watson, 2012:359; Train, 2003: 44-45). Thus, the control variables should have theoretical arguments supporting their inclusion to avoid this.

Given this, it is far more likely that the model suffers from omitted variable bias, which reduces the size of the estimates. Additionally, it is possible that the unobserved variance is not the same across all units (Train, 2003: 45). The latter is somewhat ameliorated by clustering the standard errors on states. The former is more difficult. Because the unobserved variance stems from unknown variables and affects the parameter estimates, these become less interpretable (King, Tomz, & Wittenberg, 2000; Mood, 2010). To somewhat amend this issue, I run simulations of the main model using the STATA software addition CLARIFY to produce quantities of interest that are more intuitive and somewhat less affected

by the unexplained variance (King et al., 2000; Mood, 2010; Tomz, Wittenberg, & King, 2003). While this is not a complete fix to the issue, it does produce more intuitive parameters which can be translated into substantive effects.

I thus include control variables thought to influence the onset of nonviolent conflict, and the control variables included in this study correspond closely to those applied by Butcher and Svensson (2014: 10).

The composition and size of the population has been shown to affect the likelihood of both violent and nonviolent conflict onset – states with large populations are more likely to experience conflict than those with smaller populations (Chenoweth & Lewis, 2013d; Cunningham, 2013: 300; Hegre & Sambanis, 2006). Therefore, a logged measure of the population size is introduced ($lnPopulation_{t-1}$). Additionally, nonviolent conflict is thought to be an urban phenomenon (Celestino & Gleditsch, 2013). In addition, urbanization may help overcome collective action problems related to conflict onset (Butcher & Svensson, 2014: 11), and therefore a measure of the proportion of the population living in urban areas (*Urbanization*) from the WBD (2013) is included to control for these effects (Butcher & Svensson, 2014: 11).

GDP per capita has been established as a determinant for violent conflict in several studies (Fearon et al., 2007; Fearon & Laitin, 2003; Hegre & Sambanis, 2006). The proposed mechanisms behind the effect that states with higher GDP per capita have lower likelihood of conflict onset than states with lower GDP per capita are plentiful. Some argue that GDP per capita is a measure of state capacity or rebellion capacity (Collier & Hoeffler, 2004; Collier et al., 2009; Fearon & Laitin, 2003), others that low GDP per capita is a cause of grievance. The same effects of GDP per capita have not been established for nonviolent conflict (Chenoweth & Lewis, 2013d; Cunningham, 2013), but in order to control for either mechanism, a measure of real GDP per capita (*GDPpc*) from the Penn World Table version 7.1 (Aten, Heston, & Summers, 2013) is introduced as a control variable[21].

Tucker (2007) argues that election years provide a focal point for an aggrieved population, and therefore conflict is more likely to occur in such years. Elections mobilize large segments of the populace, and may therefore instigate nonviolent conflict in the event that the election sparks a grievance, as electoral fraud might (Beissinger, 2007, 2013; Tucker, 2007). I include the dummy variable (*Election*) marking whether a state-held election

[21] The variable is included in the dataset by Butcher and Svensson (2014).

occurred in a given year created by Butcher and Svensson (2014: 11) to control for this effect[22].

As a feature of the repressive state system, large military forces may deter the occurrence of nonviolent conflict, or facilitate repressive actions against nascent nonviolent conflict, making them a 'non-occurrence'. However, authors have also emphasized the importance of military defections for nonviolent conflict, and large military forces may aid the dissidents seeking to provoke this effect (Butcher & Svensson, 2014; Nepstad, 2013). Following (Butcher & Svensson, 2014: 11), I include their measure of lagged number of military personnel for a given state-year ($MilitaryPersonnel_{t-1}$) to control for this effect[23].

Several studies have identified learning effects of conflict – i.e. previous experience with conflict increases the likelihood of conflict onset. A temporal dependency of peace on conflict has been repeatedly established – states that have enjoyed long periods without conflict are less likely to experience conflict onset than states that have had conflict in their immediate past (Collier & Hoeffler, 2004; Hegre et al., 2001; Hegre & Sambanis, 2006: 531). In addition, previous experience with either violent or nonviolent campaigns may have left behind an existing infrastructure for dissent, which facilitates new campaigns I include the cubic polynomial of the time since the last nonviolent and violent campaign created by Butcher and Svensson (2014), using the binary time-series cross-section (BTSCS) software in STATA (Beck et al., 1998; D. B. Carter & Signorino, 2010).[24]

The onset of either violent or nonviolent conflict may also be affected by the existence of other contemporary conflicts, both in the near vicinity of a state and in the world in general. Conflicts in other countries may signal an opening in the opportunity structure of which dissidents take advantage. Additionally, diffusion effects of both violent and nonviolent conflict have found empirical evidence – conflict in neighboring countries increases the likelihood of conflict (Beissinger, 2007; Celestino & Gleditsch, 2013; Hegre & Sambanis, 2006: 532-533; Weyland, 2012). Like Butcher and Svensson (2014: 11), I include variables denoting the number of nonviolent and violent conflict onsets globally, each given year (*NumberNonviolOnset*, *NumberViolOnset*). Additionally, I include two variables

[22] The variable is created by Butcher and Svensson (2014: 11), using the National Elections Across Democracy and Autocracy data (NELDA;Hyde & Marinov, 2011).
[23] The variable is created by Butcher and Svensson (2014: 11), using the National Material Capabilities Data version 4.0 at the Correlates of War Project (Singer, 1988).
[24] These measures of time dependency are labeled: NonViolStabilityYears, $NonViolStabilityYears^2$, $NonViolStabilityYears^3$, ViolStabilityYears, $ViolStabilityYears^2$, and $ViolStabilityYears^3$.

denoting conflict onset of either violence or nonviolence in a 50km radius for each state (*NeighborNonViol*, *NeighborViol*).

Various measures of institutional democracy or polity are often included in analyses of conflict (Butcher & Svensson, 2014; Collier & Hoeffler, 2004; Collier et al., 2009; Cunningham, 2013; Hegre, 2014; Hegre et al., 2001; Hegre & Nygård, 2014). Some have claimed an inverted U-curved relationship between polity and conflict onset, indicating that semi-democracies – or anocracies – are more likely to experience conflict than fully institutional democracies or autocracies (Hegre et al., 2001). These findings have been drawn into question by other researchers, who argue that the results are driven by the coding in the Polity IV scale (Vreeland, 2008). Others argue that regime instability rather than institutional polity is the driver of the effect – anocracies experience more conflict because they are more unstable (Gates, Hegre, Jones, & Strand, 2006). Including measures of institutional democracy/autocracy would in any regard be problematic in this analysis, because they are highly correlated with measures of repression. I do, however, include the measure of regime change (*RegimeChange1to3*) in the expanded model to control for the effect of regime instability upon conflict onset from Butcher and Svensson (2014), which indicates the magnitude of change on the PolityIV scale in the previous three years[25].

The model's estimates' robustness is tested by including additional controls from Butcher and Svensson (2014: 12) in an expanded model, such as fuel exports exceeding 33 percent of merchandise exports[26] (*Fuel*), region fixed effects, and regime change (*RegimeChange1to3*). I also run a simplified model with only the independent variables.

Table 3 Frequency Statistics for Dichotomous Variables

Variable	0	1	N	Missing
Not Free	2,450	1,479	3,929	4
Liberalization	3,411	385	3,796	133
Autocratization	3,448	348	3,796	133
Election	3,054	869	3,923	6
$Fuel_{t-1}$ *(33% of exports)*	2,974	764	3,738	191

Notes: All units with 1 on the variable Free are excluded from this table. The full frequency table for the dataset can be perused in Appendix Table 1

[25] The variable created by Butcher and Svensson (2014: 11) is based on the PolityIV project (Marshall, Jaggers, & Gurr, 2011).

[26] This variable is also created by Butcher and Svensson (2014: 12). To correct for the large amount of missing values, the authors used a "last known value" imputation, imputing forward from the first known value to the next known value (Butcher & Svensson, 2014: 12, 10).

Table 4Summary Statistics for Continuous Variables

Variable	Mean	Std. Dev.	Min.	Max	N	Missing
ProximitytoLiberalization	0.66	0.25	0.06	1	3,796	133
ProximitytoAutocratization	0.65	0.24	0.06	1	3,796	133
Time since Liberalization	6.04	5.98	0	33	3,796	133
Time since Autocratization	5.93	5.38	0	33	3,796	133
lnPopulation	15.96	1.565	11.77	20.99	3,913	16
Real GDP per capita	4,701.27	7,893.18	160.93	81515.41	3,616	313
Military Personnel$_{t-1}$	170.98	476.28	0	4750	3,866	63
Urbanization	42.84	22.98	2.72	100	3,809	120
NumberNonViol	2.49	3.20	0	16	3,929	-
NumberViol	2.25	2.02	0	9	3,929	-
NeighborhoodViol	0.20	0.24	0	1	3,671	258
NeighborhoodNonviol	0.05	0.13	0	1	3,671	258
RegimeChange1to3	0.40	3.27	-18	18	3,907	22

Notes: All units with 1 on the variable Free are excluded from this table. The full frequency table for the dataset can be perused in Appendix Table 2

5.3 Methodological Concerns

In addition to the aforementioned issues related to autocorrelation, two more concerns deserve our attention before we move on to the descriptive statistics, and the results of the multinomial logit models. First, I will discuss issues of endogeneity in this analysis, and then I will address the problem of missing values in an analysis concerning conflict and repression.

5.3.1 Endogeneity

Endogeneity caused by reciprocal influence, or codetermination is certainly a grave concern in any study of repression and dissent (Stock & Watson, 2012: 461-462). Undeniably, repressive measures may be imposed on a population because the regime is threatened – in fact, several analyses highlight this aspect of repression in their definition of the concept. However, the theoretical conceptualization of definition in this thesis (Chapter 3, section 3.2) shows that repressive measures may also be imposed because of perceived threats or simply

to deter any threat. In other words, we are faced with a problem of which came first – repression or dissent. As with the famous 'the chicken or the egg' conundrum, it is difficult to determine. Even more concerning is the focus on changes in repressive policies – these, and especially increases in repressiveness – may in fact be responses to dissident activity.

Nevertheless, the theoretical discussions in chapters 2, 3 and 4 suggest that repression may also change because of collapses in will or capacity to repress, or increases in the same. Thus, I attempt to ameliorate the endogeneity issues by lagging the effect of repressive change by one year, to ensure that the measurement of repressive change occurred previous to the onset of the campaigns. I do this rather than making an instrument variable – which can be an option – because it is unlikely that any variable correlated with repression would not be correlated with conflict as well. In other words, defining a valid instrument variable that explains some variation in repression, but is exogeneous – i.e. not correlated with the error term – in an analysis of repression and dissent is unlikely to be a successful endeavor. If the two criteria of relevance and exogeneity are not fulfilled, the instrument variable-approach will not produce the desired results (Stock & Watson, 2012- 480-481).

Admittedly, there are issues with this approach as well. It may be that events preceding the onset of campaign influenced the repressive change. This may be somewhat ameliorated by the onset coding in NAVO 2.0. According to Chenoweth and Lewis (2013d), the onset date of each campaign is coded "(…) as the date of the first observed event associated with the overall campaign meeting the 1,000 participant threshold" (Chenoweth & Lewis, 2013d: 419). Nevertheless, we cannot know for certain that dissident activity didn't influence the repressive change prior to this date. Additional lagging of the proximity variables might amend this uncertainty further, but there are theoretical concerns that make this approach undesirable.

While the theoretical foundation for this thesis does not specify the speed with which the dissidents should react to the repressive change, it seems probable that lagging their response with several years is a poor representation of reality. It seems more likely that the effect of, and response to, change should be higher in the immediately subsequent years. I have therefore chosen to lag the effect of change with one year, while remaining aware of the potential criticisms this approach opens up with regards to the findings in my analysis.

An equally relevant concern is (multi)collinearity. If the independent variables are correlated, the standard error will be large, and the estimates will be affected, rendering them uncertain. A high degree of multicollinearity is associated with large variance in the

estimates, making them questionable and imprecise, and establishing significant effects is made difficult (Christophersen, 2013: 77). To that end, control variables thought to influence the independent and/or the dependent variables that are not correlated with each other are included in the model.

To discover the extent to which collinearity is an issue in this analysis, VIF tests of OLS regression of the base model and expanded model are conducted. The results show that collinearity is not a significant problem. The interaction term and the independent variables *Proximity to Autocratization* and *NotFree* are necessarily highly correlated, and the temporal controls are also highly correlated with each other. However, excluding the interaction term and the temporal controls, none of the variables have a VIF value above 2.10. These results can be perused in Appendix Table 4 and 5. Some argue that a VIF value above 5 is cause for concern, others 10. In either respect, it does not appear that collinearity is of any grave concern in this analysis.

5.3.2 Missing Data

Missing data is not uncommon territory in both repression research and quantitative analyses on nonviolent conflict, or indeed in political science as a whole. It is a severe concern, because as is often the case, systematic missing values degrade the representativeness of the data (Acock, 2012: 375; Christophersen, 2013: 81). As such, several answers to the question of what to do with unknown – or missing – values for one or more variables have been proposed.

Identifying which kind of missing values we are dealing with is the first step. The independent variables in this thesis concern repressive policies in all types of polity. It is unreasonable to assume that the gaps in information are completely random, or Missing Completely at Random (MCAR). Highly repressive states are also the least likely to be forthcoming about their policies, as well as the most difficult to police because they often control media and NGO access very closely. Therefore, the missing values are not MCAR, nor are they MAR – Missing at Random. The missing values on the variables concerning repression can be presumed missing, at least in several instances, precisely because of the level of repression in that state. In other words, the missing values of the repression variables are Not Missing at Random (NMAR) (Acock, 2012: 375; Christophersen, 2013: 81).

This makes what to do with them a conundrum. Both excluding the missing variables and imputing values will bias our estimates. Faced with no ideal solution, choosing to rely on

the data known to represent reality seems preferable, rather than using the other variables in the analysis to generate values. This last option is made even less appealing when we consider the possibility that several other variables in the model may be equally affected by repressive policies, and therefore have missing values for the same units. Therefore, I have not imputed any of the variables with missing values. However, the *Fuel in Exports* variable from (Butcher & Svensson, 2014: 11) has been imputed forwards from the first known value by the authors to ameliorate a severe degree of missing. As this variable is only used in the expanded model for robustness check, I have chosen to use their imputed measure rather than the original, because the original variable without imputation restricts my dataset even further, with its 1,716 missing values in a dataset excluding '*Free'* states.

5.4 Summary

In this chapter, I have described the research design constructed to test the theoretical propositions of Chapter 4. This serves to bridge the gap between theoretical suppositions, concepts, and hypotheses, and the empirical data available to test them. Due to lack of data availability, only four of the seven hypotheses from Chapter 4 are tested in multinomial regression analyses of 3,929 state-years based on data from 149 non-free countries, with 74 nonviolent and 73 violent campaign onsets, between 1972 and 2006. The next chapter presents the results of the empirical analyses, as well as a discussion of the hypotheses, the findings, and the robustness and goodness of fit of the models.

6 Analysis

This section is tripartite. First, I offer an initial look at the relationship between the independent variables and the dependent variable in distribution plots, before the results of the multinomial logistic regression are described and interpreted. Additionally, I present more intuitive quantities of interest to further aid the interpretation of the results. The models are then subjected to a number of robustness tests and tested for fit to the data. Section three is a discussion of the findings in this analysis, including evaluations of the hypotheses.

6.1 Descriptive statistics

To get a preliminary idea of the relationship between the dependent variable, major maximalist campaign onset, and the independent variables, *Proximity to Liberalization* and *Proximity to Autocratization*, I include figures showing the distribution of campaign onsets across values of the independent variables. To aid interpretation, two figures showing the distribution of campaign onsets across the variables *Time since Liberalization*, and *Time since Autocratization* are also included.

Figure 1 shows the distribution of values on the variable measuring time since liberalization occurred for all cases (dark dash), cases where there was an onset of nonviolent campaign (solid blue), and cases where there was an onset of violent campaign (light dash),

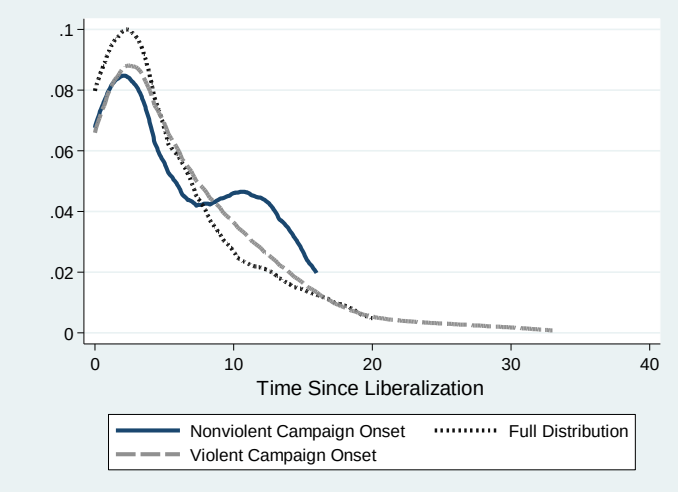

Figure 1 Distribution of global nonviolent and violent campaign onset by time since liberalization, 1972 to 2006

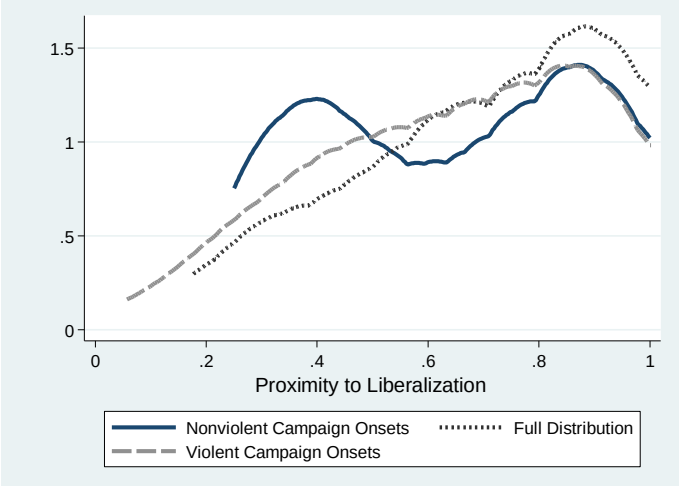

Figure 2 Distribution of global nonviolent and violent campaign onset by proximity to liberalization, 1972 to 2006

excluding units with a 1 on the *Free* dummy. Two patterns stand out. First, the majority of cases have a low value on the variable, indicating that liberalization occurs relatively frequently, and that both nonviolent and violent campaign onset most often occurs in the years immediately after liberalization.

Secondly, as time passes, the density of cases tapers off in the full sample, a pattern which is followed by the distribution of violent campaigns. However, the distribution of nonviolent campaigns has a second peak of density of cases between 10 and 15 years after liberalization. This may indicate that in cases where liberalization in non-free states is not followed by additional liberalization, and the state remains non-free, nonviolent conflict is more likely than violent conflict.

Figure 2 shows the distribution of values on the decay variable, *Proximity to liberalization*, for all cases, cases with nonviolent campaign onset, and cases with violent campaign onset, excluding the cases with *Free=1*. This figure indicates the same pattern as the above – the density is greatest at high values on the proximity variable, but the density of nonviolent cases reaches another peak below 50 % effect of liberalization.

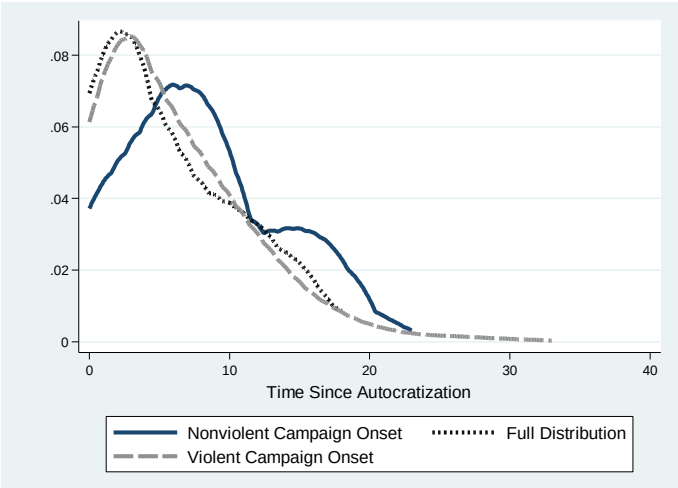

Figure 3 Distribution of global nonviolent and violent campaign onset by time since autocratization, 1972 to 2006

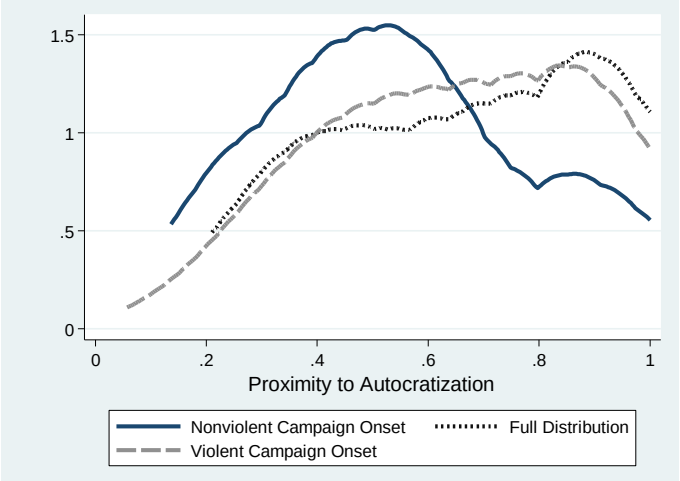

Figure 4 Distribution of global nonviolent and violent campaign onset on proximity to autocratization, 1972 to 2006

This feature is interesting because it displays the now oft-stated suggestion that nonviolent campaigns are fundamentally different from violent campaigns (Chenoweth & Lewis, 2013d; Chenoweth & Ulfelder, 2015; Cunningham, 2013). The distributions of nonviolent campaigns over values of *Time since Liberalization* and *Proximity to Liberalization* indicate that this

proposition may be true for response to liberalization as well.

Figure 3 shows the density of cases on the values of *Time since Autocratization*. Violent conflict onset follows a similar line to that of the full distribution – increasing rapidly in the initial few years, and then declining as time passes. The distribution of cases of nonviolent conflict onset, however, clearly diverges from this pattern. It appears that following an autocratization, the initial likelihood of nonviolent conflict onset is quite low. After several years pass, however, nonviolent conflict onsets become more likely. A similar pattern is visible in Figure 4, where a distinct peak indicates that a high number of cases of major maximalist nonviolent campaign onset occur around the middle values of the *Proximity to Autocratization* variable.

Thus, the distributions of nonviolent campaign onset over the autocratization variables indicate the same dissimilarity of nonviolent campaign onset from the general distribution and violent conflict onset as the distributions over the liberalization variables. Interestingly, all four graphs indicate that stability after instability increases the likelihood of nonviolent conflict, while decreasing the likelihood of violent conflict. Nevertheless, the general impressions from the density figures are that in the initial wake of liberalization, campaign onset of either denomination increases in occurrence the first few years, before tapering off, and that autocratization initially restricts the number of nonviolent onsets – but interestingly enough, not the onset of violent conflict. It appears that violent conflicts occur more frequently immediately following autocratization, and as the level of repression is stabilized and kept stabile, violent conflict becomes less likely.

The patterns indicated by the density graphs could be the product of the confounding factors described earlier. Table 5 shows the result of the multinomial logistic regression analysis for the onset of major, maximalist nonviolent campaigns globally, excluding *Free* states.

6.2 The Regression Models

The first model in Table 5, the Base Model, shows the effect of the independent variables on the likelihood of nonviolent conflict onset relative to no conflict, controlled for potential confounders. Although none of the estimates for the independent variables are significant, they indicate similar patterns to those described above. The estimated coefficient for *Proximity to Liberalization* is positive, and quite strong. Although it is not significant at

commonly accepted standards, it approaches statistical significance (p = 0.185). Likewise, the estimate for *Proximity to Autocratization* is negative and similarly strong, though great uncertainty is connected with this estimation.

The estimated coefficients of the independent variables on the likelihood of violent campaign onset are not statistically significant. However, the estimate for *Proximity to Liberalization* is positive and fairly strong, reflecting the pattern from the distribution figures above. It would appear that violent campaign onset is more likely in the immediate aftermath of liberalization – though this estimate is not significant at $p = 0.154$. The *Proximity to Autocratization* estimate is negative, but so far from an acceptable significance level that we cannot set store by it.

As the number of conflict onsets this analysis is based on is fairly limited, the potential for single cases to heavily influence the results is large. I therefore apply a method similar to jackknifing to discover possible outliers disproportionally affecting the estimates and standard errors of the Base Model. The multinomial regression analysis is repeated, excluding each state sequentially. The scatter plot in Figure 5 shows the resultant estimated coefficients and standard errors of *Proximity to Liberalization* on nonviolent conflict.

The scatter plot clearly shows that Nepal is an extreme outlier, the exclusion of which improves the efficacy of the model massively, while the others are grouped together. There are no obvious reasons why Nepal should be such an outlier, but it is beyond the scope of this

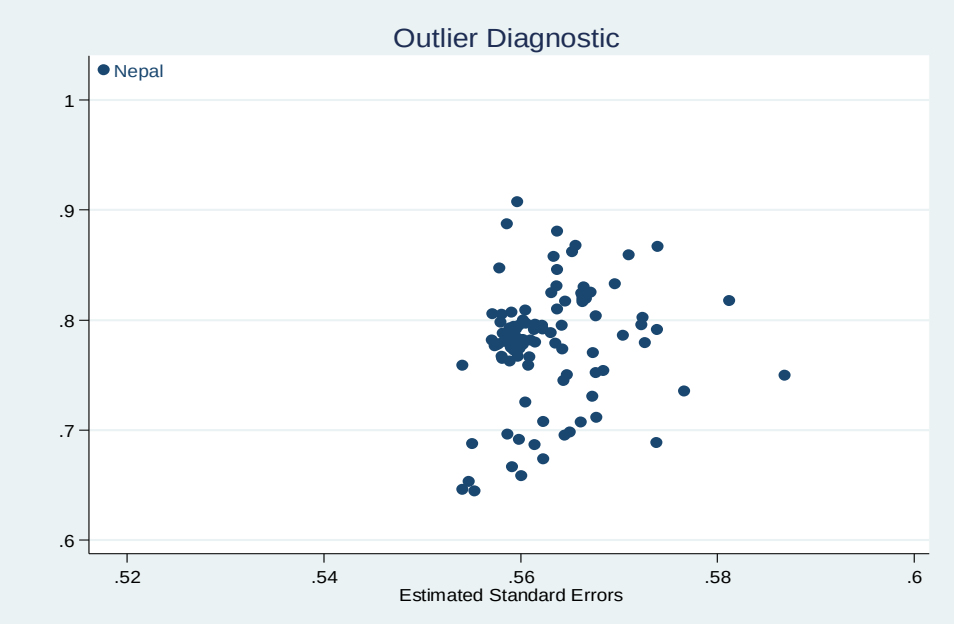

Figure 5 Outlier Diagnostics. Estimated coefficients and standard errors for Proximity to Liberalization on nonviolent campaign onset from repeated multinomial regression analyses of the Base Model, excluding each state sequentially.

Table 5 Onset of Major Maximalist Campaigns, NAVCO, 1972 to 2006

	Base Model	%	%StdX	Base Model, excl. Nepal	%	%StdX
0 (No campaign onset, ref. cat)						
1 (Nonviolent Campaign Onset)						
NotFree	-0.165 (0.950)	-15.2		-0.284 (0.955)	-24.8	
ProximitytoAutocratization	-0.747 (0.919)	-52.6	-16.3	-1.263 (0.827)	-71.7	-26.0
NotFree*ProximityAutocrat	0.008 (1.543)	0.8	0.3	0.376 (1.546)	45.7	14.2
ProximitytoLiberalization	0.747 (0.564)	111.1	20.6	0.999† (0.521)	171.5	28.6
rGDPpc	0.000 (0.000)	0.0	4.2	0.000 (0.000)	0.0	2.2
lnPopulation	0.320** (0.108)	37.6	63.5	0.322** (0.112)	38.0	64.5
Military Personnel $_{t-1}$	0.000 (0.000)	0.0	13.1	0.000 (0.000)	0.0	13.5
Election	0.817** (0.266)	126.4		0.864** (0.268)	137.3	
Urbanization	0.002 (0.008)	0.2	5.7	0.005 (0.008)	0.5	11.6
NonViolStabilityYears	0.059 (0.110)			0.058 (0.110)		
NonViolStabilityYears^2	0.000 (0.006)			0.000 (0.006)		
NonViolStabilityYears^3	-0.000 (0.000)			-0.000 (0.000)		
ViolStabilityYears	0.071 (0.115)			0.066 (0.117)		
ViolStabilityYears^2	-0.004 (0.006)			-0.004 (0.006)		
ViolStabilityYears^3	0.000 (0.000)			0.000 (0.000)		
NumberNonViolOnsets	0.164*** (0.025)	17.9	70.2	0.163*** (0.026)	17.7	69.3
NumberViolOnsets	-0.055 (0.095)	-5.4	-10.5	-0.034 (0.090)	-3.4	-6.6
NeighborhoodViol	-0.562 (0.728)	-43.0	-12.5	-0.700 (0.800)	-50.3	-15.2
NeighborhoodNonviol	0.641 (0.878)	89.9	8.5	0.585 (0.966)	79.5	7.7
Constant	-11.131*** (2.174)			-11.180*** (2.208)		

Table 5 (continued)

	Base Model	%	%StdX	Base Model, excl. Nepal	%	%StdX
2 (Violent Campaign Onset)						
NotFree	0.900 (0.889)	145.9		0.818 (0.899)	126.6	
ProximityAutocratization	-0.417 (0.714)	-34.1	-9.5	-0.550 (0.723)	-42.3	-12.3
NotFree*ProximityAutocrat	-0.789 (1.215)	-54.6	-24.2	-0.668 (1.232)	-48.7	-21.0
ProximityLiberalization	0.856 (0.600)	135.3	24.0	0.902 (0.609)	146.4	25.5
rGDPpc	-0.000 (0.000)	-0.0	-71.2	-0.000 (0.000)	-0.0	-70.8
lnPopulation	0.279** (0.100)	32.1	53.5	0.277** (0.100)	32.0	53.5
Military Personnel $_{t-1}$	-0.000 (0.000)	0.0	-12.8	-0.000 (0.000)	-0.0	-12.3
Election	-0.037 (0.295)	-3.6		-0.009 (0.295)	-0.9	
Urbanization	0.011 (0.012)	1.1	27.7	0.011 (0.012)	1.1	28.2
NonViolStabilityYears	-0.013 (0.114)			-0.013 (0.121)		
NonViolStabilityYears^2	0.003 (0.006)			0.003 (0.006)		
NonViolStabilityYears^3	-0.000 (0.000)			-0.000 (0.000)		
ViolStabilityYears	-0.053 (0.085)			-0.049 (0.088)		
ViolStabilityYears^2	0.002 (0.005)			0.001 (0.005)		
ViolStabilityYears^3	-0.000 (0.000)			-0.000 (0.000)		
NumberNonViolOnsets	-0.009 (0.059)	-0.9	-2.8	-0.011 (0.059)	-1.1	-3.5
NumberViolOnsets	0.302*** (0.044)	35.3	83.9	0.298*** (0.045)	34.7	82.2
NeighborhoodViol	0.232 (0.554)	26.1	5.7	0.173 (0.565)	18.9	4.2
NeighborhoodNonviol	-0.540 (1.227)	-41.7	-6.7	-0.463 (1.233)	-37.1	-5.7
Constant	-9.325*** (2.141)			-9.309*** (2.179)		
N	3274			3240		
Log pseudolikelihood	-535.25			-521.50		
Pseudo R^2	0.127			0.131		

Robust standard errors clustered on state in parentheses

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

Note: % denotes percent change in odds of 1 unit increase in x_i, %StdX denotes percent change in odds of 1 std increase in x_i.

thesis to conduct a case study of Nepal, and therefore, because it is such a significant outlier, Nepal is excluded from the analysis.

The second model in Table 5 shows the Base Model excluding Nepal. The results are largely the same – none of the estimated coefficients for the independent variables changed direction. However, the estimated effect of *Proximity to Liberation* on nonviolent campaign onset is now significant at a 90 % confidence level, and close to the 95 % confidence (p = 0.055). For nonviolent conflict onset, *Proximity to Autocratization* also approaches statistical significance at p= 0.127.

These results suggest that the patterns from the distribution graphs were not in entirety driven by spurious effects. Liberalization in non-free states appears to have a substantial positive impact on the likelihood of nonviolent campaign onset, while there is more uncertainty connected to the estimated negative effect of autocratization, although it is negative as indicated by the distribution graphs. For violent conflict onset, neither variable has a significant effect, although the reported direction is as expected – positive for liberalization, and negative for autocratization.

To further illuminate the relationship between the independent variables and nonviolent campaign onset, I have used the STATA software addition, CLARIFY[27] (Tomz et al., 2003). Using the simulation techniques described by King et al. (2000), I calculated the effect of one standard deviation increase from the mean in the independent variables individually on the probability of nonviolent campaign onset.

With all values held at their means, the simulated probability of nonviolent campaign onset per year is very low – only 0.99 percent. One standard deviation increase in *Proximity to Liberalization* raises the simulated probability of nonviolent campaign onset to 1.27 percent[28]. The value of the mean plus one standard deviation increase is close to the value on the decay variable 1 year after liberalization, and the mean corresponds to somewhere between four and five years after liberalization[29]. In other words, when roughly one year has passed since liberalizing repressive change, and all other variables are at their means, the

[27] CLARIFY draws 1,000 sets of simulated parameters from their asymptotic sampling distribution. These are then converted into the quantities of interest, such as first differences, predicted values, or expected values.
[28] When the dichotomous variables are held at zero, the result is very similar – the initial probability of nonviolent campaign being 0.95 percent, and the probability after one standard deviation increase from the mean in *Proximity to Liberalization* being 1.21 percent, yielding a 27.4 percent change in probability.
[29] The mean of *Proximity to Liberalization* is 0.658, and the standard deviation is 0.252. Thus, the value producing the probability after increase is 0.910, which is fairly close to the value of the decay variable at 1 year after liberalization (0.917). In other words, the probabilities after increase are at a little over a year after liberalization. The mean, 0.658, corresponds to somewhere between four (0.707) and five (0.648) years after liberalization.

probability of nonviolent campaign onset is 1.27%. For *Proximity to Autocratization*, one standard deviation[30] increase reduces the probability of nonviolent campaign onset to 0.74 percent[31] - or, when the autocratization occurred between one and two years ago, the probability of nonviolent conflict onset that year is 0.74 percent.

These results reinforce the impression left by the models in Table 5. Although the substantive effects are not as precise as may be desirable, the simulated probabilities indicate that proximity to liberalization does have a positive effect on the likelihood of nonviolent conflict – or to give a hesitant response to the research question of this thesis; based on the simulated quantities of interest, it would appear that repressive instability influences the probability of nonviolent conflict, in non-free states between 1972 and 2006.

Before the hypotheses are evaluated, additional insights from CLARIFY simulations should be highlighted. Figure 6 displays the effects of a one standard deviation increase from the mean[32] on the probability of nonviolent campaign onset for all substantive variables[33] in the Base Model, with a 90 percent CI cap[34]. The first difference effects – or percentage points change produced by a standard deviation increase in each independent variable – were also generated by simulations using CLARIFY.

The largest effect on the probability of nonviolent campaign onset is provided by *Number of Nonviolent Onsets*. One standard deviation (3.2) increase from the mean (2.5) increases the annual probability of nonviolent campaign onset by 0.65 percentage points. In other words, when the number of global nonviolent conflicts in a given year goes up from 3 to about 5 or 6, the probability of nonviolent campaign onset in a given year increases by 0.65 percentage points, all other variables held at their means. This indicates that the proposed relationship of diffusion finds support in this analysis as well.

The second largest effect is produced by one standard deviation increase in *lnPopulation,* with an increase in annual probability of nonviolent campaign onset of 0.62 percentage points. The positive relationship between large populations and increased likelihood of conflict onset is well-established, and this analysis confirms that phenomenon

[30] The mean for autocratization is 0.654, which corresponds to somewhere between four (0.707) and five (0.648) years after autocratization. The total value of the variable after one standard deviation increase, 0.895, corresponds to somewhere between one (0.917) and two (0.841) years after autocratization.
[31] When the dichotomous variables are held at zero, the result is very similar – the probability after one standard deviation increase from the mean in *Proximity to Liberalization* being 0.70 percent, yielding a 27.4 percent change in probability from 0.95 percent.
[32] Note that in this figure, all variables are held at their means.
[33] Except the cubic polynomials.
[34] The 90 percent confidence interval is chosen because of the relative rareness of nonviolent conflict.

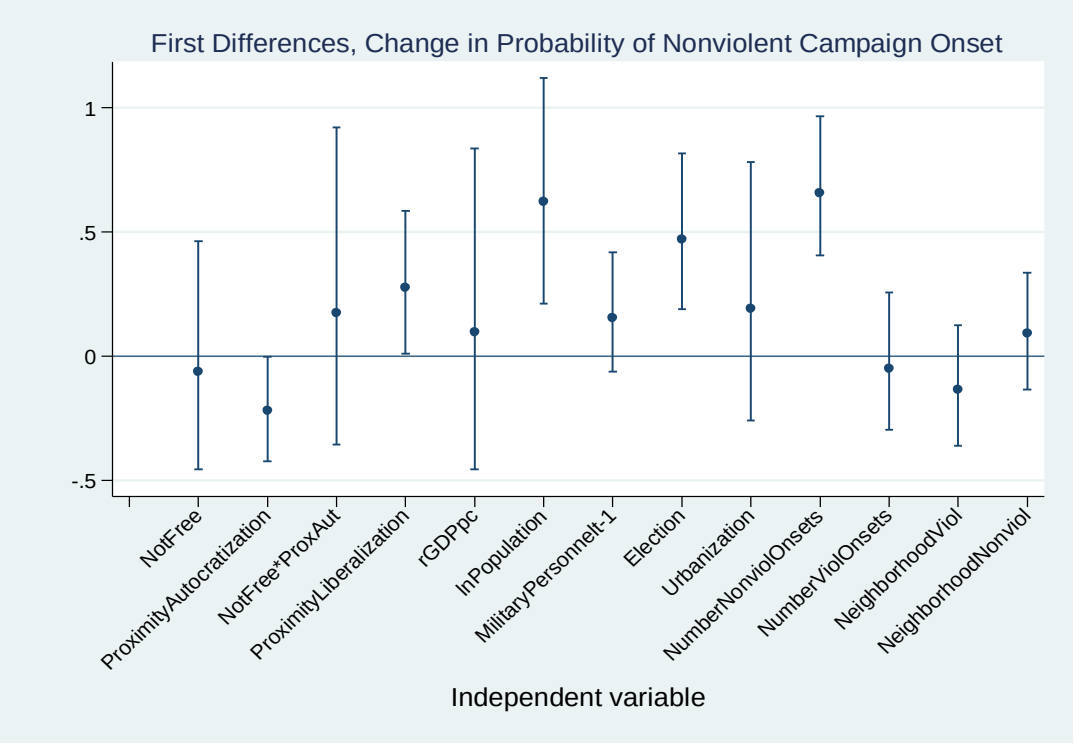

Figure 6 First differences, the effect of a one standard deviation increase on the probability of nonviolent campaign onset, 1972-2006

also holds true in an analysis restricted to non-free states (Butcher & Svensson, 2014; Chenoweth & Lewis, 2013d; Chenoweth & Ulfelder, 2015).

Election also appears to have a relatively strong effect on the probability of nonviolent campaign onset, but this effect is probably underestimated as it is dichotomous, and mean and standard deviation don't make much sense. The effect of one standard deviation increase in the *Election* variable on the annual probability of nonviolent campaign onset is reported at 0.46 percentage points.

However, as the variable is dichotomous, and a value of 0.63 on *Election* is nonsensical, its effect is likely greater. To investigate this, I ran the analysis again, setting the binary variables at 0. With all other variables at their means, the annual probability of nonviolent campaign onset in non-election years is 0.95 percent. In other words, in partly free states in non-election years, with mean values on the other variables, the probability of nonviolent campaign onset is 0.95 percent. However, in election years, the annual probability increases to 2.2 percent, which equals a 1.25 percentage point increase. This indicates that election years may indeed provide a focal point for an aggrieved population, as suggested by Tucker (2007), and that the politicized momentum may indeed be the mechanism that increases the probability of nonviolent campaign onset in partly free states (Beissinger, 2007, 2013; Tucker, 2007).

None of the other controls yield significant changes to the annual probability of nonviolent conflict onset in the CLARIFY simulations presented above. As in other quantitative studies of nonviolent conflict onset (e.g.,Chenoweth & Lewis, 2013d; Chenoweth & Ulfelder, 2015; Cunningham, 2013) GDP per capita does not have a significant influence on the annual likelihood of nonviolent campaign onset in the present model. More interestingly, neither do *Military Personnel*$_{t-1}$ nor *Urbanization*. I find no significant effect on the annual probability of nonviolent campaign onset of the size of the armed forces in non-free states, nor is the proposition that urbanized countries are more prone to nonviolent conflict onset supported, which is consistent with the findings in the analysis by Butcher and Svensson (2014: 17). The neighborhood variables are not significant either, but it is possible that their effect is captured by the measures of annual global nonviolent and violent campaign onsets.

Still, there is more information to gain from the CLARIFY simulations. Setting all independent variables at their means does not extract as poignant clues to the effect of proximity to liberalization as is possible. First, it might be interesting to discover different effects of one standard deviation increase in *Proximity to Liberalization* on simulated probability in *Partly Free* states and in *Not Free* states.

Setting the binary *NotFree* and *Election* variables to zero yields the annual simulated probability for campaign onset in partly free states in nonelection years, with all other variables set at their means. Thus, in states that remain only partly free between four and five years after liberalizing repressive change, in nonelection years, the simulated probability of nonviolent campaign onset that year is 0.95%, and the simulated probability of violent campaign onset is 0.75 %. However, states that remain partly free approximately one year after the liberalization have a simulated probability of 1.21 % for nonviolent campaign onset in that year, and 0.95 % for violent campaign onset. In other words, going from one year after liberalization to four to five years after liberalization decreases the probability of nonviolent campaign onset by 0.26 percentage points when the regime keeps a stable value in the middle of the civil liberties scale.

For states with a 6 or a 7 on the civil liberties scale four to five years after liberalization, in nonelection years, the simulated probability of nonviolent campaign onset is 0.77%, and the probability of violent campaign onset is 1.83 %. For states with a 6 or 7 approximately one year after liberalization, the simulated probability of nonviolent campaign onset is 0.99 %, while it is 2.35 % for violent campaign onset. As for states that keep a stable value in the middle range of the CL scale after liberalization, regimes that remain stable in the

NotFree[35] category after liberalization have a much higher annual probability of conflict onset approximately one year after liberalization than four to five years after liberalization. However, the simulated annual probabilities indicate that the annual probability of violent conflict onset is consistently higher than the annual probability of nonviolent conflict onset.

There are mainly two interesting features with these simulated probabilities. First, the simulated annual probabilities of conflict onset increase when proximity to liberalization is increased, both in states that are partly free and in states that are not free. Second, in states that are still highly repressive after liberalization, the probability of violent conflict is higher than the probability of nonviolent conflict, while the opposite holds true for partly free states. This is not reflected by the estimated effects in Table 5, but is nevertheless interesting. The most compelling feature of these substantive effects is that in the early aftermath[36] of liberalization, there is an increased simulated likelihood of nonviolent campaign onset.

In order to properly answer the research question '***does repressive instability increase the likelihood of nonviolent conflict onset?'*** the hypotheses from Chapter 4 are evaluated based on the results from the multinomial regression analyses and the CLARIFY simulations.

There are two tested hypotheses concerning the relationship between liberalization and conflict onset. The first, $\boldsymbol{H_1}$, states that liberalization should increase the likelihood of conflict onset in general. As I find no conclusive evidence that the probability of violent campaign onset is positively affected by *Proximity to Liberalization*, I retain the null hypothesis that liberalization does not have a significant positive effect on the likelihood of conflict onset, and reject $\boldsymbol{H_1}$.

The second tested *Liberalization*-hypothesis, $\boldsymbol{H_3}$, is more specific, and proposes a positive relationship between liberalization and the probability of nonviolent conflict onset. In the Base Model excluding Nepal, the estimated effect of *Proximity to Liberalization* on nonviolent campaign onset is positive and significant at 90 percent confidence level. As described above, the marginal effect of one standard deviation increase in *Proximity to Liberalization* raises the simulated probability of nonviolent campaign onset to 1.27 percent. Thus, there is support for $\boldsymbol{H_3}$, and the null hypothesis that no significant positive relationship between liberalization and nonviolent conflict onset can be rejected with 90 % certainty.

[35] This necessarily indicates that the value on the CL scale is 6, as liberalization has occurred, and thus a value of 7 is impossible.

[36] Reminder: The mean of *Proximity to Liberalization* is 0.658, and the standard deviation is 0.252. Thus, the value producing the probability after increase is 0.910, which is fairly close to the value of the decay variable at 1 year after liberalization (0.917). In other words, the probabilities are given a year and some months after liberalization. The mean, 0.658, corresponds to somewhere between four (0.707) and five (0.648) years after liberalization.

The remaining two tested hypotheses are concerned with the relationship between autocratization and the probability of conflict onset. Recalling the discussion in Chapter 4, section 4.4, the first of these hypotheses, $\boldsymbol{H_5}$, is essentially the POS-argument that repression is a constraining factor which reduces the likelihood of conflict. While the estimated coefficient for *Proximity to Autocratization* is negative both for nonviolent and violent campaign onset, neither parameter estimate is significant at conventional levels. For major maximalist nonviolent campaign onsets, the estimated effect approaches statistical significance, but the uncertainty regarding this finding is too great to reject the null hypothesis. Therefore I retain the null hypothesis that no significant negative relationship exists between autocratization and the probability of conflict onset.

Finally, $\boldsymbol{H_6}$ proposed that autocratization toward extreme levels of repression decreases the likelihood of conflict. In other words, if the autocratization causes the level of repression to be all-encompassing and pervasive, no opportunities for conflict exist and thus the likelihood of conflict onset is reduced by this effect. The interaction term in the Base Model tests this. Its coefficient is nowhere near significant for nonviolent campaign onset, nor for violent campaign onset[37]. Thus, I reject $\boldsymbol{H_6}$ and retain the null hypothesis that the level of repression autocratization leads into does not have an interaction with autocratization producing reduced probability of campaign onset.

Thus, on the basis of the main model in the analysis, only one hypothesis is retained with any degree of certainty – it does appear that liberalization increases the likelihood of nonviolent campaign onset. The next section tests the robustness of this finding, and the efficiency and predictive power of the model.

6.3 Testing the Model

To evaluate the estimates and the model used in this analysis, I first conduct robustness tests to assess the sensitivity of the results. The next section addresses the model's goodness of fit, evaluating the efficacy and in-sample predictive power. These last tests indicate whether the model is a good fit to the data, and provide an opportunity to compare the relative efficiency and explanatory powers of the models.

[37] Because of the extreme uncertainty regarding these estimates, I have not focused on their directions for nonviolent and violent conflict. The confidence interval is very wide for both coefficients, and thus the relevance of the direction seems less interesting.

6.3.1 Robustness

To assess the sensitivity of the estimations reported in Table 5, I conduct a number of robustness tests. First, two additional models are specified. The simplified model contains only the independent variables, no controls. The expanded model contains two additional parameters, *RegimeChange1to3* and *Fuel Exports*, as well as regional dummies to control for regional differences. The West/Europe is set as the reference category.

Second, alternate specifications of the repression dummies, *Free* and *NotFree*, are substituted for the originals. To assess whether the specification drives the analysis, I generate two more dummies, where units with a 1 on the Civil Liberties (CL) scale from Freedom House receives a 1 on *altFree*, all else set to 0, and where 1 on the dummy *altNotFree* is given to units with a CL value of 7. This includes all units with a 2 on the CL scale in the analysis, increasing the *N* of the analysis. Additionally, the interaction term between *Proximity to Autocratization* and *altNotFree* may be more theoretically valid, as the 'extreme repressive level' specified in the hypothesis may be better captured by a value of 7 on the CL scale than both values of 6[38] and 7. The results of the robustness tests are reported below.

Simplified and Expanded Models

To test the models reported in Table 5, I first ran a simplified model with only independent variables, with the same exclusions as the main model. The resulting models can be examined in Appendix Table 6. The estimated effects on nonviolent conflict are very similar to those of the Base Model excluding Nepal. The coefficient for *Proximity to Autocratization* is still negative, but is now significant at .001-level. The interaction term between *Proximity to Autocration* and *Not Free* is now negative, but remains far from acceptable significance levels. Finally, in the simplified model, the *Proximity to Liberalization* is not significant. The differences between the simplified model and the base model are probably explained by omitted variable bias in the simplified model, biasing the effects and standard errors.

The results from an expanded model including the measure of *RegimeChange1to3*, the dummy for *Fuel Exports*, and regional dummies, are encouraging. For the probability of nonviolent conflict, the coefficient for *Proximity to Liberalization* reaches a stricter significance level. Additionally, *Proximity to Liberalization* has a larger effect upon the probability of nonviolent conflict in the expanded model. In other words, the results from the

[38] A 6 on the CL scale does reflect a high level of repressiveness, but not as pervasive as a 7.

Base Model are robust, even when controlled for regime change in the previous three years, and regional effects. Thus, it does not appear that the effect of proximity to liberalization is solely based on regional differences, nor a side-effect of regime change. This then, supports the rejection of the null hypothesis of $\boldsymbol{H_3}$, and strengthens credibility that the base model estimated effects were not driven by omitted variable bias.

Interestingly, in the expanded model, the effect of *Proximity to Autocratization* on the probability of nonviolent conflict is negative and significant at 0.1-level. These increased levels of significance could be driven by the reduced unexplained variance and thus altered scale of the parameters (Train, 2003: 45). The explained variance remains relatively low, which admittedly is a weakness of the models, but which in the interest of parsimony and absence of established good models for nonviolent conflict onset I leave to future researchers to amend. Neither parameters' estimates are significant for violent conflict, nor is the interaction term, which remains below desired levels of confidence. In other words, the expanded model including a control for *RegimeChange1to3,* a dummy variable for *Fuel Exports,* and regional dummies do not alter the evaluation of the hypotheses, but rather strengthens the confidence in the rejection of all save $\boldsymbol{H_3}$.

Alternative Specifications of the Dummy Variables of Repression

It could be argued that alternative specifications of the *Free* and *NotFree* dummies may be warranted. While the current specification of a CL value of 6-7 in *NotFree* and 1-2 in *Free* may mask effects at the extreme values on the CL scale. I therefore rerun my models with new specifications of the dummies, giving the value 1 on *altFree* for a CL value of 1, and zero for all other values, and the value 1 on *altNotFree* for a CL value of 7, zero for all else. The resultant models are interesting, and can be inspected in Appendix Table 7.

First, for the probability of nonviolent campaign onset, the coefficient for the interaction term between *altNotFree* and *Proximity to Autocratization* is large and negative. In the Base Model, it is significant at 90 percent confidence level (p = 0.075), and it approaches significance in the Expanded model (p = 0.103). This indicates that when the increased level of repression effectively makes the state an extremely repressive state[39], conflict is less likely to ensue. Compared to the estimate for the *Proximity to Autocratization*, which remains negative and insignificant, this is interesting. In the previous models, it approached statistical significance, but in the Base Model and Expanded Model with

[39] With a civil liberties value of 7.

alternative specifications, it is nowhere close to any conventional standards of significance at p = 0.520 and p= 0.568, respectively. This suggests that the original specification disguised the moderating effect of extreme repression on the effect of *Proximity to Autocratization*. However, as this finding is very sensitive, I opt to act on the side of caution and retain the null hypothesis of $\boldsymbol{H_6}$ in this analysis. Nevertheless, the results from these alternative specifications are interesting, and may be an avenue for future research.

Finally, for the probability of nonviolent conflict onset, the estimated effect of *Proximity to Liberalization* remains positive and significant at 90 percent confidence level for all models, and approaches 95 percent confidence level in the Base Model with alternative specifications. This further strengthens the rejection of $\boldsymbol{H_3}$'s null hypothesis.

Overall, the robustness checks do not challenge the findings from the Base Model. The next section will assess which models provide the best efficacy, and which model is the best at predicting outcome correctly.

6.3.2 Goodness of Fit

While the robustness checks above tests the estimations for sensitivity to alternate specifications, the explanatory power of the independent variables should also be tested. The level of statistical significance may not reflect an indicators predictive power, and the empirical fit of the models is highly relevant to the validity of the estimations. The models' ability to explain variations in the data indicates their explanatory power. To assess the empirical fit of the models in this analysis, I rely on two heuristics: Aikake's Information Criterion (AIC), and Receiver Operating Characteristics (ROC) curves.

Aikake's Information Criterion (AIC)

The AIC is commonly used to evaluate the efficiency of a model, given a set of data. While it is comparable to a LR-test, the AIC has some crucial advantages. First, while the LR-test is affected positively by the inclusion of additional parameters, the AIC penalizes additional variables, which reduces the likelihood of overestimating the efficiency of an expanded model. In other words, the AIC takes into account the trade-off between goodness of fit and overly complex models, indirectly rewarding parsimony. Second, the AIC allows models to be un-nested, and for comparisons across different models for the same data material. Smaller values should, all else being equal, suggest a better fitting model (Long, 1997: 109-110).

Table 6 shows the AIC values for all models in this thesis, including those from the robustness checks. The Base Model includes both the independent variables and the control variables specified under 5.2.3 and 5.2.4 in Chapter 5. To discover the relative efficiency of this main model and the relevance of the independent variables, I also calculate the AIC values of the Base Model without the independent variables, keeping only the controls. To evaluate the efficiency of adding controls, Table 6 also reports the AIC values of a simplified model in which only the effects of the independent variables on campaign onset are estimated. Finally, the AIC values of an expanded model, including additional controls and regional dummies are included to evaluate whether these additions add efficiency to the model. All models have four AIC values in Table 6 – the AIC value is calculated for the models run with all non-free units, and for estimations excluding Nepal, both with the original specifications of *Not Free* and *Free* from 5.2.3 in Chapter 5, and with the alternate specifications described in the robustness checks above.

The first notable pattern is that the models excluding Nepal are generally more efficient than their comparable counterparts. In other words, excluding Nepal from the analysis improves the fit of the models. The Base Model with original specifications of *Not Free* and *Free*, excluding Nepal, has the overall lowest AIC value, although the AIC of the Expanded Model with the same conditions is fairly close.

Table 6 AIC values, all models

	Original Specifications of Free & NotFree		*Alternate Specifications of Free & NotFree**	
Model	**All Units**	**Excl. Nepal**	**All Units**	**Excl. Nepal**
Base Model	1150.5	1123.0	1189.9	1163.1
Base Model, Controls Only	1164.6	1139.7	1208.7	1183.5
Simplified Model	1405.9	1376.1	1467.2	1437.6
Expanded Model	1152.4	1124.3	1201.6	1175.1

Notes: All models exclude all units with Free = 1.
** Extreme values of the CL-scale. altFree = 1 if CL = 1, altNotFree = 1 if CL = 7.*

In fact, all for all four sets of AIC values, the Base Model including independent variables has the lowest AIC value. The Expanded Model is fairly close in the two sets of AIC with original specifications of the repressive dummies, and the penalization of additional

parameters probably accounts for its higher AIC value. Nevertheless, this suggests that the Base Model is not a markedly poorer fit than the Expanded Model, and that the additions in the specification of the Expanded Model do not improve the fit to data.

The Base Model ran excluding the independent variables with only controls has a consistently higher AIC value compared with the Base Model including the independent variables. This suggests that the inclusion of the independent variables improves the fit and efficiency of the model. Likewise, the consistent relatively high AIC values for the Simplified Model with only independent variables across all specifications and exclusions indicate that the inclusion of the control variables in the Base Model improves the fit of the model markedly.

The alternate specifications of the two repression dummies do not improve the fit of the model – all AIC values with alternate specifications are higher than those with original specifications. Thus, the Base Model with the original specifications of the repression dummies and excluding Nepal provides the best overall fit to the data.

Receiver Operating Characteristics (ROC) Curves

ROC plots[40] visualize the in-sample predictive power of the models. The curves display the relationship between the true positives rate and the false positives rate at different thresholds for the models. The true positives rate is the number of correctly predicted conflict onsets over the actual number of units with onsets in the data. The false positives rate is the number of incorrectly predicted onsets over all units with no conflict onset in the data. The *x*-axis in the curves is the false positives rate, with the *y*-axis representing the true positives rate (Greenhill, Ward, & Sacks, 2011: 992). Thus, curves for

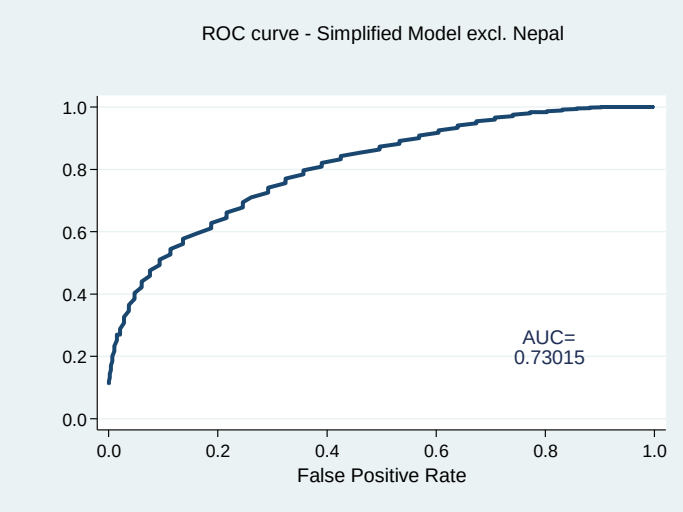

Figure 7 ROC Curve, Simplified Model (Independent Variables Only)

[40] To generate these ROC plots, I have used the mlogitroc command in STATA, which is made available online as a software addition.

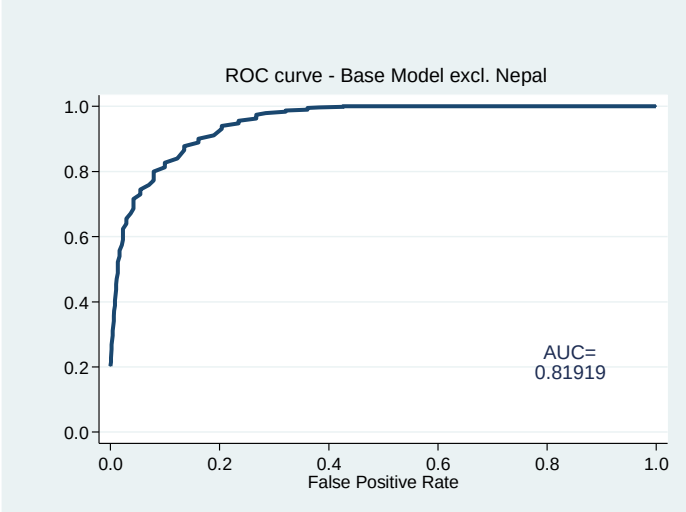

Figure 8 ROC Curve, Base Model

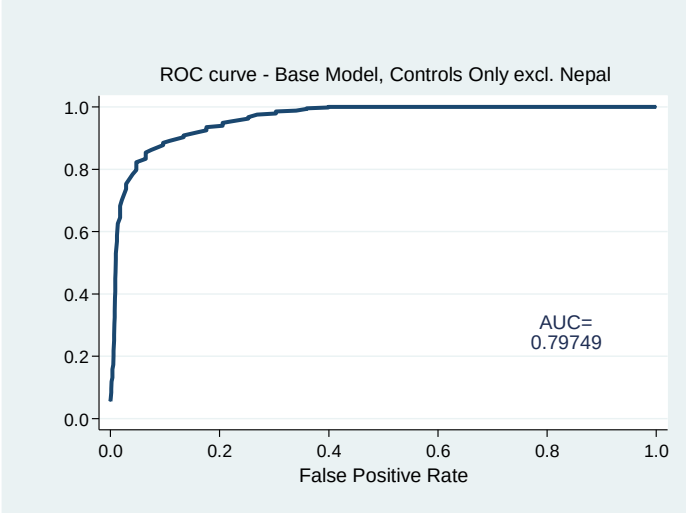

Figure 9 ROC curve, Base Model (Controls Only)

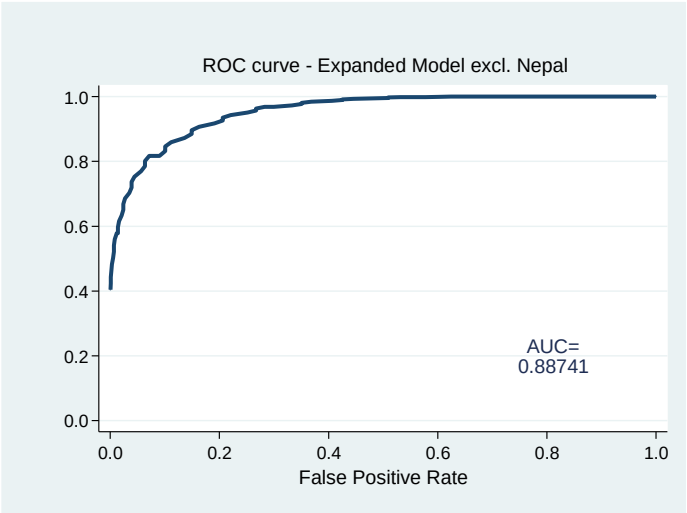

Figure 10 ROC Curve, Expanded Model

models that are adept at predicting correctly should be drawn up in the top left corner of the grid.

The area beneath the curve, the 'AUC-score', can therefore be interpreted as the model's overall predictive power, with a value of 1 indicating perfect predictive power, and a value of 0.5 signifying that the model gets as many predictions wrong as correct for each threshold (Greenhill et al., 2011: 992).

I have generated ROC curves for four models, all excluding Nepal, with the original specifications of *Free* and *Not Free*, displayed in Figures 7, 8, 9, and 10.

The Base Model in Figure 8 clearly performs better than the simplified model in Figure 7, as its curve is more drawn towards the upper left corner of the graph. The AUC score is also markedly higher for the Base Model, suggesting that including the control variables improves the rate of true positives over the rate of false positives – or the in-sample predictive power of the model.

Likewise, the model excluding the independent variables in Figure 9 is less adept at predicting

campaign onset correctly than the full base model including the independent variables, suggesting that the inclusion of the independent variables improves the model's predictive power. The difference between the two is not as great as between the model with only independent variables and the full main model, but as the AUC approaches 1 we should expect more modest adjustments. It suggests that the independent variables at least have a modest effect on the in-sample predictive power of the model.

Finally, the expanded model in Figure 10 with the additional control variables – *RegimeChange1to3, Fuel Exports*, and the regional dummies yields the highest AUC score, and thus performs the best at correctly predicting conflict onset relative to falsely predicting conflict onset of the four models. This may be because the Expanded Model is better suited to the violent campaign onsets, and therefore performs better in terms of predicting violent conflict onset than the other three.

6.3.3 Assessing the Model

The Base Model performs reasonably well across all these tests. The estimates are largely robust to the inclusion of additional variables, and although the alternative specifications of the repression dummies did change the estimates, the AIC values indicate that the Base Model with the original specifications is more efficient. The ROC-curves indicate that the expanded model including additional controls and region dummies has better predictive power than the Base Model, but since the estimates for the independent variables do not substantively differ between the two models – in both models *Proximity to Liberalization* is positive and significant for nonviolent campaign onset, and *Proximity to Autocratization* is negative (though it does reach the lowest conventional level of significance in the expanded model, while it is only approaching significance in the base model), and none of the independent variables have any significant effects on the probability of violent campaign onset – this is not of great concern. Furthermore, the fact that additional variables were included, and thus the parameter scale changed, may account for the larger estimated effects and overall increased significance of the independent variables. The AIC values indicate that the Base Model is a better fit to the data.

While predictive power is desired, adding too many control variables may result in overfitting the model, making it less applicable outside the sample it was fitted for. As several of the hypotheses in this analysis are left untested due to lack of data, this is not an ideal situation. Preferably, the models should be capable of explaining contemporary nonviolent

conflicts not included in their datasets, such as those of the Arab Spring, as well as future conflicts. While I do not expect the hypothesis that liberalization induces conflict to hold in all circumstances, it may be a fruitful inclusion to models rather than the static measure of repression often used. I leave this, as well, for future researchers to assess.

6.4 Summary

This chapter has presented the empirical assessment of four of the seven hypotheses posited based on the grievance-opportunity framework. Only one, $\boldsymbol{H_3}$, of the four receives confident empirical support – the empirical analysis in this thesis suggests that proximity to liberalization does increase the likelihood of nonviolent conflict, in somewhat repressive states. In my models, as in other quantitative studies of nonviolent and violent campaign onset, I find that there are significant differences between the two types of campaigns. While the relationship between proximity to liberalization and nonviolent conflict onset is significant, it is not so for violent conflict onset, consistent with $\boldsymbol{H_3}$, which proposed that certain features of nonviolent campaigns make them more likely in the aftermath of liberalization.

The next, and final, chapter will offer a summary view of the thesis, including its limitations, before outlining the possible avenues for future research.

7 Final Thoughts

It was the goal of this thesis to further contribute to the nascent quantitative study of the causes of nonviolent conflict, as well as offer a new suggested solution to the 'Punishment Puzzle', by answering the research question ***does repressive instability increase the likelihood of nonviolent conflict?*** To that end, an integrated theoretical framework of grievance theory and the political opportunity structure approach was offered as an alternative to the common oppositional depiction of these theories. Rather than emphasizing either grievances or political opportunities as the definitive origin of dissent, I suggest that conflict onset is a function of the two, and that changes in either factor could be the trigger for conflict. Seven hypotheses were derived from the framework, four of which were tested using multinomial logistic regression.

I found fairly strong support for the hypothesis[41] that liberalization of repression increases the likelihood of nonviolent conflict. Conversely, the statistical model applied in this analysis could not produce conclusive support[42] for the remaining three hypotheses tested. It may be that the specifications of the models, or the lack of data available, are the origins of this uncertainty, and I would encourage others to build upon my work. Nevertheless, in the spirit of Karl Popper, I choose to err on the side of caution, and retain only the hypothesis supported by statistical significance. Within this line of thinking, there are certain limitations of the present analysis that the reader should include in their consideration of this conclusion.

First, the dataset in this analysis is restricted. While data for global nonviolent and violent campaigns is available between 1945 and 2006 in the NAVCO 2.0 dataset, there is less availability of data on repression. For this analysis, the Civil Liberties scale from Freedom House was applied. The *Freedom in the World* dataset only contains annual information for all independent states between 1972 and 2014, which restricts the time period in the analysis to 1972 to 2006, about half the time span of the NAVCO data. The numbers of major maximalist nonviolent and violent campaigns in the analysis were constrained by this[43], though a larger proportion of nonviolent campaigns were retained relative to the proportion of

[41] Or rather, I found no grounds to retain my null hypothesis that liberalization does not have a positive and significant effect on nonviolent conflict.
[42] Or rather, could not falsify their null hypotheses.
[43] There are 74 major, maximalist nonviolent campaign onsets and 73 major, maximalist violent campaign onsets in the resultant dataset, with a total of 3,929 units of analysis. In the original NAVCO 2.0 dataset, there are 150 violent and 100 nonviolent campaigns.

violent campaigns. In other words, the basis for analysis is somewhat better for nonviolent campaign onset than for violent campaign onset.

Second, the NAVCO 2.0 data are not without concerns. As noted upon in Chapter 5, several aspects are noteworthy[44]. The primary concern may be the inclusion rules, and their implication for comparison of nonviolent and violent campaigns. While the threshold of 1,000 participants does diminish the issue of underreporting bias[45], it remains unclear whether the comparability of 1,000 participants in nonviolent campaigns to 1,000 participants, derived from the number of battle-related deaths, in violent campaigns is sufficient. It is an issue on which I have chosen to rely on the expertise and consensus of the scholars and experts involved in constructing the NAVCO 2.0 dataset, but which nevertheless should be remarked upon as a potential flaw of the present analysis. As it is, the analysis and its subsequent conclusions are restricted to major campaigns, with maximalist goals.

This last feature of the inclusion rules – maximalist goals – may be more harmful for this analysis than it is for others. The theoretical framework does not specify which types of goals the dissidents are likely to have – and admittedly, they may have all three specified by Chenoweth and Lewis (2013d) – but it is a fair argument that dissidence in the wake of repressive change may be most likely to purport goals of regime change or reform. This especially rings true if the repression is posited as the grievance. Reformist campaigns are not included in the NAVCO 2.0 dataset, and due to the already challenging lack of data, it is imprudent to limit the analysis to those campaigns with a goal of overthrowing the ruling regime. These factors may have confounded the findings reported in Chapter 6, and are deserving of future academic attention. With that said, there is no reason that the mechanism of repressive change should not influence dissidents who wish to remove a foreign occupier or who desire secession, albeit perhaps more likely solely as an opening or constraining factor, as the grievance is already specified.

Third, the concept of *repression* is far more complex than the operationalization in this analysis is able to reflect. Because no satisfactory measure of *personal integrity repression* is available, it was not included in the analysis. This is not as severely limiting as other issues, because the theoretical framework and hypotheses are more inclined towards civil liberties repression instability. Nevertheless, it would be an interesting avenue of future research, in

[44] For a full discussion of these, I refer back to section 5.1.1 Notes on NAVCO 2.0 and the Dependent Variable, Chapter 5.
[45] Which remains significant, and potentially harmful.

the event that data on *personal integrity repression* suitable for measuring instability becomes available.

Furthermore, the Civil Liberties scale from Freedom House, while being the chosen measure of repression, is an aggregated scale prone to subjectivity and contextual specification issues. However, I do not consider the CL scale to be any more plagued with these issues than other aggregated scales, and the open and consensus based Freedom House methodology greatly relieves the issue of subjectivity. All the same, readers should keep in mind that the scale on which the independent variables are based is not a completely objective measure of repression, although the idea of an objective scale in the case of an inherently complex and subjective concept such as 'repression' is slightly fictional[46].

Fourth, the explained variance is fairly low across the statistical models. In lieu of established decisively good models for nonviolent campaign onset, this is to be expected. Additionally, the low variance may be a figment produced by the artificiality of the time aspect in the analysis. While annual analyses are common in conflict research, our explained variance would increase if the time aspect were longer – say 10 years – and conversely, lower should we chose a shorter time frame.

Taking all these aspects into account, the analysis is still not without its strengths. As with all quantitative analyses, its generalizability, though limited by insufficient data, is far greater than any case study. Furthermore, the results are robust to alternate specifications, as well as to the inclusion of additional control variables. Finally, the Base Model[47] performs reasonably well compared to the other models in both efficacy and prediction rate, which implies a relatively sound internal validity. As the explained variance is still fairly low, however, there may still be some room for improvement in this respect.

Thus, based on the results from the multinomial regression analyses, and the model diagnostics, it appears that the answer to the research question is that for all independent, non-free states except Nepal between 1972 and 2006, repressive instability – as the liberalization of civil liberties repression – does increase the likelihood of major maximalist nonviolent campaign onset, both where the outset is highly repressive and those where it is less so. This is interesting, because one of the hypotheses that remain untested specified that the level of repression pre liberalization mattered. While this analysis does not provide a basis for

[46] After all, repression must always be relational to some ideal of 'liberal' – a state is repressive to the degree that it is not permissive of certain defined and ideal liberties.
[47] Excluding Nepal.

evaluating the relevance of pre-liberalization repression level, it does suggest that in any non-free state, liberalization increases the likelihood of conflict.

Conservatively, there is some degree of support for the argument that opening change in the political opportunity structure produces conflict. It would appear that when dissidents face a moderately to very repressive regime that seek to mend its ways, the opening opportunity structure increases the probability of nonviolent campaigns arising. Furthermore, the proximity to the liberalization matters – major maximalist nonviolent campaigns are more likely to occur in the immediate aftermath of the liberalization.

These results speak to both the literature on state repression and dissent, and to the quantitative research of the causes of nonviolent conflict. While the former is an established branch of comparative statistical conflict analyses, the latter is still developing. We do not yet know as much about the origins of nonviolent dissent as we do violent. What we do know is that there seems to be fundamental differences in their causes, as well as in their success rates (Chenoweth & Lewis, 2013d; Chenoweth & Stephan, 2011; Chenoweth & Ulfelder, 2015). This subsidiary finding is also confirmed here, although there are still great gaps left open for analysis as to what makes violent and nonviolent campaign onset differ.

Furthermore, Chapter 4 argued for an integration of two of the most prominent branches of theory in conflict studies – grievance theory and political opportunity structure approaches (Gurr, 1968, 1970; Tarrow, 1998). Like Shadmehr (2014), I argue that the theories are complementary rather than opposing, and propose a grievance-opportunity function, in which changes in either increases the likelihood of conflict onset. I believe this approach both to be consistent with the original theories, as well as fruitful for future research.

Finally, this thesis sought to contribute to the illumination of the 'Punishment Puzzle' described by Davenport (2007a:8) – while the effect of dissent on repression is established as positive, despite a plethora of research no conclusive answer has been made to the question of the effect of repression upon dissent. As I argued in Chapters 3 and 4, this phenomenon may have a fairly intuitive answer. While the repression measured in dissent-upon-repression research is responsive, repression-upon-dissent analyses must take into account the many-faceted nature of the concept. The search for one consistent effect of repression on dissent may be futile, considering that repression can be deterrent or coercive, preemptive or responsive, overt or covert. Depending on what is surveyed, the results should be expected to differ. I argue that the grievance-opportunity function of change may provide an opportunity to focus on the changes in preemptive levels of repression, and find partial support for this.

As indicated above, this thesis leaves exciting avenues of research open for future research, in the advent of increased data availability. First and foremost, the conditional hypotheses left untested in this thesis due to insufficient data deserve attention from future scholars interested in the relationship between repression and dissent. The lack of data precluded them from the present analysis, but both their theoretical foundation and the possible implications should they prove to be supported empirically are intriguing. While repression research has largely been focused on repression as a responsive governmental behavior, the theoretical framework presented here argues that it also has a deterrent and proactive dimension. In relation to the finding in this analysis that major maximalist nonviolent campaign onset is made more likely in the aftermath of civil liberties liberalization in non-free states, it would certainly be interesting to investigate whether the initial repressive level makes a difference in probability of conflict onset – as predicted by Alexis de Tocqueville in 1859, and hypothesized but left untested here.

Second, as mentioned, the mechanism of repressive change is restricted to civil liberties repression in this analysis, which leaves interesting possibilities for future research. First, personal integrity repression could be included in the theoretical conflict function, although that was beyond the scope of this thesis. If data become available, a test of whether there are significant differences between changes in civil liberties repression and personal integrity repression with regards to the probability of conflict onset would be interesting.

Finally, the findings in this thesis are restricted with regards to external validity. Several limitations have been placed upon the conclusions that could reasonably be drawn based on the data material and methods available. Replication and improvements on the model to further assess the strength and plausibility of the suggested relationship between liberalization and conflict onset would be welcome. A new iteration of the NAVCO data is in the coming, and thus opportunities to test the reliability and external validity of the findings presented here should be plentiful.

As a final note, I would like to emphasize that this thesis does not attempt to fully explain all contention, nor is it able to. It does, however, contribute a small piece of a large and ever-changing puzzle, which in and of itself is sufficient justification for the endeavor. Discovering one unitary conflict explanation is quite probably impossible. However, that does not diminish the value of every single contribution to our collective understanding of what conditions promote conflict – the knowledge that it is complicated, diffuse, and perhaps ultimately unpredictable, should only further encourage the search for puzzle pieces.

Literature

Ackerman, P., & DuVall, J. (2000). Nonviolent power in the twentieth century. *Political Science & Politics, 33*(02), 147-148.

Acock, A. C. (2012). *A Gentle Introduction to Stata* (Revised 3 ed.). College Station, TX: Stata Press.

Adcock, R., & Collier, D. (2001). Measurement validity: A shared standard for qualitative and quantitative research. *American political science review, 95*(03), 529-546.

Alimi, E. (2009). Mobilizing under the gun: theorizing political opportunity structure in a highly repressive setting. *Mobilization: An International Quarterly, 14*(2), 219-237.

Aten, B., Heston, A., & Summers, R. (2013). Penn World Table Version 7.1. *Center for International Comparisons of Production, Income, and Prices at the University of Pennsylvania*.

Beck, N. (2001). Time-series-cross-section data: What have we learned in the past few years? *Annual review of political science, 4*(1), 271-293.

Beck, N., & Katz, J. N. (1995). What to do (and not to do) with time-series cross-section data. *American political science review, 89*(03), 634-647.

Beck, N., Katz, J. N., & Tucker, R. (1998). Taking Time Seriously: Time-Series-Cross-Section Analysis with a Binary Dependent Variable. *American Journal of Political Science, 42*(4), 1260-1288. doi: 10.2307/2991857

Beissinger, M. R. (2007). Structure and example in modular political phenomena: The diffusion of bulldozer/rose/orange/tulip revolutions. *Perspectives on Politics, 5*(02), 259-276.

Beissinger, M. R. (2013). The semblance of democratic revolution: Coalitions in Ukraine's orange revolution. *American political science review, 107*(03), 574-592.

Butcher, C., & Svensson, I. (2014). Manufacturing Dissent Modernization and the Onset of Major Nonviolent Resistance Campaigns. *Journal of Conflict Resolution*, 0022002714541843.

Carey, S. C. (2006). The dynamic relationship between protest and repression. *Political Research Quarterly, 59*(1), 1-11.

Carey, S. C. (2010). The use of repression as a response to domestic dissent. *Political Studies, 58*(1), 167-186.

Carter, A., Clark, H., & Randle, M. (2006). *People Power and Protest since 1945: A Bibliography on Nonviolent Action*. London: Housmans

Back to the future: Modeling time dependence in binary data, (2010).

Celestino, M. R., & Gleditsch, K. S. (2013). Fresh carnations or all thorn, no rose? Nonviolent campaigns and transitions in autocracies. *Journal of Peace Research, 50*(3), 385-400.

Chenoweth, E., & Cunningham, K. G. (2013). Understanding nonviolent resistance: An introduction. *Journal of Peace Research, 50*(3), 271-276.

Chenoweth, E., & Lewis, O. A. (2013a). Nonviolent and Violent Campaigns and Outcomes (NAVCO) Data Project v. 2.0 Codebook. University of Denver.

Chenoweth, E., & Lewis, O. A. (2013b). *Nonviolent and Violent Campaigns and Outcomes Dataset, v. 2.0*.

Chenoweth, E., & Lewis, O. A. (2013c). *Notable changes in campaign coding between NAVCO v.1.1 and NAVCO v2.0*. NAVCO Data Project.

Chenoweth, E., & Lewis, O. A. (2013d). Unpacking Nonviolent Campaigns: Introducing the NAVCO 2.0 Dataset. *Journal of Peace Research, 50*(3), 415-423.

Chenoweth, E., & Stephan, M. J. (2011). *Why Civil Resistance Works: The Strategic Logic of Nonviolent Conflict*. New York: Columbia University Press.
Chenoweth, E., & Ulfelder, J. (2015). Can Structural Conditions Explain the Onset of Nonviolent Uprisings? *Journal of Conflict Resolution*, 0022002715576574.
Christophersen, K.-A. (2013). *Introduksjon til statistisk analyse: regresjonsbaserte metoder og anvendelse*. Oslo: Gyldendal.
Cingranelli, D. L., & Richards, D. L. (2010). The Cingranelli and Richards (CIRI) human rights data project. *Human Rights Quarterly, 32*(2), 401-424.
Collier, P., & Hoeffler, A. (2004). Greed and grievance in civil war. *Oxford economic papers, 56*(4), 563-595.
Collier, P., Hoeffler, A., & Rohner, D. (2009). Beyond greed and grievance: feasibility and civil war. *Oxford economic papers, 61*(1), 1-27.
Cunningham, K. G. (2013). Understanding strategic choice: The determinants of civil war and nonviolent campaign in self-determination disputes. *Journal of Peace Research, 50*(3), 291-304.
Cunningham, K. G., & Beaulieu, E. (2010). Dissent, Repression, and Inconsistency. In E. Chenoweth & A. Lawrence (Eds.), *Rethinking Violence: States and Non-State Actors in Conflict* (pp. 173-195). Cambridge, MA: MIT Press.
Dahl, M., Gates, S., Gleditsch, K. S., & González, B. (2014). *Apples and Oranges? Actor Profiles and The Choice of Violent and Non-Violent Tactics*. manuscript.
Danneman, N., & Ritter, E. H. (2014). Contagious rebellion and preemptive repression. *Journal of Conflict Resolution, 58*(2), 254-279.
Davenport, C. (1995). Multi-dimensional threat perception and state repression: An inquiry into why states apply negative sanctions. *American Journal of Political Science*, 683-713.
Davenport, C. (2004). The promise of democratic pacification: An empirical assessment. *International Studies Quarterly, 48*(3), 539-560.
Davenport, C. (2007a). State repression and political order. *Annu. Rev. Polit. Sci., 10*, 1-23.
Davenport, C. (2007b). State repression and the tyrannical peace. *Journal of Peace Research, 44*(4), 485-504.
Davenport, C., & Armstrong, D. A. (2004). Democracy and the violation of human rights: A statistical analysis from 1976 to 1996. *American Journal of Political Science, 48*(3), 538-554.
Davenport, C., & Trivedi, P. (2013). Activism and awareness Resistance, cognitive activation, and 'seeing'untouchability among 98,316 Dalits. *Journal of Peace Research, 50*(3), 369-383.
Davies, J. C. (1962). Toward a theory of revolution. *American Sociological Review*, 5-19.
DeNardo, J. (1985). *Power in Numbers*. Princeton: Princeton University Press.
Earl, J. (2006). Introduction: Repression and the social control of protest. *Mobilization: An International Quarterly, 11*(2), 129-143.
Earl, J. (2011). Political repression: Iron fists, velvet gloves, and diffuse control. *Annual review of sociology, 37*, 261-284.
Fearon, J. D., Kasara, K., & Laitin, D. D. (2007). Ethnic minority rule and civil war onset. *American political science review, 101*(01), 187-193.
Fearon, J. D., & Laitin, D. D. (2003). Ethnicity, insurgency, and civil war. *American political science review, 97*(01), 75-90.
Fein, H. (1995). More murder in the middle: life-integrity violations and democracy in the world, 1987. *Hum. Rts. Q., 17*, 170.
Francisco, R. A. (1995). The Relationship between Coercion and Protest An Empirical Evaluation in Three Coercive States. *Journal of Conflict Resolution, 39*(2), 263-282.

Francisco, R. A. (1996). Coercion and Protest: An Empirical Test in Two Democratic States. *American Journal of Political Science, 40*(4), 1179-1204. doi: 10.2307/2111747
Francisco, R. A. (2004). After the massacre: Mobilization in the wake of harsh repression. *Mobilization: An International Quarterly, 9*(2), 107-126.
Frantz, E., & Kendall-Taylor, A. (2014). A dictator's toolkit Understanding how co-optation affects repression in autocracies. *Journal of Peace Research*, 0022343313519808.
FreedomHouse. (2015a). Freedom in the World 2015. Retrieved 02.19, 2015, from https://freedomhouse.org/report/freedom-world/freedom-world-2015#.VijmgH7hDIU
FreedomHouse. (2015b). Freedom in the World 2015 Methodology (Vol. 2015): Freedom House.
FreedomHouse. (2015c). *Individual country ratings and status, FIW 1973-2015*. Retrieved from: https://freedomhouse.org/report-types/freedom-world
Gates, S., Hegre, H., Jones, M. P., & Strand, H. (2006). Institutional inconsistency and political instability: Polity duration, 1800–2000. *American Journal of Political Science, 50*(4), 893-908.
Ginkel, J., & Smith, A. (1999). So you say you want a revolution? A game theoretic explanation of revolution in repressive regimes. *Journal of Conflict Resolution, 43*(3), 291-316.
Goldstein, R. J. (1978). *Political Repression in Modern America From 1870 to the Present*. Cambridge Mass.: GK Hall & Company.
Goldstone, J. A., & Tilly, C. (2001). Threat (and opportunity): Popular action and state response in the dynamics of contentious action. In R. Aminzade, J. A. Goldstone, D. McAdam, E. J. Perry, W. H. J. Sewell, S. Tarrow, & C. Tilly (Eds.), *Silence and voice in the study of contentious politics* (pp. 179-194). New York: Cambridge University Press.
Greenhill, B., Ward, M. D., & Sacks, A. (2011). The separation plot: A new visual method for evaluating the fit of binary models. *American Journal of Political Science, 55*(4), 991-1002.
Gurr, T. (1968). A causal model of civil strife: A comparative analysis using new indices. *American political science review, 62*(04), 1104-1124.
Gurr, T. (1970). *Why Men Rebel*. Princeton, N.J: Princeton University Press.
Hegre, H. (2014). Democracy and armed conflict. *Journal of Peace Research, 51*(2), 159-172. doi: 10.1177/0022343313512852
Hegre, H., Ellingsen, T., Gates, S., & Gleditsch, N. P. (2001). *Toward a democratic civil peace? Democracy, political change, and civil war, 1816–1992.* Paper presented at the American Political Science Association.
Hegre, H., & Nygård, H. M. (2014). Governance and conflict relapse. *Journal of Conflict Resolution*, 0022002713520591.
Hegre, H., & Sambanis, N. (2006). Sensitivity analysis of empirical results on civil war onset. *Journal of Conflict Resolution, 50*(4), 508-535.
Henderson, C. W. (1991). Conditions affecting the use of political repression. *Journal of Conflict Resolution, 35*(1), 120-142.
Hibbs, D. A. (1973). *Mass Political Violence: A Cross-National Causal Analysis*. New York: Wiley.
Hirsch, E. L. (1990). Sacrifice for the Cause: Group Processes, Recruitment, and Commitment in a Student Social Movement. *American Sociological Review, 55*(2), 243-254. doi: 10.2307/2095630
Hyde, S. D., & Marinov, N. (2011). Which elections can be lost? *Political analysis*, mpr040.
Jakobsen, P. V. (2013). Coercive Diplomacy *Contemporary Security Studies* (pp. 239-255): Oxford: Oxford University Press, 3. edition.

Karatnycky, A., & Ackerman, P. (2005). How freedom is won: From civic resistance to durable democracy. New York: Freedom House
Kendall-Taylor, A., & Frantz, E. (2014). How Autocracies Fall. *The Washington Quarterly, 37*(1), 35-47.
King, G., & Roberts, M. E. (2015). How Robust Standard Errors Expose Methodological Problems They Do Not Fix, and What to Do About It. *Political analysis, 23*(2), 159-179. doi: 10.1093/pan/mpu015
King, G., Tomz, M., & Wittenberg, J. (2000). Making the most of statistical analyses: Improving interpretation and presentation. *American Journal of Political Science*, 347-361.
Koopmans, R. (1997). Dynamics of repression and mobilization: The German extreme right in the 1990s. *Mobilization: An International Quarterly, 2*(2), 149-164.
Lichbach, M. I. (1987). Deterrence or escalation? The puzzle of aggregate studies of repression and dissent. *Journal of Conflict Resolution, 31*(2), 266-297.
Lichbach, M. I., & Gurr, T. R. (1981). The Conflict Process A Formal Model. *Journal of Conflict Resolution, 25*(1), 3-29.
Long, S. J. (1997). *Regression Models for Categorical and Limited Dependent Variables* (Vol. 7). Thousand Oaks: SAGE Publications.
Marshall, M. G., Jaggers, K., & Gurr, T. R. (2011). Dataset Users' Manual. Polity IV Project. *Centre for Systemic Peace*.
McAdam, D. (1999). *Political Process and the Development of Black Insurgency, 1930-1970* (2nd ed.). Chicago: University of Chicago Press.
McAdam, D., McCarthy, J. D., & Zald, M. N. (1996). *Comparative perspectives on social movements: Political opportunities, mobilizing structures, and cultural framings*: Cambridge University Press.
McAdam, D., & Tarrow, S. (2000). Nonviolence as contentious interaction. *PS: Political Science & Politics, 33*(02), 149-154.
McAdam, D., Tarrow, S., & Tilly, C. (2004). *Dynamics of Contention*. New York: Cambridge University Press.
McLauchlin, T., & Pearlman, W. (2012). Out-Group Conflict, In-Group Unity? Exploring the Effect of Repression on Intramovement Cooperation. *Journal of Conflict Resolution, 56*(1), 41-66.
Mesquita, B. B. D., Downs, G. W., Smith, A., & Cherif, F. M. (2005). Thinking inside the Box: A Closer Look at Democracy and Human Rights. *International Studies Quarterly, 49*(3), 439-457. doi: 10.2307/3693602
Meyer, D. S. (2004). Protest and political opportunities. *Annual review of sociology*, 125-145.
Mood, C. (2010). Logistic regression: Why we cannot do what we think we can do, and what we can do about it. *European Sociological Review, 26*(1), 67-82.
Moore, W. H. (1998). Repression and dissent: Substitution, context, and timing. *American Journal of Political Science*, 851-873.
Moore, W. H. (2000). The Repression of Dissent A Substitution Model of Government Coercion. *Journal of Conflict Resolution, 44*(1), 107-127.
Muller, E. N., & Weede, E. (1990). Cross-National Variation in Political Violence: A Rational Action Approach. *The Journal of Conflict Resolution, 34*(4), 624-651. doi: 10.2307/174182
Nepstad, S. E. (2013). Mutiny and nonviolence in the Arab Spring Exploring military defections and loyalty in Egypt, Bahrain, and Syria. *Journal of Peace Research, 50*(3), 337-349.
Nordås, R., & Davenport, C. (2013). Fight the youth: Youth bulges and state repression. *American Journal of Political Science, 57*(4), 926-940.

Oliver, P. (2008). Repression and crime control: Why social movement scholars should pay attention to mass incarceration as a form of repression. *Mobilization: An International Quarterly, 13*(1), 1-24.
Olson, M. (1965). *The Logic of Collective Action*. Cambridge: Harvard University Press.
Opp, K.-D., & Roehl, W. (1990). Repression, micromobilization, and political protest. *Social Forces*, 521-547.
Pierskalla, J. H. (2009). Protest, deterrence, and escalation: The strategic calculus of government repression. *Journal of Conflict Resolution*.
Poe, S. C., & Tate, C. N. (1994). Repression of human rights to personal integrity in the 1980s: a global analysis. *American political science review, 88*(04), 853-872.
Rasler, K. (1996). Concessions, repression, and political protest in the Iranian revolution. *American Sociological Review*, 132-152.
Regan, P. M., & Henderson, E. A. (2002). Democracy, threats and political repression in developing countries: are democracies internally less violent? *Third World Quarterly, 23*(1), 119-136.
Ritter, E. H. (2014). Policy Disputes, Political Survival, and the Onset and Severity of State Repression. *Journal of Conflict Resolution, 58*(1), 143-168. doi: 10.1177/0022002712468724
Schelling, T. C. (2008 [1966]). The Diplomacy of Violence. *Essential Readings in World Politics*, 338-346.
Schock, K. (2005). *Unarmed Insurrections: People Power Movements in Nondemocracies* (Vol. 22): University of Minnesota Press.
Schock, K. (2013). The practice and study of civil resistance. *Journal of Peace Research, 50*(3), 277-290.
Shadmehr, M. (2014). Mobilization, Repression, and Revolution: Grievances and Opportunities in Contentious Politics. *The Journal of Politics, 76*(03), 621-635.
Sharp, G. (1973). *The Politics of Nonviolent Action*: Boston: Porter Sargent.
Singer, J. D. (1988). Reconstructing the correlates of war dataset on material capabilities of states, 1816–1985. *International Interactions, 14*(2), 115-132.
Snyder, D. (1976). Theoretical and methodological problems in analysis of governmental coercion and collective violence. *Journal of Political and Military Sociology, 4*(2), 277-293.
Stock, J. H., & Watson, M. W. (2012). *Introduction to Econometrics* (3 ed.). Essex: Pearson Education.
Sutton, J., Butcher, C. R., & Svensson, I. (2014). Explaining political jiu-jitsu Institution-building and the outcomes of regime violence against unarmed protests. *Journal of Peace Research*, 0022343314531004.
Svensson, I., & Lindgren, M. (2011). Community and consent Unarmed insurrections in non-democracies. *European Journal of International Relations, 17*(1), 97-120.
Tarrow, S. (1998). *Power in Movement: Social Movements and Contentious Politics* (2 ed.). New York: Cambridge University Press.
Tilly, C. (1978). *From Mobilization to Revolution*. New York: McGraw-Hill.
Tocqueville, A. d. (1955). The Old Régime and the French Revolution (S. Gilbert, Trans.). Garden City N.Y.: Doubleday Anchor.
Tomz, M., Wittenberg, J., & King, G. (2003). CLARIFY: Software for interpreting and presenting statistical results. *Journal of Statistical Software, 8*(1), 1-30.
Train, K. E. (2003). *Discrete Choice Methods with Simulation*. New York: Cambridge University Press.
Tucker, J. A. (2007). Enough! Electoral fraud, collective action problems, and post-communist colored revolutions. *Perspectives on Politics, 5*(03), 535-551.

Vreeland, J. R. (2008). The effect of political regime on civil war Unpacking anocracy. *Journal of Conflict Resolution, 52*(3), 401-425.
Weyland, K. (2012). The Arab Spring: Why the surprising similarities with the revolutionary wave of 1848? *Perspectives on Politics, 10*(04), 917-934.
Wilkinson, P. (1971). *Social Movement*. London: Pall Mall.
Wolfe, A. (1973). *The Seamy Side of Democracy: Repression in America*. New York: David McKay.
Wood, R. M., & Gibney, M. (2010). The Political Terror Scale (PTS): A re-introduction and a comparison to CIRI. *Human Rights Quarterly, 32*(2), 367-400.
Young, J. K. (2009). State capacity, democracy, and the violation of personal integrity rights. *Journal of Human Rights, 8*(4), 283-300.
Zanger, S. C. (2000). A global analysis of the effect of political regime changes on life integrity violations, 1977-93. *Journal of Peace Research, 37*(2), 213-233.
Zunes, S. (1994). Unarmed insurrections against authoritarian governments in the Third World: a new kind of revolution. *Third World Quarterly, 15*(3), 403-426.

Appendix

Appendix Table 1 Frequency Table for Dichotomous Variables, All Units

Variable	0	1	N	Missing
Not Free	3,857	1,479	5,336	4
Liberalization	4,672	495	5,167	173
Autocratization	4,801	366	5,167	173
Election	3,950	1,292	5,242	98
Fuel$_{t-1}$ (33% of exports)	4,170	848	5,018	322

Notes: For all state-years, 169 states, 1972-2006

Appendix Table 2 Summary Statistics for Continous Variables, All Units

Variable	Mean	Std. Dev.	Min.	Max	N	Missing
ProximitytoLiberalization	0.63	0.27	0.057	1	5,167	173
ProximitytoAutocratization	0.60	0.26	0.057	1	5,167	173
lnPopulation	15.89	1.61	11.77	20.99	5,301	39
Real GDP per capita	8,257.85	10,372.88	160.93	81515.41	4,993	347
Military Personnel$_{t-1}$	164.95	442.97	0	4750	5,252	88
Urbanization	48.33	24.12	2.72	100	5,144	196
NumberNonViol	2.50	3.19	0	16	5,340	-
NumberViol	2.19	2.01	0	9	5,340	-
NeighborhoodViol	0.19	0.25	0	1	4,826	514
NeighborhoodNonviol	0.05	0.13	0	1	4,826	514
RegimeChange1to3	0.44	3.08	-18	18	5,210	130

Notes: For all state-years, 169 states, 1972-2006.

Appendix Table 3 Auxillary Model, First Approach

	(1) Auxillary Model, First Approach	
	0 (no campaign onset, ref.cat)	
	1 (Nonviolent campaign onset)	2 (Violent campaign onset)
NochangeNOTFREE	(ref.cat)	(ref.cat)
NochangeFREE	-2.008*	-1.385
	(0.917)	(1.328)
NochangePARTLYFREE	-0.589	-1.510**
	(0.415)	(0.552)
ChangeFREEtoPARTLY	-14.595***	-15.338***
	(0.764)	(0.829)
ChangeFREEtoNOT	-12.842***	-13.522***
	(0.849)	(0.908)
ChangePARTLYtoFREE	-14.605***	-14.707***
	(0.631)	(0.747)
ChangePARTLYtoNOT	0.589	-14.779***
	(1.146)	(0.514)
ChangeNOTtoPARTLY	0.712	-0.217
	(0.728)	(0.973)
ChangeNOTtoFREE	-13.143***	-16.541***
	(1.476)	(1.311)
lnPopulation	0.264+	0.405+
	(0.137)	(0.216)
Election	0.948**	0.785+
	(0.298)	(0.425)
Military Personnel	0.000	-0.001
	(0.000)	(0.001)
Nvstabyrs	-0.001	0.020
	(0.011)	(0.017)
Vstabyrs	-0.007	-0.024
	(0.015)	(0.019)
NumberNonviolOnset	0.132***	-0.079
	(0.030)	(0.071)
NumViolOnset	-0.215+	0.354***
	(0.117)	(0.071)
Urbanization	0.019*	0.038**
	(0.010)	(0.015)
Fuel Exports	-0.747+	-0.569
	(0.402)	(0.703)
realGDPpc	-0.000	-0.000*
	(0.000)	(0.000)
Neighborhood_viol	0.461	1.075
	(0.611)	(0.838)
Neighborhood_nonviol	-0.083	1.303
	(1.222)	(1.378)
Constant	-8.877***	-12.024**
	(2.408)	(3.715)
N	2917	

Standard errors in parentheses
+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
Notes: NochangeNOTFREE reference category.
79 observations completely determined. Standard errors questionable.

Appendix Table 4 VIF Test, Base Model Independent Variables

Variable	VIF
realGDPpc	2.08
Urbanization	1.92
lnPopulation	1.57
MilitaryPersonnel	1.56
Proximity to Liberalization	1.46
Proximity to Autocratization	1.39
NotFree	1.22
Neighborhood_viol	1.08
NumberViolOnsets	1.07
NumberNonviolOnsets	1.06
Neighborhood_nonviol	1.03
Election	1.03
Mean VIF	1.37

*Notes: Interaction term NotFree*Proximity to Autocratization and temporal polynomials excluded*

Appendix Table 5 VIF Test Expanded Model Independent Variables

Variable	VIF
realGDPpc	2.09
Urbanization	1.95
lnPopulation	1.57
MilitaryPersonnel	1.56
Proximity to Liberalization	1.49
Proximity to Autocratization	1.43
NotFree	1.30
Fuel Exports	1.09
Neighborhood_viol	1.08
NumberViolOnsets	1.08
RegimeChange1to3	1.06
NumberNonviolOnsets	1.06
Neighborhood_nonviol	1.03
Election	1.03
Mean VIF	1.35

*Notes: Notes: Interaction term NotFree*Proximity to Autocratization and temporal polynomials excluded*

Appendix Table 6 Auxillary Models, Robustness Tests Expanded and Simplified Model

	Expanded Model, excl. Nepal	%	%StdX	Simplified Model, excl. Nepal	%	%StdX
0 (No conflict onset, ref.cat)						
1 (Nonviolent campaign onset)						
NotFree	-0.417	34.1		-0.071	-6.9	
	(0.898)			(0.778)		
ProximitytoAutocratization	-1.443†	-76.4	-29.1	-2.153***	-88.4	-40.5
	(0.874)			(0.610)		
NotFree*ProximityAutocrat	0.587	79.9	22.9	-0.141	-13.2	-4.9
	(1.458)			(1.248)		
ProximitytoLiberalization	1.247*	247.9	36.9	0.874†	139.6	24.7
	(0.608)			(0.483)		
rGDPpc	0.000	0.0	8.3			
	(0.000)					
lnPopulation	0.436**	54.7	96.2			
	(0.140)					
Military Personnel $_{t-1}$	0.000	0.0	6.7			
	(0.000)					
Election	0.866**	137.7				
	(0.279)					
Urbanization	-0.007	-0.7	-15.6			
	(0.011)					
NonViolStabilityYears	0.057					
	(0.111)					
NonViolStabilityYears^2	0.002					
	(0.006)					
NonViolStabilityYears^3	-0.000					
	(0.000)					
ViolStabilityYears	0.086					
	(0.135)					
ViolStabilityYears^2	-0.005					
	(0.007)					
NonViolStabilityYears^3	0.000					
	(0.000)					
NumberNonViolOnsets	0.178***	19.5	78.2			
	(0.029)					
NumberViolOnsets	-0.036	-3.5	-7.0			
	(0.093)					
NeighborhoodViol	-0.227	-20.3	-5.2			
	(0.810)					
NeighborhoodNonviol	0.140	15.1	1.8			
	(1.180)					
RegimeChange1to3	-0.018	-1.8	-5.8			
	(0.052)					
Fuel Exports	-0.327	-27.9	-12.4			
	(0.430)					
North and Southeast Asia	-1.407*	-75.5				
	(0.617)					
Central and South Asia	-2.399*	-90.9				
	(1.000)					
Middle East and North Africa	-1.603**	-79.9				
	(0.600)					
Sub-Saharan Africa	-1.775***	-83.0				
	(0.497)					
Latin America	-1.029**	-64.3				
	(0.348)					
Oceania	-13.439***	-				
	(1.126)	100.0				
Constant	-11.655***			-3.159***		
	(2.514)			(0.466)		

(continued)	Expanded Model, excl. Nepal	%	%StdX	Simplified Model, excl. Nepal	%	%StdX
2 (Violent campaign onset)						
NotFree	0.871	139.0		1.156	217.6	
	(0.950)			(0.906)		
ProximitytoAutocratization	-0.384	-31.9	-8.8	0.161	17.4	4
	(0.754)			(0.757)		
NotFree*ProximityAutocrat	-0.775	-53.9	-23.9	-0.944	-61.1	-28.5
	(1.249)			(1.291)		
ProximitytoLiberalization	0.830	129.3	23.2	1.334*	279.6	40.1
	(0.634)			(0.580)		
rGDPpc	-0.000†	-0.0	-77.5			
	(0.000)					
lnPopulation	0.284*	32.8	55.1			
	(0.114)					
Military Personnel $_{t-1}$	-0.000	-0.0	-18.2			
	(0.000)					
Election	-0.117	-11.1				
	(0.309)					
Urbanization	0.009	0.9	22.1			
	(0.013)					
NonViolStabilityYears	0.019					
	(0.139)					
NonViolStabilityYears^2	0.002					
	(0.007)					
NonViolStabilityYears^3	-0.000					
	(0.000)					
ViolStabilityYears	-0.044					
	(0.088)					
ViolStabilityYears^2	0.002					
	(0.005)					
NonViolStabilityYears^3	-0.000					
	(0.000)					
NumberNonViolOnsets	-0.008	-0.8	-2.7			
	(0.059)					
NumberViolOnsets	0.299***	34.9	82.7			
	(0.044)					
NeighborhoodViol	0.013	1.3	0.3			
	(0.663)					
NeighborhoodNonviol	-0.607	-45.5	-7.4			
	(1.503)					
RegimeChange1to3	0.045	4.6	16.3			
	(0.059)					
Fuel Exports	0.043	4.4				
	(0.357)					
North and Southeast Asia	-0.997	-63.1				
	(0.651)					
Central and South Asia	-0.151	-14.1				
	(0.576)					
Middle East and North Africa	0.166	18.0				
	(0.391)					
Sub-Saharan Africa	-0.612	-45.8				
	(0.388)					
Latin America	-0.837	-56.7				
	(0.590)					
Oceania	0.891	143.7				
	(0.821)					
Constant	-9.147***			-5.222***		
	(2.500)			(0.648)		
N	3226			3762		
Log pseudolikelihood	-506.13			-678.05		
Pseudo R^2	0.156			0.019		

Notes : Clustered standard errors in parentheses, analysis of all states Free = 0
† $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Appendix Table 7Auxillary Models, Alternative Specifications of Repressive Dummies

	Alternative specifications Base Model, excl. Nepal	Alternative specifications Expamded Model
0 (No conflict onset, ref.cat)		
1 (Nonviolent conflict onset)		
altNotFree	2.137*	1.750†
	(1.067)	(1.008)
ProximitytoAutocratization	-0.435	-0.426
	(0.675)	(0.745
NotFree*ProximityAutocrat	-3.599†	-3.179
	(2.019)	(1.951)
ProximitytoLiberalization	1.065†	1.077†
	(0.551)	(0.595)
rGDPpc	-0.000	-0.000*
	(0.000)	(0.000)
lnPopulation	0.276**	0.340**
	(0.106)	(1.24)
Military Personnel $_{t-1}$	0.000*	0.000
	(0.000)	(0.000)
Election	0.785**	0.761**
	(0.264)	(0.271)
Urbanization	0.006	-0.002
	(0.007)	(0.009)
NonViolStabilityYears	0.059	0.041
	(0.123)	(0.128)
NonViolStabilityYears^2	0.000	0.002
	(0.006)	(0.006)
NonViolStabilityYears^3	-0.000	-0.000
	(0.000)	(0.000)
ViolStabilityYears	0.036	0.045
	(0.111)	(0.121)
ViolStabilityYears^2	-0.003	-0.003
	(0.005)	(0.006)
ViolStabilityYears^3	0.000	0.000
	(0.000)	(0.000)
NumberNonViolOnsets	0.159***	0.166***
	(0.025)	(0.026)
NumberViolOnsets	-0.029	-0.027
	(0.087)	(0.090)
NeighborhoodViol	-0.475	-0.209
	(0.670)	(0.645)
NeighborhoodNonviol	0.451	0.342
	(0.924)	(1.045)
RegimeChange1to3		-0.007
		(0.052)
Fuel Exports		-0.275
		(0.440)
North and Southeast Asia		-0.843
		(0.595)
Central and South Asia		-1.855*
		(0.930)
Middle East and North Africa		-0.814
		(0.526)
Sub-Saharan Africa		-1.428**
		(0.461)
Latin America		-0.694*
		(0.350)
Oceania		-12.441***
		(1.024)
Constant	-11.047***	-10.756***
	(2.155)	(2.375)

(continued)	Alternative specifications Base Model, excl. Nepal	Alternative specifications Expamded Model
2 (Violent conflict onset)		
altNotFree	1.530*	1.357†
	(0.649)	(0.710)
ProximitytoAutocratization	-0.304	-0.136
	(0.616)	(0.664)
NotFree*ProximityAutocrat	-1.404	-1.283
	(0.961)	(0.935)
ProximitytoLiberalization	0.976	0.799
	(0.603)	(0.624)
rGDPpc	-0.000*	-0.000
	(0.000)	(0.000)
lnPopulation	0.294**	0.291*
	(0.103)	(0.115)
Military Personnel $_{t-1}$	-0.000	-0.000
	(0.000)	(0.000)
Election	-0.048	-0.055
	(0.290)	(0.290)
Urbanization	0.012	0.009
	(0.011)	(0.013)
NonViolStabilityYears	-0.003	0.032
	(0.125)	(0.145)
NonViolStabilityYears^2	0.003	0.001
	(0.006)	(0.007)
NonViolStabilityYears^3	-0.000	-0.000
	(0.000)	(0.000)
ViolStabilityYears	0.061	-0.053
	(0.085)	(0.085)
ViolStabilityYears^2	0.002	0.002
	(0.005)	(0.005)
NonViolStabilityYears^3	-0.000	-0.000
	(0.000)	(0.000)
NumberNonViolOnsets	-0.007	-0.003
	(0.057)	(0.057)
NumberViolOnsets	0.305***	0.305***
	(0.044)	(0.044)
NeighborhoodViol	0.093	-0.059
	(0.541)	(0.634)
NeighborhoodNonviol	-0.539	-0.483
	(1.21)	(1.249)
RegimeChange1to3		0.055
		(0.056)
Fuel Exports		0.042
		(0.334)
North and Southeast Asia		-0.831
		(0.633)
Central and South Asia		-0.091
		(0.569)
Middle East and North Africa		0.266
		(0.399)
Sub-Saharan Africa		-0.477
		(0.369)
Latin America		-0.674
		(0.536)
Oceania		0.236
		(0.649)
Constant	-9.881***	-9.610***
	(2.202)	(2.474)
N	3,778	3761
Log pseudolikelihood	-541.54	-531.57
Pseudo R^2	0.137	0.152

Notes : Clustered standard errors in parentheses, analysis of all states Free = 0
† $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$