THE COUNTERINSURGENCY DILEMMA: THE CAUSES AND CONSEQUENCES OF STATE
REPRESSION OF HUMAN RIGHTS IN CIVIL WARS

Jason Michael Quinn, B.A., M.A.

Dissertation Prepared for the Degree of

DOCTOR OF PHILOSOPHY

UNIVERSITY OF NORTH TEXAS

May 2010

APPROVED:

T. David Mason, Major Professor
James Meernik, Committee Member
Ideen Salehyan, Committee Member
J. Michael Greig, Committee Member
John Todd, Chair, Department of Political Science
Michael Monticino, Dean of the Robert B.
Toulouse School of Graduate Studies

In this project a theory of adaptive differential insurgency growth by the mechanism of repression driven contagion is put forth to explain variation in the membership and spatial expansion of insurgencies from 1981 to 1999. As an alternative to the dominant structural approaches in the civil war literature, Part 1 of the study proposes an interactive model of insurgency growth based on Most and Starr’s opportunity and willingness framework. The findings suggest that state capacity, via its impact on state repressive behavior, plays an important gatekeeping function in selecting which minor insurgencies can grow into civil war, but contributes little to insurgency growth directly. In Part 2 of the study, I directly examine variation in insurgency membership and geographical expansion as a function of repression driven contagion. I find that repression increases the overall magnitude of insurgency activity within states, while at the same time reducing the density of insurgency activity in any one place. Despite an abundance of low intensity armed struggles against a highly diverse group of regimes around the world, I find an extremely strong and robust regularity: where repression is low – insurgencies don’t grow.
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ACKNOWLEDGEMENTS

I would like to thank all my committee members for their time and support. A special thanks to T. David Mason for ten years of mentorship and friendship; thank you for taking an interest in me many years ago and seeing it through to the end. You are up there with Elvis. I would also like to thank my parents, Mike and Billie Quinn, for all of their help which spans several decades. The biggest thanks of all has to be given to my wife, Donna, for her many years of emotional support and sacrifice. Please don’t make me pay you back. Last, but far from least, I would like to thank my 3 year old son Ayden Michael Quinn for making me have to work so much harder and for holding the universe together at its center.
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INTRODUCTION

1. Which Insurgencies Survive and Prosper?

Within minutes of entering Cuba, in the first encounter with Batista’s army at Alegria de Plo in December 1956, sixty of the eighty rebels of the 26th of July movement were dead. Che Guevara, Fidel Castro, and approximately twenty other armed guerrillas barely managed to escape into the Sierra Maestra mountains (Moreno 1970). Six months later, Guevara records in his diary that successful recruitment efforts had increased the size of the tiny rebel group to two hundred persons - an increase in group size over one thousand percent in just six months (Moreno 1970:123). By the end of the civil war, a brief twenty-six months later, the rebel group was between two and three thousand strong (Macaulay 1978). According to Guevara’s diaries, recruitment for the largest part came from those groups of peasants “…who had to endure the persecution of Batista’s military units…They fled to us for refuge…” (Che Guevara, quoted in Ratliff 1966:291).

Having met unprecedented success in Cuba, Che chose Bolivia as his next target for revolution in Latin America. In November of 1966, he and his small imported guerilla foco arrived in La Paz, Bolivia by way of Uruguay and set up a rebel base camp in the remote Nancahuazu valley. Yet after six months in the Bolivian mountains, Guevara laments in his diary on his inability to acquire a single solitary new recruit. When the Bolivian army captured Guevara and the remainder of his men on October 8, 1967, there were seventeen men remaining. What accounts for the stark difference in the recruitment success and growth of these two rebel groups over roughly the same period of time? After all, the same leadership and the same training existed in both experiments; even the terrain was very similar. As
Moreno (1970:132) rightly concludes, the differences in recruitment success between the two cases must have involved factors which “...lay beyond the immediate reach of the foco....” In other words, the determinants of membership growth were largely external or independent of the rebel group itself.

What might these external conditions have been? The empirical findings from the quantitative literature on civil war onset and the relevant country-level data from Bolivia and Cuba provide little insight into this rare natural experiment. By most accounts, Che should have had better luck building an insurgency in Bolivia than in Cuba. In the year Che entered Bolivia, the average Bolivian income was several times smaller than average incomes in Cuba in 1957, which were among the highest in all of Latin America. Bolivia had roughly eight times more mountainous territory than Cuba providing an abundance of cover for establishing rural bases. (I might add that as Che’s revolutionary experiment in the Bolivian mountains was failing miserably, next door in Peru, Abimael Guzman was building the socio-political support base that would produce one of the most destructive, deadly and protracted insurgencies in all of Latin America). What best explains the differential growth rates of these and other proto-insurgencies?

The most consistent finding from the extant civil war literature is that weak states provide hospitable environments for conducting guerrilla warfare and that is why we see most civil wars located inside poor states. Almost without exception, however, quantitative studies of civil war onset have examined only those insurgencies which did succeed in growing into a formidable challenge to an incumbent regime. An analogy might be early risers in a nascent insurgency that give a group of scholars a post-dated check for a given threshold of severity to
be cashed at a later date when the aggregate level of violence has been jointly produced by the warring parties. A fundamental problem is that scholars end up ignoring the checks that bounce, which are, arguably, the most important cases for conflict management and counterinsurgency policy.

In Part I of this study, I make a simple change to the prevailing research question in the civil war onset literature, shifting the emphases from predicting the geographical location of successful insurgencies (i.e., civil war), to explaining which insurgencies succeed or fail, given the opportunity. I find that changing the research question from “where do insurgencies survive and prosper?” to “which insurgencies survive and prosper?” produces two sets of empirical findings that are so different from one another, the most plausible explanation is that they are two separate research domains. How could the conditions that predict the location of successful insurgencies from a sample of all nation-states be fundamentally different from the conditions associated with whether a particular time spell of insurgency activity produces a civil war or not? One hypothesis involves selection effects. As I will demonstrate throughout this study, the conditions that reliably predict which countries have the highest risk of civil war have little to do with insurgency success or failure. Instead, country characteristics have an indirect function of selecting which insurgencies will enter a much smaller sample where growth is a possibility. I will demonstrate that country characteristics serve mostly a gate-keeping function only; once an oppositional movement passes through the gate, few of the conditions theorized as favoring insurgency growth operate as expected. The conditions that create insurgencies and the conditions that lead to insurgency growth appear to operate differently. In addition to yielding a better understanding of insurgency growth, a theoretical approach that controls for
selection effects while attempting to follow the escalatory phases of civil conflict from the beginning of organized violence to its end point, also helps resolve several persistent anomalies that exist in the civil war literature which stem from apparent disconnects in tracing the escalatory process. The findings from this project offer a new and consistent explanation for these anomalies and how they fit into the overall puzzle of insurgency growth.

2. Insurgency Growth and Anomalies in the Civil War Research Program

One particularly perplexing anomaly involves the role of ethnic demography and the risk of civil war. This anomaly, and the several that follow, can be illustrated more clearly with the use of Figure 1. The figure shows a continuum of escalating civil conflict, with overlapping populations, starting with a population of protest events and ending with a population of civil wars. The events that make up the total population of cases in each category of severity are drawn from a smaller group of cases from the previous stage. Out of a very large population of oppositional protests, for example, a much smaller number of these environments will produce terrorism campaigns. Most terrorism campaigns will either remain static or devolve, while a much smaller number will grow into minor insurgencies. Likewise, out of an existing population of minor insurgencies, the majority of them will either remain a minor conflict or be terminated at some point, while a much smaller number will grow into major civil wars. Thus, each stage of escalation has a small number of cases that will advance to the next stage of escalation with the population of cases at each stage getting smaller and smaller as the probability of successful escalation declines.

The anomaly regarding a society’s degree of ethnic fragmentation and its risk of civil war involves the selection process whereby minor insurgencies grow into major civil wars. The
puzzle is clearly evident in the sensitivity analyses of civil war onset performed by Hegre and Sambanis (2006) which contrasts one dataset which includes minor conflicts to another dataset which is limited to major civil wars only.

Put simply, the amount of ethnic fragmentation within a national society, depending on the ratio of minor conflicts to major conflicts in the sample being analyzed, is either the most significant concept yet discovered in predicting civil conflict, or not significant at all. Using the PRIO/Uppsala Internal Armed Conflict Dataset (Gleditsch et al. 2002) that contains 252 civil conflicts, Sambanis tested 88 variables associated with 18 different conceptual categories. The
The most significant category was ethnic fragmentation. Almost every variable used to capture ethnic fragmentation was highly significant and positively correlated with the onset of internal armed conflict. (e.g., “ethnic heterogeneity index,” “linguistic component of ethnic heterogeneity index,” “ethnic fractionalization index,” “ethnolinguistic diversity,” “ethnic fractionalization squared,” “religious component of ethnic heterogeneity index” and “ethnolinguistic diversity squared”). It is important to mention that the PRIO dataset has a much lower threshold of war-related casualties for inclusion (i.e., 25 deaths per year) and as a result contains 127 more civil conflicts than Sambanis (2000) and over 100 more conflicts than Sambanis (2004).

When the same onset model was estimated using the Sambanis (2004) civil war dataset, which contains only civil wars that have produced at least one thousand battle deaths (N=152), not a single one of the seven previously mentioned variables reached statistical significance. Given that major civil wars obviously start out as smaller minor conflicts, how could ethnic fragmentation be so important at one level of severity and completely lacking at the next level? This finding suggests that the majority of major civil wars, for some unknown reason, grow out of a small group of minor conflicts not located in ethnically fragmented states, which would be highly unusual given that ethnic fragmentation appears to be the strongest predictor of minor conflicts. This anomaly strongly insinuates that the conditions that create minor insurgencies and the conditions associated with minor insurgencies growing into major insurgencies are either different conditions or conditions that operate differently.

A second anomaly involves the role of mountainous terrain in increasing the risk of civil war. Mountainous terrain actually presents two puzzles related to the origins of civil war: one
temporal; one spatial. The first discrepancy is similar to the one described above; mountainous terrain mysteriously becomes significant at one point in the escalation process but appears to be unimportant before that point. The second discrepancy is that even when mountainous terrain begins to become important, its effects appear to operate only at the cross-national level of comparison and not at the sub-national level or the conflict level.

Many studies have found a significant positive relationship between civil war onset and the percentage of overall territory within the state that is mountainous (Hegre and Sambanis 2006; Fearon and Laitin 2003; Blimes 2006; Ross 2006; Hendrix and Glaser 2007; Lujala et al. 2005; Suzuki 2007). Many other studies, however, have not found this relationship. The results from Hegre and Sambanis (2006) provide an answer to this discrepancy although not an explanation. Their analysis shows that the mixed findings concerning mountainous terrain also result from the ratio of minor to major civil conflicts in the sample under investigation. Mountainous terrain is not significant when the sample being analyzed contains a large number of minor conflicts but is significant when predicting only major civil wars (Hegre and Sambanis 2006). Thus, the pattern is the mirror image of the pattern that ethnic fragmentation follows. Mountainous terrain might assist insurgents in escalating their fight, as suggested by Fearon and Laitin (2003), although this relationship has not been tested. Doing so would require predicting which minor conflicts become major conflicts, as opposed to trying to predict civil war from a pooled dataset containing every-available country year. It is also not clear why mountainous terrain would not also benefit the escalation processes that produce minor conflicts. Would not the presence of mountainous terrain assist violent protesters or terrorists who wanted to wage a larger guerrilla campaign?
Nevertheless, recent research by Bugaug and Lujala (2005) seriously undermines the argument that mountainous terrain provides any significant utility in the production of insurgency. Using GIS mapping software to determine the amount of mountainous terrain within the actual borders of civil conflict zones, they find that conflicts tend to be fought away from the mountains. They found that “...conflict zones are – contrary to general belief – less mountainous and forested than the countries in which they occur...” (2005:410). Buhaug and Lujala performed two sets of regression analyses on the duration of civil conflicts, one conducted at a cross-national level of analysis and the other conducted at a sub-national level of analysis (i.e., comparing civil conflicts instead of the countries that house them). When comparing countries to each other, the amount of mountainous terrain within the nation had a significant positive impact on duration. When comparing conflicts with each other, using the amount of mountainous terrain in the actual conflict zone, no significant effect was found. In other words, mountainous terrain significantly effects conflict dynamics even when the mountainous terrain is located in another part of the state - away from the conflict zone.

Buhaug and Rod (2006) created a GIS based dataset of African conflicts where the unit of analysis is 100x100 km cells superimposed over the continent. The percentage of mountainous territory inside each cell was coded and logistic regressions were performed in an effort to predict the location of territorial conflicts and governmental conflicts. For territorial conflicts, the coefficient for mountainous terrain was highly significant but negative; territorial conflicts mostly existed in “sparsely wooded low-lands” (2006:327) away from the mountains. As for the location of revolutionary conflicts, mountainous terrain was not significant. In summary, when predicting the actual location of civil wars, mountainous terrain either doesn’t
matter or has an adverse effect on insurgency production. Buhaug and Lujala interpret their finding as evidence of an ecological fallacy (i.e., aggregate data on a country does not accurately depict local “on the ground” conditions inside the country). In other words, the relationship is spurious. I find much evidence in this study that the relationship is, in fact, not spurious. The findings in chapter 2, 6 and 7 provide an explanation that is perfectly consistent with the temporal minor/major conflict discrepancies and the spatial country/conflict discrepancies.

The last anomaly involves the role of poverty and insurgency production and appears to follow these same patterns of discrepancy (i.e., temporal and spatial) although it is less demonstrable at this point (future chapters show these discrepancies much clearer). If we assume that poverty both increases the level of grievances in a society while decreasing the capacity of the state to accommodate those grievances and to effectively deal with oppositional violence before it gets out of hand, then poverty should produce insurgencies in a roughly linear fashion. As poverty increases, oppositional activity goes up while the ability of the government to manage that oppositional activity effectively goes down. Numerous studies of low intensity conflicts, such as Hamilton and Hamilton (1983:50), have found that domestic terrorism increases with economic development. Even while controlling for a democratic government, they found that higher levels of GNP per capita increased the amount of domestic terrorism within states. Li and Schaub (2004) found their index of government capability (an index of GDP per capita, military size and military spending) significantly associated with higher levels of terrorism. Thus, higher levels of resources, both economic and military related, were associated with more political violence. There is also some evidence that while most civil wars
occur in poor states, the intrinsic benefits of poverty to insurgency growth at the conflict level are lacking. Evidence is discussed in more detail in chapters 1, 2, 6 and 7.

This study puts forth a theory and several empirical tests that solve these anomalies, or more accurately, reconciles them with the bulk of non-anomalous empirical findings from the civil war literature. I am able to show that covariates associated with state capacity (such as poverty and mountainous terrain) and commonly seen as facilitating insurgency growth are actually selection mechanisms that sort nascent insurgencies into two populations – a control group and a treatment group. In the control group, violent oppositional groups are pursued by police and state investigators in an environment where the state maintains respectable human rights conditions. In contrast, the treatment group is comprised of a small number of violent oppositional groups who were operating in states where the government responded to oppositional activity with repressive violence. I argue that only insurgencies that make it into the treatment group will ever have a chance of becoming major civil wars. State repressive violence is a necessary condition for insurgency growth (although not always sufficient). Specifically, selection bias results from the fact that the assignment of nascent insurgencies either to the control group (non-repressive environments) or the treatment group (repressive environments) is not random (Achen 1986) but determined by state capacity.

Making causal inferences in the presence of selection effects can be problematic. As Heckman argues “...formal statistics unaided by context and subject-matter considerations... cannot solve causal problems...” Instead, the “...decision about the appropriate statistical procedure requires information outside of statistics. Such judgments are inherently controversial.” (1989:166). In this project, I put forth a theory of adaptive differential
insurgency growth by the mechanism of repression driven contagion arguing that whether or not a nascent insurgency grows into a major civil war, which allows the conflict to become a case in any large-N cross national analysis, depends on variables that selected for insurgency opportunity and for a state repressive response many years before the civil war started and these variables are largely unrelated to insurgency growth directly (but are correlated with growth).

Consider the most robust empirical relationship found in the civil war onset literature, the ubiquitous link between poverty and civil war susceptibility. Many studies have found a negative and statistically significant correlation between national economic prosperity and the likelihood of civil war (Abouharb 2005; Barbieri and Reuveny 2005; Besançon 2005; Blimes 2006; Buhaug 2006; Caprioli 2005; Collier and Hoeffler 2002a; Collier and Hoeffler 2004a; Collier and Hoeffler 2004b; De Soysa and Neumayer 2007; Eberle et al 2003; Elbadawi and Sambanis 2002; Fearon 2005; Fearon and Laitin 2003; Gleditsch 2007; Hauge and Ellingsen 1998; Hegre and Sambanis 2006; Henderson and Singer 2000; Hendrix and Glaser 2007; Jusu 2005; Krause and Suzuki 2005; Raleigh and Urdal 2007; Reynal-Querol 2004; Ross 2006; Salehyan 2007; Sambanis 2001; Thyne 2006). Examining a multitude of insurgency dynamics throughout this project, such as cross-national variation in opportunities for escalation and civil war production, as well as variation in the membership size of insurgencies, variation in the geographical dispersion of insurgencies, as well as the density of insurgency operations within the state, I consistently find poverty (and rough terrain) unimportant in explaining insurgency growth. Therefore, poverty is the most significant condition when comparing countries but
largely unimportant when comparing conflicts within countries that have them. How can we construct a research design that can make sense of this causal disconnect?

There are only three patterns that could explain such a relationship and the evidence points to the importance of selection effects. One possible pattern is that poor states are inherently associated with more overall oppositional violence; hence, there are more opportunities for insurgeries to grow in poor states, and a few will if for no other reason than luck. A second possibility is that poor states do not experience more oppositional group violence but do produce very favorable environments for conducting guerrilla warfare. Thus, poor states do not generate more insurgency opportunities, but when violent opposition groups do occasionally emerge, they are atypically successful in poor states. A third explanation may be that poverty does not produce more opportunities for rebellion nor better conditions for insurgency growth. Instead, poverty’s only connection to insurgency growth is indirect: it selects for state repressive behavior (Poe & Tate 1994).

The first claim - there are simply more opportunities in poor states for insurgeries to get going and some will – is easily tested. The Global Terrorism Database contains thousands of known political opposition groups committing violent attacks against government targets within states both inside and outside of civil war contexts. By creating a cross-national sample of country years outside of civil war, we can explore whether there is any relationship between poverty and insurgency opportunities. If we find that poor states do not experience more oppositional violence than other states, outside of civil war contexts, this falsifies Claim 1, but not Claim 2.
Claim 2 is that poverty may be unrelated to opportunity, but poor states are incompetent at counterinsurgency and thus provide favorable conditions for insurgency growth. Thus, given a sample of states all facing oppositional group violence outside of civil war, insurgency growth will be the greatest inside poor states. If poverty provides intrinsic benefits to insurgents and we have a large number of insurgency opportunities to follow over time, then insurgencies in poor states should be more likely to grow into major civil wars. There is also a second, more direct way, to test whether poverty leads to growth: we can look at rebel recruitment in a sample of ongoing civil wars. Weak counterinsurgency efforts and weak control over national territory should boost rebel recruitment and hence boost rebel group size. Moreover, as Collier and Hoeffler (2003) specifically argue, poverty lowers the opportunity costs of participation in insurgency, thereby facilitating rebel recruitment.

Claim 3 argues that poverty, or weak state capacity, is influential at some point in time in the *decision calculus* of the state on how to respond to oppositional group violence but provides little direct enhancement to insurgency growth. Weak states are much more likely to respond to oppositional movements with repressive violence, but once the decision to repress has been made, poverty or state capacity has little explanatory power in explaining which insurgencies prosper. In chapter 2, I find that poverty is not important in predicting which insurgencies will escalate into major civil wars. In chapter 6, I find that poverty is not important in explaining variation in rebel group size during civil wars. In chapter 7, I find that poverty provides no benefits regarding the geographical expansion of insurgencies. On the contrary, insurgencies tend to expand easier in more resource-rich environments.
Another benefit of controlling for selection effects in the study of insurgency and civil war is that it creates a robust and stringent framework for evaluating, not only selection effects, but also related issues, such as endogeneity, reverse causality and bi-directionality in conflict processes. The present study contributes to the “repression-dissent paradox” literature by examining insurgency and counterinsurgency using Most and Starr’s (1993) “Opportunity and Willingness” framework for conflict processes. The repression-dissent paradox revolves around researcher’s disagreements on whether repression generally escalates or deters dissident mobilization and the conditions associated with escalation or deterrence. Brockett (1993:458) aptly summarizes a common perception in the literature, namely that, “…regime violence smothers popular mobilization under some circumstances, but at other times similar (or even greater) levels of violence will provoke mass collective action rather than pacify the target population. This paradox remains even when the usual explanatory variables, such as the level of socioeconomic grievances or political regime type, are held constant.” Similarly, Lichbach (1987) finds that “…deterrence works. And then again deterrence doesn’t work.” More recently, Hafez (2006:72) writes,

assessing the impact of repression on movement behavior is as difficult as it is important. Theoretically, there is little agreement regarding the logical consequences of state repression on movement behavior. Some contend that repression increases the costs of collective action so as to make it unlikely. Others maintain that repression generates additional grievances that motivate further mobilization to punish an “unjust” opponent.

Brockett’s solution to the repression-dissent paradox is to focus on the timing of the state’s use of repressive violence. According to Brockett, those movements that are most likely to survive and grow are those that are in an ascendency phase of mobilization when the state responds with repressive violence. I take Brockett’s argument one logical step further: those
groups best able to survive organized violence are those best able to use organized violence themselves. In chapter 1, I argue that those movements that are most likely to survive and grow in the face of state repression are those groups in an ascendency phase of violence.

This neatly complements Mason & Krane’s (1989) emphasis on a rebel group’s ability to provide safe haven to victims of state violence as an important excludable private good that rebels can offer in exchange for support. Rebel groups that are best able to provide protection should be those groups who are already engaging in violence themselves. This study contributes new evidence that the repression-dissent paradox is largely categorical rather than paradoxical. When states respond with repressive violence to oppositional groups already organized for violence, the result is almost always escalating violence.

3. Several Puzzles, Two Temporal Phases, One Theory

In this project I put forth a theory of adaptive differential insurgency growth by the mechanism of repression driven contagion. The principal objective is not to prove that repression is usually counterproductive; that fact has been well documented in the qualitative literature on civil war. Instead, I desire to show how physical actions of the state produce parallel physical actions in an insurgency that produces insurgency growth. By doing so, I hope that we might also gain a better understanding of why states repress if it is so often counterproductive. I hope to show that (1) the construction of a large socio-political support base is the most important factor in sustaining an insurgency, and (2) that such a support base must be built, in large part, before the civil war begins. Furthermore, and most importantly, I argue that it cannot be built without systemic repressive violence by the state’s military and police forces. If a sizable number of initial recruits come from the socio-political support
system which is built, in large part, before the civil war begins, and the size of the support system depends on the presence and degree of state repressive violence, then an important factor in explaining the size of a rebel group during a civil war is how repressive the state was before the civil war began. If repression is a selection mechanism determining which insurgencies have the potential to grow into civil wars, then repression should have the largest amount of explanatory power long before the civil war starts. Once a civil war starts, contemporaneous levels of state repression during the conflict may help sustain the insurgency, but if repression selects small insurgencies to become large ones, the amount of variation in repression inside a sample of states in civil war will be much smaller than the amount of variation in a sample of states outside civil war contexts. In other words, if repression is close to a necessary condition for insurgency growth, successful insurgencies will almost always be housed within repressive states.

As for the spatial expansion of insurgencies, it is largely taken for granted that most contemporary civil wars are fought using guerrilla warfare (i.e., a socio-political support base at the center funding irregular military units operating at the periphery), although almost none start out that way. Insurgency expansion, instead of resulting from deliberate pre-war planning by oppositional leaders, is more often an unintended by-product of forced displacement as insurgency leaders, rank and file members, and suspected supporters flee expanding state violence. Spatial moves into areas of less government repression result in a consistent pattern of parallel tactical adaptations which ultimately generate irregular warfare. Once in motion, harsh repression and insurgency expansion create feedback loops, where repression causes further geographical expansion, which in turn generates more and more
targets for the state to repress via denunciations from informants in areas of overlapping territorial contestation (Kalyvas 2006).

State repression may not be a sufficient condition for insurgency growth but I argue it is a necessary one. While some insurgencies facing harsh repression will lack the necessary antecedents for growth, insurgencies inside non-repressive or mildly-repressive regimes generally do not grow beyond a few hundred members. Using a variety of different data and empirical tests conducted at several different levels of analysis, I find a persistent and robust relationship: where state repression is low, insurgencies don’t grow.

4. Organization of the Project

To better understand the relationship between state behavior and civil war, two questions must be addressed. First, which states inside and outside of civil war contexts are the most repressive? Second, what are the consequences of harsh repressive violence by states in conflict settings, both outside and inside of civil war? I apply these two questions to two samples of states: one group of states experiencing oppositional group violence but not civil war, and another group of states fighting ongoing civil wars. Examining the causes and consequences of state repressive violence outside of civil war and inside of civil war yields four motivating questions that structure the project:

Part I: Outside Civil War

A) Among a group of states, all facing spells of low-intensity oppositional group violence outside of civil war, which states will respond with harsh repression and which states will maintain respectable human rights conditions?
B) How important is the behavioral response of the state in determining which spells of oppositional group violence, outside of civil war, will escalate into civil war in the future?

Part II: Inside Civil War

A) Among a group of states fighting ongoing civil wars, which states have the most repressive counterinsurgency campaigns?

B) How important is the harshness of the state’s counterinsurgency response in a civil war in determining which rebel groups will be able to successfully expand their recruitment efforts and the geographic scope of their operations?

I demonstrate the causal relationship between state repressive behavior and insurgency growth in Part I and Part II of the project. Part I looks at state behavior and oppositional group violence outside of civil war with the objective of predicting, in advance, which opportunities for insurgency growth will in fact produce a civil war. In Part II I use a dataset of every available country year spent in civil war (N=658) from 1981 to 1999, to examine insurgency growth both in terms of membership and geographical expansion. I argue that when state repression increases, so does the size of insurgency organizations that are in a structural position to benefit from such repression, as more and more of the surrounding population comes to oppose the incumbent regime. As rebel groups become larger and stronger, they become better able to achieve territorial control, which further enables them to capitalize on the state’s use of violence (i.e., growth begets growth). Chapters 3 and 4 explore this particular dynamic of state repression and the relative military capabilities of the combatants as part of a larger research design that examines cross-national variation in the harshness of counterinsurgency.
Examining state repression of physical integrity rights during civil war as the dependent variable, I argue that state capacity, the military balance of power, and ecological factors such as geography and the nature of the dispute, are related to the territorial dynamics of the war, which in turn, affect the scope and severity of state repressive violence.
PART I

OUTSIDE OF CIVIL WAR: EMERGENCE AND ESCALATION OF VIOLENCE
Chapter 1

Opportunity and Willingness in Insurgency,

Counterinsurgency and Civil War

One of the most consistent claims in the civil war literature is that weak states form the core risk set of nations susceptible to civil war. Two leading theoretical approaches in the area of civil war onset (Collier 2000; Collier and Hoeffler 2004; Fearon & Laitin 2003) attribute the outbreak of civil war to the presence of a hospitable environment for the survival and production of insurgency. In a series of works, Collier (2000) has put forth a general argument that the causes of rebellion matter far less than those conditions which make rebellion viable. Collier’s “rebellion as business” approach argues that civil conflict is most likely when the opportunity costs of participation are lowest, that is, when rebel work pays more than what the legitimate market is offering recruits for their time and energy. According to the logic of this “greed” hypothesis, all societies have a number of people who would be attracted to rebellion as an occupational choice, given the right payoff. As Collier, Hoeffler and Soderbom (2004:256) argue, “Rebellions will occur where and only where they are profitable (although they need not be motivated by profit). They will be profitable where revenues during conflict are atypically high and costs atypically low.” In a similar argument, Fearon and Laitin (2003) argue that certain environments tend to favor the production of guerrilla warfare:

Decolonization from the 1940s through the 1970s gave birth to a large number of financially, bureaucratically, and militarily weak states...The conditions that favor insurgency – in particular, state weakness marked by poverty, a large population, and instability – are better predictors of which countries are at risk for civil war than are indicators of ethnic and religious diversity or measures of grievances such as economic inequality, lack of democracy or civil liberties, or state discrimination against minority religions or languages (2003:88).
The parsimony of these capacity-centered theories of civil war best explains their current prominence in the literature. Knowing little about a particular state except how poor it is, their models predict where civil wars have occurred quite well. Knowing a particular state’s history, region, culture, political and civil liberties, religious divisions, ethnic divisions, class inequalities, trading status, or export profile adds only marginally to our predictive ability. While these works have redirected the research program on civil war onset, I argue that both approaches overlook a critical factor: the strong statistical link between state capacity and state behavior.

I identify three major problems that stem from the over-emphases on and misspecification of state capacity in predicting civil war onset. First, such approaches are tautological and deterministic. They are tautological in that they model insurgency and civil war as outcomes rather than as processes and are thereby unable to explain counterfactuals (i.e., where civil wars could have occurred but did not). Second, both approaches treat every state as if it has the same risk of experiencing civil war, ignoring whether actual opportunities for civil war exist in the form of insurgencies that may or may not escalate depending on favorable conditions. Third, both approaches assume that all states respond similarly to insurgencies. This is apparent in the tendency of both approaches to model civil war in terms of the environmental conditions that make rebellion more or less practical, as with Fearon and Laitin’s emphasis on the ability of insurgents to avoid the state and Collier’s emphasis on markets. Neither approach attempts to control for the statistically robust linkage between state capacity and state repressive behavior (Poe and Tate 1994). Furthermore, as I will show, most of the assumed linkages between state capacity and “favorable conditions” for insurgency do not hold
up to empirical scrutiny once they are isolated and examined as individual testable postulates. In fact, the reverse appears to be better supported: weak states are among the riskiest environments for insurgents and their supporters. The results suggest that state capacity is at best an indirect cause of civil war to the extent that state capacity influences state behavior.

As a solution to these problems I propose a process-oriented approach that controls for structure and agency (or opportunity and willingness) in the study of insurgency and civil war (Sprout and Sprout 1964; Most and Starr 1989). As an alternative to the structural determinism that currently dominates the civil war literature, where actions are driven almost entirely by the structural conditions of the environment, I propose an interactive model of civil war based on Most and Starr’s (1989) reformulation of the “the ecological triad.” Originally put forth by Sprout & Sprout (1965; 1968) as a way to study the interactions between agents and structures, the ecological triad calls

for the study of both entity and environment, and most importantly, how the two are related...[D]ecision makers or small groups...are surrounded by factors that structure the nature of the decision, the options available, the consequences, costs, and benefits of those options...[T]his can be captured only by looking at all three parts of the ecological triad (Most and Starr 1989:29).

Most and Starr (1989) reformulated the ecological triad to better fit conflict processes by focusing on opportunity and willingness in agent/structure interactions. Opportunity refers to the actual possibilities for action (i.e., what a human being or group can do is constrained by an objective reality and possibilities within the environment). Willingness refers to the decision-making process that ultimately shapes behavior within that environment. The fundamental premise of the framework is that the environmental level, the decision making level and the
interaction of both opportunity and willingness are equally necessary to explain an event or outcome.

I construct a three stage analysis that examines (1) which states in the international system face actual opportunities for insurgencies to develop (The Risk Set), (2) the willingness of states to engage in harsh state repression during these opportunities (The State’s Response), and (3) the interaction of state repressiveness with insurgency opportunity in indentifying which opportunistic environments for insurgency are more or less likely to produce a future civil war (The Consequences).

1.1 Three Problems with State Capacity-Centered Theories of Civil War

1.1.1 Environmental Structuralism is Tautological and Deterministic

The current dominant theoretical approaches to civil war argue that nations with physical conditions that are favorable for conducting irregular warfare are more likely to experience civil war. Since an insurgency could not have been successful without favorable conditions, little is really explained. If observation has revealed that certain environments produce more civil wars than other environments and scholars assigned a label to those environments (i.e., weak states) then we have a useful correlate for locating civil wars. But if we then go on to say that the label we gave those environments best explains why they have so many civil wars, then we are left with circular reasoning. Question: Where do insurgencies survive and prosper? Answer: Where they have the most favorable conditions. Question: Where do they have the most favorable conditions? Answer: Where they survive and prosper. How do we get out of this apparent tautology?
It would require a research design that takes into account both opportunity and willingness for every state and every violent oppositional group. First, the analysis would identify opportunities for escalation, the true risk set for civil war. Second, using only these insurgency opportunities, it would examine the willingness of the state in its behavioral response. The inclusion of opportunity and willingness in civil war modeling removes any and all tautology by identifying the environments where escalation into civil war is a possibility and then predicting well in advance which environments will produce civil war.

Varying the parameters of organized violence by expanding the temporal margins encapsulating it allows one solution. Like previous works, I am interested in predicting the onset of major civil wars, but theoretically I treat onset as an indicator of a highly successful mobilization process. I am equally interested in the other side of the dependent variable: those insurgencies that were crushed, failed to get off the ground beyond a few acts of terrorism, or gradually devolved from a lack of social support. Although the term ‘insurgency’ is often used synonymously to describe various types and degrees of civil conflict (i.e., revolts, rebellions, revolutions, terrorism, guerrilla warfare and civil war) its most frequent usage in counterinsurgency involves the early stages of an armed struggle against a governmental authority, that has not yet escalated to civil war. Ideally, insurgency should be operationalized as a phase that generally starts with terrorism by known oppositional groups and, if able to survive and grow (which rarely happens), might develop into a major civil war. This definition of insurgency is quite common in the literature on counterinsurgency and in ethnographic depictions of escalating political violence.
Galula’s influential *Counterinsurgency Warfare* (1964) was one of the first works to treat insurgency as a phase in a hierarchy of oppositional violence that sometimes ends in civil war. Galula (1964:4) defines an insurgency as “…a protracted struggle conducted…step by step, in order to attain specific intermediate objectives leading finally to the overthrow of the existing order…its beginnings are so vague that to determine exactly when an insurgency starts is a difficult legal, political, and historical problem….” Much in response to the works of Mao on insurgency tactics and strategy, other scholars began to examine insurgency as part of a process of escalation. Kitson (1971), for example, treats insurgency as a strength building exercise whereby the insurgents use guerrilla warfare to build a formidable military organization that one day might defeat the government using more conventional combat strategy and tactics. “[B]ut in the earliest stages” writes Kitson, “….the war is fought by people who strike at a time and place of their own choosing and then disappear” (1971:95). In international law, insurgency generally refers to violence “…against a constituted government that falls short of revolution, rebellion, or civil war” (Scruton 1982:226). Hamilton (1998:21) provides one of the most theoretically useful definitions of insurgency, one that gives the use of irregular warfare an equal standing with social and political mobilization while emphasizing timing and process:

Insurgency is a political-military conflict...[P]olitical subversion, selective terrorism, and guerrilla operations play an integral, if not primary, role in the outcome...Insurgency is a strategic political development that implements these tactics as a means to sustain itself until further development can occur. Consequently, an insurgency, as a type of war, may lead to and be part of a large conventional conflict, revolution, or civil war...Insurgencies do not have the capacity to be a revolution or civil war, but with staying power and continued support can ignite either.

1.1.2 Focusing on Capacity Ignores Realistic Opportunities for Escalation

Civil war models have not included realistic opportunities for low level insurgencies to
escalate into civil war. Onset studies have almost all attempted to predict the onset of civil war in every available country year. The role of mountainous terrain in insurgency is expected to have the same effect on the risk of civil war in the Swiss Alps as in the Gorongosa Mountains of Mozambique or the Peruvian Andes. Fleeing to the mountains is not necessary in most states that experience oppositional group violence. Previous research designs leave no way to consider counterfactual situations where an insurgent organization had an opportunity to grow but failed, or where a state had the opportunity to harshly repress but refrained from doing so. This is mostly the result of a lack of data – we only have datasets made up of those civil conflicts that met a relatively high threshold of violence.

Ignoring opportunity in theoretical approaches to civil war creates two important sample selection problems. First, by not considering oppositional violence against the state, short of civil war, previous empirical models have included all available country years in their sample, a strategy which equates the absence of civil war onset in one country that faced no oppositional violence whatsoever to the absence of civil war in another country where oppositional violence was taking place but did not escalate into civil war. Regardless of how dismal the popularity of the current regime may be, dissidents cannot support or join an armed movement that has yet to emerge. Insurgencies cannot escalate into civil war without the opportunity. Thus, we have not been able to reliably evaluate the effect of state behavior or environmental conditions in counterinsurgency campaigns because we have collected and analyzed data for only one side of the dependent variable, i.e., insurgencies that did succeed in making it to civil war.
Several recent works have attempted to examine civil war onset as the end point of an interactive process between a government and rebel group that begins with low level dissident activity (Regan and Norton 2005; Young 2008). Both of these studies include every available country-year in their analyses and thus suffer from the same selection problem as discussed above. More importantly, because these works attempt to follow civil war as a process but use a pooled sample of all available country-years, they create a second sample selection problem. They try to follow escalating violence in a sample where civil wars are already ongoing, leaving no way to differentiate between violence that leads to civil war and violence that results from civil war. I argue that a process model needs to start with periods of time within states where civil war does not exist and then predict where and when civil war will develop in the future.

Regan and Norton (2005), for example, attempt to advance process-oriented approaches to civil war by examining the impact of the same group of independent variables at three different levels of civil conflict: the prevalence of non-violent protest, the severity of rebellion, and the presence of civil war for every country-year. They find that state repression decreases non-violent protest but tends to escalate rebellion. All three dependent variables in their models (non-violent protest, rebellion, and civil war), however, are analyzed using the same sample of pooled country-years. Thus, the impact of state capacity and state repression on non-violent protest is calculated using a sample of country years that includes ongoing civil wars. Because levels of state repression will always be higher during civil war, using a sample of country-years that contains ongoing civil wars to study non-violent protest behavior is very problematic. Let us assume, for example, that state repression is either positively correlated with protest activity or has no relationship at all with protest. If state repression is higher during
civil war (which we already know to be the case; see Poe & Tate 1994), and levels of non-violent protest are lower during civil war, then a pooled cross-sectional sample of country-years containing civil war and non-civil war years will always produce a strong correlation between lower levels of protest and higher levels of repression. Similarly, let us assume that repression does not make civil war more likely, but instead states fighting ongoing civil wars are more repressive than states not fighting ongoing civil wars. In a pooled sample of country years, some containing civil war and some not, the prevalence of civil war in any given year will always be strongly associated with higher levels of repression. Given the sample design, Regan and Norton’s (2005) findings are almost unavoidable.

Another recent process oriented approach is Young’s (2008) path to civil war model. Using a cross-national sample of all available country years from 1976 to 1999, Young finds higher levels of dissident behavior inside weak states (e.g., states with less popular support, fewer expenditures, previously higher levels of state repression and previous dissident activity). Weak states are also found to be the most repressive states and the most likely to experience the onset of civil war. All in all, Young’s findings confirm and expand the weak state argument. Weak states experience more opposition and are more likely to repress, which creates a cycle of violence leading to civil war.

Young (2008) makes an important contribution in explaining why some weak states, but not other weak states, develop civil war. I find, however, that the design suffers from the same limitations as Regan and Norton’s (2005) study, namely, that it purports to be a process model, which follows escalating violence over time, while in reality it merely examines different levels of conflict severity during the same period of time using a sample of states simultaneously
experiencing very different levels of conflict severity. Young’s analysis starts by modeling
dissident activity using Banks’ (1996) data. However, Banks’ index includes the presence of
guerilla warfare as the peak measure. The index is then applied to a sample of all available
country years from 1976 to 1999 that includes all ongoing civil wars. Thus, Young’s path to civil
war begins with civil war; he estimates a sample that contains ongoing civil wars using a
categorical dependent variable which contains civil war. Strong correlations were found
between repression, weak states and civil war; the problem is that we already knew that civil
wars occur in weak states and that states fighting ongoing civil wars are far more repressive
than states not fighting ongoing civil wars (Poe & Tate 1994).

1.1.3 Focusing on Conditions for Guerrilla Warfare Assumes a Uniform State Response

I also challenge the assumption that how states respond to insurgency is so uniform that
the only variation worth considering is the ability of the rebels to avoid the state’s coercive
machinery. Focusing primarily on conditions favorable to guerrilla warfare makes sense only if
cross-national research showed that state behavior was relatively consistent across states. This
implicit assumption of uniformity in state repressiveness that is presumably mediated by state
capacity in producing variation in the probability of civil war onset across states has in no way
been demonstrated empirically.

Statistical models of civil war onset point directly to variation in state repression as an
important, although omitted, cause of civil war. Some of the most important variables
identified in recent models predicting civil war onset are the same variables previously found to
be predictors of higher levels of state repression of physical integrity rights among a cross-
national sample of states. Numerous studies on the determinates of state repression of physical
integrity rights have found that weaker states with lower levels of economic development, lower levels of democratization and larger populations engage in significantly greater levels of physical integrity violations against their populations relative to wealthier and more democratic states (Poe & Tate 1994; Poe, Tate & Keith 1999; Harrelson-Stephens & Callaway 2003; Richards, Gelleny and Sacko 2001). Fearon and Laitin (2003), in what would become the most-cited study on the onset of civil war, found these same variables to be strong predictors of the onset of civil war. They argue that insurgency is most likely to occur within states that are financially and organizationally weak because these conditions are best suited to the survival of insurgency. In particular they argued that,

Most important for the prospects of a nascent insurgency…are the government’s police and military capabilities and the reach of government institutions into rural areas. Insurgents are better able to survive and prosper if the government and military they oppose are relatively weak... (2003: 80).

While lower levels of income may proxy a number of conditions that affect the probability of civil war, the authors explicitly interpret their findings on poverty as indicative of a state with weak coercive machinery: “We believe that the strong results for per capita income...are due largely to its acting as a proxy for state military and police strength relative to potential insurgents...” (2003:80). In a later study, Hegre and Sambanis (2006:514), performing sensitivity tests on models of civil war onset, also include income per capita “as a measure of the economic opportunity costs of the war...or of some aspect of state capacity....” They too find that economically weaker states have the highest probability of civil war onset, and the relationship is robust across alternative specifications.
1.2 Unpacking the Weak State - Survive and Prosper Argument

The “weak state” label has been utilized by a range of scholars studying the developing world. Thomas (1987) defines a state’s strength or weakness in terms of its coercive and non-coercive institutional capacities. For Buzan (1991) a weak state lacks institutional capacity but also lacks ideational and popular support; in weak states people do not identify with the state. Focusing on the consequences of these deficits, Migdal (1988) defines a weak state as one that cannot subordinate traditional local authority structures to the institutional authority of the state. Due to its lack of capacity, control, and support, the state’s plans are continuously co-opted, captured, or derailed by informal social networks that tie citizen loyalties to local strongmen whose authority is based upon patronage. Using these definitional components of the weak state, I will demonstrate several problematic linkages in the weak state/insurgency-survival argument that, when considered together, point toward an omitted variable bias within the civil war onset literature.

According to Achen (1986) a common form of selection bias involves important omitted variables that select cases into a treatment group. The consequence of omitted variable bias is the production of a statistical model where the findings of the model can fit the overall argument extremely well while the causal pathways or premises of the argument are not empirically supported (Green 1993). Consider the conclusion that insurgents are more likely to “survive and prosper” (Fearon and Laitin 2003:80) in weak states. If we put this conclusion at the end of a deductive-nomological argument, in which the conclusion follows logically from several interrelated propositions which themselves contain empirical content that should be testable, it would closely approximate the following form:
(a) Weak states are financially weak and/or lack popular support, which translates into an overall weakness in counterinsurgency,

(b) Counterinsurgency weakness lowers the level of individual risk to active insurgents and potential recruits, making it easier for insurgent organizations to keep their current supporters and attract new supporters,

(c) Thus, insurgencies are more likely to “survive and prosper” in weak states.

The first proposition, that financial weakness translates into a weak coercive machinery or weak counterinsurgency capabilities, while intuitive, loses support when held up to several strands of available evidence. Anecdotally, we know that many poor states have very large armies; in poor states, labor is more abundant than capital. An extremely important role in successful counterinsurgency campaigns is played by sentries. Simply having a presence establishes authority and compliance. Of the countries that over the course of the last decade have consistently been in the top ten in terms of military size, only one (the United States) is in the upper quartile of all nations in GDP per capita. Six of the ten largest militaries in the world (China, India, North Korea, Pakistan, Iraq, and Vietnam) are in the bottom half of all nation-states when ranked according to GDP per capita. Three of the top ten are in the bottom 25 percent all of states in GDP per capita (2007 estimate). Moreover, eight out of the ten states with the largest militaries in the world have fought a major civil war since 1945. As can be seen in Figure 1.1 in the top left corner, the largest militaries in the world are atypically found in the poorest states.

Second, there is the related proposition that, at the very least (and perhaps independent of the above proposition), insurgents in weak states should be better able to
avoid the state’s coercive machinery, which lowers the level of individual risk faced by active rebels, future recruits and members of the insurgent organization’s civilian support base. Logistically, if nothing else, weaker states lacking in coercive capacity and social control should have more difficulty taking the fight to the rebels where they operate. If weaker states do in fact have a harder time locating and engaging the rebels, this should result in fewer encounters between the two combatants during civil war, leading to fewer battles and, in turn, fewer battle-related deaths per year. In one of the first studies on the severity of civil wars, however, Lacina (2006) found a negative correlation between per capita income and yearly battle-deaths:
as income goes up, conflict severity goes down or, conversely, as income decreases, deaths increase. After controlling for selection effects in modeling the severity of civil warfare, Heger & Salehyan (2007) found GDP per capita to be a strongly significant predictor of lower levels of battle deaths in civil war. In other words, weaker states have the most deadly conflicts. The graph at the top right corner of Figure 1.1 is a sample of every country year spent in civil war from 1945 to 1999 (using the list of civil wars in Sambanis 2004) with each state’s GDP plotted against the number of people killed in the civil war each year. There appears to be an extremely strong relationship between state weakness and the risk of dying in a civil war. Empirically, it appears that the hardest places to “to survive and prosper” are in weak states during a civil war.

What about popular support for the regime, social control, and the risk of dying in civil war? Recent empirical work has also demonstrated that the weaker the government’s level of popular support, the more severe the state’s response to internal conflict. Heger and Salehyan (2007) found conflict severity to be significantly related to the size of the government’s coalition of supporters. They found higher levels of conflict severity as the size of the government’s winning coalition decreased. States with weaker coalitions had more deadly conflicts. This provides even more support for the generalization that rebels do not face less risk to their lives in weaker states, whether we measure weakness in terms of the size of the government’s army, economic resources of the state, or in terms of some level of societal or popular support for the government in power.

If the scope and severity of state repression is an indicator of coercive capabilities, which logically it should be, then weaker states typically invest more, not less, resources into
their coercive machinery and are more likely to put them to use (see Stanley 1996). Cross-
national, empirical works on the determinants of state repression of physical integrity rights
(Poe & Tate 1994; Poe, Tate & Keith 1999; Harrelson-Stephens & Callaway 2003; Richards,
Gelleny and Sacko 2001) inform us that economically weaker states with lower levels of
democratization and larger populations are the most likely states to inflict severe and
widespread levels of repressive violence upon their populations. The graph at the bottom left
corner of Figure 1.1 plots GDP against the CIRI physical integrity index (reversed scale) for all
available country years from 1981 to 1999. There certainly appears to be a very strong linear
relationship between how weak a state is and how repressive it is.

Nicaragua, El Salvador, Guatemala, and Cambodia provide easy illustrative examples of
economically underdeveloped states that became devoted killing machines when faced with an
armed oppositional challenge. The simple fact that weak states are the most repressive states
in the international system, yet civil war is most likely to occur in weak states, implies a
posteriori that harsh state repression – instead of being a deterrent to participating in an
insurgency - appears to be positively correlated with insurgency participation. Weak states tend
to produce an administrative style that Migdal (1988) refers to as the “politics of survival.”
Lacking control over their population and territory, they try to force control and compliance
through violence. This produces a structural environment where people face greater, not lesser,
risk to their lives from state violence. Conditions favorable to guerrilla warfare, such as
mountainous terrain, could allow rebel sanctuary in strong states and weak states alike. Only in
the presence of harsh state repression, which occurs mostly in weak states, would terrain and
geography become relevant for survival.
1.3 A Theory of State Repression and Insurgency I: Emergence and Escalation

To make historical sense, any viable conception of revolution must take into account that those who initiate, lead, provide mass support for, and ultimately benefit from revolutions are often very different groups of people.


There is good reason, in fact, for holding that rebellion is one of the least likely consequences of exploitation. If exploitation alone were a necessary and sufficient condition of rebellion, much of Southeast Asia and the Third World would surely be in a semi-permanent state of civil war...The structural context of revolt, the paths of survival and nonrevolt, and the anatomy of repression are, for this purpose, the central issues that merit attention.


The process by which state repression results in the escalation of insurgencies involves two levels of analysis which are not mutually exclusive. The former involves the generally available “paths of survival and nonrevolt,” which James Scott speaks of, or simply the options available to at-risk populations to avoid state repression at the structural level. The available paths of survival and non-revolt will determine whether an oppositional group militarizes. Once a nascent insurgency has emerged, the “anatomy of repression” being committed in the government’s counterinsurgency campaign (i.e., the harshness and frequency of the state’s repressive violence) and the effects on victims and members of their social networks at the micromobilization level will determine whether the insurgency is able to grow.

A micromobilization approach would examine common acts of state repressive behavior in counterinsurgency campaigns and their effects on the ability of the rebels to recruit new supporters. For example, mass political detentions in counterinsurgency campaigns are quite
common, yet there is a lack of empirical evidence on the effects of mass detention. Does the experience of detainment generally increase or decrease the probability of those individuals becoming active insurgents if and when they are released? Does torturing an individual during detainment (even if it is limited to psychological methods or non-injurious physical stress) deter future participation in an insurgency or increase the likelihood of future participation? What is the available universe of data and cases on how a social network generally responds to the death of one of its members via execution or disappearance in the context of a counterinsurgency campaign? (Part II of the project will explore these questions in more detail).

1.3.1 Paths of Survival and Nonrevolt

Although the qualitative literature on civil wars, revolutions, and rebellions has become increasingly state-centric in recent years, contemporary empirical modeling of civil war has largely ignored the behavior of the state in the years preceding civil war. Research has shown contradictory findings on the question of whether government repression increases or decreases oppositional activity. Lichbach (1987) proposed an elegant solution to the repression/dissent paradox: state repression of nonviolent behavior will decrease nonviolent activities but likely increase violent ones (and vice versa). We might re-state it even more generally: repression of one tactic will result in a move to avoid such repression causing a shift to alternative tactics. Lichbach stops the process, however, with the shift to the use of violence by rebel groups. In addition to causing a shift from non-violent to violent tactics, I argue that unrelenting state repression forces a consistent progression of tactical changes by oppositional groups that generally starts with terrorism, moves into insurgency, and culminates in civil war. The underlying motive of each tactical shift is to avoid or flee repressive violence, which causes
a spatial shift in where the insurgents operate. As insurgents are pushed deeper into areas devoid of repression, they have to adapt their tactics to their new environment. Rather than a tactical shift resulting in a parallel geographical move consistent with the use of the new tactic, such as irregular warfare in rural environments, I argue that the causal arrow should be reversed. Rather than picking the ideal geographic location for a particular tactic, tactical changes are more often an unintended consequence of trying to avoid state violence.

Harsh state repression leads to fairly consistent and predictable moves in the escalation process. Most oppositional groups first go violent with the use of pre-insurgency forms of terrorism, such as bombings or other sudden but brief strikes against police, government and military facilities. Not yet strong enough or able to engage in irregular warfare, due to an insufficiency of arms, recruits, and secure bases to which they can retreat, early risers in an insurgency engage in assassinations and kidnappings of unpopular elites and/or bombings of facilities related to governmental authority. The geographical center of the antecedent political organization and its support base will generally be the same location as the first acts of terrorist violence, which again, are usually directed against unpopular local officials, police stations or military outposts. The state may respond with repressive violence in the rebel group’s area of origin in an effort to quickly identify and remove active participants. This results in the most active participants fleeing the area to avoid capture, resulting in the dispersal of the leadership to more rural staging areas, a process which will establish the future foundations for guerrilla warfare.

At this early stage, the government usually lacks a clear grasp of the nature or source of the opposition and, consequently, lacks an appropriate response plan. As a result, governments
commonly engage in sporadic counterinsurgency operations clustered around the rebel group’s area of origin before returning to their own primary bases or to the capital. Thus, repressive regimes target the exact geographic area and population segments where the rebel group should have the highest levels of active and passive support. When the government leaves the area, the insurgents filter back into the towns and villages and begin recruiting new members. Residents fearful of the government’s return, particularly young men, now have some place to go due to repression generated displacement.

Noticeable changes in the dynamics of a civil conflict will be generated, first and foremost, by changes in the targeting, timing, or severity of state repression. Targeting involves whether the regime is generally selective in whom it represses (Mason and Krane 1989; Mason 1989; Kalyvas 2006). The timing of state repression involves how long the state waited in repressing the movement which determines the extent to which the rebels can capitalize on the state’s use of violence (Snyder 1976; Brockett 1995: Hafez 2003, 2006; Hafez and Wiktorowicz 2004). The severity of repression involves how harsh and frequent the acts of repression are (Gurr 1970; Lichbach and Gurr 1981; Muller 1985; Muller and Weed 1990). The three conditions are heavily intertwined, in that the timing of state repression affects the regimes targeting strategy, which in turn affects the severity of repression. Early repression tends to be more selectively targeted since the regime better knows the location of those in the oppositional group before violence escalated.

Mason and Krane (1989), focusing principally on the targeting patterns and severity of state repression, present a model of the process by which repressive state violence escalates, rather than deters, support for a nascent insurgency. Constructing a model of nonelite
response to state repression based on death squad violence in El Salvador, they argue that whom the state is targeting with repressive violence may be the most important condition for understanding its likely consequences. Mason and Krane’s approach is based on an attempt to model the decision calculus of politically neutral “nonelites” faced with variation in targeting patterns by the regime (e.g., leadership targeting, rank and file targeting, indiscriminate targeting). They find that state repression of oppositional groups usually begins with the targeting of the group’s leaders. As long as the regime restricts its repression to the leadership of the group, state violence can have the effect of slowing additional active support but will do little to shift the overall distribution of popular support (active or latent) for the insurgents over to the incumbent regime. Nor is leadership-targeted repression likely to reduce the level of engagement of opposition members who are already actively involved because the grievances that gave rise to the movement in the first place are not resolved by repression.

![Civil War Production Diagram]

Figure 1.2 Civil War Production as a Process of Avoiding State Repression with Tactical Adaptation
Moreover, those leaders who avoid repression now have an incentive to shift to violent tactics of their own, since to continue a program of nonviolent opposition would leave them vulnerable to the state’s use of violence. Thus, Mason and Krane (1989) contend that the likely outcome of eliminating the leadership will be the rise of new leadership that transforms the movement into a more militarized organization.

When the remaining insurgents escalate levels of violence against the state as retribution for the attacks against their leadership, the state is much more likely to expand the scope of its repressive violence to known or suspected rank and file members of the opposition. This move by the state will serve to solidify the active participation among the rebel organization’s current membership base as those “...who have supported the opposition in the past...are not likely to reduce their support for them in the future if the government expands its repression to include rank and file participants...” (1989: 482). Rather than reducing support to the opposition, repression of the rank and file will likely cause a shift toward further group militarization to provide for the self-defense of the current membership. By expanding repression to the rank and file, the regime ties the self preservation of the current membership to the militarization of the opposition organization. Active group members who are at risk of being victimized by the state’s campaign of repression now have little choice but to cling to the group. Escalating the fight becomes “the only way out” (Goodwin (2001).

Since the state has been selecting targets for repression based on known or suspected participation in rebel activity, support for the insurgency now becomes completely covert. In a dialectical process, targeted repression destroys the very environment needed for its use by forcing all elements of the opposition underground. To the extent that the government
becomes unable to locate targets for repression or distinguish active rebels from the neutrals surrounding them, state repression becomes increasing indiscriminate, inflicted upon those actively providing support to the insurgents and the politically neutral alike. In their effort to find suspected insurgents who do not wish to be found, state repression inevitably becomes more generalized. It is at this juncture, that state repression will increase active support for the insurgency:

as the level of repressive violence escalates and becomes more indiscriminate, the option of remaining uninvolved is eventually precluded because nonelites can no longer assure themselves of immunity from repression by simply remaining politically inert. Under such conditions, they can be induced to support rebel organizations by the promise of protection from indiscriminate violence by the state (1989:176).

When and where it becomes increasingly difficult for the average person to avoid state repressive violence and a rebel group exists that is able to offer protection as a selective incentive in exchange for support, rebel recruitment will prosper and the insurgency will grow. The combination of harsh state repression with the ability of the rebels to provide safe havens to potential victims of that repression contributes to insurgency growth by making it easier for the rebels to recruit new members.

Brockett (2005) adds the element of timing to the process of repression and insurgency escalation. Based on his analysis of the confrontational dynamics of oppositional movements and repressive regimes in Central America, Brockett (2005:327) concludes that “…if state violence is increased after a protest cycle/cycle of contention is well underway, this repression is more likely to provoke even higher levels of challenge, both nonviolent and violent, rather than deter contention.” Brockett argues that insurgency escalation is most likely when harsh state repression follows a period of mobilization. Taking Brockett’s idea even further we might
argue that escalation is most likely when harsh state repression occurs, not only during the ascendency phase of mobilization, but when the oppositional group has already formed within its organization an active militarized wing that is engaging in violence against government targets. A rebel group already organized for violence against the regime will be in the best possible position to capitalize on the widespread discontent, outrage, and fear generated by the state’s use of harsh and indiscriminate repressive violence. Thus Brockett’s focus on timing neatly complements Mason and Krane’s emphases on security dilemma’s and safe havens in that the capabilities of the rebel group at the time the state is engaging in repressive violence is an important precondition of their ability to protect themselves – and others.

1.4 Comparative Qualitative Evidence of the Defensive Escalation Model

The proposed theory of adaptive differential insurgency growth by the mechanism of repressive driven contagion meshes well with the general conclusions of many comparativists who have independently examined dozens of civil wars and periods of escalation in great detail. A large amount of corroboration and substantiation exists between the qualitative and case literatures on rebellions, revolutions, civil wars and insurgencies and this general mechanism of adaptive differential growth, i.e., harsh state repression intersecting with the emergence or existence of a rebel group able to provide protection and the material requisites for engaging in organized violence against the state.

We might start with a negative proof. The other side of the theoretical coin would predict a decline in insurgency growth commensurate with a decline in the rebel group’s ability or inclination to provide effective protection from the state’s armed forces. Palmer (1992) provides case evidence for this dynamic in the case of Sendero Luminoso, arguing that, of the
many peasant communities in the Pampas River valley who initially cast their lot with Sendero, the alliances lasted only as long as the state’s military forces were kept at bay by the rebel group. According to Palmer (1992:5) “...when Sendero organizers abandoned community residents during a government military attack...the insurgents lost their momentum in the area.” Isbell (1992) tells a similar story about the village of Cancha Cancha who gave Sendero wide support in its first two years, but later withdrew it. “The end of the brief period of support,” according to Isbell (1992:72), “came when Sendero fled over the hills and left the peasants to face repression by the armed forces, including massacres of entire villages, random disappearances, and a return of semifuedal practices.”

In a rare sort of natural experiment, Berg (1992) was able to observe, first hand, Sendero’s evolving relationship with the peasantry of the Andahuaylas province in Peru both before and after the arrival of the state’s counterinsurgency forces in 1983. In 1982, before the military had arrived, Berg (1992: 96) wrote that, “The evidence from Andahuaylas suggests that the peasants have a great deal of sympathy for the actions of Shining Path...but not a great deal of active support.” Returning to the area four years later, in the midst of the state’s counterinsurgency campaign, he found a much greater level of active support for Sendero. “One indication of this...” he remarks (1992:96) “...is the changing terminology used by peasants when referring to members of Shining Path. In 1982 they were known as “terrorist” (terroristas, terros, terukuna) or sometimes, sarcastically, as los universitarios. In 1985, in contrast, they were often called “comrades” or “buddies” (companeros).”

Strong (1992:93) echoes for Ayacucho and other provinces what Berg was witnessing in Andahuaylas, “The more blood was spilled, the more Shining Path prospered...Despite all the
armed forces’ torture and massacres and burning down of villages, the number of Shining Paths attacks in the Ayacucho department in 1984 ... were double those in 1982.” Over the course of Berg’s four year absence, around 7,126 people were killed in and around the provinces of Sendero’s origin (Strong 1992:92). Although Sendero engaged in its own share of repression, Berg (1992) notes, among others, that the residents he talked to viewed Sendero violence as much more selective than that of the police or military. As one villager put it: “When the guerillas struck, it was against people whose “crimes” were well known or against specific targets such as cooperatives. On the other hand, the police arrested and interrogated blindly...” (1992:98).

A resident from one of the valley towns in the Upper Huallaga shared with Gonzales (1992:111) what he considered to be the greatest service provided by Sendero to the people of his area: “The best thing...is that now the police don’t abuse us like they used to and nobody steals a thing. You can leave your car in the middle of the road unlocked for several days, go back and actually find it completely intact.” Keeping the government out of their hinterland was a top priority of the Shining Path. According to Strong, the departments of Ayacucho, Apurimac and Andahuaylas, which became the primary areas of Sendero activity, were “liberated” of almost all police presence in the first two years of the insurgency. According to Gonzales (1992:111), there was one way in and one way out of the Upper Huallaga and “...Shining Path completely controlled the valley road...At the entrance to towns such as Paraiso and Tocache, Sendero manned the control posts armed with submachine guns.” Keeping the government out was sometimes accomplished with creativity. In 1987, the rebels dug trenches across the only road into the Upper Huallaga valley every one hundred yards for roughly 15
miles (a total of 273 trenches between Tingo Maria and Nuevo Progreso). It took the government several days to fill in the ditches and get trucks and patrols back into the area.

Through interviews with hundreds of former residents and insurgents in El Salvadoran provinces where the Farabundo Marti National Liberation Front (FMLN) maintained a presence, Wood (2003) explored the reasons why individuals participated in the Salvadoran insurgency. She found two common patterns among those respondents who claimed active participation. Most of those who joined the insurgency had family histories of personal victimization from state violence and lived in close proximity to zones “liberated” by insurgent forces. Wood presents a clear display of defensive mobilization: victimization from harsh state repression coupled with the existence of rebel controlled areas where people could take refuge or plot their revenge. Those targeted by the state had a very narrow spectrum of options in their attempts at self-preservation. Those suspected by government forces, notes Wood, had almost no choice other than fight or flight:

- some activists who had been denounced to government forces, together with nonparticipants who had merely been denounced as insurgents by grudge-bearing enemies, faced difficult choices of fleeing the area (as many did) or of joining the insurgency (as some did). One reason to do so was that the FMLN as an organized armed force might provide protection from government forces (2003:116).

Wood (2003:116) adds that the oftentimes dispersed nature of the FMLN rebel group made it difficult for them to effectively provide security, but she nevertheless notes that the government’s arbitrary and brutal repression reinforced and deepened the insurgents’ framing of the government as a profoundly unjust authority. State violence deployed against unarmed civilians and sometimes against uninvolved family members or other residents legitimated the choice to rebel against the state, and to use arms in doing so (2003:116).
Defensive mobilization is a common model of insurgency escalation, not only in rural insurgencies where rebels can hide in the geographic peripheries of the state, but also in largely urban-based conflicts such as Ireland, Algeria, and Egypt, many of which also have a strong religious dimension. Conducting an oral history of the Provisional Irish Republican Army (IRA) through interviews with both the founders of the movement as well as rank and file members of the insurgent organization, White (1993:11) concluded that, “Their accounts show them to be less a product of family tradition and revolutionary ideals than a product of state violence.

In response to state repression, the respondents embraced the violence of the IRA.” Examining Islamist-based insurgencies in Egypt, Algeria, and the Philippines, Hafez (2003:71) argues that, “The nature of state repression equals in importance the degree of system accessibility in shaping the strategic orientation of Islamist movements.” According to Hafez (2003:91),

The contemporary histories of Algeria and Egypt point to an important link between repression and rebellion. In both countries Islamists rebelled not merely because they were aggrieved or excluded from political participation but because they felt their organizational and physical survival was threatened by overwhelming repression. In both instances, insurgency was a defensive reaction to protect organizational gains and personal lives against predatory states.

In the Philippines, “...mere grievances were not sufficient to produce mass rebellion. It took outright physical threat to the lives of ordinary Muslims in the early 1970’s to produce rebellious organizations with popular support...” (Hafez 2003:95). Additional support comes from O’Leary and Silke’s (2007:409) comparative study of insurgency in Spain, Nepal, Peru, Sri Lanka, Northern Ireland, and India. They argue that, “[T]he result in almost every situation of generalized repression has been an increase in the intensity of the violence and an increase in wider support for the insurgents.” Utilizing a comparative-historical method of inquiry into
revolutions and rebellions, Skocpol (1994:266) notes a persistent regularity in the cases she has studied, namely that, repressive regimes...

...without intending to do so, valorize the potential oppositional role of armed revolutionaries. Because such regimes are so closed, they readily turn to vicious repression when faced with demands for even the most moderate political or economic adjustments. Thus closed authoritarian regimes place a premium on the things armed revolutionaries are best prepared to do – namely, provide opponents of a regime with the means of self-defense, such as guns, clandestine networks, safehouses, and even liberated territory within which to survive and carry on oppositional politics ... [F]rom the viewpoint of would-be revolutionaries, the ideal situation is to face an exclusionary and repressive authoritarian regime that lacks strong control of its entire territory or borders... (1994:267).

Finally, examining positive and negative cases of revolutionary movements in El Salvador, Nicaragua, Guatemala, Philippines, Bolivia, Columbia, Chile, Peru, Honduras, Indonesia, Malaya and Vietnam, Goodwin (2001:245) concludes his study will the following:

...the “root cause” of armed rebellions that seek the overthrow of the state – as distinct from other forms of political conflict – is not poverty, exploitation, or inequality per se. Rather, armed revolutionary movements result from the violent suppression of the peaceful political activities of aggrieved people who have the capacity to rebel. (emphasis in original).

1.5 Summary and Hypothesis

When the state uses violent repression against an oppositional group, especially one that already has an active military wing, there will likely be a shift in support within the group to the more militant wing as people perceive the state’s use of violence as evidence that changing the regime through conventional or nonviolent means is unlikely. If the dissidents were already using violent tactics against the state when the state began to engage in harsh systematic repression, it is unlikely that the rebel group would abandon its use of violence. The rebel group now faces a classic security dilemma, in that any effort by individuals to return to non-violent activities or to defect from the rebel group makes those group members vulnerable to state
violence. In addition, the continued use of non-violent activities by the conventional wing of the movement will likely diminish. Thus, we propose the following:

Hypothesis 1: When a state engages in harsh levels of repression within a period of oppositional group violence, outside of civil war, the state faces a greater risk of a civil war developing.
Chapter 2
Research Design and Empirical Test I: Emergence and Escalation into Major Civil War

In this chapter a model of civil war is constructed that examines opportunity, willingness and the development of civil war in three interrelated analyses. In all three analyses, a time series cross sectional (TSCS) data structure is used with the country-year as the unit of analysis. The goal of the first analysis (Defining the Risk Set) is to identify those states that have an actual opportunity for insurgency escalation to occur by modeling oppositional group violence, outside of civil war. To define the risk set for insurgency escalation, I identify which states (all outside of civil war) enter an opportunity spell of oppositional group violence (the gatekeeping stage) as well as how many attacks they experience during the spell (severity stage) using a two-step Heckman model.

Once I have defined the risk set and those variables important in determining the onset of an opportunity spell and its severity, the second analysis (The State’s Response) selects on these opportunities, using both a statistical selection model as well as a manual split population model, and then examines as the dependent variable the state’s repressive response to these spells using data from the CIRI Human Rights Data Project (Cingranelli and Richards 1999).

The third and final analysis (The Consequences) takes as its sample the 116 states that did pass through the gatekeeping stage and enter an opportunity spell of oppositional group violence. Using this sample of opportunity spells for insurgency growth I estimate a series of logistic regression models that predict which opportunities will produce a future civil war.
2.1 Operationalizing Oppositional Group Violence

In order to identify the presence or absence of low intensity oppositional group violence for every state, a database of violent attacks directed at government targets by identified oppositional groups was needed. The Global Terrorism Database (LaFree and Dugan 2008) is appropriate for this purpose in that it contains not only the most comprehensive number of violent attacks for the time period of the current study (54,664 attacks between 1981-1999) but also contains information on the groups that committed the attacks. The Global Terrorism Database (GTD) is a compilation of all known violent attacks by groups “…to attain a political, economic, religious, or social goal through fear, coercion, or intimidation…” recorded globally from 1970 to 1997 by the Pinkerton Global Intelligence Service (PGIS) and from 1998 to 2004 by LaFree and Dugan (2008) and the Inter-university Consortium for Political and Social Research at the University of Michigan.

Extensive filtering and trimming of the 54,664 attacks recorded between 1981 to 1999 was done to match the data to the theoretical purpose of the present study using three dimensions: (1) the group that committed the attack; (2) the specified target of the attack; and (3) the description of the attack. In a minority of cases the perpetrator of the attack could not be identified (i.e., “unknown”) or was described by a generic grouping such as “protester” or “extremist.” I do not want to include events where the perpetrator was unknown for two reasons that are directly related to insurgency recruitment. If the group could not be identified, then presumably, its goals were not clearly political. Therefore, the group is not a candidate for escalation. Any group that attacks without naming itself or claiming responsibility cannot profit from the attack in terms of attracting followers. The purpose of early attacks is to advertise
resolve to potential supporters. How can supporters gravitate to a group that does not identify itself? For the current analyses, all events perpetrated by unknown groups and all events falling under generic groupings were excluded. Only violent attacks conducted by oppositional groups identified by a group name were included. Second, all acts of violence not targeted, either directly or indirectly, toward a governmental authority were excluded. This included acts where the target was specified as “unknown” as well as attacks against abortion clinics, sporting events, and other non-governmental targets. Third, attacks with an “unknown” or ambiguous description were excluded. Only events describes as “assassinations,” “bombings,” “highjackings,” “hostage taking,” “kidnapings,” and “facility attacks,” where the militant group was identified by a proper name and the attack was directed at a governmental authority or was relevant to state-capacity (e.g., transportation and utility facilities) are included in the dataset.

This three pronged elimination process culled over 21,000 events from the final dataset. Ultimately, 33,187 acts of oppositional group violence against governmental targets were included for the years 1981 through 1999. At least one attack took place in 1,210 country years. Around twenty-four percent of the attacks were directed at targets specified as the “government.” The “police” were the targets of another twenty-two percent of the total. In sixteen percent the target was listed as being of a “military” nature. Attacks upon targets related to “transportation” and “utilities” comprised roughly six percent and three percent of all attacks, respectively. The remainder of attacks (all around one percent) were perpetrated against “businesses,” “journalists,” “religious figures,” and “educational” targets. While these attacks did not directly involve governmental authority, they all involved attacks on facilities or
societal elites commonly targeted by insurgents and are thus considered as relevant in the state’s decision calculus on how to respond. The most frequent description given involved attacks upon “facilities” (forty-five percent) followed by “bombings” (thirty-two percent). In nineteen percent of the events “assassination” was the intended goal, followed by “kidnapping” (four percent).

Of the 1,210 country years in which oppositional group violence took place between 1981 to 1999, sixty percent of those years housing violence were outside of a civil war context, using the dates of onset and termination for civil wars specified by Sambanis (2004). In other words, the majority of oppositional group violence directed against states from 1981 to 1999 did not occur during an ongoing civil war. Instead, the attacks occurred within states that never experienced a civil war during the period, or they occurred before or after a state fought a civil war. This large distribution of violent attacks outside of civil war environments creates an ideal pattern of empirical variation with which to model the role of opportunity and willingness in insurgency and counterinsurgency.

2.2 Defining the Risk Set: Cross-National Variation in the Onset and Severity of Oppositional Group Violence Outside of Civil War, 1981-1999

The sample used in the first analysis contains every available country year for all major nation-states in the world from 1981 to 1999, with the exception of those years in which a state was fighting in an ongoing civil war (658 years were spent in civil war from 1981 to 1999). Using the dates of onset and termination of civil wars in Sambanis (2004), all years spent, even partially, in civil war were dropped from the dataset. It is crucial that “civil war violence” not be included in the sample because our goal is to identify periods of existing oppositional group violence where civil war has the opportunity to develop in the future. What will be predicted in
the final analysis (i.e., civil war) cannot be contained within the units that the subsequent predictions will be based upon. If we were to include civil war years in the sample and then model oppositional group attacks, we would be essentially modeling civil war onset with a disaggregated measurement of violence instead of a binary one.

The purpose of the first analysis is to discover which states enter an opportunity spell of oppositional group violence outside of civil war and what influences the severity of violence within the spell. To accomplish this task, I use a Heckman two-step selection model containing a gatekeeping equation and a severity equation. Since the severity of an opportunity spell is a measure of the success of the oppositional groups in carrying out attacks, it is necessary to control for selection bias when evaluating the severity of violence as oppositional groups will presumably evaluate the probability of being successful in their endeavors before committing themselves. Additionally, we have no other way of knowing, apart from a two-step selection model, whether the conditions associated with the onset of an opportunity spell are the same conditions associated with the severity of violence within an opportunity spell.

In the first step, or the gatekeeping step, the dependent variable is a binary coding of the onset and duration of an opportunity spell within a state. The coding procedure combines the onset and duration of violence and thus is the same procedure that has been used to capture the prevalence of violence in the civil war literature (Elbadawi and Sambanis 2002). A state enters an opportunity spell with an initial attack and continues in that opportunity spell until 1999 unless a civil war is experienced which causes all remaining years, including the year of civil war onset, to be dropped from the sample. Until an initial attack is suffered by the state, the state receives a code of zero for every year. Once an attack is suffered by the state, the
remaining years receive a code of one for the duration of the opportunity spell. Of the 142 states in the sample which we follow for a period of nineteen years, there were 116 states that experienced at least one attack and thus entered into an opportunity spell. The average duration of the 116 opportunity spells was ten years. The dependent variable in Model 1 (the gatekeeping stage) is the prevalence (i.e., onset and duration) of opportunity spells.

After obtaining the estimates from the gatekeeping phase, the two-step Heckman procedure uses the estimates regarding which states experience an opportunity spell of oppositional group violence to compute the estimates for the level of severity inside the opportunity spell. If the process that leads a state into an opportunity spell is correlated with the severity of the spell, positively or negatively, the Heckman model corrects for this bias. In Models 2 and 3, the dependent variable is the number of oppositional group attacks occurring per year (Severity) with two different indicators measuring regime type. The mean value of Severity was three attacks per year with a minimum of 0 and a maximum of 201. All independent variables included are from the Fearon & Laitin (2003) and Hegre & Sambanis (2006) replication files.

2.2.1 Findings

Table 2.1 displays the results of a two-step Heckman selection model examining cross-national variation in the prevalence and severity of oppositional group violence against states from 1981 to 1999. Two general themes stand out across the three models. First, the conditions associated with the onset and duration of oppositional group violence and the conditions associated with the severity of oppositional group violence differ considerably which illustrates the importance of using a two-step selection model for understanding an escalatory
process. As for the severity of oppositional group violence, Models 2 and 3 show that state capacity, whether measured by military size, military spending, economic development, or political development, has little explanatory power. In Model 1, the gatekeeping phase, however, almost every one of these indicators is statistically significant. The presence of minority rule by an ethnic group is a strong predictor of entering a period of oppositional group violence, although it has little effect on the severity therein.

Other indicators are statistically significant in both stages, but with contradictory affects. Societies with higher levels of ethnic fragmentation appear to be less likely to experience oppositional group violence overall, but when the state does enter an opportunity spell the severity of violence is likely to be much higher than in other states. This suggests that there is little reason to assume that the same conditions that increase a country’s risk of experiencing oppositional group violence will also influence the success of the oppositional groups in escalating their activities. Of those indicators that do have significant and consistent effects in both phases, the results suggest that states that are more repressive, with larger populations, and larger amounts of mountainous terrain are more likely to experience oppositional group attacks than other states and the level of violence is likely to be greater as well.

If we conceptualize the purpose of this analysis as defining a true risk set for insurgency escalation, a second general theme that emerges from the findings is that weak states don’t generally stand out as vulnerable relative to other states. New states (less than 2 years old) are not any more vulnerable to oppositional group violence than more established states.
Table 2.1  Defining the Risk Set: Heckman Two Step Selection Model of Oppositional Group Violence Outside of Civil War, 1981-1999

<table>
<thead>
<tr>
<th>Variable</th>
<th>Model 1 Gatekeeping Stage</th>
<th>Model 2 Severity Stage</th>
<th>Model 3 Severity Stage</th>
</tr>
</thead>
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<tr>
<td></td>
<td>DV: Prevalence of oppositional group violence (1,0)</td>
<td>DV: Number of yearly attacks</td>
<td>DV: Number of yearly attacks</td>
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<td>State Repression (t-1)</td>
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<td>(0.306)**</td>
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<td>(0.000)***</td>
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<td>(0.001)</td>
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<tr>
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<td></td>
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<td></td>
<td>(0.543)**</td>
<td>(7.315)**</td>
<td>(6.956)**</td>
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<td>-35.106</td>
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<td>(0.658)</td>
<td>(8.511)**</td>
<td>(8.280)**</td>
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<td>0.071</td>
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<td>(0.001)**</td>
<td>(0.025)**</td>
<td>(0.025)**</td>
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<td>-1.410</td>
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<td>(10.625)</td>
<td>(10.616)</td>
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<td>(0.681)**</td>
<td>(0.618)**</td>
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<td>3.585</td>
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<td>(0.160)***</td>
<td>(2.223)</td>
<td>(1.912)</td>
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<td>Uncensored obs</td>
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<tr>
<td>Number of states</td>
<td>142</td>
<td>142</td>
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</tbody>
</table>

Standard errors in parentheses; ** significant at 5%; *** significant at 1%  
Note: Model 1, the gatekeeping equation or selection equation, is intended to capture the risk of entering a spell of oppositional group violence; once a state passing through the “gate” with its first attack, every year thereafter is coded a 1. States receive a code of zero for every year until they experience an attack; if an attack never occurs, the state receives zeros for the entire temporal domain.
The conflict history of a state is also not a reliable predictor of the presence or severity of oppositional group violence. In Model 1, we see that of those indicators that are important in identifying the risk set for insurgency opportunity, most run contrary to weak state arguments. Higher GDP per capita, which is the most frequently used indicator to proxy state capacity (Fearon and Laitin 2003) and the opportunity costs of dissent, is associated with a higher risk of experiencing oppositional group violence and the coefficient is highly significant. As seen in Models 1 and 3, more democratic regimes also appear to be disproportionately vulnerable to oppositional group violence. In Model 3 a democratic dummy variable is substituted for the Polity score, indicating a country’s status as a full democracy (+6 to +10) in the calendar year. Democracies do appear to be the most vulnerable states to experiencing oppositional group violence. Democratic states experienced, on average, 3 or 4 more attacks per year than non-democratic states. Pape (2003:350) offers the following rational for democratic susceptibility to political violence:

> democracies are often thought to be especially vulnerable to coercive punishment. Domestic critics and international rivals, as well as terrorists, often view democracies as “soft,” usually on the grounds that their publics have low thresholds of cost tolerance and high ability to affect state policy...they must also be confident that their opponent will be at least somewhat restrained. While there are infamous exceptions, democracies have generally been more restrained in their use of force against civilians.

The strongest predictor of the severity of oppositional group violence against government targets is the level of ethnic fractionalization in the nation. Highly fragmented states experienced around 23 more attacks per year than states with more ethnically homogenous societies. As indicated by the squared term, ethnically polarized societies, in which several large ethnic groups co-exist, are associated with the most political violence;
political violence is lowest in societies with extremely low levels and extremely high levels of ethnic fragmentation.

2.2.2 Discussion and Conclusion

Conceptualizing major civil wars as “successful insurgencies” requires a research design that attempts to identify the earliest possible stages of organized violence and in doing so creates a pool of insurgency “opportunities” that may or may not succeed in escalating into civil war. As such, the primary purpose of this analysis was to identify the risk set for insurgency opportunity. In the next stage of the analyses (The State’s Response), I examine the state’s repressive behavior in the presence of oppositional group violence. I do so by selecting on the opportunity spells identified in this analysis using two different methods.

2.3 The State’s Response: Examining Cross-National Variation in State Repressive Behavior to Oppositional Group Violence, Outside of Civil War, 1981-1999

In this, the second analysis, opportunity and willingness are both considered by selecting on the opportunity spells identified in the previous analysis (Defining the Risk Set) and examining the state’s behavioral response. The dependent variable across all four models is the CIRI Human Rights Data Project’s 9 point physical integrity index (I reversed the scale so that higher numbers indicate worse human rights conditions and lower numbers indicate more respect for human rights). The CIRI database contains yearly measurements of human rights abuses for all major nation-states based on information regarding the human rights practices of states found in Amnesty International’s Annual Reports and the U.S. State Department Annual Country Reports on Human Rights Practices. The CIRI physical integrity index (physint) is a 9 point additive scale derived from a Mokken scale analysis of the four indicators of physical
integrity: (1) torture, (2) extrajudicial killings, (3) disappearances, and (4) political imprisonment.

Two modes of selecting on opportunity are used: a Heckman two-step selection model (Models 1 and 2) and a manual mode of selection (Models 3 and 4) that drops from the sample all the states that failed to enter an opportunity spell of oppositional group violence and all of the country years that preceded the year in which a state entered into an opportunity spell. In Models 1 and 2, a Heckman two-step selection model estimates, first, the conditions associated with the severity of violence within an opportunity spell and then uses that selection bias term to correct the estimates in the outcome equation, which in this case, the dependent variable is the CIRI Project’s 9 point physical integrity index. It is important to select for the severity of oppositional group violence when examining state repressive behavior since the oppositional actors must have considered the likelihood of a harsh repressive response before acting. Additionally, the states that are not experiencing any oppositional violence have no need to repress. The dependent variable in the first step equation for Models 1 and 2 is the number of oppositional group attacks per year. The average number of attacks per year was around 3 attacks with a minimum yearly value of zero and a maximum of 201. Since the same dependent variable was just estimated in the previous analysis (see Table 2.1, Models 2 and 3) the results for the first step equation are not duplicated here. Table 2.2 shows only the second step outcome equation (Models 1 and 2) which predicts the repressiveness of the state while selecting for the number of oppositional group attacks. According to Heckman (1979), who provides a method of accounting for selection effects under some scenarios, selection bias (depending on the source) can rarely be entirely overcome using statistical methods only.
Investigating the theoretical source of the bias, which often takes the form of omitted variable bias in both the control group and the treatment group, and partitioning the dataset are often required (Heckman 1979). As a robustness test of the Heckman model, as well as a way of providing a more intuitive display of what the Heckman selection models are accomplishing, I also manually dropped from the sample all country years outside an opportunity spell and examined the repressiveness of the state using an AR(1) regression with a single lag OLS of residuals and panel corrected standard errors (Models 3 and 4). Thus, if a state does not experience any oppositional group violence, it is not included in the sample used to estimate Models 3 and 4. This resulted in the elimination of 26 states from the sample because they never experienced any oppositional violence between 1981 and 1999. Every country year that preceded a state’s entry into an opportunity spell was also dropped resulting in a country-year reduction of roughly 40 percent.

The explicit purpose of this robustness test is to give a much more strictly defined answer to the question: given a cross-national sample of states that are all facing actual oppositional group violence, outside of civil war, how will the different states respond? Even when using the Heckman selection model, it still might be possible that states that never experienced any oppositional group violence, and thus have no reason to repress, are posing a potential bias. After all, we are not concerned with how repressive states are in general, but how states response to oppositional violence and the consequences of state behavior.

2.3.1 Findings

Two more general themes emerge from the results shown in Table 2.2. Comparing the first two models to the last two models, we see that almost identical results are achieved using
the two different methods of selection. The Heckman two-step model, which takes the
difference between the estimates of the first selection equation and the estimates from the
second outcome equation, produces almost exactly the same results as manually dropping from
the sample all country years not inside an opportunity spell of oppositional group violence. This
result builds confidence that our results are not an artifact of using the Heckman selection
model while also providing a more intuitive understanding of what the Heckman model
accomplishes.

Recalling from the last analysis that more democratic states with higher levels of
economic development were the most vulnerable to attack, the second general theme that
emerges from Table 2.2 is that those states most likely to see oppositional group violence are
the least likely to repress it. Conversely, those states least likely to see oppositional group
violence are most likely to repress it. After selecting for which states are vulnerable to
experiencing oppositional group violence, weak states stand out as the most repressive regimes
and the explanation is not that weak states face greater levels of oppositional violence. Most of
the states that experienced a large number of attacks over the time period (e.g., Germany,
Italy, France, Greece, Belgium, Japan, Ecuador, Chile, Venezuela, and Spain) would not be
considered weak states. Moreover, how harshly a state responds to oppositional group
violence does not even appear to be driven by the number of yearly attacks, as indicated by the
weak results for the variable which counts the number of attacks in each year (Yearly Attacks).
The coefficient for Yearly Attacks suggests that it would take roughly 200 attacks in a single year
to increase the state’s CIRI score one point.
Across almost all of the indicators relevant to state capacity in all four models, weak states stand out as the most repressive regimes. Higher GDP per capita (logged) is strongly correlated with lower levels of state repression in every model. States with a greater capacity for military spending are also found to be generally less repressive. Higher levels of democracy within a state is one of the strongest statistical predictors of lower levels of repression. The coefficient of .098 indicates a full range of roughly 2 points moving from the least democratic states to the most democratic states in the sample. States with a history of civil war were also significantly more repressive. The variable which counts previous peace years (i.e., years since the last civil war) was strongly significant and indicates roughly a .21 decrease in repressive behavior for every decade that civil war is avoided.

Across all four models mountainous terrain is highly significantly and associated with harsher levels of repression. For every one percentage point increase in mountainous terrain we would expect a .01 increase in the state’s repression score. States with larger populations are also significantly more likely to respond to oppositional group violence with repression. The presence of minority rule, operationalized as the percentage of the population that differs in ethnicity from the ethnic group in power, reaches strong statistical significance with large effects. Since the variable is proportional, we can interpret the coefficients as indicating a range of variation on the dependent variable between 1 to 2 points. Higher levels of ethnic fractionalization within the state were not significantly associated with the level of repression. While ethnic fractionalization appears to increase the number of attacks against a state in an opportunity spell, it does not appear to strongly influence how the state responds to such violence.
Table 2.2 The State's Response to Oppositional Group Violence Outside of Civil War, 1981-1999
Two Modes of Selecting on Opportunities for Escalation

<table>
<thead>
<tr>
<th></th>
<th></th>
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<th></th>
<th></th>
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<tbody>
<tr>
<td></td>
<td>Regime type: 21 point Polity index</td>
<td>Regime type: Democracy (1,0)</td>
<td>Regime type: Polity index</td>
<td>Regime type: Democracy (1,0)</td>
</tr>
<tr>
<td></td>
<td>Selection Equation DV: # of Attacks</td>
<td>Selection Equation DV: # of Attacks</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Yearly Attacks</td>
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<td>0.006</td>
<td>0.005</td>
<td>0.005</td>
</tr>
<tr>
<td></td>
<td>(0.002)**</td>
<td>(0.002)**</td>
<td>(0.003)</td>
<td>(0.003)</td>
</tr>
<tr>
<td>Military Size (K)</td>
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<td>0.000</td>
<td>0.000</td>
<td>0.000</td>
</tr>
<tr>
<td></td>
<td>(0.000)**</td>
<td>(0.000)**</td>
<td>(0.000)</td>
<td>(0.000)</td>
</tr>
<tr>
<td>Military Spending (M)</td>
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<td>-0.000</td>
<td>-0.000</td>
<td>-0.000</td>
</tr>
<tr>
<td></td>
<td>(0.000)***</td>
<td>(0.000)***</td>
<td>(0.000)***</td>
<td>(0.000)***</td>
</tr>
<tr>
<td>GDP per cap (ln)</td>
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</tr>
<tr>
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<td>(0.092)***</td>
<td>(0.100)***</td>
<td>(0.069)***</td>
<td>(0.069)***</td>
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<tr>
<td>Peace Years</td>
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<td>-0.015</td>
<td>-0.016</td>
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<tr>
<td></td>
<td>(0.004)***</td>
<td>(0.004)***</td>
<td>(0.005)***</td>
<td>(0.004)***</td>
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<tr>
<td>Ethnic Fract</td>
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<td>0.763</td>
<td>0.853</td>
<td>0.328</td>
</tr>
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<td>(1.001)</td>
<td>(1.070)</td>
<td>(1.118)</td>
<td>(1.074)</td>
</tr>
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<td>Ethnic Fract2</td>
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<td>-2.492</td>
<td>-1.860</td>
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<td></td>
<td>(1.156)**</td>
<td>(1.274)</td>
<td>(1.403)</td>
<td>(1.348)</td>
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<td>Mountainous</td>
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<td>0.017</td>
<td>0.010</td>
<td>0.009</td>
</tr>
<tr>
<td></td>
<td>(0.003)***</td>
<td>(0.003)***</td>
<td>(0.003)***</td>
<td>(0.003)***</td>
</tr>
<tr>
<td>New State</td>
<td>-1.996</td>
<td>-1.964</td>
<td>-1.110</td>
<td>-1.153</td>
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<td>(0.619)***</td>
<td>(0.579)***</td>
<td>(0.538)**</td>
<td>(0.610)</td>
</tr>
<tr>
<td>Population</td>
<td>0.438</td>
<td>0.736</td>
<td>0.457</td>
<td>0.408</td>
</tr>
<tr>
<td></td>
<td>(0.109)***</td>
<td>(0.094)***</td>
<td>(0.063)***</td>
<td>(0.060)***</td>
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<tr>
<td>Minority Rule</td>
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<td>2.762</td>
<td>1.111</td>
<td>1.230</td>
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<tr>
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<td>(0.328)***</td>
<td>(0.274)***</td>
<td>(0.202)***</td>
<td>(0.205)***</td>
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<td>Polity</td>
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<td>-0.095</td>
<td>-0.095</td>
<td>-0.095</td>
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<tr>
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<td>(0.019)***</td>
<td>(0.011)***</td>
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<td>-0.645</td>
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<td>-1.340</td>
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<td>(0.215)***</td>
<td>(0.215)***</td>
<td></td>
<td>(0.157)***</td>
</tr>
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<td>-2.609</td>
<td>-2.427</td>
</tr>
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<td>(2.373)</td>
<td>(1.845)***</td>
<td>(1.029)**</td>
<td>(0.978)***</td>
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<td>2018</td>
<td>1198</td>
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<td>Number of states</td>
<td>142</td>
<td>142</td>
<td>116</td>
<td>116</td>
</tr>
</tbody>
</table>

Standard errors in parentheses; ** significant at 5%; *** significant at 1%
Note: The dependent variable in the selection equation for Models 1 and 2 (not shown) is the number of oppositional group attacks per year, including the variables from Table 2.1 that were significant predictors of the number of oppositional group attacks per year. In Models 3 and 4 the population was limited to only those states that entered a spell of oppositional group violence with a first attack. Thus, in Models 3 & 4 I am manually selecting on “opportunity” by dropping from the sample the 26 states that did not experience any attacks in the gatekeeping stage of the previous analysis (Defining The Risk Set).
2.3.2 Discussion and Conclusion

By selecting on those states suffering periods of oppositional group violence outside of civil war, this analysis sought to set up a group of states that faced, more or less, an equal opportunity environment for engaging in repressive violence. The repressive behavior of the states was found to be anything but equal. Irrespective of the number of attacks suffered per year, the most repressive states shared certain basic characteristics, namely: a) an economically underdeveloped economy, b) a large population, c) a non-democratic government, d) a minority ethnic group in power, e) a history of civil war, and f) larger amount of mountainous terrain. In sum, the most significant variables in predicting civil war onset in the extant civil war literature are the best variables at predicting a harsh repressive response to oppositional group violence outside of civil war contexts. Conversely, states with democratic governments, majority rule, developed economies, smaller populations, no recent history of civil war, and more manageable geographic terrain were significantly more likely to maintain respectable human rights conditions even in the face of some severe campaigns of oppositional group violence. In the next stage, I take our pool of known “insurgency opportunity spells” (N=116) along with the known response of the state and see which environments produce civil war.


In the previous analysis, I examined variation in state repressive behavior while selecting on 116 periods of oppositional group violence. In the final analysis, I am interested in seeing which of these 116 insurgency opportunities produced civil war and where civil war failed to develop despite the intentions and hard work of hundreds of violent oppositional groups. I have argued in this project that it is the response of weak states to oppositional violence – their
resort to repression – and not state weakness itself that largely determines the ability of oppositional groups to recruit and build a sustainable support system for civil war. In this analysis I compare opportunity spells of violence that receive the treatment of state repression to opportunity spells where the state maintained respectable human rights conditions.

Each country year inside of our spells of insurgency opportunities was coded as to whether or not civil war onset occurred (1 or 0) in the next year (t+1), three years in the future (t+3), five years in the future (t+5), and seven years in the future (t+7), creating 4 separate logistic regressions. For onset at t+1, the independent variables for 1981 would be used to predict civil war onset in 1982. For t+7, the independent variables for 1981 would be used to predict the onset of civil war in 1988. Dates of onset were taken from Hegre & Sambanis (2006).

2.4.1 Independent Variables

The primary independent variable is the severity of government repression of physical integrity rights for every available nation-state as measured by the CIRI Human Rights Data Project (Cingranelli & Richards 1999). The CIRI physical integrity index (physint) is a 9 point additive scale derived from the four indicators of physical integrity (torture, extrajudicial killings, disappearances and political imprisonment) based on information regarding the human rights practices of states found in Amnesty International’s Annual Reports and the U.S. State Department Annual Country Reports on Human Rights Practices. All other independent variables included are from the Fearon & Laitin (2003) and Hegre & Sambanis (2006) replication files.
2.4.2 Findings

Table 2.3 displays the results of four logistic regression models of civil war onset which differ based on how far in advance civil war onset is predicted. Across all 4 models, state repression, which operationally includes considerations of the incidence and frequency of torture, detainment, disappearances and extra-judicial killings by the state in every country year, is the strongest predictor of civil war onset. As seen in models 2, 3, and 4, the harshness of state repression is a powerful predictor of civil war as far as seven years in the future. The measure of state repression taken three years before the onset of civil war (Model 2) appears to have the largest effect of the same variable in the other 3 models. We might conclude that, one the state embarks upon a program of harsh repression it takes about three years on average for rebel groups to militarize and build a base of political cadres and social support sufficient to mount an organized armed challenge to the state.

Controlling for escalation opportunities along with the inclusion of state repression leads to considerable changes in the expected effects several other variables, given their status in the extant civil war literature. Population size, which has been one of the most consistent and powerful predictors of civil war onset in previous models, fails to reach statistical significance by a large margin. Since population has been previously shown to be a powerful predictor of state repression of physical integrity rights in cross-national samples (Poe and Tate 1994), its fall from significance with the inclusion of state repression in the model suggests that the variable may have been a consistent predictor of civil war in previous research because it was effective at predicting repression rather than civil war onset.
GDP per capita, the standard variable for state capacity or the opportunity costs of rebellion, does not reach statistical significance in any model. The results of the analysis suggest that previous works on onset have been overly-deterministic with regards to state capacity and the economic viability of rebellion. After controlling for state behavior within environments of actual oppositional group violence short of civil war, state capacity and poverty do not predict where insurgencies will survive and prosper. This finding suggests two explanations. First, the correlation between lower levels of state capacity and civil war onset in previous research was, in some part, driven by the fact that wealthy western democracies experience virtually no civil wars and controlling for opportunity removes this bias. Second, the primacy of state capacity in previous civil war modeling appears to be the result of a strong relationship between state capacity and state behavior, especially state repressive behavior.

Interestingly, when controlling for the opportunity to rebel, coupled with how repressive the state is, a higher proportion of mountainous territory within the state appears to be negatively associated with the onset of civil war. Given the fact that mountainous terrain was strongly associated with greater levels of state repression in stage 2, this finding also suggests that mountainous terrain has been predicting harsh repression and not necessarily civil war in previous research. When repression and mountainous terrain are included in the same model, the effect of mountainous terrain is reversed.

The number of years that have passed since a previous civil war (or the number of years a state has experienced civil peace) is negatively associated with civil war onset and highly significant. The variable indicating a new state (less than 2 years old) was dropped from the logit analysis as its failure (0) predicted the failure (0) of the dependent variable perfectly.
In other words, there were no civil wars in new states during the period that could be included in the sample. Since our minimum lag period is one year, civil wars that emerged in the first year of a state’s existence could not be included since the country did not exist in the year before it experienced the civil war (e.g., Moldova, Azerbaijan). Furthermore, data are not available for most of the new Soviet satellite states in their first year of existence.
Due to the fact that the direct interpretation of coefficients and odds ratios for individual variables is less than intuitive in logistic regressions, the marginal effects of the variables were calculated. The marginal effects were computed for model 1 (t+1) by holding all variables at their mean values other than the variable of interest and calculating the difference in the probability of onset with the variable of interest held at its minimum value versus its maximum value. Table 2.4 provides the marginal effects for the variables that are significant in Model 1.

A state that has a maximum repression score of eight, indicating that it violated all four personal integrity rights of its citizens “frequently” in the given year, has a 6,192 percent higher probability of civil war onset in the next year than states that scored the minimum index value (0). The marginal effects of previous years in peace is much smaller but nevertheless indicates a 70 percent decline in risk moving from the state with the longest history of civil war avoidance to those states that had recent civil wars. States where over 94 percent of the territory is mountainous had a 1207 percent lower probability of experiencing civil war onset than state’s with little mountainous terrain. While at odds with previous finding on civil war onset, the finding becomes less iconoclastic when interpreted in light of the results for state repression and if we make a distinction between irregular warfare (a pure military method) and guerrilla warfare (the use of irregular warfare in a broader socio-political dissident movement). While rough terrain is useful in conducting irregular warfare, it is certainly not a necessary condition. Moreover, too much rough terrain may be detrimental to the flow of resources between irregular units and their much larger civilian support base. Too much territorial fragmentation may also impede the creation and expansion of social networks and mobilization in general.
Table 2.4  Marginal Effects for Significant Variables in Model 1

<table>
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<tr>
<th>Variables</th>
<th>Probability</th>
<th>Change (%)</th>
</tr>
</thead>
<tbody>
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<td>Repression</td>
<td></td>
<td></td>
</tr>
<tr>
<td>0 (min)</td>
<td>.00113</td>
<td></td>
</tr>
<tr>
<td>8 (max)</td>
<td>.07111</td>
<td>+ 6,192.92</td>
</tr>
<tr>
<td>Peace years</td>
<td></td>
<td></td>
</tr>
<tr>
<td>0 (min)</td>
<td>-.0004229</td>
<td></td>
</tr>
<tr>
<td>54 (max)</td>
<td>-.0001248</td>
<td>- 70.48</td>
</tr>
<tr>
<td>Mountainous</td>
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</tr>
<tr>
<td>0 (min)</td>
<td>-.0003921</td>
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</tr>
<tr>
<td>94.3 (max)</td>
<td>-.00003</td>
<td>- 1,207</td>
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<tr>
<td>Polity</td>
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2.5 Conclusion

One of the primary motivations behind Part I (chapters 1 and 2) was to deal with problems stemming from selection effects, endogeniety and reverse causality in the study of civil war violence in a transparent, controlled and robust manner by controlling for the opportunity for insurgencies to escalate and predicting civil war several years in advance. We can be confident in the finding that harsh state repression is the primary producer of insurgency growth, not just because states that experience civil war are more repressive than states that do not. Instead, it has been shown that repressive states are more likely to experience civil war relative to states that also faced violent opposition but responded to that challenge with less repressive measures. By controlling for opportunity and willingness in
modeling civil war onset and by predicting civil war up to 7 years in advance we can also have more confidence that our independent variables have not been detrimentally altered by escalating violence.

Most importantly, an opportunity based approach allows us to say with much more certainty that repression causes civil war rather than civil war causes repression. One way to begin to model such a puzzle is to look at all opportunities to repress and see if oppositional violence that was not met with repression failed to escalate. From a normative standpoint, it would have been disappointing to discover in Stage 2 (The State’s Response) that most nation-states experiencing oppositional group violence will attempt to crush it with repressive violence. Most states in a period of oppositional violence (some facing 50, 100, 200 attacks per year by rebel organizations) maintained respectable human rights conditions. States that maintained respectable human rights conditions almost without exception avoided civil war. Their insurgencies might have survived, but they were unable to grow, absent the fear and outrage generated by the perception of a repressive rogue state.

In stark contrast, a minority of states responded to their nascent insurgencies, according to textbook counterinsurgency doctrine, by forming a “parallel insurgency.” States that tried to “terrorize” the terrorists were most likely to see civil war in their immediate future. Assassinating leaders and supporters of the opposition group, the state’s parallel insurgency would lead to a series of geographical and tactical adaptations that would produce civil war.
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Note: The number of attacks listed is for the duration of the opportunity spell. The CIRI score listed is the average score for the opportunity spell.
Part II

INSIDE CIVIL WAR: INSURGENCY GROWTH
Chapter 3

The Harshness of the State’s Counterinsurgency Response: Cross-National Variation in State Repression of Physical Integrity Rights during Civil War, 1981-2005

3.1 Introduction

The dynamics of violence during civil war has garnered a considerable amount of attention in recent years. Efforts have focused on explaining the severity of combat in civil conflicts (Heger & Salehyan 2007; Lacina 2006) with particular clusters of researchers focusing on the disproportionate killings of civilians or non-combatants (Downes 2008; Eck & Hultman 2007; Humphreys & Weinstein 2006; Kalyvas 2006; Weinstein 2006) and the less frequent but extreme forms of state violence such as mass killings (Valentino 2000, 2004, Valentino, Huth & Balch-Lindsay 2004) and state sponsored politicides and genocides (Bartrop 2002; Colaresi & Carey 2008; Dadrian 2004; Dutton, Boyanowsky & Bond 2005; Krain 1997, 2005; Fein 1993; Harff 2003; Mann 2000; Montalvo & Reynal-Querol 2008; Winton & Unlu 2008). Absent from this large body of literature on civil violence is an attempt at explaining cross-national variation in state repression of physical integrity rights during civil war despite the fact that these specific types of violations (e.g., torture, political imprisonment, forced disappearances and extra-judicial killings) have been the staples of many counterinsurgency campaigns. This is empirically reflected in the finding that state involvement in civil war is one of the strongest, if not the strongest, predictor of widespread physical integrity violations by nation-states (Poe & Tate 1994; Poe, Tate & Camp-Keith 1999).

Explaining why some states are far more repressive in their counterinsurgency responses than other states experiencing civil conflicts should be of interest to a diverse range
of scholars and policy makers interested in human rights, civil war dynamics, repression, and counter-insurgency doctrine. Consider the cases of Peru and Bangladesh, both of which fought similar sized insurgencies (4 to 5 thousand active rebels in both cases) across almost exactly the same time period (1980-1996 and 1974-1997, respectively). The Peruvian state employed a harshly repressive counterinsurgency campaign against Sendero Luminoso in which tens of thousands of Peruvian citizens were killed by extra-judicial execution or forced disappearance, most notably in the Ayacucho province. By contrast, the government of Bangladesh responded far less harshly in its counterinsurgency responses, despite the fact that the rebel organization it faced (Shanti Bahini) was slightly larger than Sendero, and Bangladesh’s national army was half the size of the Peruvian army. At least among these two cases, the extant human rights literature would clearly predict Peru to be the less repressive state. Peru had a democratic polity score during most of the conflict and a population size several times smaller than Bangladesh. Nor do the two cases fit neatly into any weak state argument; Bangladesh, the less repressive state, was considerably weaker, both in income per capita and the size of its military.

This paper examines what characteristics of a civil war and the states engaged in them are important in explaining variation in state repressive behavior both within and across countries.

The chapter is organized in the following manner. First, using descriptive statistics, I briefly examine the severity of physical integrity violations during civil war and the amount of variation to be explained. Second, relying heavily on the works of Wickham-Crowley (1990), Mason (1989a, 1989b), and Kalyvas (2006), I develop a theoretical model of state repression during civil war that helps explain the differential levels of state repressive violence within and across conflicts. Third, I propose a series of testable hypotheses derived from the theoretical
approach. Fourth, a regression analysis tests these expectations against every episode of civil war between 1981 and 2005 using data on worldwide physical integrity violations from the CIRI Human Rights Data Project (Cingranelli & Richards 1999). Eighty-nine civil wars in fifty-eight countries (taken from Sambanis 2004) are ultimately included in the analysis, creating 658 pooled country-years spent in civil war.

3.2 The Severity of Human Rights Abuses during Civil War

In the first cross-national study examining physical integrity violations that included the presence of a civil war within the nation-state as an explanatory variable, Poe & Tate (1994) found it to be the strongest predictive variable in their model, aside from the dependent variable lagged one year. In an expanded study, covering most countries from 1976 to 1993, Poe, Tate & Camp-Keith (1999) again found civil war involvement to be the strongest predictor of the repression of physical integrity rights by states, as did Harrelson-Stephens & Callaway (2003) in a more recent study. Krain (1997) found civil war settings to be the best predictor of the onset of state sponsored genocides and politicides. Valentino (2004) found similar results for the onset of mass killings of civilians perpetrated by governments.

A preliminary examination of state repression of physical integrity rights using data from the CIRI Human Rights Data Project (Cingranelli & Richards 1999) shows that the overwhelming majority of the worst country-years for physical integrity violations over the last two and a half decades occurred inside a civil war. Out of the 4,601 country-years in the CIRI project from 1981 to 2005 there are only 184 years that receive the worst possible score on the project’s nine point physical integrity index (physint), meaning that all four personal integrity rights (torture, political imprisonment, extra-judicial killings and disappearances) were violated.
frequently by the state in that year. Of those 184 years, 166 (over 90 percent) fall between the
dates of onset and termination of a civil war found in the Sambanis (2004) civil war dataset,
which includes every major civil war fought since 1945 that produced at least one thousand
battle deaths.

Examining the nation-states that perpetrated the worst possible levels of repressive
violence in the CIRI project yields a total of 44 states (out of a total of 195 states) that had at
least one year which received the worst possible score for physical integrity violations by the
CIRI project. Of these 44 states, all but 7 were fighting a civil war in the given year. In other
words, 84 percent (37 of 44) of the nation-states coded by the CIRI project as grossly violating
the physical integrity rights of its citizens between 1981 and 2005 were fighting a civil war at
the time.

![Figure 3.1. Variation in State Repression of Physical Integrity Rights during Civil War](image-url)
Having pointed out that the overwhelming majority of the worst years for human rights violations committed by states over the last two and a half decades occurred during civil war, it is equally important, from a research standpoint, to show the substantial level of empirical variation that exists in state repressive behavior during periods of civil war. The Sambanis (2004) civil war dataset contains 658 country years spent in civil war for the time span for which we have CIRI human rights data. Figure 3.1 shows a distribution graphic of those 658 years and their respective scores on the CIRI nine point physical integrity index. The original scale has been reversed so that lower values represent lower levels of repression and higher values correspond to greater levels of repression. As mentioned, the majority of the worst years for physical integrity violations found in the CIRI project (e.g., those years receiving a score of 8 on a reversed CIRI \textit{physint} index) occurred during a civil war. Nonetheless, those years as a group are a minority among total civil war years, representing around a quarter of all years spent in civil war. As can be seen in the same graphic, more than half of all years spent in civil war (.25 to .75) received a score somewhere between four and seven on a reversed CIRI \textit{physint} index with around ten percent of civil war years receiving a score between zero and three. Thus, the dependent variable to be used in the present analysis displays substantial variation even during years when a civil war was on-going in a nation. In the following sections, I review several theoretical approaches that help explain the substantial range of variation in state repressive behavior across conflicts.

3.3 General Theories of State Repression: Regime Strength and Threat Perception

In light of distinctive terminologies for the dependent behavior, whether emphasizing human rights (Henderson 1991; Mitchell and McCormick 1988; Poe and Tate 1994); state
terrorism (Stohl and Lopez 1984) or state repression (Davenport 1995a, 1995b, 1996a, 1996b; Rasler 1986), explanations for why states engage in repressive behavior fall generally within a rational choice framework (e.g., Frohlich & Oppenheimer 1970; Leites and Wolf 1970; Lichbach 1984, 1987; Poe 2004; Mason 1989a, 1989b; Tullock 1971). Rational choice approaches assume that, alongside some incentives for collaboration, government and military leaders engage in repression to decrease support to oppositional movements when the expected benefits outweigh the expected costs. Specifically, leaders seek out variable information on threat levels and dissident activity along with attributes of the environment that help them create estimates of the expected utility of using repressive violence.

A burgeoning human rights literature has identified a number of macro-level variables that have been consistently powerful in explaining differential levels of physical integrity violations across states. Democratic states generally have better human rights records than non-democratic states. States with a higher economic standing fare better in respecting human rights, relative to poorer nations. Third, states involved in international war have been found to be more likely to repress their populations. Fourth, states with larger populations tend to have worse human rights records than states with smaller populations. Lastly, past levels of repression are strongly predictive of future levels of repression. Considerable evaluation and replication in the forms of alternative indicators, different time spans, different samples of states, different human rights indexes and different statistical methodologies have consistently found these variables important in explaining levels of physical integrity violations by nation states (e.g., Abouharb & Cingranelli 2006; Bueno De Mesquita et. al. 2005; Davenport 1995,
Poe (2004) presents a leadership decision-making model which attempts to integrate these variables into a single theoretical or heuristic framework of why state leaders choose to repress. Poe argues that leaders monitor and weigh levels of domestic threat (T) against the strength of their regime (S). The ratio of domestic threat to regime strength (T/S), will determine the utility of using repression. Put simply, variables that increase perceptions of state security and state strength, such as higher levels of democracy and economic development, should translate into fewer acts of repression. By contrast, those conditions that increase the governing elite’s fears of political survival, such as poverty, autocracy, or oppositional violence will likely lead to lower levels of perceived security, and as a result, greater amounts of repressive behavior. No prior research on the repression of physical integrity rights, however, has examined whether these indicators will have the same effect in a subsample of states engaged in civil war. We can begin to make this distinction by testing these variables against our sample of country years spent in civil war from 1981 to 2005. Thus, I propose the first set of control variables to be included in the analysis (e.g., statist measurements found to be significant predictors of physical integrity violations in previous scholarship).

Control 1. The lower the level of democratization in the civil war state, the greater the levels of repression committed by the state.

Control 2. The greater the level of poverty in the civil war state, the greater the levels of repression committed by the state.
Control 3. The greater the population in the civil war state, the greater the levels of repression committed by the state.

Control 4. Involvement in a concurrent international war by the civil war state should be associated with greater the levels of repression committed by the state.

3.4 State Repression Inside of Civil War: Territorial Contestation

Seeking to explain state repressive behavior across large samples of states, general theories of state repression (e.g., Poe and Tate 1994) have appropriately focused on how state’s might perceive threats, on oppositional activity and how vulnerable the state may perceive its own security to be. We know that state weakness and oppositional threats increase repression when examining large samples of states, but we know very little about what increases the harshness of state repression among a sample of states already fighting an ongoing civil war. While the state repression literature, as a whole, has emphasized threat perception, a much smaller literature on the dynamics of violence within civil war has consistently emphasized territorial contestation in explaining variation in repression within civil war contexts. As Wickham-Crowley (1990:225) notes,

Terror is particularly common in guerrilla warfare because there is an aggregation and mixture of combatant, noncombatant, and support system into a very small social and geographical space. The nature of the support system...consists in large part...of the peasantry itself. Whether the peasantry acts willingly or not, there is often a very deep social and geographical overlap in guerrilla warfare between the support system (the source of military intelligence, food, supplies, and recruits) and the civilian population, with a large overlap between the civilian population and the combatants as well.

State repression of noncombatants is quite frequent during civil war, while an anomaly outside it, due to the fact that both sides in a civil war have to extract resources and recruits from the same civilian population. This simple dynamic makes the primary tactical objective of
both the state and the rebel group during a civil war to out bid each other in the construction of
civilian support systems. Thus the most severe levels of state repression in the CIRI project are
clustered closely around civil war events, as the targets of state repression shift from leaders
and active participants in oppositional activity toward the general population. Such a shift
rarely occurs outside of civil war.

The most important factors in explaining levels of state repression against civilian
populations during civil war, according to Wickham-Crowley (1990:226), are the depth of
“system overlap” that is present between the government, the guerrillas, and civilian support
areas, “…as well as the lack of information on the part of government soldiers necessary to sort
out these categories…” (1990: 230). As Mason (1989a: 470) notes, it is in these areas of dual or
overlapping sovereignty that both the government and the rebels “…compete for the support
and loyalty of citizens by providing them with flows of goods and services…and by threatening
them with sanctions if they are detected free riding or lending aid, political loyalty, or other
forms of support to the rival elite group.”

Much of the violence against noncombatants results from the inability to separate the
rebels from the civilian populations that frequently surrounds them - either by coincidence or
by design (Azam & Hoeffler 2002). Mason (1989: 486) finds a common pattern that
“...continually increasing the scope and intensity of coercion as a means of dealing with
crowding effects means that the targeting of that coercion is likely to become less precise and
more arbitrary....” Valentino, Huth & Balch-Lindsay (2004) have noted the tendency of states to
engage in greater amounts of violence against noncombatants when the opposition relies more
heavily on evasive tactics rather than engaging in more conventional combat. Ultimately, it is
the intensity of competition between the government and rebels within the civilian collaboration market that helps explain the variance in repression suffered by civilian populations, as the leadership from each side “...endeavors to shift the preferences of nonelites in its favor and to induce them to provide the supportive behaviors that are necessary to sustain that elite’s political organization and programs” (Mason 1989: 471). Kalyvas (2006) provides even more detailed theoretical insight into how this tactical objective is related to territorial contestation and why it leads to differential levels of violence targeted disproportionately against civilians.

Kalyvas (2006) puts forth a general theory of political violence during civil war where both the government and insurgents “...seek to maximize territorial control subject to the local military balance of power....” According to Kalyvas, gaining and keeping control over territory requires the exclusive collaboration of individual civilians who, in turn, maximize various benefits subject to survival constraints. Irrespective of their sympathies...most people prefer to collaborate with the political actor that best guarantees their survival rather than defect by helping the rival actor (2006:12).

Kalyvas argues that both actors in a civil war seek hegemonic control over physical territory because such control consolidates collaboration from the resident population as residents tend to cooperate with whichever side best controls the area where they live. By doing so, they avoid repression from the locally dominant group while also gaining protection from the rival political actor. Hence, territorial control leads to collaboration, which in turn, consolidates territorial control. Different strategies of civil war violence, according to Kalyvas (2006:12), result from the twin processes of segmentation and fragmentation of sovereignty: territory is divided into zones monopolistically controlled by rival actors (segmentation) and zones where
these actors’ sovereignty overlaps (fragmentation). The type of sovereignty or control that prevails in a given region affects the type of strategies followed by political actors.

When territorial control within the state is segmented, an insurgent organization has been successful in achieving territorial and collaborative control in one or more regions of the country, replacing the government’s institutions there with their own. The degree of state presence in the area coupled with the organizational strength of the rebels will determine their level of control and hence the general level of segmentation. The level of rebel control will determine the magnitude of their demands upon the population segments residing within their jurisdiction, ranging from a passive tolerance to active participation in the rebel army or a post in the insurgent’s shadow government. In areas of fragmented control and overlapping loyalties, where each side has some control, but not complete control, each actor can use repressive violence to force collaboration with their organization. We expect state repressive behavior to vary across space and time because “…political actors do not need to use violence where they already enjoy high levels of control and cannot use selective violence where they have no control whatsoever; having no access to information…”(Kalyvas 2006:12). Instead, actors use repressive violence “…in contested areas, where they have incomplete control” (Kalyvas 2006:12-13).

To test his theory of territorial contestation and civil war violence, Kalyvas (2006) performs a micro-comparative study on the Greek civil war displaying the theory’s internal validity alongside an abundance of anecdotal evidence from many civil wars which strongly suggests external validity. There has not yet been any quantitative attempt, however, to theoretically model the specific conditions or processes that might lead to different patterns of territorial control during civil wars; nor has there been any related effort to examine variation
in state repressive behavior during civil war in any cross-national fashion. Two important correlates of Kalyvas’ theory should be explored further: the origins of each type of territorial control, and second, what factors or conditions of a civil war are consistent with the identification or estimation of the level of contested territory within a civil war state.

Variation in state repressive behavior during civil war should be contingent upon, first, characteristics of the civil war, or the state housing it, that either create or correspond with levels of segmented versus fragmented territorial control. These theoretical expectations can be formalized into two broad hypotheses.

Hypothesis 1 (Origins) Conflict dynamics theorized as generating larger amounts of fragmented territorial control and smaller amounts of segmented territorial control should be associated with greater levels of repression committed by the state.

Hypothesis 2 (Estimation) Conflict variables theorized as generating larger amounts of fragmented territorial control and smaller amounts of segmented territorial control should be associated with greater levels of repression committed by the state.

3.5 The Origins of Segmented versus Fragmented Territorial Control during Civil War: Conflict Geography, Relative Capabilities and Social Control

In the following sections I use Boulding’s (1962) concept of a state’s “loss of strength gradient” to illustrate how the dominant pattern of territorial control that develops within a civil war should be influenced by the reach of the state and the location of the primary rebel base area. I theorize that the origins of either segmented territorial control (i.e., where both the rebels and the state enjoy high levels of control in different areas) or fragmented territorial control (i.e., where the rebels largely hide in areas more or less under government control) is
largely a function of the reach of the state into its national territory measured along three dimensions: geographical distance, military capabilities and social control.

3.5.1 The Reach of the State, part 1: Geographical Distance and the Loss of Strength Gradient

Since the level of government control over national territory is not uniform throughout the state, the dominant pattern of territorial control that develops during a civil war should be contingent, to some degree, on the location of the rebel’s primary base area. Kalyvas suggests that the relationship between the level of government control in a given area and the amount of violence employed by the state in the area is often curvilinear with respect to the position of the state and the position of the rebel group. State repression is lowest in areas where government control is high and rebel control is low, and in areas where government control is low and rebel control is high. Thus, it is the level of control enjoyed by both actors in the same area that determines the potential for the state to use repressive violence in an effort to shift the balance of power in the area. Assuming that (a) government control diminishes in a roughly linear fashion as we move outward from the capital, and (b) that the highest density of rebel control should be close to its primary base area (although the rebel’s operating range may be much larger), the position of the rebel’s primary base should determine, in large part, the potential for what kind of territorial pattern will become dominant within the state. Stated differently, the position of the rebel base, given the state’s loss of strength gradient over its territory (Boulding 1962; Buhaug 2007) will produce the potential for either extreme segmentation or extreme fragmentation.

A government’s ability to project its force, and the accompanying control over territory and the populations residing therein, should diminish with distance in a more or less linear
fashion (Boulding 1962). At the sub-national level, the density of military power should be highest around the city which houses the government’s leaders, and it should gradually diminish as we move away from the capital. Out of a finite military budget, an adequate amount of military spending will be devoted to protecting the lives of the leadership in and around the capital, with the remainder of resources being spread out over the rest of the nation’s territory. Thus, the density of military power will be highest around the capital.

![Figure 3.2 Loss of Strength Gradient over Territory with Two Possible Rebel Base Locations](image)

Figure 3.2 attempts to show how the dominant pattern of territorial control that develops during a civil war should be influenced by the position of the rebel base area. The star represents the capital of the state, where the leaders usually reside, and each cell within the square represents a certain amount of territory (say, 50 square miles). Within each cell is a
number that can be used to measure the amount of any government resource related to maintaining control over that area (e.g., political supporters, military capability, and public goods). For the present discussion, think of the numbers inside the cells as the number of government supporters that live in a 50 square mile area (in the thousands). Thus, around the capital city, where the government’s resources have the highest density, the government has 100,000 supporters. Hundreds of miles away the number of government supporters that can be found in any 50 square mile area may be very low.

Consider the rebel base located at the right side of the grid (along the eastern border of the country). Because the capital of the state is located at the western end, the government has few supporters in the extreme eastern realms of the state. Let us assume that the rebels have around 15,000 supporters in their core area and that their support diminishes in a similar pattern with distance. Although the pattern of territorial control that ultimately develops under such a scenario will depend on a number of factors, this eastern rebel base produces at least the potential for a dominant pattern of segmentation to develop. Under segmented control, there is little “system overlap,” meaning few supporters of each regime living in the same geographic unit. As shown, the areas under rebel control have very few government supporters, and areas under full government control have very few rebel supporters. In short, the eastern rebel base should produce an overall dynamic where the government is, more or less, secure in the areas it controls and the rebels are, more or less, secure in their area. A rebel base located far away from the capital should create a structural pattern where rebel control at the local level is maximized and government control is minimized. Like competing firms, the rebels are more likely to achieve monopolistic control in a particular civilian collaboration
market the lower the amount of government competition that exists in the area. As long as rebel expansion is confined to areas with a low government presence, the overall pattern of control within the country can be best described as segmented. In stark contrast, a rebel base located near the capital creates a zero-sum game where any increase in rebel control comes at the expense of the government.

The second leftward oriented rebel base should produce a different structural tendency, one that is expected to produce fragmentation. The western rebel base is located in an area of the country which contains a high density of government supporters. The rebel base is located in a region of the state which contains around 30,000 government supporters per 50 square miles. Almost regardless of the amount of support the rebel’s enjoy in and around this primary base area, such a location will produce heavy amounts of overlapping loyalties and fragmented control simply because the rebels have a stronghold right in the middle of an area in which the government enjoys a large degree of support. This will produce a large number of defections and denunciations, exponentially increasing the government’s list of insurgency supporters that could be targeted for removal and/or punishment. Thus, I propose the following expectation: Hypothesis 1: Shorter geographical distances between rebel base areas and the capital of the state should lead to larger amounts of system overlap and fragmented territorial control resulting in greater levels of repression committed by the state.

3.5.2 The Reach of the State, Part 2: Military Capabilities and the Loss of Strength Gradient

Predicting the effects of the balance of military capabilities between combatants on levels of state repression during civil war presents an interesting puzzle. In the human rights literature we find that higher levels of state capacity, typically proxied by GDP per capita, are
significantly correlated with lower levels of physical integrity violations (Poe and Tate 1994). The same literature has found that states in civil war engage in higher levels of physical integrity violations. In the civil war literature, state capacity (also proxied by GDP per capita) is a strong predictor of the onset of civil war (Fearon & Laitin 2003). Here we have several mutually reinforcing findings regarding state capacity, civil war, and state repressive behavior from two separate bodies of literature. In global samples of countries we find that (a) weaker states are more repressive than stronger states, (b) weaker states are more vulnerable to civil war than stronger states, and (c) states in civil war are more repressive than states not in civil war. The expectation is clear: weaker states should engage in higher levels of repression and stronger states should engage in lower levels of repression.

This may not hold, however, for states involved in civil war. If state repressive behavior during civil war is a function of territorial contestation, the exact opposite relationship appears to be more logically sound. The fact that the state is in a civil war is evidence of endogenous conditions within the state conducive to the growth of an insurgency. I argue that the same conditions that make a state susceptible to civil war in the first place will also limit the state’s ability to fight the insurgency once it is underway. The weaker the state fighting a civil war, the weaker the ability of the regime and its national military to police the peripheral regions of the state, which should result in greater rebel consolidation in those areas. If we assume that weak states control some areas but not all areas within their borders and that rebels tend to seek out areas with the lowest levels of government control, then segmented territorial control should be more likely in weaker states. On the other hand, we should expect greater military strength by the state to lead to greater levels of fragmented territorial control as stronger states will
have more de facto control over a larger share of the state's territory from the beginning of the conflict.

Figure 3.3 Military Capabilities and the Loss of Strength Gradient

Using the same grid of government control from the previous section, the fundamental difference between a weak state and a stronger state, following the logic of the loss of strength gradient, can be seen by removing the areas on the outskirts of the grid where the government has little to no control. The leaders in both weak states and strong states alike will use a large proportion of their resources to secure their own lives in the capital, with the remainder of military resources spread out over the country. Stronger states will have a larger amount of military resources to spread. Therefore, the areas on the control grid with little to no government control will become smaller as the state’s military capabilities become larger. The
gray regions in Figure 3.3 represent areas devoid of government control that will be present only in a very weak state. In stronger states, those areas will not be available for rebel settlement. As the strength of the states increases, the grey areas will expand even further inward so that even the farthest reaches of the state’s periphery have a substantial government presence.

Thus, in weaker states higher degrees of segmentation are made possible by the availability of areas with little to no effective government control. In stronger states the only option for the pattern of territorial control that will develop involves varying degrees of fragmentation. In strong states, where the government has some control in almost all areas, the rebels have no choice (aside from transnational bases) but to operate in areas under some degree of government control.

Hypothesis 2: Greater government military strength should result in larger amounts of fragmented territory resulting in greater levels of repression committed by the state.

3.5.3 The Reach of the State, part 3: Ethnic Polarization, Marginalization and Social Control

Up to this point, the government’s grid of control over its territory has been treated as the output of a mechanical formula based on the amount of resources put into it. There is also a substantial literature on state capacity and political development that emphasizes the role of identity and political preferences as determinative forces in a state’s ability to achieve control throughout its territory. Migdal (1988) argues that it is primarily the differences between societies, rather than the differences between states, that is most important for understanding why so many states cannot achieve control over their populations and territory. As Migdal (1988:33) notes, “In parts of the Third World, the inability of state leaders to achieve
predominance in large areas of their countries has been striking. ...The ineffectiveness of state leaders who have faced impenetrable barriers to state predominance has stemmed from the nature of the societies they have confronted....” The impenetrable barrier that Migdal refers to is “...the pattern of fragmented social control...” (1988:40), the common denominator of which, Migdal suggests, is high levels of ethnic fractionalization. Migdal notes that well over half of all Third World countries score either “very high” or “high” in measures of ethnic and linguistic fractionalization, “...while less than a third of other countries fall into these categories...” (1988:38).

For the state, the consequences of linguistic and ethnic fragmentation is a model of state-society relations where the state is but “one organization in a mélange within the boundaries in which it seeks to rule” (Migdal 1988:40). Social fragmentation reduces a government’s ability to induce from the population “compliance, participation, and legitimation,” the three primary areas of social control, according to Migdal (1988:40). Within the boundaries of the state, there is a national government, village leaders, bosses, chiefs, lords and rebel groups - all with their own rules of the game - offering peasants survival strategies in return for collaborative support and material surpluses. The strategies that people choose depend on the material incentives and the level of coercion that the different actors can bring down upon them. Ethnically fragmented societies, which deny state leadership mobilization capabilities and popular support, tend to produce a particular administrative and political style that Migdal terms the “politics of survival” (1988:213). Lacking the ability to penetrate society, to regulate social relationships, to extract and appropriate resources effectively, and lacking support from any sizable segment of the population that would lend some level of legitimacy,
leaders of fragmented states are more likely to seek social control through repression and coercion. In both Migdal (1988) and Kalyvas (2006), we find almost the same mechanism: higher levels of fragmentation increase the level of competition with state authorities who respond with political violence in attempts to force collaboration and deter support to the government’s competitors.

There is also evidence from other studies that ethnic marginalization produces greater levels of state violence in conflict settings. Heger and Salehyan (2007) utilize data on the ethnicity of the head of state and the proportion of the population that shares that ethnicity to show that smaller ethnic coalitions supporting the government increases the severity of civil conflicts. Other studies have found measures of ethnic fractionalization to be significant in lowering the provision of public goods and services offered by states to their populations (Mauro 1995; Alesina, Baqir & Easterly 1997; Easterly & Levine 1997). These findings suggest that ethnic fractionalization and ethnic marginalization contribute to a social fragmentation which lowers the legitimacy of the state.

How does the presence of ethnic fragmentation and marginalization fit into the previous two discussions on the state’s loss of strength gradient? The ability of a state to project its power and control should still be subject to logistical constraints similar to the previous two discussions. Assuming a similar baseline grid, we can visualize the impact of ethnic fragmentation and marginalization by superimposing onto that grid societal power centers run by local strongmen, where the government may not only lack control but face the potential for active opposition. In other words, there is latent ethnic conflict, which can be activated by changes in the balance of power.
Figure 3.4 shows a scenario where the grid of government control is the same as in previous examples -- that is, a gradual decay function -- except that I have superimposed onto the grid three ethno-political communities. One is supportive of the government and close to the capital. The other two have little to no support for the government, and given the opportunity, would like to be in power themselves. The largest bloc closest to the capital shares the same ethnicity with the current head of state and receives a disproportionate amount of the state’s resources relative to the other groups. This privileged bloc contains 300,000 more government supporters than would otherwise be expected in this region if control was maintained through coercion alone. The other two blocs would like to control the state themselves. But because they are no match for the national military, they suffer in silence and the government largely ignores them.

Figure 3.4 Ethnic Multi-Polarity and Government Control over Territory
If it were not for the situation of ethnic multi-polarity, the position of the rebel base area in this scenario might otherwise be unproblematic for the government. The rebels are operating mainly on the outskirts of government control and would be easy enough to ignore. However, given the two disloyal ethnic blocs nearby, the situation could become quite explosive. The particular location of the rebel base is less important in this example. What is important is the potential for large changes in the balance of power depending on these formerly ignored blocs of non-loyalists or semi-loyalists. If both of the eastern blocs were to join the rebels, the government could be in serious jeopardy. If the northern bloc joined the rebels and the southern bloc attempted to stay out of the conflict, then control over this neutrality-seeking southern bloc could be the tipping point for control over most of the state, given the large amount of territory with no government control at all. Presumably, in this scenario, there is no limit to the amount of repressive violence the government may choose to inflict in order to maintain control over the southern bloc.

Once a civil war erupts in an ethnically fragmented state, the potential scope of state repressive violence is much greater. I argue that ethnic multipolarity works in much the same way as multipolarity has been theorized to work in international relations theory. Multipolarity may or may not be associated with more conflict or violence, but when a conflict does arise in a multipolar system, it is generally recognized as making the conflict bigger and more violent as groups form alliances and counter-alliances seeking to provide for their own security in an environment where the balance of power is shifting (Wayman 1984). Thus I propose the following hypotheses:
Hypothesis 3: Greater levels of ethnic fractionalization should result in larger amounts of fragmented territory, resulting in greater levels of repression committed by the state.

Hypothesis 4: Greater levels of ethnic marginalization from power should result in larger amounts of fragmented territory, resulting in greater levels of repression committed by the state.

3.6 Estimating Levels of Segmented versus Fragmented Control during Civil War: The Size, Nature and Severity of the Conflict

In addition to theorizing the origins of segmented versus fragmented control of territory during civil war, I put forth in the following sections a number of indicators which I argue can be used to estimate general levels of segmented versus fragmented territorial control during civil war.

3.6.1 The Size of the Conflict Zone

The overall size or scope of a conflict should be coterminous with the amount of contested territory inside the civil war state. Buhaug and Gates (2002) have compiled geographical data on the size and scope of the combat zone for most civil wars that have been fought since 1945. Using the rebel base area as a center point, they measure a conflict zone as the geographic area that extends from the rebel base to the farthest known combat areas. The borders of the conflict zone, in other words, are the farthest battlefield sites from the rebel’s base area, giving us a good indicator of the overall amount of territorial space under contestation.

Because the size of the conflict zone reflects the interdependent relationship between the geographic penetration of both the state and the rebels throughout the country, it is
perhaps the best indicator we could have of the amount of territory under contestation during a civil war. A large conflict zone is not only indicative of rebel operations in extended areas of the country but also that the government has a military presence in those areas and is challenging the rebels militarily. As a result, larger conflict zones should be evidence of a larger amount of fragmented or contested space within the state, where both combatants have some limited and overlapping control. In contrast, a conflict where the rebel group has little territorial and collaborative control outside its home area, while the rest of the nation’s territory falls generally under the control of the government, translates into a smaller conflict zone where battlefield sites are close to rebel controlled areas. Smaller conflict zones are evidence of segmented territorial control where the rebels exercise considerable control in their home region while operating very little out of it.

Hypothesis 5: Larger conflict zones should be indicative of larger amounts of fragmented territory, resulting in greater levels of repression committed by the state.

3.6.2 The Nature of the Conflict: Ethnicity and Territoriality

Civil wars are typically categorized according to whether the primary cleavage between the government and rebel group is based in an ethnic or ideological divide and whether the desire of the rebel group is to take control of the national government (i.e., revolution), or part of the state’s existing territory (i.e., secession). In accordance with the proposed theory, ethnic conflicts and territorial conflicts (while found to be disproportionately at risk for rare but severe acts of violence such as genocide) should experience less day-to-day repression for several reasons related to how the nature of such conflicts influences the scope of a state’s counter-insurgency efforts.
Ethno-regional conflicts, by their very nature, should result in less fragmented territory since the insurgents themselves and their pool of potential recruits tend to be geographically segregated from the population at large, thereby, decreasing the amount of fragmented space in addition to solving the “identification problem” in the targeting of repression. Studying the onset of ethnic conflict, Toft (2006) found that concentrated ethnic groups are significantly more conflict prone than dispersed groups. Thus, when we see an ethnic conflict, it is likely being fought by a geographically concentrated group. Most ethnic conflicts are also territorial disputes, and all territorial disputes are ethnic conflicts. Indeed, the underlying cause of most ethnic conflicts involves the failure of the central government to recognize the group’s claim to a preexisting territorial or regional sovereignty. As a prerequisite, most ethnic conflicts usually involve some degree of segmented territorial control and thus should be associated with less state repression. In addition, scholars have found that ethnic and territorial conflicts tend to be fought further away from the capital city (Buhaug & Gates 2002) and generally have lower levels of severity (Hegre & Salehyan 2007). Eck and Hultman (2007) found the killing of civilians by government forces to be more common during revolutionary rather than territorial conflicts. Territorial conflicts, which do not directly threaten the government’s existence, are likely to be perceived as less of a risk. As a result “…fewer troops are deployed in these regions and less priority is placed on rooting out rebel forces…” (Eck and Hultman 2007:241).

When the basis of collaboration or defection is rooted primarily in the acceptance or rejection of an ideology, rather than a visual ethnic affinity, it also becomes increasingly difficult to determine who is on which side. In civil wars fought over the control of the national government, (i.e., revolutions), the entire population and territory of the state can be
implicated in the fight. As a result, generalized repression should be more likely to develop in ideological civil wars due to the identification problem (i.e., rebels are less identifiable). Since ethnic insurgents are more likely to be geographically concentrated and are comprised of a recognizable group, repression by the state will tend to be limited not only to members of the particular group but also to the geographic region where that group resides. In contrast to ethnic conflicts, where the government can monitor suspects based on ethnic markers, effective deterrence should be harder for the state to achieve in ideological conflicts since the government cannot monitor and punish those whom it cannot identify. The decreased probability of being identified may also make defection to the rebel group more frequent in ideological conflicts, which in turn increases the demand and number of targets for state repression. As violence begins to escalate in ideological conflicts and active rebels are not generally distinguishable from the neutrals surrounding them, state repression is more likely to be inflicted upon those engaged and not engaged in the insurgency (Mason and Krane 1989b). The more difficult it is for the military to distinguish rebel from neutral, the wider the state is likely to cast its net of repressive sanctions (Mason and Krane 1989b). A summary of these expectations would take the following forms:

Hypothesis 6: Civil wars where the primary cleavage between the combatants is ideological (not racial) should result in larger amounts of fragmented territory and more generalized targeting resulting in greater levels of repression committed by the state.

Hypothesis 7: Civil wars fought for control of the central government (revolutions) should result in larger amounts of fragmented territory and more generalized targeting resulting in greater levels of repression committed by the state.
3.6.3 The Severity of Contestation: Insurgency Activity and Deaths

One of the most visible indicators of the level of contestation and overlapping territorial presence within a civil war is the severity of combat. Lacina & Gleditsch (2005:148) argue that battle deaths, as an indicator, is “the best measure of the scale, scope, and nature of the military engagement that has taken place...and how frequently and widely they engage each other.” A larger number of battle deaths in a conflict suggest larger areas of overlapping military presence in the state which translates into more physical opportunities for the two sides to meet, engage in combat, killing more people. The same pattern should also hold for insurgency attacks since an attack, by its very nature, requires a target considered to be worthy to the government and insurgents in the area capable of executing the attack. A greater level of engagement should also lead to higher levels of threat perception - a state might increase its use of repression out of possible desperation (Downes 2008; Valentino, Huth, and Balch-Lindsay 2004; Davenport 1995b). The frequency of insurgency attacks every year provide the state a highly visible and costly indicator of the magnitude of insurgency activity but be unrelated to how much territory is being contested by the insurgents. How might we disentangle threat from territorial contestation with regard to the impact of insurgency activity on state violence? Taking into account, not only the number of insurgency attacks per year, but also the number of cities within the state housing an insurgency attack, could also give us the opportunity to gauge the importance of threat versus territorial contestation during civil war. Taking into account how many cities within the state housed insurgency attacks in the year provides us with another estimate of how much territory is being contested within the state. Whereas the size of the conflict zone (Buhaug and Gates 2002) gives us a measurement
involving the borders of insurgency activity, the number of cities with insurgency attacks gives us a measure of how much space within that zone contains both government targets worthy of attack and insurgency cells carrying out those attacks. Thus I propose the following:

Hypothesis 8: Larger numbers of yearly battle deaths should be indicative of larger areas of fragmented territory, resulting in greater levels of repression committed by the state.

Hypothesis 9: Larger numbers of insurgency attacks should be indicative of larger areas of fragmented territory, resulting in greater levels of repression committed by the state.

Hypothesis 10: Larger numbers of cities experiencing insurgency attacks should be indicative of larger areas of fragmented territory, resulting in greater levels of repression committed by the state.
Chapter 4

Research Design and Empirical Test II: The State’s Counterinsurgency Response

Although state repressive behavior often takes milder forms, such as limiting civil and political liberties, repression is defined here as the purposive infliction of physical violence against a person for political reasons. For reasons already discussed, states fighting civil wars not only engage and combat rebel armies but frequently use widespread physical violence against civilian populations. The most severe and common forms of physical repression include torture, imprisonment, disappearances and extra-judicial killings. States have so frequently employed these same “physical” elements of repressive violence that they have come to be referred to in the human rights literature as violations against the “physical integrity” of the person (Poe & Tate 1994). This group of “physical integrity rights” has gained the status of a special group of human rights, at the most representing an essential core of a universal conception of rights, while at the very least being necessary for any other political or civil right to be fully enjoyed.

The most widely used empirical databases of physical integrity violations committed by states are the Political Terror Scale (Gibney 2004; Poe & Tate 1994) and the CIRI Human Rights Data Project (Cingranelli & Richards 1999). The dependent variable used in the present analysis is the magnitude of government repression of physical integrity rights measured by the CIRI project (Cingranelli & Richards 1999). The CIRI physical integrity index is made up of multiple components which allow us to test the extent of correlation between violence captured in an independent variable, such as battle deaths, and similar types of violence as captured in the components of the index. The CIRI physical integrity index is a nine point additive scale derived
from the four ordinal indicators of physical integrity (torture, extrajudicial killings, disappearances and political imprisonment) based on information regarding the human rights practices of state governments found in Amnesty International’s Annual Reports and the U.S. State Department’s Annual Country Reports on Human Rights Practices. Each country is assigned 0, 1, or 2 points according to whether physical integrity violations occurred “frequently,” “occasionally” or “have not occurred.” For this paper, the original scale was reversed to ease comparability with PTS in which higher values represent higher levels of state repression and lower values represent lower levels of state repression.

4.1 Operationalization of Independent Variables

Distance. This variable is a measure of the distance in square kilometers (logged) between the conflict center point (rebel base) and the capital city of the state, taken from Buhaug and Gates (2002). Where more than one conflict was ongoing in the same country-year, the variable indicates the average distance between the state’s capital and several rebel base areas.

Military Capabilities. I include two different indicators of the state’s military capability and one indicator of the rebel group’s military strength. Rebel Strength is an ordinal variable taken from Cunningham, Gleditsch, and Salehyan’s data on rebel groups. This variable measures the strength of the rebel group relative to the government’s military capabilities (see Heger & Salehyan 2007). Their variable has a five-part coding of rebel strength (much weaker, weaker, parity, stronger, much stronger) which takes into account troop levels, external support, control of territory, and organizational cohesion. There are no states in the sample with multiple ongoing conflicts where the coding of relative strength differed among rebel
groups. *Military Size* is the size of the government’s military in units of one thousand soldiers and *Military Spending* is the size of the state’s defense budget in units of one thousand dollars. Both are taken from the Composite Index of National Capability (CINC) (Singer 1987).

Ethnic Fractionalization. This is a proportional measurement of ethnic fractionalization for each state in each year taken from Fearon and Laitin (2003). The variable ranges from 0 to 100 indicating the probability that two randomly selected persons from the state would be from two different ethnic groups. I also include the squared term to control for a parabolic relationship. The presence of several relatively large ethnic groups is thought to increase the potential for ethnic dominance which generates security dilemmas and higher levels of conflict (Elbadawi and Sambanis 2002; Ellingsen 2000; Reynol-Querol 2002; Sambanis 2001).

Minority Rule. The degree of minority rule in the state for each year is taken from Fearon, Kasara and Laitin (2007). The variable represents the percentage of the plurality group that does not share the same ethnicity as the head of state (for our sample the range is .04 to .98).

Size of the Conflict Zone. This is a proportional variable measuring the size of the conflict zone as a percentage of the state’s overall territory. It is taken from Buhaug and Gates (2002). Buhaug and Gates (2002) measure the radius of the conflict zone as the distance from the rebel base area (center point) to the farthest known battlefield sites.

Severity of Conflict. The variable *Battle Deaths* is a measure of battle deaths in each country year taken from Lacina & Gleditsch (2005). The number of insurgency attacks the state suffered in every year (Attacks) and the number of cities housing insurgency attacks per year (Cities) were taken from the Global Terrorism Database (LaFree and Dugan 2008). For those
years where a state had deaths, attacks, and cities with attacks from more than one conflict, I took the sum of all deaths, attacks and cities in the given year (I check the effects of this with a robustness test in the analysis section).

War Type. I created two variables to distinguish the nature of each conflict. The first variable (Ideological War) indicates whether the primary cleavage between the combatants was ideological or ethnic, taken from Sambanis (2001). I created a companion dummy variable (Both Types) controlling for the presence of both an ethnic and non-ethnic conflict in the same country year. The second war type variable, (Territorial War) denotes whether the nature of the incompatibility between the combatants involves a territorial dispute or a fight for control over the central government (i.e., revolution) derived from the UCDP/PRIO Armed Conflict Dataset (Gleditsch et al. 2002). Again, I created a companion control variable indicating the presence of a revolutionary conflict and a territorial conflict fought in the same country year. There are sixty-one country years in the sample where a state fought both an ethnic and a non-ethnic civil war, or both a revolutionary and a territorial conflict, in the same year.

4.2 Control Variables

The Poe and Tate Model. The level of democracy in the civil war state is measured using the Polity IV democratization index (Jaggers & Gurr 1995). Gdp per capita is taken from the World Bank’s World Development Indicators and adjusted to a year 2000 base. Population for the years 1981 to 1999 is taken from Fearon & Laitin (2003) and updated through 2004 with the World Bank’s Development Indicators. Interstate war participants are taken from the Correlates of War interstate war file (Sarkees 2000).
War Duration. The variable \textit{(War Duration)} is a count of duration in years beginning at the date of onset for each civil war. If a civil war began in 1975, its duration in 1981 when it enters our temporal domain would be 6 years. I include a control variable \textit{Conflict Dyads} to control for multiple ongoing conflicts in the same year.

Number of Ongoing Civil Wars. The variable \textit{Conflict Dyads} measures how many civil wars each state is fighting in each year according to Sambanis (2004). Of the 658 country years spent in civil war between 1981 and 2005, there are 77 years in which a state fought more than one civil war in the same year (India, Ethiopia, Philippines, Burma, Sri Lanka, and Indonesia). This variable ranges from 0 to 3.

4.3 Methodology

The unit of analysis employed is the country year (N=658) with the population of civil war cases taken from Sambanis (2004). For a civil conflict to be classified as a civil war by Sambanis, it must meet the following criteria: (a) the war has caused more than one thousand battle deaths; (b) the war represented a challenge to the sovereignty of an internationally recognized state; (c) the war occurred within the recognized boundary of that state; (d) the war involved the state as one of the principle combatants; (e) the rebels were able to mount an organized military opposition to the state and to inflict significant casualties on the state. The resulting pooled cross-sectional time-series dataset contains variables for 58 countries, engaged in 89 civil wars, covering the years 1981 to 2005.

In a recent forum on the analysis of time series cross sectional data (TSCS), Wilson & Butler (2007:102) argue that to properly model TSCS data, researchers should (at a minimum) consider “...the question of unit heterogeneity...the assumption that panels can be pooled into
one data set with a common intercept and slope coefficient...” along with “...alternative
dynamic structures, either in terms of theoretical arguments or empirical tests....” The most
common way of testing for unit effects, or unobserved heterogeneity, is to perform a Hausman
test. The test statistic for the data employed in this analysis is far from significant (Prob>chi2 =
0.8919), giving us strong confirmation that we need not consider fixed effects when using this
series. I also tested the data for autocorrelation which, more than anything else, distinguishes
time series methodology from a cross-sectional regression with no time component. There is
evidence of a first order serial correlation (H0: no first-order autocorrelation; Prob > F = 0.004)
but no second order serial correlation. The data were fitted to several different estimators and
specifications appropriate for TSCS data with first order serial autocorrelation as discussed by
Keele & Kelly (2006). All yielded consistent results. Model 1 gives the results of an AR (1)
regression with a single lag OLS of residuals and panel corrected standard errors. The second
model substitutes the Revolution/Territorial dummy variable in place of the Ideological/Ethnic
variable. Model 3 presents the results of a robustness test of the primary model after dropping
seventy-seven country years in the sample where the state fought more than one conflict in the
same year. Model 4 shows the results of a robustness test of the primary model which includes
each state’s physical integrity score taken five years before the onset of each civil war.

4.4 Findings and Discussion

4.4.1 The Origins of Territorial Control: The Reach of the State

Table 4.1 displays the coefficients, standard errors and significance levels for five models
using the previously described statistical procedures; all of the models appear to fit the data
extremely well. In Model 1, three variables (Distance, Military Size, and Ethnic Fractionalization)
theorized as influencing the origins of territorial control by estimating the reach of the
government into its national territory are statistically significant predictors of the level of state repression during civil war. The variable that measures the distance between the center of the conflict zone and the state capital (logged) was negatively correlated with the level of physical integrity violations and strongly significant. The coefficient indicates that for a one hundred percentage point increase in distance between the rebel base and capital city, we would expect a unit change in the CIRI scale of -.283. The size of the state’s military was a highly significant predictor of higher levels of state repression, as expected, with modest effects. A one standard deviation increase in military size could be expected to increase CIRI repression scores by .284. As theorized, rebel bases closer to the capital and larger government militaries should translate into more contested territory and system overlap. Stronger states should also be more proactive in trying to curb insurgency growth and expansion. Conversely, when the military is small and unable to effectively police a large territory, territory should become more segmented as rebel groups should be more successful at achieving territorial control in areas of the nation. Both Military Spending and Rebel Strength did not reach statistical significance.

Contrary to threat-based models of state repression, we see that the strength of the rebel group relative to the state is not as important as might be expected. One way to interpret this finding is that any coding of rebel strength will be heavily determined by how strong the state is. A rebel group with 12,000 members might be much weaker than the government in one country while only slightly weaker in another, and at parity with the government in a few very weak states.
Table 4.1       Physical Integrity Violations During Civil War, 1981-1999

<table>
<thead>
<tr>
<th>Variable</th>
<th>Model 1 DV: Physint Index (0-8)</th>
<th>Model 2 DV: Physint Index (0-8)</th>
<th>Model 3 DV: Physint Index (0-8)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Estimation Method:</td>
<td>Replication of primary model dropping 69 country years containing multiple civil war episodes</td>
<td>Sample limited to civil wars starting in 1985 or after to include every state’s repression score 5 years before onset</td>
</tr>
<tr>
<td></td>
<td>Panel - Specific AR(1) Regression with Single Lag OLS of Residuals with PCSE</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Distance (ln)</td>
<td>-0.276 (0.085)***</td>
<td>-0.291 (0.087)***</td>
<td>-0.159 (0.073)**</td>
</tr>
<tr>
<td>Military Size (K)</td>
<td>0.002 (0.000)***</td>
<td>0.002 (0.000)***</td>
<td>0.001 (0.001)</td>
</tr>
<tr>
<td>Military Spending (M)</td>
<td>-0.000 (0.000)</td>
<td>-0.000 (0.000)</td>
<td>-0.000 (0.000)</td>
</tr>
<tr>
<td>Relative State Strength</td>
<td>0.135 (0.161)</td>
<td>0.101 (0.165)</td>
<td>-0.028 (0.303)</td>
</tr>
<tr>
<td>Ethnic fract</td>
<td>2.458 (1.162)**</td>
<td>2.575 (1.198)**</td>
<td>4.850 (2.186)**</td>
</tr>
<tr>
<td>Ethnic fract Squared</td>
<td>-2.062 (1.222)**</td>
<td>-1.847 (1.343)**</td>
<td>-6.234 (2.337)*****</td>
</tr>
<tr>
<td>Marginalized</td>
<td>0.099 (0.331)</td>
<td>-0.068 (0.366)</td>
<td>0.480 (0.417)</td>
</tr>
<tr>
<td>Conflict Zone Size</td>
<td>0.820 (0.311)***</td>
<td>0.748 (0.311)**</td>
<td>1.009 (0.413)**</td>
</tr>
<tr>
<td>Battle Deaths (thous)</td>
<td>0.018 (0.008)**</td>
<td>0.017 (0.007)**</td>
<td>0.021 (0.024)</td>
</tr>
<tr>
<td>Cities with Attacks</td>
<td>0.005 (0.002)**</td>
<td>0.006 (0.002)**</td>
<td>0.004 (0.004)</td>
</tr>
<tr>
<td>Number of Attacks</td>
<td>0.001 (0.001)</td>
<td>0.001 (0.001)</td>
<td>0.002 (0.001)</td>
</tr>
<tr>
<td>Ethnic Division</td>
<td>0.091 (0.222)</td>
<td>0.055 (0.243)</td>
<td>-0.680 (0.469)</td>
</tr>
<tr>
<td>Territorial Dispute</td>
<td>-0.244 (0.216)</td>
<td>-0.382 (0.201)</td>
<td>0.676 (0.315)**</td>
</tr>
<tr>
<td>Number of Conflict Dyads</td>
<td>0.340 (0.211)</td>
<td>n/a</td>
<td>0.090 (0.277)</td>
</tr>
<tr>
<td>Duration</td>
<td>-0.010 (0.015)</td>
<td>0.001 (0.014)</td>
<td>0.022 (0.034)</td>
</tr>
<tr>
<td>Income per capita (ln)</td>
<td>-0.153 (0.086)</td>
<td>-0.227 (0.089)**</td>
<td>-0.354 (0.080)***</td>
</tr>
<tr>
<td>Population (ln)</td>
<td>0.052 (0.121)</td>
<td>0.011 (0.122)</td>
<td>0.294 (0.180)</td>
</tr>
<tr>
<td>Polity IV Index</td>
<td>-0.029 (0.015)**</td>
<td>-0.028 (0.017)</td>
<td>-0.094 (0.027)*****</td>
</tr>
<tr>
<td>Interstate War</td>
<td>1.313 (0.346)*****</td>
<td>1.576 (0.316)*****</td>
<td>0.283 (1.384)</td>
</tr>
<tr>
<td>Physint Onset -5 Years</td>
<td></td>
<td></td>
<td>0.131 (0.096)</td>
</tr>
<tr>
<td>Constant</td>
<td>5.867 (1.245)*****</td>
<td>6.646 (1.278)*****</td>
<td>5.285 (2.155)*****</td>
</tr>
<tr>
<td>Observations</td>
<td>519</td>
<td>461</td>
<td>196</td>
</tr>
<tr>
<td>R-squared</td>
<td>.73</td>
<td>.77</td>
<td>.77</td>
</tr>
</tbody>
</table>

Robust standard errors in parentheses; ** significant at 5%; *** significant at 1%
I expect that the strength of the rebel group relative to the government, as a variable, will not be as important when included in a model that contains several variables intended to measure state capacity and the reach of the state. We can test this interpretation by removing Military Size and Military Spending and re-estimating the model (not shown). Rebel Strength is now significant at the 10 percent level of confidence with a negative coefficient of .20. Since the rebels are strong only in cases where the government is very weak, we find essentially the same relationship, state repression is lowest when the government is weak. In sum, a preponderance of state military power leads to higher levels of repression as hypothesized.

The single strongest predictor of harsh levels of state repressive violence during civil war is the degree of ethnic fractionalization in society. The coefficient of 3.00 represents the average increase in state repressive behavior, as reflected in the CIRI physical integrity index, moving from a country with a relatively homogenous population to a country that is significantly more fragmented. An included squared term for ethnic fractionalization is also significant suggesting a parabolic relationship between state repression and ethnic fractionalization. While this means that at some point (the vertex) higher levels of ethnic diversity causes repression to decrease, it also means that the initial curve where repression increases with increases in ethnic diversity is much steeper. Overall, a parabolic relationship implies that state repression is highest when the state governs a society comprised of several relatively large ethnic groups (polarization) and lowest when society is ethnically homogenous or characterized by numerous small groups.
4.4.2 Estimating Territorial Control: Conflict Characteristics

Of the second category of independent variables, those used to estimate general levels of contested territory during the conflict, all but one are significant and in the expected direction. The size of the conflict zone, as a percentage of the total area of the state in square kilometers, is highly significant with substantive effects. The coefficient of .89 represents the average increase in state repressive behavior we might expect to encounter moving from a country with a relatively small conflict zone, (e.g., Papua New Guinea, Bangladesh) to a country where the conflict zone encompasses most of the state’s physical territory (e.g., Columbia, Guatemala, Peru). The findings on the war type variables indicate that whether a civil war involved ethnic division between the combatants, or whether the conflict was fought over territory or control of the national government were not significantly related to the repressive behavior of the state. In models and 2, we see that for every one thousand combat deaths per year we would expect an increase of roughly .015 in the level of repression committed by the state. Repression would increase .12 with a one standard deviation (7.9 thousand) increase in combat deaths per year. State repression would increase by roughly .0015 for every one hundred combat deaths per year. Thus, the severity of combat seems to exert a small effect on levels of state repression during civil war. This suggests, at least, that combat violence and repressive violence are to some degree distinct forms of violence. This makes sense, however, if the bulk of day-to-day counterinsurgency is carried out by police and state security forces rather than the national military which I would argue is more often the case (see Wilkinson 2006).
The number of insurgency attacks the state experienced every year, a highly visible indicator of threat, fails to reach statistical significance in any model despite the extremely large amount of variation in insurgency activity across conflicts. The number of cities that experienced insurgency attacks in the year, however, was a significant predictor of more repression. We can expect state repression to increase by .005 for every city in the state that experiences one or more insurgency attacks. While the unit to unit effects are not very large, the full range of effect on state repression, moving from the state with the fewest cities experiencing attacks in a year to the state with the most cities experiencing insurgency attacks would yield an increased repression score of about 1.5 points on the CIRI index (288*.005).

Next, I examine the effects of the control variables on state repressive behavior during civil war.

4.4.3 Control Variables

The results suggest that how long a civil war has been going on has little to do with how repressive the state is during the conflict - both measures of conflict duration failed to reach statistical significance. As expected, the number of separate civil wars ongoing in the state in a given year is a strong predictor of greater levels of human rights violations by the state. The variable which counts the number of ongoing civil wars in a year is strongly significant with a coefficient of .635. States in the sample that fought two civil wars in the same year (e.g., Myanmar, Philippines, and Angola) had repression scores over a point greater (2*.63) than states fighting a single civil war. Those fighting three civil wars (e.g., India, Ethiopia) had repression scores nearly 2 points greater.

How did those variables found to be important in predicting levels of physical integrity violations in past research perform for a sample of nations fighting in civil war? The level of
democratization was significant with levels of state repression falling roughly -.036 for every one point increase in the 21 point Polity IV index. Thus, higher levels of democracy would be associated with decreased levels of state repression, by roughly half a point, on average. Although autocracies are the most susceptible regimes to civil war, there are 203 country years in the sample where the state was democratic with a Polity score of six or above. I substituted a threshold dummy variable for democracy which failed to reach statistical significance. A squared term for the polity index was substituted in another re-estimation (not shown) and the variable was strongly significant suggesting that mixed regimes in the middle of the polity scale tend to be the most repressive. Population size was not statistically significant.

Involvement by the civil war state in an international war does appear to make the state more repressive, as previous research has shown. The important question regarding this finding is whether involvement in international war led to more domestic human rights abuses, or whether repressive leaders are more belligerent in their international interactions as well. Sharing this concern, Sobek, Abouharb and Ingram (2006) found empirical evidence that domestically repressive states are generally more conflict prone internationally. Since there are only three states in our 1981-2005 sample that experienced an international war while a civil war was on-going within their borders, (Iran, Iraq and Syria) we can easily compare their records, before and during the interstate war, to shed some light on this question. A difference of means test comparing the three year period preceding involvement in international war with the three year period after involvement was not statistically significant for any of the three cases. It is also worth mentioning that the three cases are rivals and the presence of a civil war may have helped instigate the international war.
Higher levels of income per capita, which has been a consistent predictor of respect for human rights in previous research, failed to reach statistical significance in the two primary models. That variable did achieve significance in Models 3 and 4 where robustness tests lowered the number of states and country years included in the regression model. Although a sample comprised of states experiencing civil war will be a sample of disproportionately less wealthy states, those states that are economically stronger or weaker do not appear to differ significantly in their propensity to repress during civil war.

4.5 Robustness Tests

In this section, the robustness of the findings is tested by examining the extent of change in the results when restricting the number of yearly observations and again when including a measure of each state’s repressive behavior five years before the onset of each civil war. A concern not yet explored regards the effects of aggregating data from multiple conflicts in the same country year. While the CIRI index is based on a national level of analysis, many of the independent variables theorized as influencing state repressive behavior are conflict specific, such as the size of the conflict zone. When multiple civil wars are fought in the same state in the same year, the data were aggregated to a national level of analysis. For example, if a state in our sample is fighting two civil wars simultaneously in a given year, where one conflict zone takes up 20 percent of the state’s overall territory and the other 30 percent, I consider 50 percent of the state’s territory to be under contestation. This could create a potential bias, however, as values for those affected variables will usually be larger in years where multiple conflicts are present than in years where only a single civil war was fought. Model 3 displays the results of a re-estimation of the primary model (Model 1) dropping from the analysis the 77
country years which contain multiple civil wars. The findings are consistent with model 1 and somewhat stronger. The amount of explained variance increases and the significance levels of several key variables, including state repression, are strengthened.

Model 4 shows the results of a second robustness test intended to control for how repressive the state was before the onset of civil war. Taking into account how repressive the state was before the war is especially important given that the primary model includes not only conflict characteristics but also country characteristics (e.g., ethnic fractionalization, population) which could have an impact on how repressive the state was to begin with. The results of model 4 give us a strong confirmation that cross-national differences in repressive behavior by states during civil war are not simply the result of how repressive the states were to begin with. The variable \textit{physint (onset -5)}, which measures each state’s CIRI score 5 years before the year of onset, fails statistical significance by a large margin ($p>z = .947$).

4.6 Conclusion

This chapter represents the first empirical effort at explaining cross-national variation in state repression of physical integrity rights during civil war. The severity of state repression during civil war was modeled as a function of territorial contestation within the state. It was theorized that in conflicts where control over territory and populations is more fragmented, repression by the state will be greater due to a higher number of defections and denunciations in areas of overlapping control. Conversely, where territorial control is more segmented, the number of denunciations will be lower and without informants willing to provide information to the state, repressive violence has no target.
Variables theorized as being associated with the origins of more fragmented territorial and collaborative control within the state, such as rebel bases closer to the capital, stronger government military capabilities, and more ethnically heterogeneous societies, were shown to be significant predictors of higher levels of state repression. Those variables theorized as being correlates of greater amounts of fragmented territorial control, such as larger conflict zones and a larger number of cities housing insurgency activity were also shown to be significant predictors of higher levels of state repression.

I assume that territorial control is more segmented for civil wars where rebel bases are far away from the capital, where the state military is smaller, where the conflict zone is more limited, where insurgency attacks occur in fewer cities, and where society is less divided along ethnic lines. Where the government and the rebel group have more autonomy in their spheres of influence and less “competition” (Mason and Krane 1989), “system overlap” (Wickham-Crowley 1990), and “fragmented sovereignty” (Kalyvas 2006), their need to inflict harsh repressive violence to force collaboration will decrease. The targeting of noncombatants may also become less frequent under segmented territorial control if the defense of held territory makes combat more conventional in nature rather than based primarily on guerilla tactics. This interpretation dovetails with Valentino, Huth & Balch-Lindsay’s (2004) study on civilian killings during interstate wars, extra-systemic colonial wars and civil wars. They found more episodes of mass killings in all types of conflicts that were characterized more heavily by guerrilla warfare as opposed to those conflicts with more conventional military engagements. The same dynamic might hold true for a sample universe of only civil wars; segmentation leads to more
conventional combat which in turn generates lower levels of repressive violence against civilians.

Overall, the findings suggest that territorial dynamics are important for understanding the harshness of state repressive behavior during civil war. More politically-based explanations, such as the lack of democratic accountability or economic underdevelopment, are found to have modest overall effects when compared to those indicators theorized as tapping into the competitive struggle over collaborative and territorial control between combatants in civil wars. Future research should focus on possible feedback loops and bi-directionality in the relationship between the relative strength of government and rebel forces and state repression.
Chapter 5
State Repression and the Size and Spatial Expansion of Insurgencies

5.1 Introduction

The balance of power between rebel groups and governments has been theorized as influencing a wide range of civil war dynamics, from severity (Hultman 2007; Eck and Hultman 2007; Kalyvas 2006; Heger and Salehyan 2007); to duration and outcome (de Rouen & Sobek 2004); termination (Mason, Weingarten & Fett 1999; Mason and Fett 1996) to the prospects for post-conflict peace (Quinn, Mason and Gurses 2007). Yet, despite the theoretical significance given to the size of insurgencies, there is an absence of empirical works on the dynamics of insurgency growth over the duration of civil wars. The only works on the determinants of rebel recruitment success have to date been entirely theoretical or game theoretic in nature (Gates 2002). In Part I I argued and offered empirical evidence that insurgencies are most likely to emerge and escalate in the presence of harsh repressive violence by the state. The theory proposed in this chapter begins where Part I ended – with the onset of civil war.

By following the growth of rebel organizations during civil war, I first attempt to shed some light on a neglected topic in the study of civil war and insurgency: rates of differential recruitment among rebel groups. What factors facilitate the growth of insurgencies once they are underway? What determines their size? Given the logic that rebel organizations need to recruit as many troops as possible, what accounts for the large variation in rebel group sizes across conflicts? How were the rebels of the Mozambican National Resistance (RENAMO) and the Moro National Liberation Front (MNLF) in the Philippines able to command over 20,000 troops at their peak, while the Islamic Armed Movement (MIA) in Algeria and the Palipehutu in
Burundi never exceeded 2000, and the Zapatista Army of National Resistance (EZLN) in Mexico never exceeded 200 members?

5.2 The Anatomy of Repression and the Micromobilization of Rebel Recruitment

In Part I I argued and found strong empirical evidence that insurgencies generally are unable to grow without the presence of harsh state repression, even when controlling for insurgency opportunity, state capacity, poverty and geography. I argued that when insurgencies succeed in escalating into civil war, it is more often an unintentional by-product of the consolidation and geographical displacement of active group members trying to flee from expanding state repression rather than the result of any premeditated plan by the leadership of the insurgency or the persuasiveness of their ideological appeal. Once active membership is consolidated and geographical displacement has put the material survival of the insurgents almost entirely in the hands of a political and social support system (if one exists) by cutting off all avenues of normal employment to active insurgents, how does the group recruit new members?

Following a deductive approach used by Opp (1990) to study the growth of social movements, I focus on the motivational effects of victimization from state repression on an individual’s decision to join an already existing and viable insurgency organization. Specifically, I attempt to show that when states engage in repressive counterinsurgency and attempt to locate suspected insurgents by repressing the suspect’s social network, social processes are generated that facilitate rebel recruitment, geographical expansion, as well as the expansion of overall popular support to the insurgency. I examine in detail how the anatomy of state repression (torture, political imprisonment, extra-judicial killings and disappearances) leads to
the growth of insurgencies primarily through the mechanism of facilitating rebel recruitment. As I will show, repression has powerful psycho-social effects, both on its victims and on members of the victim’s social network that makes them more susceptible to the recruitment efforts of insurgent organizations. Similar to Opp’s (1990) description of “stigmatization” in the growth of social movement networks, I find that individuals who have been targeted by the state are more likely to end up joining the insurgency regardless of their initial preferences toward doing so. Within the recruitment process are both push and pull factors affecting an individual’s likelihood of joining an insurgency. Individuals who have reason to believe that they will be targeted by the state -- especially individuals who have already been detained, imprisoned or tortured by the state and released -- are likely to receive positive treatment from the insurgency organization while being shunned, to some extent, by others within their immediate social setting who believe, with good reason, that interacting with this person will increase their own likelihood of being targeted. A similar process is described elsewhere in the social movement literature as “bridge burning.” Thus, there are rewards or positive incentives given to individuals who have been the victims of state repression that pull them toward participation, while at the same time barriers are often raised if the individual tries to return to the normal social networks of everyday life.

5.3 Torture

The marines kept me blindfolded without food and water for fourteen days, hanging me up by my hands tied behind my back until my eyes were popping out...I was punched and half drowned. Eventually they decided I was not guilty and freed me. I was lucky -- a prisoner accused of blowing up a bridge had his hand cut off in front of me. He confessed.

Jose Navarret, a merchant denounced to Peruvian armed forces by a jealous neighbor
Closely connected to detainment, the most common human rights violation perpetrated by governments fighting insurgencies is the use of torture (Cingranelli & Richards 1999). Torture is the use of: “...[I]solation, humiliation, psychological pressure, and physical pain...to obtain information, to break down the prisoner, to destroy a victim’s sense of self-esteem, and to intimidate those close to him or her...” (Punamaki 1988:83). Police and military interrogators use torture as a method of extracting intelligence from individuals who have incentives to withhold such information. It is also used as a device for social control, (i.e., as a punitive measure against an individual or as a collective punishment against threatening population segments). The former is most prevalent, while the latter is mostly confined to authoritarian regimes (Wantchekon and Healy 1996). Wantchekon and Healy (1996) explain the prevalence of torture as a signaling game of incomplete information involving the state, the torturer, and the victim. They show that once torture is sanctioned, its use generally becomes more and more prevalent and increasingly sadistic. One of the principle reasons for this is the incentive, even among weak victims (who should not ordinarily require torture to be cooperative), to hold out initially in order to determine whether the interrogator is professional or sadistic. This incentive structure makes torture prevalent and sadistic even when both are unnecessary to acquire information from the detainee.

Studies that attempt to explain the motivations behind the use of torture and works that debate whether, and under what conditions, torture might be condoned, are quite common. Studies on the effects of torture, especially those that provide some insight into the benefits of torture as a counterinsurgency tactic, are few and far between. This is surprising and unfortunate, given that torture is the most common human rights violation in the world. The
little evidence available suggests that the benefits of torture to counterinsurgency are negligible while the psychological effects of torture on the victims are powerful. French General Massu, the hero of the Battle of Algiers, is often accredited with making the systematic use of light torture a standard modern counterinsurgency technique. Later in life he publicly expressed his regret for using torture admitting “...that it had served no useful or necessary function in combating terrorism” (Macmaster 2004:9). Furthermore, Macmaster (2004:9) notes that it is “...widely acknowledged that state violence had...served to drive most of the Algerian population into the arms of the FLN....” The “French Algerian model” of interrogation was applied in Operation Phoenix in Vietnam and proved ineffective at gathering accurate intelligence according to the writings of at least one colonel who worked in the program (Macmaster 2004).

5.3.1 The Psychological Effects of Torture on Victims

Almost every single study done on the victims of torture are clinical in nature, having nothing to do with the treatment of patients. As a result, the studies can be extremely useful to students of political violence, in that the purpose of the studies is to identify and detail the extent of injury so that a plan of treatment may be designed. Thus, we have several case studies that examine both what the state did to the victims and, perhaps most important for the present purposes, the psychological damage and effects that the victim displayed or shared with the researcher.

In a 1999 mental health study of 1,368 Kosovo Albanians, Cardozo et. al. (2003) surveyed each respondent about traumatic experiences they endured during the recently ended civil conflict there. The sampling procedure used to choose the respondents was
scientific, random and conducted at a national level of analysis. Thirty population clusters, 15 rural and 15 urban, were randomly chosen throughout the country of Kosovo (population 1.6 million). Using grids of each cluster, households were randomly selected and all available adults within the household were interviewed about possible traumatic experiences during the civil war.

Amazingly, over 48 percent of the Kosovo residents chosen at random in 1999 reported that they had been tortured by security forces during the conflict. Over 26 percent of the sample said they had a family member or friend murdered by the state. The researchers found that more than 25 percent of the respondents suffered from post traumatic stress disorder or PTSD. Two of the mental health questions asked to every respondent in the survey were: “Do you have feelings and fantasies of taking revenge over what has happened to you and your family during the war?” and “Do you think you will really act on those feelings?” (2003:354). The researchers found that those who were diagnosed with PTSD were the most likely group to harbor revenge fantasies. Half of the male respondents and 43 percent of females said that they harbored revenge fantasies. Among those respondents, 64 percent of men and 49 percent of women said that they would act on their desire for revenge if they were given an opportunity. A follow-up survey one year later, showed only a slight decline in the frequencies of these responses among men and a slight increase among women.

In another clinical study, Laws and Iacopino (2002) surveyed 192 self-identified torture victims (168 men and 24 women) located through the Human Rights and Democracy Forum (HRDF), a non-governmental organization which was operating in Punjab, India. Survey respondents came from 126 different towns in 17 different districts of Punjab. No
compensation of any kind was given to the respondents for their testimonies. The researchers first asked the respondents why they thought they had been tortured by the police. Forty percent of the respondents reported that security forces had tortured them to get information regarding the identities and locations of Sikh insurgents. Twenty-two percent said they were tortured as punishment for allegedly providing food and shelter to an insurgent. Thirteen percent said they were wrongly suspected by the authorities of being a Sikh insurgent. Only three individuals identified themselves as Sikh insurgents. A handful of remaining people said they were tortured as a result of an interpersonal conflict with the police that was unrelated to the civil conflict between the Sikh separatist and the Indian government.

The primary emphasis of the Punjab study was clinical in nature, giving us an abundance of details on the exact methods of torture and the injuries sustained by the victims in Punjab. The authors found that, “[T]he descriptions of torture were highly similar.” Upon interrogation, almost all of the respondents were forced to undress and were whipped with straps or wooden sticks. After an introductory beating, 75 percent of the respondents reported an unusual method of leg stretching. Two policemen, each holding one of the victim’s legs, would pull the legs as far apart as physically possible without causing a major injury. Although extremely painful, in all but a few cases, no lasting injury was sustained (thirteen individuals reported a dislocated hip or knee). Sixty-three percent of the respondents reported being hung from the ceiling with their hands tied behind their backs while standing on a chair or table. Again, this method produced extreme pain in the shoulders, but in only a few cases did it produce an acute injury. Twenty-seven percent reported the use of electric shocks from a small generator. A minority of respondents reported various other methods used on occasion such as mock
executions and being held under water. It seems apparent that the methods chosen by the authorities were those that trial and error had shown maximized pain while minimizing visible scaring and acute injury. Less than 20 percent of the group had empirically verifiable physical injuries: 7 individuals had visible knee problems, 3 had a visible dislocated or fractured hip, one an ankle injury, 3 had a visible wrist injury, one a dislocated shoulder, one a broken humerus, 15 had evidence of healed bone fractures and 8 had healed burn scars. The survey revealed that torture was carried out in at least 84 different police stations in Punjab. The authors noted one police officer (anonymously) who estimated that 4,000 to 5,000 people were tortured at his particular police station between 1985 to 1990. This was only one of the 84 stations where torture was reported as having occurred from the respondents.

Punamaki (1988), a Finnish psychologist, surveyed 40 released Palestinian political prisoners in the West Bank and Gaza Strip regarding the prevalence of torture, coping methods, and symptoms they had experienced. The former prisoners who agreed to be part of the study were located while Punamaki was conducting a follow up study on the mental health of Palestinian women. Thus, the forty ex-prisoners were located in what appears to be a quasi-random fashion in that they happened to be family members of some of the women participating in the mental health study. Background information taken on the subjects revealed that one-third of the group had been detained for less than one year, while another third had been detained over five years. Ten of the forty had been “administrative detainees,” a legal category by which detainees can be held legally without trial for six-month periods with six-month renewals. According to interviews with the subjects, the use of light torture, both physical and psychological, against the detainees appeared to be a common practice during
interrogation sessions and less so during actual imprisonment. Seventy-seven and seventy-eight percent, respectively, said they were whipped and experienced cold water treatments while detained. Seventy percent reported being hung by the hands for extended periods of time. The most frequently used interrogation tactics were psychological in nature: humiliation directed against their nationality (95 percent), threats against themselves (92 percent), or against their families (68 percent), and being confronted with false confessions reportedly given by other detainees (70 percent). Eighty-two percent said they were offered deals towards their release if they agreed to confess to their own guilt or the guilt of others.

Most of the ex-prisoners in Punamaki’s study mentioned more than one coping resource for getting through their ordeal. More than half (57 percent) emphasized their ideological struggle as a personal resource. Punamaki lists “feeling of international and national solidarity, support by one’s fellow prisoners, and one’s family...” as the most frequently mentioned coping resources. Punamaki also examines coping methods noting that, “Most of the actual coping strategies were collective in nature. In only a few cases were individual or intrapsychic modes of coping mentioned” (1988:90). Punamaki found an inclination toward violent conduct in 50 percent of the respondents along with difficulties in controlling emotions (47 percent) as a persistent symptom of their ordeal. As the author admits, it was not possible to examine or report on the post-confinement level of ideological commitment or participation of the subjects in rebel activity without jeopardizing their personal security. Admission of such support could be used against the individuals leading to re-incarceration. While post-confinement activity was not reported, the findings nonetheless, suggest a general pattern of increased solidarity and participation in collective action amongst the prisoners, at least while in confinement, along
with an increased propensity toward emotionality and inclinations toward violence after confinement.

5.3.2 Discussion

What can we learn from these few case studies of torture? They exhibit several commonalities. First, they suggest that when torture is sanctioned by the state as a counterinsurgency device, it does tend to become extremely widespread, expanding exponentially as each victim provides information streams leading to subsequent victims. Incidentally, around the time of Punamaki’s study of former Palestinian prisoners, the Red Cross, which has permission from the Israeli government to visit every detainee in custody, reported that since the beginning of the conflict, over 500,000 individual Palestinians have been detained by Israeli security forces (out of a population around the time of 1.5 million Palestinians living in the occupied territories). Extrapolating these numbers leads to an estimated one in three Palestinians (mostly males) that have been detained. Based on the sheer vastness of these detention levels, we can infer with some confidence that the majority of detained individuals could not have been insurgents themselves, but instead were at most members of a suspected insurgent’s social network.

If we assume that most detainees are not active insurgents and we assume that the probability of torture is more or less random among detainees, then the evidence from the other cases suggests more explicitly that the majority of individuals who are tortured in a counterinsurgency campaign are also not insurgents. The Punjab torture study is especially illuminating in that only three of the 192 individuals who were tortured were self-described insurgents. Even if we presume that many of the respondents were in fact Sikh militants but
wanted to conceal that from the surveyors by lying, they could have easily done so by merely claiming (as some did) to be wrongfully accused. Only 13 percent of those tortured made the claim that they were wrongly accused. Instead, the overwhelming majority of those tortured (62 percent) said that they were tortured to get information on the location of a suspected insurgent or as punishment for allegedly giving support to a known insurgent. When considered together, the cases suggest several basic generalities related to the micromobilization of rebel recruitment. First, detainment and torture during a counterinsurgency campaign is inflicted on a surprisingly large pool of individuals. The next logical area of research involves a deeper examination of the effects of torture on its victims, especially in light of the strong assumption that most of the victims are not insurgents. Unfortunately, no studies exist that examine the effects of torture on future insurgency participation.

Given the lack of studies on the effects of torture on post-episodic insurgency participation, we can gain some insight by examining studies on post traumatic stress disorder (PTSD) and direct exposure to political violence since “[t]here is ample evidence that political prisoners...as well as victims of torture and other forms of organized violence...show high levels of posttraumatic stress (PTS) symptoms...” (Salo et al. 2004). As evident in the Kosovo case previously discussed, a common correlate of PTSD is the harboring of revenge fantasies. Twenty five percent of those surveyed in Kosovo had PTSD, and this group was the most likely group to have revenge fantasies. Horowitz (2007:24), a clinical psychologist who treats post-traumatic revenge fantasies, provides a working definition: “[S]ymptomatic revenge fantasies go beyond normal bitter thoughts; they are unwanted, uncontrollable, dangerous, or intensely evocative of shame or guilt” and “...give a sense of restored purpose and control in an
otherwise shattered life…” (2007:27). While most of the emotional responses to trauma (shock, outcry, denial, de-realization and depression) usually fade or diminish over time, Horowitz argues that revenge fantasies are extremely unique in that they “…tend to persist in late phases of psychological trauma.” Horowitz argues that, “[R]evenge fantasies are persistent because they also provide additional positive emotional effects. The victim can feel good about gaining a sense of power and control by planning vengeance and may experience pleasure at imagining the suffering of the target and pride at being on the side of some spiritual primal justice…” (2007:26).

There is also a substantial body of psychological research that shows that personal exposure to traumatic political violence increases both ideological commitment and participation in oppositional activity, especially in adolescents. In a sample of Palestinian youths, Punamaki & Suleiman (1990) found that an individual’s level of exposure to political violence increased the frequently of “active and heroic” coping mechanisms among the respondents. In another study of 385 Israeli Jewish youths, Punamaki (1987) found a strong correlation between greater levels of direct exposure to political violence and more “hawkish” attitudes towards war and national security.

Particularly interesting was her finding that those respondents with the most hawkish attitudes toward war and the enemy also expressed the least fear of war and violence relative to the other respondents. Using measures of anxiety, insecurity, depression, and personal failure as indicators of mental well-being, Punamaki found that exposure to political violence increased these symptoms of mental illness only among those who had weak ideological commitments prior to the exposure to violence. She notes, “The model substantiates that the
more children had experiences of political violence, the stronger ideological commitment they showed...and the stronger the ideological commitment they expressed, the less they suffered from psychological problems...” (1996:65). She found that those with strong ideological commitments, operationalized as patriotism, a defiant attitude toward the enemy and the glorification of war, not only experienced the least psychological damage when exposed to violence but experienced higher levels of ideological commitment. Those individuals with stronger ideological commitments at the time they began to witness traumatic violence were better able to process the traumatic events in such a way that further justified and strengthened their own ideological commitment. This is especially relevant to our discussion of the effects of torture on the individual since the selection process by which regimes select targets for interrogation should be biased toward those segments of the population with stronger ideological sympathies for the insurgents. Punamaki’s findings are also especially relevant given the studies that show the disproportionate participation of young adults in insurgency organizations (Urdal 2006).

Punamaki notes the correlate between her findings and a similar pattern noted by Bettelheim (1943) based on his own observations in the Dachau concentration camp during World War II. Bettelheim noted that their appeared to be two groups of Jews, who despite the extremely traumatic situation, seemed to be the least vulnerable psychologically: the extremely religious, and those who were communists. Both groups, he argued, could contextualize their suffering as part of a larger scheme or plan increasing their self esteem and ability to cope. The most vulnerable were those Jews who lacked any type of strong ideological commitment, whether religious or political. This group, according to Bettelheim, suffered the greatest
psychological damage because they were least able to come to terms with what was happening, why it was happening, and why it was happening to them.

5.4 Detainment/Political Imprisonment

Extensive detainment and political imprisonment by states leads to insurgency growth, I argue, by supporting the ecology of rebel recruitment. The widespread imprisonment of suspected insurgents in close quarters, for months or years at a time, has been repeatedly observed as providing a networking structure for insurgency recruitment and radicalizing prisoners. In the early stages of the Algerian civil war, mass detainment was seen as greatly facilitated the organizational efforts of the insurgents (Hafez 2003). Hafez (2003) refers to the make-shift political prisons used to house thousands of suspected rebels as “insurgent universities.” Once inside the camps, thousands of angry young men entered an insurgent social network where they made contacts and acquired knowledge on how to retaliate against the regime that put them there. In one of the only studies of detainment in counterinsurgency campaigns, O’Leary and Silke (2007:413) note that,

> Overall, the management of prisoners has been rather poorly understood by most governments. Only in a few cases, and especially Spain, did governments consciously tackle the de facto “universities of terror” that result from concentration...Concentrated groups, almost by definition, become more disciplined, more soldier-like, and more likely to come under the control of their leaders and organizations.

The authors found that in the deviant case, the Spanish government’s use of a dispersal policy which attempted to separate suspected insurgents during detainment “...led to a substantial increase in the number of prisoners who left ETA, and significantly hammered ETA’s ability to train, supervise, and motivate prisoners who chose to remain” (2007:413). Dawes
found that adolescent political prisoners in South Africa developed stronger political commitments during and after imprisonment than they had held prior to being imprisoned.

Consider Strong’s (1992) description of the “typical” Sendero experience in prison during the Peruvian civil war; one saturated with political socialization:

The jailed rebels consider prison a place of political action, a temporary inconvenience or work accident to be turned into political advantage...Visitors to El Fronton and Lurigancho were struck by the rebel inmates’ orderliness and cleanliness; how no prison guard ventured into their cellblocks; how slogans were chanted while the prisoners stood in military squares or they marched up and down below portraits of Marx, Lenin, and Mao...There were indoctrination classes...The surreptitious usurpation of power within the prisons was a microcosm of what was happening in the rest of Peru (1992:144-145).

Many of the Sendero detainees were eventually released following their regimented indoctrination and training in prison. While Berg (1992:95) was conducting field work in the Andahuaylas province of Peru in 1985, he noted the return of twenty-nine people to the village in the month of June, having been released from prison “where they had been held for four years with all charges dropped for lack of evidence.” Ironically, Berg notes that most of the charges against the detainees did not hold up in court because the evidence usually consisted of suspicion rather than physical acts or confessions obtained through torture which turned out to be incompatible with the state’s evidence of the crime. Thus, indiscriminate detainment, usually of young males who fit the rebel stereotype, provided insurgency leaders behind bars with a never-ending surplus of recruits.

Strong argues that “[P]oor police work, depending largely on the extraction of confessions under torture, and inept or terrified judges and attorneys as well as an inadequate judicial structure meant that many rebels were released. The failure of the democratic system in turn encouraged the security forces to take the law into their own hands, at both a personal
and institutional level” (1992:150). This produces another feedback mechanism: harsh repression yields false confessions and false denunciations that eventually lead to many detainees being released from confinement for a lack of evidence. The military, perceiving this as the judiciary undermining their work, takes sentencing into their own hands, increasing the frequency of killings and disappearances.

5.5 Disappearances and Extra-judicial Killings

While the use of torture is theorized as having a radicalizing effect on many of its victims, this effect is presumably much less pronounced regarding the victim’s family members. Torture victims often withhold information about what happened to them. As the cases cited earlier demonstrate, the damage is mostly psychological rather than physical and thus not easily shared or communicated to others. The same is not true, however, with having a friend or family member disappeared or executed by the state. Forced disappearances differ from extra-judicial executions only in that an effort is made to conceal what happened to the person in an effort to achieve deniability on behalf of the state. By not claiming responsibility for disappearances, however, governments that commit them are also typically constrained in their ability to raise barriers to collective action and civic organizations that spring up in efforts to locate the missing individuals. To repress such efforts would amount to a de facto admission that the government was responsible for the disappearances.

Where the state has established covert death squads as a means of removing individuals supporting the opposition, the taking of individuals becomes frequent and mechanistic. Kumar and Mahmood (1998), working with the Committee for Coordination on Disappearances in Punjab, report that in one district of Punjab alone more than 2,000 people had been
disappeared. Later it was discovered that those individuals had been killed during interrogation or while in custody and had been cremated. According to the missing persons registry of the Croatian Commission for Detained and Missing Persons, (Jurcevic, Allen, and Dahl 2007:370) 3,052 persons were formally reported as missing during the civil war in Croatia which lasted 46 months, or an average of 66 persons per month, or between 2 and 3 persons per day over the course of the conflict. Of the 3,052 persons reported missing, 2,046 (67%) were executed and later exhumed from 136 mass graves that have been identified in the country. The demographic characteristics of the population of disappeared persons indicate that the overwhelming majority of those abducted were males (79 percent), ethnic Croats (89 percent) between the ages of 18 and 49 (60 percent).

Many qualitative works have identified a notable relationship between the practice of killings by the state and rebel recruitment. At least one study found a consistent and direct relationship between individuals who had a family member killed by the state and the subsequent recruitment of that individual by the insurgents. Wood (2006) specifically notes that among the campesinos she interviewed in former conflict zones in El Salvador, those respondents most likely to be self-described insurgents were those who had a family member killed by the state’s security forces and who lived near areas “liberated” by the insurgents from a Salvadoran military presence. Indeed, the majority of insurgent campesinos interviewed by Wood joined the FMLN after the murder of a family member by the government’s death squads. Mason and Krane note a similar pattern of repression and rebel recruitment success in the same case:

The growth of the FMLN’s active forces and its base of covert support during the late 1970’s suggest...a shift in peasant loyalties...with the escalation of indiscriminate
repressive violence. By 1979, the FMLN had an estimated 6,000-8,000 active guerrillas, as many as 100,000 part-time militia, and a reported one million sympathizers who provided covert support in the form of food, intelligence, and sanctuary (1989:188).

In their comparative study of political violence in Central America, Booth, Walker and Wade (2006) discuss the effects of indiscriminate state repression on the successful recruitment efforts of the FSLN rebel organization in the Nicaraguan civil war. Following increases in harsh repressive violence the military ranks of the FSLN “...flourished, ballooning from less than 500 troops under arms in mid-1978 to between 2,500 and 5,000 by June 1979” (2006:74). They specifically note the effects of extra-judicial killings, saying that,

The guard openly targeted youths from the early teens up because they were suspected of pro-Sandinista sympathies. The military and police dragged hundreds from their homes and the streets and murdered them. From 1977 on, the National Guard conducted an ever-intensifying war against the people of Nicaragua. Such repression eventually drove thousands, especially young people, to join the FSLN (2006:73).

Berg’s (1992:94) field work in the province of Andahuaylas in Peru in 1981 and again in 1985 provides an intriguing view of an area with substantial sympathies toward Sendero observed at two different points in time: the beginning of armed conflict and then again four years later. Berg observed a striking change in demography over the course of his four year absence:

When I returned in 1985, I was able to follow the course of this new, more brutal stage of the counterinsurgency campaign...In 1982 young people in their late and early twenties...had been a conspicuous part of village life, but now they were absent...It became evident, however, that the major reason for the absence of young people was that they had been killed, jailed, or driven away by the police (or had joined Sendero) in the previous three years.” According to Berg (1992:94) one hundred counterinsurgency forces occupied the district in 1983 and their primary activity “…consisted of rounding up, arresting, and interrogating suspected guerrillas... the primary targets were young men.

While indiscriminate repression, especially extra-judicial executions, have been
observed to benefit rebel recruitment, such killings are far more anomalous than targeting the leadership of oppositional movements. What if the government limits repressive violence to the leadership of rebel organizations? Despite the frequency of leadership targeting in counterinsurgency campaigns, it is under-theorized and there is little evidence of its effectiveness. Are rebel groups like snakes, which can be decapitated and incapacitated, or are they more like starfish, able to consistently grow new leaders should current leaders be severed from the body of the organization?

Killing or jailing the leadership of insurgencies does not usually lead to a notable decline in the organization’s strength, nor a collapse of the organization’s authority structure. It is also not typically followed by a decrease in the amount of violence the rebel group produces. Instead, a new covert leadership emerges and begins work towards further militarizing the group. Why do the initial acts of targeting the leadership so often fail in generating insurgency decline? The answer to this puzzle involves an important distinction in the command or authority structure of rebel organizations versus government and military organizations.

Military commanders in Peru, for example, were so entrenched in their belief that removing the leadership of Sendero would decapitate the organization that when the organization failed to decline after much of its leadership had been killed or captured, the military leaders assumed that the leadership must be running the organization from inside the prisons. After most of the leaders inside the prisons were then killed, and Sendero’s activities still increased, the military continued to make capturing the few remaining leaders of Sendero a top priority. In the case of Sendero, however, its command structure made targeting the leadership a null stratagem. As Tarazona-Sevillano (1992:183) notes,
One of the organizational attributes that Sendero possesses is resiliency, being capable to date of generating and regenerating leadership. The insurgency continues to grow despite the Peruvian government’s claims that it has captured or killed many top Sendero leaders. Shining Path’s expansion suggests that the system is reinforced by a standing cadre of militants able to step into leadership positions as needed.

Kerkvliet (1977:47) makes similar observations about the Filipino government’s early efforts to dissolve the peasant uprisings that would later become the Huk rebellion by jailing and killing its top leadership. The organization, not only endured, but responded with fresh growth:

The largest of the peasant organizations, KPMP and AMT, had lives of their own, independent of particular leaders. They were not personal followings of particular leaders. For example, several top KPMP leaders were imprisoned on sedition and other charges between 1931 and 1933. Among them were Juan Feleo and Jacinto Manahan, both long-time and widely acknowledged KPMP activists. Yet while Feleo and others were in prison or in exile, until 1938, the KPMP swelled as agrarian unrest grew more intense (1992:47).

One of the more extraordinary things about leadership targeting in the Huk rebellion was the fact that the Huks were a legitimate fighting force against the Japanese occupation and became viewed as a threat by the Filipino government only after the Japanese occupation ended. This led to a very unusual situation where the government knew the identities of almost all the Huk leaders who were then individually pursued by government forces. In 1945, fourteen “…prominent Huk leaders…” and “…the top two commanders, Luis Taruc and Casto Alejandrino…” had been jailed in an attempt to prevent “Huk domination” in Central Luzon (1977:112). Other prominent Huk leaders such as Patricio del Rosario and Juan Feleo were disappeared. What was the effect on the Huk rebellion’s immediate future of wiping out most of the leadership? “In both size and organizational strength, the peasant rebellion grew between 1946 and late 1948” (Kerkvliet 1977:174). Following the increases in repression by
the Filipino government, the number of Huk guerrillas increased from a few hundred in 1945-46 to somewhere between 5 and 10 thousand in roughly two years (Kerkvliet 1977:174). By 1951 the Huk rebellion reached its peak size of an estimated 15,000 guerrillas (1977: 210).

Due to their covert or illegal status, we should expect rebel groups to have a more tiered leadership and membership. In stark contrast to the more politically based appointments of military and government officials, the upper ranks of rebel organizations have proven themselves loyal, trustworthy and effective over years of service to the group. Sendero members, for example, had to “rise through the ranks, demonstrating their commitment to the insurgency, which often means participating in a terrorist act or some other violent, life threatening activity” (Tarazona-Sevillano 1992:182). By the time a leader has risen to the upper ranks of the organization, “…they already have both a profound understanding of the movement and valuable hands on experience” (1992:184).

Likewise, Kerkvliet found that those wanting to join the Huk rebellion had to first prove themselves as trustworthy and non-belligerent. Once the Huks were established they could be more selective about their membership, as evident from the statement of one Huk rebel from the town of San Ricardo:

At first almost anyone could join the Hukbalahap. But then there were several men who abused barrio people, misused their guns, and so on. We even had some spies in our midst. So, we started to screen potential recruits. Then, new recruits were put on probation – they weren’t given guns right away. In this way, our discipline improved even while our numbers increased. So the Hukbalahap guerrillas had a good reputation with the people (Kerkvliet 1977:71).

What happened to the recruits that were rejected by the Huks is a case in point. According to Huk leaders, many of them joined the government’s squadrons (Kerkvliet 1977:72). The general view of the government units was that they “...mainly included men who
were either close to the government or were hoodlums whom the Hukbalahap had rejected.”

These differences in the command structure and membership credentials did not go unnoticed by the peasants interviewed by Kerkvliet in numerous towns and villages across Central Luzon.

In San Ricardo, villagers recalled that “The Huks were aggressive” toward the incumbent regime and extremely well supported by the populace. “Practically everybody in San Ricardo,” Kerkvliet concluded, “was in the Hukbalahap” (1977:72). In contrast, the government squadrons were widely considered cowards and despised for their level of abuse against the locals. According to one San Ricardo villager, the government squadrons usually “…just hid. They were cowards.” (1977:72). While the Huks were seen as being “…from the barrios…,” the military police and civilian guards were made up of men generally under the control of the local landlords. One year before the Huk rebellion began, the national government transferred control of the police and civilian guards over to the local governments, which meant even less accountability for their repressive behavior (Kerkvliet 1977:148).

Similarly, the killing of Sendero leaders by the Peruvian government did not slow the rebel group. Killing Sendero leaders, if anything, seems to have increased support to the organization as well as the frequency of violent attacks perpetrated by the rebel group against government targets. An excellent example is the prison massacres of 1986 that killed many top Sendero leaders. Tarazona-Sevillano (1992:183-184) describes the event:

In 1986, a large number of known key leaders were being held in the Lurigancho and El Fronton prisons, and many speculated that the movement was being run from inside the prisons themselves. When 256 Senderistas were killed in uprisings at these prisons in June 1986, it appeared that the organization might disintegrate, as many leaders were among the dead....On the contrary it quickly adjusted and soon went on the offensive again. After extensive study of the movement, police have concluded that Sendero’s leadership is not limited to a few individuals, but extends through a well-trained hierarchical structure.
The same cannot be said of the Peruvian military (and perhaps the militaries in most weak states) who are frequently lacking in commitment, training, experience, and whose overall command structure is highly centralized. At the height of armed conflict in Peru in 1987, President Garcia removed the individual heads of each branch of the armed forces and replaced them with a single minister of defense. The military’s funds were diminished and re-allocated to the local police, putting a strain on an already rivalrous relationship. According to Strong (1992:152) the Peruvian military, for all intents and purposes, had no professional class, presumably due to the low wages paid for military service. To make matters worse, the Peruvian military had a series of formal rules in place that served to limit the overall experience of its leaders and recruits in the areas where they served. The Peruvian military had a one-year rotation schedule for senior officers. According to Strong (1992:151), this policy almost guaranteed inexperience and apathy: “No sooner have officers worked their way into their new posting than they become preoccupied with the next one.” In addition, all armed forces recruits were sent to geographical areas away from their home area, apparently to limit the desertion rate.

Unlike the attacks on Sendero’s leadership, the assassinations of government and military officials by Sendero consistently diminished the government’s authority and presence in the areas where the rebels were operating. Sendero assassinated 166 government figures in 1989 alone. According to Hazelton and Hazleton (1992:216), “[a]t least eighty districts and four provinces had no municipal authorities, and political parties found it difficult to recruit candidates for office in many areas.” The pattern was no different for military leaders. Nearly a

What about the rank and file government soldiers? According to Strong (1992:150), sixty percent of the 120,000 members of Peru’s armed forces were two year draftees who received almost nothing aside from food and barracks for their service. Those who volunteered did so generally to avoid starvation. By the end of the 1980’s, Peruvian soldiers lacked basic material necessities such as bullets, boots and uniforms. Military desertions in the emergency zones ran between thirty and forty percent. As Sendero was reaching its peak size, the army was operating at thirty-six percent of its former operational capacity, according to government analysts (Strong 1992:151).

5.6 Contemporaneous State Repression and the Geographical Expansion of Insurgencies

In addition to creating a micro-mobilization process of rebel recruitment growth, especially in and around the rebel group’s area of origin, I argue that harsh repression also transplants that dynamic of recruitment to other geographical areas through a process of *forced migrant contagion*. This is one of the principal ways rebel groups are able to expand geographically into other parts of the nation. While cross-national contagion effects have gotten some attention in the civil war literature (Salehyan 2007), how rebel groups expand geographically within the same state has received almost no attention. Given the fact that most rebel groups could be accurately classified as “niche firms” (i.e., motivational grievances are localized) successful geographical expansion is not easily explained. Nor do I think that many rebel groups could have originated in areas into which they were later able to expand successfully. Since the socioeconomic conditions that give rise to oppositional groups are
usually regional or local in nature, as are territorial and ethnic divides, the geographical expansion of rebel groups requires an explanation. In ethno-regional conflicts especially, rebel groups may grow numerically commensurate with the population density in the group’s area of origin, but geographical expansion should be limited to that group’s homeland.

I argue that harsh state repression can solve this problem of finite expansion opportunities through a mechanism of forced migrant contagion. Only in the presence of harsh state repression will individuals who fit the rebel’s recruitment profile, in terms of class, ethnicity and birthplace, migrate in any sizable numbers throughout the nation, where both the police and rebel recruiters are waiting. Repression in the rebel group’s area of origin, especially indiscriminate repression, can lead to the forced displacement of people, particularly young males, from the rebel group’s area of origin to other parts of the nation. Once relocated, these forced migrants are frequently picked up by police or military as suspected rebels since they are from the rebel group’s area of origin and match the state’s stereotype of an insurgent.

This creates a large feedback loop where forced migration (resulting from repression in one part of the state) leads not only to the dispersal of potential rebel recruits, but to the spatial expansion of state repression as well. In sum, forced migration transplants the defensive mobilization dynamic to other areas of the state where it did not, and could not, have existed before.

5.7 A Brief Illustrative Case: Sendero Luminoso’s Expansion from Ayacucho into Lima and Huancayo

An example of this dynamic can be seen by following Sendero Luminoso’s expansion into Lima, the capital city, as well as other parts of the state beyond Ayacucho. Before government troops arrived the movement had been isolated in the Andean mountains for
nearly twenty years. Evidence from the case literature strongly suggests that the geographical expansion of Sendero was an *unintended* consequence of generalized state repression in the countryside which generated forced migration to urban areas. At the time, urban areas were not being targeted by the police and military. This is yet another example of how selective violence eliminates the environment needed to produce it. Limiting repressive violence to one geographical area creates an incentive for those most likely to be targeted to leave the area.

According to Smith (1992:133),

In December 1982, the elected civilian government of Fernando Belaunde Terry (1980-1985) ordered the armed forces to enter into action in Ayacucho and declared the region an emergency zone (EMZ). The military’s indiscriminate repression against the Ayacucho peasants had another repercussion that would ease Sendero’s expansion into Lima. Thousands of Ayacucho peasants fled from the region. In 1985, the Peace Commission estimated there were fifty thousand forced migrants, mainly settled in Lima, Ica, and Huancayo.

The largest recipient cities of forced migrants from Ayacucho, according to Smith and several other sources, were Lima and Huancayo, areas of no insurgent activity at the time. What happened to these thousands of forced migrants upon arriving in Lima and Huancayo and what were the consequences in terms of insurgency activity?

“Once in Lima,” says Smith (1992:133) “carrying a voting identification card with Ayacucho marked as birthplace was a guarantee of two weeks in the security police’s prisons and even torture.” Sendero responded to this new sector of victims with a policy of making “house calls” which usually began with an offer of support, starting the gradual evolution of a relationship of reciprocity between Sendero and internally displaced peasants in Lima. By 1985, “…Lima surpassed Ayacucho as the region with the most subversive activity, with a doubling of incidents over the year before…” (Smith 1992:134).
Manrique’s (1998) description of Sendero’s expansion into Huancayo is almost identical to Smith’s account of what happened in Lima. “During the same period that Shining Path was forced into a general retreat in Ayacucho by the Armed Forces’ genocidal tactics...” notes Manrique (1998:198), “there was a marked increase in terrorist actions in the central sierra. Beginning in January 1984, Huancayo and its adjoining districts were subject to recurrent nighttime bombings.” According to Long (2001:165), by 1990, the city of Huancayo “...had become a centre of violence...filling up with refugees from the surrounding southern highland regions, where Sendero had its garrisons.” In sum, insurgency activity met with harsh repression tends to be transplanted rather than diminished; a decrease in one area often will correspond to increases in another locations. The previous arguments and case evidence on the anatomy of state repression and its effects on rebel recruitment and geographical expansion can now be formalized into three hypotheses:

Hypothesis 1a (Membership Expansion): Harsh state repression will facilitate rebel recruitment resulting in larger insurgency organizations.

Hypothesis 1b (Membership Expansion): Levels of state repression before the civil war begins should have the largest impact on how large the insurgency becomes.

Hypothesis 2 (Geographical Expansion): Harsh state repression will result in a forced migration of people from the dominant insurgency locales resulting in a lower density and a higher rate of dispersal of insurgency activity.

5.8 Completing the Ecological Triad of Insurgency Growth

Harsh state repression, I argue, is one of the most important factors in explaining the growth of insurgencies. It is not, of course, the only important factor and its impact could be
mediated by other environmental conditions, such as opportunity structures. Opportunities for insurgency growth may be facilitated by obvious conditions such as state weakness or geography. Opportunity structures also includes the level of competition between the government and rebel group as they both seek to extract resources and recruits from the same civilian population. Governments and rebel groups not only compete in a military contest of might but must sustain themselves in a competition for support. In an environment of competition, the government in most cases has a clear military advantage which should translate into a recruitment advantage. The rebel organization, with more modest resources, will try to recruit from a population segment where the probability of defection is the lowest (Gates 2002). This will typically be population segments farthest from the government’s geographical center and closest to the rebel group’s geographical center. How competitive the government is in the recruitment process determines in large part the size of the rebel’s recruitment pool.

Second, we must control for ecological variables affecting the recruitment pool given their prominence in the social movement literature. Are the insurgents in a tactical position to organize and channel popular discontent into armed oppositional action against the state? I argue that rebel recruitment is heavily dependent on structural ecology (i.e., the number, and distribution of spatial interactions between recruiters and recruits). Recruitment efforts should be the most successful in areas with the highest number of recruiter-recruit interactions. Larger populations should produce more interactions between recruiters and recruits. Lastly, no theory of insurgency growth would be complete without tending to the rebels dilemma (Lichbach 1995). In addition to the impact of state repression, I emphasize individual
motivations for joining an insurgency such as private goods and selective incentives. How has the collective action problem been overcome? Despite grievances, why would otherwise conservative and risk avoidant nonelites participate in such a risky endeavor?

5.8.1 State Capacity, part 1: Coercive Machinery

Whether we are examining the great historical revolutions of past centuries or contemporary guerrilla insurgencies in the developing world, understanding participation in collective violence requires an examination of the state, particularly state incapacity as a political opportunity structure allowing for the emergence of dual or multiple sovereignty. As Goodwin (2001) notes, variation in state resources explains differential levels of success in reforming or responding to threats which give rise to oppositional challenges.

Most contemporary civil wars involve guerrilla warfare, the central feature of which involves the tactical avoidance of the state’s strength. As such, recent empirical works predicting civil war onset have tended to focus on those variables that limit the state’s ability to respond effectively to domestic threats before they get out of hand. Fearon and Laitin (2003), for example, give primacy to factors that have detrimental effects on the state’s capacity to police its territory effectively. Collaboration with insurgents is less likely to be detected and punished the smaller the scope of the state’s counterinsurgency capacity. The smaller the overall size of the state’s army, the fewer the number of troops and units that can be used in domestic counterinsurgency operations. The resulting decrease in general deterrence should make recruitment by the rebels easier as potential recruits downplay the odds that their participation will be monitored and punished by the state.
Hypothesis 3: The weaker the government’s army, the greater the level of insurgency growth.

5.8.2 State Capacity, part 2: Strong Societies and Weak States

There is a substantial literature on state capacity and political development that emphasizes variation in the state-society relations as a determinate force in the state’s ability to gain compliance and social control throughout its national territory. Migdal (1988) discusses the detrimental effects of social fragmentation on state capacity. Migdal (1988) argues that, “In parts of the Third World, the inability of state leaders to achieve predominance in large areas of their countries has been striking...The ineffectiveness of state leaders who have faced impenetrable barriers to state predominance has stemmed from the nature of the societies they have confronted...” Migdal notes that “Well over half the Third World countries are either very high or high in ethnic and linguistic fractionalization, while less than a third of other countries fall into these categories...” (1988:37). The impenetrable barrier that Migdal refers to is a highly fragmented society, the common denominator of which might be high levels of ethnic fractionalization.

According to Migdal, ethnically fragmented societies, which deny state leadership mobilization capabilities and popular support, tend to produce a particular administrative and political style that Migdal terms the “politics of survival” (1988:213). Lacking the ability to penetrate society, to regulate social relationships, and extract and appropriate resources effectively, and lacking support from any sizable segment of the population that would lend some level of legitimacy to the state, leaders of fragmented societies seek to establish social control through repression and state terror. In both Migdal (1988) and more recently in Kalyvas...
higher levels of fragmentation are theorized as increasing state repression in attempts to force collaboration and deter support for the government’s competitors.

There is also evidence from other bodies of literature that ethnically divided societies are more conflict prone. Conducting sensitivity analyses on civil war onset with several different model specifications, Hegre and Sambanis (2006) found levels of ethnic fractionalization to be the single strongest predictor of minor armed conflict since 1945. Using a new measure of ethnic fractionalization that takes into account the size of the ethnic group in power (EGIP), Cederman and Girardin (2007) find that minority rule is a highly significant predictor of civil war onset in a sample of 88 countries covering 3,327 country years. Fearon, Kasara and Laitin (2007:191), examining a sample of 160 countries from 1945 to 1999, find that ethnic minority rule in a given country year increased the odds of civil war onset about 2.75 times for each year continued under minority rule. Heger and Salehyan (2007), utilizing data on the ethnicity of the head of state and the proportion of the population that shares that ethnicity, found that the smaller the ethnic coalition supporting the government, the more ruthless the state’s response to civil conflicts. Other studies have found measures of ethnic fractionalization significant in decreasing rates of economic growth and lowering the provision of public goods offered by states (Mauro 1995; Alesina, Baqir & Easterly 1997; Easterly & Levine 1997).

In sum, greater levels of ethnic fractionalization are expected to increase the frequency of civil conflict (Hegre and Sambanis 2006), lower state capacity, legitimacy, and autonomy, decreasing the ability of the state to monitor and gain compliance from its population (Migdal 1988). All of these factors and processes should be generally favorable to insurgency growth as
they provide a steady source of grievances against the state, while also decreasing the state’s control over population segments and territory.

Hypothesis 4: Greater levels of ethnic fractionalization should lead to greater levels of insurgency growth.

5.8.3 State Capacity, part 3: The Tyranny of Distance

The idea that mobilization for collective action requires some variant of “free spaces” (Polletta 1999), “autonomous regions,” “liberated zones” (Radu & Arnold 1990:29), “evacuated areas,” “sanctuaries” (Manwaring & Prisk 1988) or “safe havens” is well established in the collective action literature and the qualitative literature on rebellions, revolutions and civil wars. Like competing firms, rebel groups will be better able to expand their business into new geographic areas, acquiring new clientele, the lower the amount of competition by the government in the area. The geographic location of the rebel group’s base area should be fundamentally important to their recruitment success as geographic distance is one of the best proxies we have for the level of dyadic competition between the government and rebel groups in the recruitment and collaboration market. Renamo, which was able to achieve a peak size of over 20,000 regular troops, operated almost entirely out of the northern-most provinces of Mozambique, while the capital city of Maputo lies in the southernmost tip of the country. Examining patterns of territorial control during the civil war in Peru, Koc-Menard (2007) found that Sendero consolidated control in the areas of the Peru where counterinsurgency forces were the weakest. According to Smith (1992:18) the Andean mountains and jungle regions that became Sendero’s “operational priority” were for all practical purposes a stateless area of Peru. “In a cold-hearted analysis of costs and benefits, this region had little importance for the Lima
government. It did not warrant the investment of political, economic, military, and human resources to maintain effective control there.”

In addition to state capacity, geographical distance should impact the ability of the state to project its coercive force, which in turn affects the ability of the rebel group to provide safe haven. The greater the distance the rebels are able to operate from the state’s headquarters, the greater their opportunity for achieving territorial consolidation and recruitment success. Recruitment should become easier for both the government and the rebel organization the greater the extent that they are drawing recruits from different pools or segments of the population (Gates 2002).

Hypothesis 5: The greater the distance between the rebel base and the capital city of the state, the greater the level of insurgency growth.

5.8.4 State Capacity, part 4: Popular Support for the Incumbent Regime

Insurgencies should be more difficult to sustain the higher the percentage of the population that supports the incumbent regime relative to the overall population. When the government is controlled by members of an ethnic group that represents the majority of the state’s total population we should expect insurgency growth to be more difficult. Conversely, as the percentage of the population that may feel ethnically marginalized from state power increases, it should be easier for insurgencies to expand. The proportion of the population that shares the ethnic identity of the head of state should be an important proxy for general levels of popular support for the government.

Hypothesis 6: The greater the percentage of the population marginalized from state power, the greater the level of insurgency growth.
5.8.5 Population Dynamics and Rebel Recruitment

Those who initiate and those who provide support for a rebellion “...are often very different groups of people...” (Aya 1979:45). Once a rebel group is formed, it seeks recognition, collaboration and participation from surrounding segments of the population. Just because an insurgent organization has the opportunity to exist alongside the state doesn’t necessarily mean the group is in the best situation to recruit or expand. There must be groups of people in a tactical position to participate. Studies in the field of human ecology have generally found that spatial distribution is fundamentally important in shaping the opportunity structure of social action; proximity is a necessary structural basis of persuasion and coercion. Higher numbers of people living closer together increase the chances for intentional and unintentional contact which increases active group making and the potential for mobilization (e.g., Tilly 1978; Tarrow 1994; Case 1981; Whyte 1956; Festinger, Schachter, and Back 1950). Researchers have found this to be especially true among ethnically homogenous groups (e.g., Zhao 1998; Gans 1967; Newcomb 1961). Similarly, I argue that an important factor in the ability of a rebel group to expand is the proximity of potential recruits and the ability of the rebels to monitor and sanction non-compliance. Rebels operating in states with greater populations should have the highest frequency of recruiter-recruit interactions, the greatest ease monitoring and sanctioning non-compliance, and as a result the highest probability of expansion. In contrast, rebels operating in sparsely populated countries should have the greatest difficulty in cultivating and expanding their social networking structures.

Hypothesis 7: I expect that the larger the population within a state, the greater the level of insurgency growth.
5.8.6 War Type and Insurgency Growth

Civil wars can be categorized according to whether the objective of the rebel group is to take control of the national government (i.e., revolution), or part of the state’s existing territory (i.e., secession) as well as whether the primary cleavage between the government and rebel group is ethnic or ideological. Almost all territorial conflicts are ethnically based, but not all ethnic conflicts are territorial; roughly half of all conflicts with an ethnic divide are fought for control of the national government. Recent scholarship has found that war type and how the war may be fought are related. Territorial conflicts, for example, tend to be fought further away from the capital city of the state than revolutionary conflicts (Buhaug & Gates 2002). Territorial conflicts also generally have lower levels of conflict severity than conflicts fought over the control of the national government (Hegre & Salehyan 2007). Toft (2006) has argued that shared ethnicity and group concentration make mobilization easier to achieve. Her research has shown that dispersed ethnic groups are far less likely to make secessionist demands against the state than ethnic groups that are geographically concentrated. Conversely, Brown (2000) has found that intermixed groups (non-concentrated) are less likely to make secessionists demands, presumably because of the group’s lack of previous autonomy. Instead, ethnic groups that are intermixed with other groups are more likely to want control over the central government (i.e., have revolutionary goals) since autonomy or control over the territory where they reside does not automatically make them more secure from attacks from rival groups. Kaufman (1996) in particular argues that intermingled population settlements create intense security dilemmas that force ethnic groups into defensive enclaves. In such an ethnic security dilemma, control of the national military becomes paramount. Thus, intermixed
ethnic groups are theorized as more likely go after control of the state than regionally concentrated groups who are better able to defend themselves.

If recruitment is heavily dependent on spatial ecology, and ecology determines rebel objectives, then the same conditions that give rise to a particular type of civil war should generate differential rates of recruitment success and hence rebel group size. Territorial conflicts should present a more suitable ecology for rebel recruitment since population concentration, density, and homogeneity have already been found to increase the mobilization potential of protest groups and social movements (Zhao 1998).

On the other hand, revolutionary conflicts, both ethnic and non-ethnic, tend to be fought by more dispersed groups (Horowitz 1985). The bulk of the fighting in revolutionary conflicts takes place closer to the state capital since gaining control over the capital is the goal of revolutionary warfare (Buhaug and Gates 2002). Generally speaking, the intermixed or dispersed nature of the recruitment pool combined with the increased competition emanating from the nearby state capital should inhibit the ecology of rebel recruitment, leading to smaller rebel organizations. While ethnicity increases the mobilization potential of concentrated groups, it has also been theorized as lowering the mobilization potential of dispersed or intermixed groups (Horowitz 1985) by increasing the government’s ability to monitor and sanction.

Hypothesis 8: Ethnic conflicts and territorial conflicts should experience easier growth than ideological conflicts or revolutionary conflicts.
5.8.7 Opportunity Costs and Rebel Recruitment

Irrespective of the political opportunity structures within a state that allow a rebel group some operational space to exist and the ecological conditions that effect the rebel group’s ability to seek out new recruits, a person’s choice to participate in a rebel organization, is, according to rational choice approaches, a self-interested calculation based heavily on the availability of private goods that are unavailable to non-participants. The logic of rational choice approaches to collective action rest on the basic assumption that non-elites would prefer to remain uninvolved in political violence using their time and energy to secure subsistence for their families while avoiding death and injury. Nonetheless, non-elites do join rebel organizations by the tens of thousands and participate in anti-regime violence. Why? According to Lichbach (1995) collective action is less likely to occur without the presence of selective incentives or private rewards, because the probability of success (defeating the government) is not high, is not dependent on any one person’s contribution, and the benefits of success are not limited to or contingent upon their personal participation. Overcoming the rebel’s dilemma requires private goods or private punishments. Otherwise everyone will attempt to free ride on the work of others.

Collier (2000) argues rebellions must be economically viable in order to attract a sufficient number of recruits. A rebellion-as-business approach would expect the size of rebel groups to be directly linked with the costs of participation, that is, the loss of income a potential rebel might face by joining an insurgency versus going to work in a factory or farming. Collier (2000) models rebellions as quasi-criminal activity where the opportunity for financial gain through looting is a primary motivation to participants. When the difference between
legitimate income and rebel income is negative, that is, rebel activity is paying better than the market, rebel groups should have no problem in recruiting new members. According to Collier, rebellions are atypically profitable when the rebels control natural resources and can extort rents from the sale and export of the primary commodities. Costs are atypically low when the country is impoverished and underdeveloped. Potential recruits have less of a stake in the status quo, the lower their income prospects.

Hypothesis 9: Lower levels of economic development should lead to greater levels of insurgency growth

The level of democratization of a state should play an important role in the decision to join a violent insurgency as democracy is itself an indicator of what other options are available for achieving social and political change short of using violence. Autocratic and exclusionary regimes are much more likely to be viewed as illegitimate by large segments of the population linking popular discontent with regime change. As Opp (1990:524) argues, if the current “…political order is regarded as a public evil…” then the creation of an alternative political order could be “…regarded as a public good.”

Hypothesis 10: Insurgency growth will be greater in non-democratic states.

5.8.8 War Duration and Selective Incentives

Many selective incentives offered by rebel groups, such as protective services, as well as income, will operate on an economy of scale. The longer a rebel group has been in business the more resources it typically has acquired and can offer recruits. If a rebel group grows larger over time, that increase in size will also confer an advantage in its ability to control territory, populations and the resources within it, thereby tying the survival strategies of the resident
population to the survival of the rebel group (Migdal 1988). In addition, the longer a war lasts, the more damage is done to the traditional economy, further lowering the opportunity costs of rebel recruitment.

It also takes time to develop the kinds of social networks that are critical to all recruitment processes, whether the group is conventional or non-conventional. The longer a rebel group has been in operation, the more extensive its organizational advantage and social networks. Studies based upon interviews with former insurgents have shown that most recruits knew their recruiter on a personal level before joining the insurgent group, i.e., the recruiter and recruit were part of a social network that preceded the act of recruitment (Florez-Morris 2007). In an interesting correlate, criminologists and sociologists have explained the growth of urban youth gangs as a process of person to person contagion. Gangs grow over time due to a steady accumulation of individuals, based on proximity, into the “lifestyle.” Once in, they tend to stay due to processes of stigmatization along with the natural desire for status and recognition among a peer group (Moore 1978; Horowitz 1983).

Hypothesis 11: The longer a conflict lasts, the greater the level of insurgency growth.

5.9 Conclusion

Using quantitative psychological studies on the mental health effects of torture and detention of victims during civil war as well as qualitative studies of civil wars, I have attempted to show in this chapter, that insurgencies grow, in membership size and in operating range, in repressive environments. The membership size of an insurgency is a function of both the birth weight of the rebel group, and the ability of the rebel group to recruit during the conflict. The size of the rebel group at the start of the civil war should be a function of how
repressive the state was in the years leading up to the war, whereas the ability to sustain the group should depend more on contemporaneous levels of repression during the conflict. As for the spatial expansion of insurgencies, I argued that insurgency expansion is often the unintentional by-product of forced displacement as insurgents and their supporters attempt to flee expanding levels of repression. Spatial moves into areas of lower risk result in a consistent pattern of dispersion of insurgency activity within the state. In chapters 6 and 7, I test these claims using data on the membership size of 89 insurgencies as well as data on the magnitude, dispersion and density of their operations.
Chapter 6

Research Design and Empirical Test III: Insurgency Membership Size

Measuring the size of an insurgency is no easy task. There are several alternative ways of measuring a dynamic and interactive process such as insurgency growth, each with its own virtues and its own set of problems. For instance, we could take the average size of an insurgency organization by dividing the all the available yearly estimates by the number of years. While a reliable measurement for some cases, numerous other cases have a more skewed distribution where the rebel group started out small or remained a consistent size for an extended period of time only to reach a far larger peak size in a relatively short period of time. Columbia provides a good illustration of this problem in establishing group size. The Revolutionary Armed Forces of Columbia (FARC) had an average size of 4,600 troops in their first 5 years and 17,000 in their last 5 years, producing an average of 9,282 over the entire time period for which we have data.

Another option is to focus on a group’s peak size: the largest possible value to occur within the series. Focusing on peak size produces similar problems in that the peak size for most rebel groups is not representative of the mean or mode for the group. The largest problem posed by taking a snapshot measurement, such as the peak size, is determining when the independent variables will be measured. If the independent variables are measured at onset, they may precede the peak estimate by ten years or more. If taken near the time of the peak estimate, the ongoing civil war has most likely already ravaged the economy, lowered population growth, and thus changed many of the independent variables. If we attempt to
solve this problem by averaging our independent variables, now both our dependent variable and explanatory variables may be biased.

The necessity of trying to account for multiple ongoing conflicts in a single country year adds another layer of complexity to the problem of measuring insurgency size. Over twelve percent of all country years spent in civil war between 1981 and 2005 had multiple ongoing civil wars with multiple rebel groups operating at the same time. If we try to include both groups separately in the analysis, then countries with two ongoing civil wars will be in the sample twice over the same time span and states with three ongoing civil wars will be in the sample three times. In other words, when using a cross-sectional measurement of insurgency size, such as average size or peak size, country years that contain more than one ongoing civil war will be in the sample more than once. Including every state that is experiencing multiple ongoing conflicts more than once in the sample would present a major selection bias, not to mention the statistical problems of including the same data for the same years multiple times in a regression. In addition, trying to account for multiple ongoing civil wars in the same year inevitably creates situations where multiple rebel groups operating within a state will have peak sizes or average sizes at different points in time. The question would then become at what time do we take the measurements of the independent variables?

6.1 Dependent Variable/Unit of Analysis

Taking into account the theoretical approach and statistical considerations, the most practical, parsimonious, least problematic solution to the above problems would be to make the unit of analysis the sum of all rebel troops the state faces in every country year, creating a pooled time series cross-sectional dataset (PTSCS). The advantages to such a format are
numerous. First, it is inclusive. By focusing on the total number of rebel troops a state faces in civil war in each year, we won’t lose any information or country years. The dependent variable will be the total number of rebel troops a state faces in every year and we can control for multiple ongoing conflicts by adding a counter variable for the number of active rebel-government dyads. Additionally, a pooled time series format allows us to measure our independent variables consistently for all years, such as, using a one year lag for all independent variables. If groups are more likely to reach their peak size towards the end of a civil war, and civil wars diminish the economy over time, then peak size will naturally be correlated with lower levels of economic performance. By forgoing any attempt at creating a median or peak size for our rebel groups, the dependent variable in every country year will be explained by contemporaneous measurements of the independent variables.

Most importantly, this format fits naturally with our theoretical approach, given that our primary independent variable of interest is state repression of physical integrity rights as measured by the CIRI Human Rights Data Project. Since the CIRI physical integrity index is generated at a national level of analysis, our dependent variable should also reflect a national level of analysis, such as the total number of rebel troops the state is fighting in each year. This format also allows us to avoid having to account for mergers or alliances between rebel groups which can cause large fluctuations in yearly group size. Coalitions of rebel groups, also a particularly common phenomenon in civil wars, are also effectively accounted for by summing the membership across all rebel groups in every year.

Data collection on insurgency membership size was based primarily on the following resources: UCDP case descriptions (after 1989), Cunningham, Salehyan and Gleditsch’s (2008)
data on rebel groups and *Civil Wars of the World: Major Conflicts Since 1945* (deRouen and Heo 2006). For information on the independent variables see chapter 4 section 4.1.

6.2 Methodology

The unit of analysis is every available country-year spent in civil war between 1981 and 1999 (N=435) with the population of civil war cases taken from Sambanis (2004). The resulting pooled cross-sectional time-series dataset contains variables for 58 countries, engaged in 89 ongoing civil wars. A Hausman test was not significant indicating that we need not consider fixed effects when using this series. I also tested the data for autocorrelation and found evidence of a first order serial correlation (H0: no first-order autocorrelation; Prob > F = 0.000). Model 1 gives the results of an AR (1) regression with a single lag OLS of residuals and panel corrected standard errors. The second model substitutes a Revolution/Territorial dummy variable in place of the Ideological/Ethnic variable.

6.3 Findings

Models 1 and 2 predict the membership size of insurgencies for every available year using a static measure of state repression taken 3 years and 5 years before the start of the war. All other independent variables are lagged one year. In table 6.1 we see strong evidence that how repressive the state was before the civil war began is highly significant. As I found in Part I predicting civil war onset, how repressive the state was three to five years before the civil war starts is perhaps more important than any other time. When a state responds to oppositional group violence, outside of civil war, with repressive violence and a civil war erupts three years in the future, we can expect the insurgency to be roughly 2205 members larger for every one point increase in the state’s pre-war CIRI physical integrity score. As we move back to 5 years
before the civil war, the effect is even larger. The full range of variation moving from the least repressive state in the sample to the most repressive state corresponds to an insurgency membership size increase of over 25,000.

Using measures of state repression years before the civil war started to predict the size of the insurgency for every year during the civil war is an eclectic but powerful design for dealing with reverse causality and endogeneity. It yields much greater confidence that insurgencies have very deep roots and that state repressive violence drives the process of insurgency growth. By impacting the magnitude of grievances, the degree of mobilization and, hence, the size of the socio-political support system (which I argue is primarily built before the war), greater state repressive violence in essence generates insurgencies with a larger potential for growth. If pre-war repressive violence is a necessary condition for civil war, as I have argued throughout this study, then the amount of variation in repression in the years before the war will be greater and have more explanatory potential for rebel force size than the level of repression just prior to or during the war. Besides Battle Deaths per year, which barely misses statistical significance, no other independent variables are significant in Models 1 and 2. It is important to note that requiring a civil war to have a 3 year and 5 year pre-war repression score reduced the sample of country years and the number of civil war cases by roughly half (i.e., only civil wars with start dates between 1984-1986 and after could be included). Thus, the significance levels for the rest of the independent variables should be regarded with caution in Models 1 and 2.
### Table 6.1 Insurgency Membership Size: Pre-War and Contemporaneous Measures of State Repression, 1981-1999

**Estimation Method:** AR(1) Regression with a Single Lag OLS of Residuals and PSCE

<table>
<thead>
<tr>
<th></th>
<th>Model 1</th>
<th>Model 2</th>
<th>Model 3</th>
<th>Model 4</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>DV: Troop Size</td>
<td>DV: Troop Size</td>
<td>DV: Troop Size</td>
<td>DV: Troop Size</td>
</tr>
<tr>
<td></td>
<td>State Repression score taken 3 years</td>
<td>State Repression score taken 5 years</td>
<td>lagged one year War type: Ethnic</td>
<td>lagged one year War type: Revolution or</td>
</tr>
<tr>
<td></td>
<td>before onset</td>
<td>before onset</td>
<td>or ideological</td>
<td>Territorial</td>
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<td>State Repression</td>
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<td>3,132.966</td>
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<td></td>
<td>(863.939)**</td>
<td>(1,130.894)**</td>
<td>(145.409)***</td>
<td>(139.151)***</td>
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<td>-5.596</td>
<td>-4.669</td>
<td>-5.306</td>
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<td></td>
<td>(4.587)</td>
<td>(19.056)</td>
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<td>Military Spending</td>
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<td>0.000</td>
<td>0.000</td>
<td>0.000</td>
</tr>
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<td>(0.000)</td>
<td>(0.000)</td>
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<tr>
<td>GDP per cap</td>
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<td>-2,717.345</td>
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<tr>
<td></td>
<td>(2,414.080)</td>
<td>(2,087.230)</td>
<td>(1,131.322)</td>
<td>(1,667.802)</td>
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<td>Duration</td>
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<td>-268.773</td>
<td>129.911</td>
<td>81.778</td>
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<td></td>
<td>(275.355)</td>
<td>(322.937)</td>
<td>(114.849)</td>
<td>(138.299)</td>
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<td>Ethnic fract</td>
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<td>8,505.835</td>
<td>450.198</td>
<td>1,846.682</td>
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<tr>
<td></td>
<td>(6,366.714)</td>
<td>(7,174.692)</td>
<td>(2,662.477)</td>
<td>(1,966.045)</td>
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<tr>
<td>Mountainous</td>
<td>-58.779</td>
<td>-105.443</td>
<td>-72.908</td>
<td>-57.113</td>
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<td></td>
<td>(83.951)</td>
<td>(95.064)</td>
<td>(39.032)</td>
<td>(51.917)</td>
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<td>Population (ln)</td>
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<td>979.551</td>
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<td>(5,922.735)</td>
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<td>Minority rule</td>
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<td></td>
<td>(4,680.503)</td>
<td>(6,825.247)</td>
<td>(1,673.774)</td>
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<td>Polity Index</td>
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<td></td>
<td>(162.924)</td>
<td>(129.243)</td>
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<td>Distance (ln)</td>
<td>339.229</td>
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<td>180.081</td>
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<td></td>
<td>(406.521)</td>
<td>(402.424)**</td>
<td>(381.121)</td>
<td>(363.799)</td>
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<td>Ethnic conflict</td>
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<td>12,890.728</td>
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<td>1,931.919</td>
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<tr>
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<td>(5,433.434)</td>
<td>(7,730.731)</td>
<td>(2,906.848)</td>
<td>(1,256.002)</td>
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<td>Dyads</td>
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<td>-1,030.988</td>
<td>886.873</td>
<td>1,931.919</td>
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<td></td>
<td>(2,159.093)</td>
<td>(2,289.072)</td>
<td>(1,352.263)</td>
<td>(1,256.002)</td>
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<td>Revolution</td>
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<td>-1,030.988</td>
<td>886.873</td>
<td>1,931.919</td>
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<td></td>
<td>(2,159.093)</td>
<td>(2,289.072)</td>
<td>(1,352.263)</td>
<td>(1,256.002)</td>
</tr>
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<td>Deaths (1000s)</td>
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<td>197.431</td>
<td>297.367</td>
<td>303.548</td>
</tr>
<tr>
<td></td>
<td>(120.712)</td>
<td>(122.801)</td>
<td>(72.623)***</td>
<td>(75.869)***</td>
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<td>Constant</td>
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<td>(17,157.590)</td>
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<td>43</td>
<td>43</td>
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<tr>
<td>Number of panels</td>
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<td>23</td>
<td>43</td>
<td>43</td>
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<tr>
<td>R2</td>
<td>.25</td>
<td>.30</td>
<td>.35</td>
<td>.24</td>
</tr>
</tbody>
</table>

*Note: Standard errors in parentheses; ** significant at 5%; *** significant at 1%*
As Models 3 and 4 show, contemporaneous state repression (lagged one year) is also a highly significant predictor of larger insurgencies, although not as strong as pre-war levels of repression. The coefficient of 411.96 indicates that for every one point increase in the CIRI nine point physical integrity index during the war, we can expect the size of insurgencies to be around 412 members larger. Moving from the least repressive state in the sample to the most repressive, we would expect the size of the insurgency to increase by roughly 3,708 rebel troops. Similar results are found in Models 3 and 4 for all the remaining independent variables. The one exception is the number of Battle Deaths per year, which is now highly significant and associated with larger insurgencies. For every one thousand battle deaths occurring in a year, insurgency membership increases around 300 members. If we speculate that the distribution of fatalities between the government and the rebel group is roughly a 50/50 ratio, then for every 500 rebels that are killed by the state, 300 new recruits take their place; a replacement rate of nearly 60 percent. An increase in battle deaths of one standard deviation (7.9) is associated with a size increase of 2,251 insurgents. If we consider both state repressive violence and the number of casualties each year as a two-dimensional gauge of the severity of a state’s counterinsurgency campaign, the combined effects on insurgency recruitment is powerful. Counterinsurgency campaigns that produce levels of state repression and battle deaths at the 75th percentile will face insurgencies with roughly 13,000 more insurgents than counterinsurgency campaigns where repressive violence and battle deaths both fall around the 25th percentile.

A counterargument would be that larger insurgencies provide more opportunities for deaths to occur. Moreover, larger insurgencies might prompt a larger overall government
counterinsurgency response, meaning the total pool of troops from both actors will be larger which presumably will produce more deaths. The literature on conflict severity during civil wars, however, provides mixed support for these claims. Using the same data on battle deaths as the present analysis, Lacina (2006:286) found that “...Variables proxying state and rebel strength have no predictive power” in explaining the severity of civil wars. The military capabilities of the state were found to be insignificant in all of Lacina’s models of severity. Heger & Salehyan (2007) did find a positive correlation between rebel strength and conflict severity but their rebel strength variable is a measurement of relative capabilities between the rebel group and the government; hence, it is heavily, if not mostly, determined by the weakness of the state rather than the strength of the rebel group. As for a direct measure of state capacity and conflict severity, they found a significant negative relationship between higher levels of GDP per capita and conflict severity. In other words, as GDP per capita increased, battle deaths decreased. Where rebel strength is high, the state will almost by necessity be extremely weak and weak states generally experience much deadlier conflicts than economically stronger states. As a more precise measure of capacity and its impact on severity, we can examine the relationship between military size and yearly battle deaths directly. The correlation coefficient between Battle Deaths (1000’s) and Military Size (1000’s) in the sample is small with a negative sign (-0.024). The larger the government’s military, the fewer battle related deaths each year.

Indicators of state capacity, including GDP per capita, failed to reach the 95 percent level of statistical significance. Neither the size of the state’s military nor the state’s defense budget appears to be associated with insurgency size. Rather than interpreting this finding as
military size being unimportant, it would probably be more accurate to say that the number of troops a state commits to counterinsurgency is not generally a function of the total number of available troops. We may never know how many troops it takes to achieve effective deterrence in counterinsurgency because states never send enough troops for effective deterrence, and we lack data on how many troops are committed.

Mountainous terrain does reach the 10 percent level of significance in Model 3 with a p-value of .062. Once again we find an unexpected negative coefficient. For every one percent increase in mountainous terrain, insurgency membership decreases by 73 members. This increases our confidence that the findings regarding the same variable in the analysis of civil war onset (in Part I) were not an anomaly or an artifact of choosing cases based on opportunity. While mountainous terrain can provide a retreat for insurgents and a physical structure for decreasing their visibility, difficult terrain could be an impediment to rebel recruitment for the same reason. As a further test of the effects of terrain on insurgency growth, I included other geographical variables, such as the percentage of the state that is covered in forests (taken from Buhaug & Gates 2002). Arguably, forests provide more cover than mountains that may or may not be forested. I also included Buhaug & Gates’ measures of the total number of roads throughout the state and the ratio of land area to the number of roads. Both were found to be insignificant. Successful rebel recruitment, which determines the membership size of insurgencies, does not appear to be influenced, to any noticeable degree, by geographic conditions favorable to guerrilla warfare. Mountainous terrain, forested terrain and the general accessibility of the terrain appear to have little effect on the ability of rebel groups to successfully recruit.
In Part I we found that ethnic fractionalization and ethnic minority rule stood out as major predictors of which states faced oppositional group violence, short of civil war. But after controlling for state repressive behavior in modeling the onset of civil war, both variables lost their power. This suggested that ethnic demographics provided the tinder for civil conflict but not the spark. The same result is found in the present analysis: ethnic demographics have negligible effects on insurgency membership size when controlling for state repression. As seen in Models 3 and 4, conflicts with an ethnic divide between the combatants are neither larger nor smaller than non-ethnic conflicts and the difference between revolutions and secessions is not different than zero.

6.4 Conclusion

In the introduction, I presented a puzzling natural experiment where the same guerrilla leader experienced stunning success in the mountains of one Latin American country in recruiting peasants into a formidable guerrilla army only to suffer an isolated defeat on another Latin American mountain range. The findings of this paper shed some additional light on Che’s failure in Bolivia and on hundreds of other insurgencies that failed to grow by demonstrating that the determinants of rebel recruitment success do indeed lie beyond the immediate reach of the foco. Regardless of whether the conflict is rooted in an ethnic cleavage, or how poor the population may be, or how large or small the state’s army, or the experience and charisma of the insurgencies leader(s), the only significant and substantive independent variables effecting insurgency size are those directly related to the harshness of the state’s counterinsurgency campaign – especially before the insurgency began. Given the results from Table 6.1 it is less surprisingly that Che failed to successfully recruit a single new insurgent in Bolivia.
In the previous chapter I argued that there is little reason to expect insurgencies to expand beyond their area of origin, given that the socio-economic or ethno-political grievances that generate most conflicts are idiosyncratic and localized. Yet, many insurgencies not only expand from their point of origin but continue to expand until almost the entire country is contested. As a solution to this puzzle, I put forth a theory that harsh state repression of a rebel group’s area of origin frequently results in a dispersion effect caused by forced displacement. Those individuals most likely to be targeted by the state (based on modal characteristics of a rebel stereotype) will flee the area, presumably moving the minimum distance required to escape being targeted. Thousands of others do likewise and in a relatively short period of time a national or regional social network is established based on kinship, neighborhood, region or ethnicity. Specifically, this displacement has the effect of shifting the dominant locale of rebel operational activity from harshly repressed areas into either non-repressed areas or areas of greater population density which crowds the state’s repressive machinery thereby lowering an individual’s risk of being targeted by the state.

Figure 7.1 shows the expected consequences of state repressive violence on insurgency activity and how this leads to an expanding geographical range of insurgency activity. The graph shows two increasing functions and once decreasing function. A steady increase in insurgency attacks alongside a steady increase in the number of cities housing attacks signifies an overall dispersion effect where insurgency activity is becoming less and less dense.
To adequately test this theory of insurgency expansion, two indicators are needed: one showing a general dispersion effect and another showing a decrease in the density of insurgent activity in any one area. Two measures are needed in order to show that harsh state repression does not cause a decrease in overall insurgency activity but rather causes a decrease in the density of insurgency activity in any one area. This in turn produces a dispersal effect (i.e., rebel attacks spreading to a greater number of geographical units). To measure the dispersal and density of insurgency activity, three streams of information are needed: 1) total number of rebel attacks per year within each state fighting a civil war, 2) the total number of cities/towns in which the attacks took place in every year, and 3) how many attacks occurred in each
Using data from the Global Terrorism Database (GTD) (Lafree and Dugan 2008) I created two dependent variables, Dispersion and Density.

Dispersion is a count of how many cities or towns were recorded as having experienced an insurgency attack in the year. Density is the percentage of attacks that took place in the dominant city or location with the most attacks relative to the total number of attacks that took place in the state that year. Density was constructed by first identifying the dominant locale of insurgency activity for every year (i.e., the one city or town with greatest number of attacks) and then dividing the number of attacks that took place in that location by the total number of attacks that took place throughout the nation. For example, in 1994, Bangladesh saw thirty-six attacks that took place across twenty-two cities. However, thirty-two of the thirty-six attacks (eighty-nine percent) occurred in just one city: Dhaka. For this year, Bangladesh would have a dispersal score of twenty-two cities and a density score of eighty-nine percent. In sum, dispersal is a count of the geographic units that experienced any attacks (in this case, cities and towns), and density is the percentage of attacks in the dominant locale relative to the total attacks that took place in the year. Since the differences between minimum and maximum values were great across all three dependent variables (some states had only one attack in a year while others had hundreds), they were all log transformed. The results, however, were almost identical regardless of whether I used the original metric or logged forms of the dependent variables.

7.2 The Model

This analyses makes use of the same dataset used in chapter 4 - a pooled cross-sectional time series set of every year spent in civil war by all major nation-states (N=654). As in the
previous analysis, the data are fitted to an AR(1) linear regression model with panel corrected standard errors to correct a first order serial correlation in the dependent variable. In Models 1 and 2 the dependent variable is Dispersion with Model 1 showing differences between ethnic and ideological conflicts and Model 2 showing the difference between revolutions and territorial conflicts. War type and war goals are separated, as in previous analyses, due to multicollinearity (e.g., all territorial conflicts are ethnic but not all ethnic conflicts are territorial). In Models 3 and 4 the dependent variable is Density and the two models also differ with respect to war type.

7.3 Findings and Discussion

7.3.1 The Magnitude of Insurgency Operations

In Model 1, we see several conditions associated with the magnitude of insurgency activity during civil wars. The non-significance of Duration immediately suggests that we should not assume that insurgencies become more widespread simply as a function of how long they last. Rather, factors exogenous to the rebel organization appear to better explain why some insurgencies grow and others do not. State repression appears to be significantly related to more insurgency activity with substantive effects. Since the dependent variable is log transformed, we can interpret the coefficient as indicating a 14 percent increase (100*0.140) in the number of insurgency attacks for every one unit increase in the 9 point CIRI physical integrity index. Thus, the most repressive states in the sample experienced roughly 112 percent more attacks than the least repressive states in the sample. The variable Distance, which measures how far the rebel base area is from the state capital in kilometers, appears to be linked to lower amounts of insurgency activity. Insurgency attacks decline by .203 for every
one percentage point increase in geographical distance between the rebel base area and capital. This suggests that greater levels of rebel autonomy decrease the number of insurgency attacks against the state.

Table 7.1 Conditions that Influence the Magnitude, Dispersal and Density of Insurgency Activity

<table>
<thead>
<tr>
<th>Variable</th>
<th>Model 1 DV: Magnitude (Total number of attacks each year)</th>
<th>Model 2 DV: Dispersion (Cities housing attacks) War Type: Ethnic or Ideological</th>
<th>Model 3 DV: Dispersion (Cities housing attacks) War Type: Revolution or Territorial</th>
<th>Model 4 DV: Density (Percentage of total attacks in the dominant locale) War Type: Ethnic or Ideological</th>
<th>Model 5 DV: Density (Percentage of total attacks in the dominant locale) War Type: Revolution or Territorial</th>
</tr>
</thead>
<tbody>
<tr>
<td>State Repression</td>
<td>0.140 (.033)**</td>
<td>0.142 (.030)**</td>
<td>0.148 (.033)**</td>
<td>-0.047 (.018)**</td>
<td>-0.052 (.019)**</td>
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<tr>
<td>Military Size (K)</td>
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<td>0.000 (0.000)</td>
<td>0.000 (0.000)</td>
<td>0.000 (0.000)</td>
<td>0.000 (0.000)</td>
</tr>
<tr>
<td>Military Spending</td>
<td>-0.000 (0.000)</td>
<td>-0.000 (0.000)</td>
<td>-0.000 (0.000)</td>
<td>0.000 (0.000)</td>
<td>0.000 (0.000)</td>
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<td>GDP per capita</td>
<td>0.538 (0.091)**</td>
<td>0.454 (0.098)**</td>
<td>0.471 (0.095)**</td>
<td>-0.163 (.061)**</td>
<td>-0.176 (.060)**</td>
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<td>Duration</td>
<td>-0.004 (0.011)</td>
<td>0.001 (0.009)</td>
<td>-0.004 (0.008)</td>
<td>-0.012 (.006)**</td>
<td>-0.009 (.006)**</td>
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<td>Ethnic fract</td>
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<td>0.144 (0.266)</td>
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<td>0.004 (0.237)</td>
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<td>Mountainous</td>
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<td>0.001 (0.008)</td>
<td>0.004 (0.237)</td>
<td>0.004 (0.208)</td>
</tr>
<tr>
<td>Population (ln)</td>
<td>0.238 (0.003)</td>
<td>0.253 (0.002)</td>
<td>0.322 (0.003)</td>
<td>-0.121 (0.002)</td>
<td>-0.114 (0.002)</td>
</tr>
<tr>
<td>Minority rule</td>
<td>-1.463 (0.356)**</td>
<td>-1.236 (0.378)**</td>
<td>-1.473 (0.382)**</td>
<td>0.028 (0.192)</td>
<td>-0.018 (0.181)</td>
</tr>
<tr>
<td>Polity index</td>
<td>0.069 (0.010)**</td>
<td>0.075 (0.010)**</td>
<td>0.091 (0.011)**</td>
<td>-0.010 (0.006)**</td>
<td>-0.009 (0.007)</td>
</tr>
<tr>
<td>Distance (ln)</td>
<td>-0.203 (0.057)**</td>
<td>-0.161 (0.054)**</td>
<td>-0.075 (0.071)</td>
<td>0.067 (0.035)**</td>
<td>0.068 (0.037)</td>
</tr>
<tr>
<td>Ethnic conflict</td>
<td>-0.500 (0.289)**</td>
<td>-0.502 (0.299)**</td>
<td>-0.012 (0.172)</td>
<td>-0.000 (0.125)**</td>
<td>-0.001 (0.088)</td>
</tr>
<tr>
<td>Battle Deaths (K)</td>
<td>0.016 (0.009)</td>
<td>0.017 (0.009)</td>
<td>0.013 (0.010)</td>
<td>-0.000 (0.004)</td>
<td>-0.001 (0.004)</td>
</tr>
<tr>
<td>Conflict Dyads</td>
<td>0.109 (0.247)</td>
<td>0.095 (0.221)</td>
<td>-0.001 (0.165)</td>
<td>-0.281 (0.125)**</td>
<td>-0.129 (0.088)</td>
</tr>
<tr>
<td>Revolution</td>
<td>0.938 (0.187)**</td>
<td>0.938 (0.186)**</td>
<td>0.938 (0.187)**</td>
<td>0.938 (0.186)**</td>
<td>0.938 (0.186)**</td>
</tr>
<tr>
<td>Constant</td>
<td>-2.678 (1.162)**</td>
<td>-3.037 (1.104)**</td>
<td>-5.037 (1.037)**</td>
<td>6.040 (0.732)**</td>
<td>5.864 (0.756)**</td>
</tr>
<tr>
<td>Observations</td>
<td>406</td>
<td>407</td>
<td>407</td>
<td>406</td>
<td>406</td>
</tr>
<tr>
<td>R2</td>
<td>.57</td>
<td>.62</td>
<td>.69</td>
<td>.85</td>
<td>.84</td>
</tr>
</tbody>
</table>

Standard errors in parentheses; ** significant at 5%; *** significant at 1%
None of the variables measuring the physical strength of the state’s military capability (Military Size, Military Spending) reached statistical significance. The capacity of the state to find and kill insurgents had no detectable effect on the magnitude of insurgency activity. According to Model 1, higher levels of democracy and higher levels of GDP per capita lead to more, not fewer, insurgency attacks each year. I found this same relationship in chapter 2 examining a sample that included every available country-year from 1981-1999 outside of civil war. Now the same relationship is again found inside a much smaller sample of states fighting major civil wars. The amount of insurgency attacks can be expected to increase by 0.53 for every one percentage point increase in GDP per capita. For every one unit increase in the Polity IV index, the magnitude of insurgency attacks is 7 percent greater. Hence, the full range of variation on the dependent variable is around 140 percent moving from the least democratic group of states in the sample to the most democratic states.

Our demographic variables show that the amount of ethnic fractionalization appears to have little effect on the magnitude of insurgency activity while population size and minority rule do appear to be important. A one percentage point increase in population size increases the number of insurgency attacks during civil war by 0.23. The presence of minority rule is associated with less insurgency activity. The full range of effect on the dependent variable would be a decrease of 140 percent in insurgency attacks moving from a country where the head of state shares the same ethnicity as 98 percent of the population to a country where only 4 percent of the population shares the same ethnicity as the head of state. In other words, the magnitude of insurgency activity is significantly smaller under minority rule. It may be the case that the same ethnic demographics which tend to produce minority rule also tend to produce.
ethnically based or territorial conflicts which might have lower magnitudes of insurgency attacks on government targets based on where the insurgents reside relative to the government’s loci of power in the state. First and foremost, ethnic conflicts and territorial conflicts tend to be fought by concentrated, often segregated, ethno-political groups. Since the conflict zone is where the ethnic groups also lives, there may be a lack of government targets coupled with the disincentive of damaging the group’s own territory. According to model 1, the difference in magnitude of insurgency activity between ethnic conflicts and non-ethnic conflicts is not different than zero. However, substituting a dummy variable for whether the conflict was a revolution or territorial dispute (results not shown) produced quite strong results. The distinction between revolutions and territorial disputes was highly significant (p-value < 0.000) with territorial conflicts having 117 percent fewer attacks than revolutions. Mountainous terrain does not appear to influence the magnitude of insurgency attacks.

7.3.2 The Dispersion of Insurgency Operations

Models 2 and 3 give the result of an AR(1) regression of Dispersion. The models appear to fit the data well with an R-square of .62 and .69. Starting with those variables most directly related to counterinsurgency, we see that State Repression, the principal variable of interests, is highly significant with a coefficient of .142. This means the number of cities that experience insurgency attacks in an ongoing civil war can be expected to increase by 14 percent (100*.142) for every one point change in the CIRI physical integrity index - holding all other variables constant. We can expect that the number of cities where insurgency operations have successfully spread to will be 112 percent higher in the most repressive regimes in the sample in contrast to the least repressive regimes. The distance between the rebel base area and the
capital of state is also highly significant and negatively related to conflict dispersal. As argued in chapter 3, greater distances between the combatants should decrease both the ability and the desire of the state to inflict repressive violence. Since both \textit{Dispersal} and \textit{Distance} are logged transformed, we can conclude that a one percent increase in distance between the rebel base and capital city will yield a -0.203 percent decrease in the number of cities housing insurgent attacks. In model 3, we find, however, how \textit{Distance} drops from significance when controlling for revolutionary versus territorial conflicts. This is precisely what would be expected given that Buhaug and Gates (2002) found that revolutions tend to be fought closer to the capital city since taking the capital is the goal of revolutionaries.

The severity of combat, measured in terms of battle deaths in the thousands, is marginally significant and carries a coefficient of .009. The size of the government’s military and amount that the state spends on defense also appear to have little influence on the ability of the rebels to expand their operations from city to city. Military size is not significant and military spending, although significant, carries a coefficient that, even when rescaled from units of a thousand to units of a million, is still nearly zero (.00001). One hundred million dollars in defense spending would be associated with roughly a .000 percent decrease in the number of cities housing insurgent attacks. One billion dollars in military spending is associated with approximately a one percent decrease in the number of cities experiencing insurgency attacks. Across the board, the military strength of the state appears to confer very little gain when the mode of combat is predominantly irregular warfare coupled with terrorism.

How do ethnic demographics, the goals of the rebel group and the nature of the conflict affect insurgency expansion? The degree of minority rule in the state is one of the most
significant variables in the model with large substantive effects. The minority rule variable reflects the percentage of the population that does not belong to the same ethnic group as the head of state. Since the presence of minority rule is predominantly seen in societies where no one ethnic group has a clear numerical advantage, it is best to interpret minority rule as an outcome resulting from coalitions of ethnic minorities. The coefficient in Model 2 indicates that the presence of minority rule decreases the number of cities experiencing insurgency attacks by 146 percent relative to states ruled by a member of the ethnic majority. Thus, ethnically marginalized societies tend to produce more concentrated or less dispersed insurrections. One explanation for this finding is that secessionist conflicts are more likely in states where political parties are ethnically based and elections often result in the exclusion of a majority of the population from governmental power. Ethnic groups whose size is not large enough to secure a plurality of votes are subject to being governed by a coalition of smaller ethnic groups. Such groups may conclude that their interests would be better served by achieving regional autonomy or independence. This interpretation is further supported by findings on the war type variables. In Model 3 we find that revolutions are significantly more dispersed than territorial conflicts. Revolutions are on average 94 percent more dispersed than conflicts fought over a territorial dispute.

States with larger populations will have slightly more dispersed civil wars, on average, than states with smaller populations. In our sample of states fighting ongoing civil wars, a one percentage point increase in population size corresponds to a 0.19 percentage point increase in the number of cities experiencing insurgency attacks. Moving from the state with the smallest
population in the sample to the largest population in the sample we should witness a dispersal increase of roughly 19 percent.

Surprisingly, the Duration of civil wars is not significantly related to dispersal dynamics. In other words, it would be a mistake to view the size of an insurgency or the number of cities housing insurgency activity as natural growth given the time to do so. The non-significance of Duration supports one of the primary motivations underlying the study, i.e., that all else equal, there are few good reasons to expect insurgencies to expand naturally or inevitably. After controlling for the variables that do cause insurgencies to expand or stay concentrated, time has no important effect.

Lastly, I examine the other two variables which throughout the study have been treated as indicators of state capacity: GDP per capita and the Polity Index. Thus far, measures of military strength have either had no effects on the ability of insurgents to expand activities. Presumably, insurgents should have a harder time expanding their operations in economically stronger states with more democratic governments, as these states should be both stronger and have more popular support throughout the state, increasing the state’s control geographically. Once again, the opposite appears to be the case for both variables. GDP per capita is highly significant and associated with more dispersed conflicts. A one percent increase in income per capita translates into a 0.45 percentage point increase in Dispersion. The level of democracy in the state is also a highly significant predictor of greater dispersal rates with large substantive effects. Each one unit increase in the 21 point Polity IV index increases Dispersion by 7.5 percent (100*.075).
7.3.3 The Density of Insurgency Operations

Models 4 and 5 display the results for Density, the second indicator of geographical expansion. The Density of insurgency activity in every year was created by first identifying the dominant insurgency locale (the city with the most insurgency attacks) and then dividing the number of attacks in the dominant locale by the total number of attacks that took place in the country-year. The average Density of insurgency activity for the sample was 39 percent; meaning that 39 percent all attacks took place in one dominant locale every year with a standard deviation of 27 percent. Hence, in roughly 68 percent (+/- 1 SD) of civil wars, there is one dominant insurgency locale that houses between 12 percent and 66 percent of yearly attacks in the state. An R-squared of .85 and .84, in models 4 and 5, suggest that the models can account for the variation in the density of insurgency activity across civil wars quite well.

In model 4, we see that State Repression has a highly significant negative coefficient meaning that the density of insurgency activity with reference to a dominant insurgent locale is smaller the more repressive the state’s counterinsurgency campaign. As indicated by the coefficient for state repression, the density of insurgency activity decreases by 5 percent (100*.047) for every one unit increase in the CIRI physical integrity index. While harsh state repression is associated with increases in both the number of attacks (Model 1) and the number of cities housing attacks (Models 2 & 3), repression decreases the number of insurgency attacks that take place in any one place. Conversely, we could say that when state repression is low, insurgency activity tends to stay concentrated in one location rather than disperse. Moving from the dominant insurgency locales in the least repressive states to the dominant insurgency locales in the most repressive states in the sample, we could expect the density of insurgency
activity to decrease approximately 38 percent. Our second indicator of counterinsurgency harshness, *Battle Deaths*, is not significant. Military size and spending have almost no detectable effect on the density of insurgency activity. Military spending is marginally significant but even when re-scaled from units of a thousand to units of a billion, the coefficient is still zero out to two decimal places (.0067).

Where state capacity is greater, the density of insurgency activity is expected to be higher, since the ability of insurgents to move operations to other cities should decrease with greater state control over its territory. Higher incomes available to insurgents should also raise the opportunity costs associated with expanding insurgency operations to other cities. In model 4, we see that the density of insurgency activity is significantly smaller given higher levels of economic development in the state. The density of insurgency activity within the dominant insurgent locale decreases by 0.163 of a percentage point for every one percentage point increase in GDP per capita. The level of democracy in the state does not reach statistical significance.

Turning to the demographic variables, we see that the distance of the rebel base camp from the state capital does not influence the density of attacks. States with larger populations experienced more attacks (Model 1) in more cities (Model 2) than states with smaller populations, but fewer attacks in the dominant locale as a percentage of total attacks in the state. This suggests that insurgents can benefit from crowding effects by moving operations to other cities. In sparsely populated areas, the movement of insurgents is presumably more likely to be noticed by government forces than in populous countries where the insurgents can more easily blend into the crowd.
Higher amounts of mountainous terrain in a nation appear to be largely unimportant to the ability of the rebels to expand their geographical range. This conclusion holds across all models. While mountainous terrain is no doubt helpful as a temporary hideout for insurgents, such terrain has little value in expanding operations. Presumably, this may be due to the simple fact that there are few government targets worth attacking in the mountains. Neither war type nor the goals of the rebel group appear to be related to the density of insurgency activity.

Finally, we see that our control variables are in fact significant. The proportion of attacks in the dominant insurgency locale does seem to be influenced by how long the war lasts. Each year a civil war continues the percentage of attacks in the dominant locale decreases around 1.2 percent a year. Finally, our control variable for the number of ongoing civil wars in the state is a significant predictor of a lower density of attacks, as expected, and the marginal effects are very large. The coefficient on the number of active rebel-government dyads in the year indicates a decrease in the number of attacks that occurred in the dominant city by twenty-eight percent if a new civil war starts during an ongoing civil war. If three civil wars are fought at the same time, the number of attacks in the city with the highest number of attacks will be 56 percent lower than if one civil war is being fought. The fact that the data clearly show a very large difference in the density of insurgency activity when multiple civil wars are being fought at the same time is an excellent demonstration that the model is highly responsive to differences in the density of insurgency activity, especially given that the number of conflict dyads was not significant in predicting which states would have more attacks or how many cities would experience attacks. Interestingly, the number of ongoing wars in a given year in a nation tells us almost nothing about the amount of aggregate insurgency activity the state will
face or how many cities will house attacks, but it is a good indicator of the density of insurgency violence.

In light of the strength of the relationship found between the density of insurgency violence and the number of active conflict dyads, it is imperative that we go back and test the robustness of our findings dropping from the analysis every country year with more than one ongoing civil war. While I do not expect the findings to change since the results were obtained while controlling for the number of dyads in every model, nevertheless, the presence of two separate civil wars should produce density patterns very different from a single civil war. States with two ongoing civil wars will have two different dominant insurgency locales, but my coding procedure allows only one dominant locale per year. This coding rule ensures that the dominant locale will not have a very large percentage of the total number of attacks within the state, since one locale is divided by the total number of attacks that resulted from two or more civil wars instead of one. Table 7.2 shows a robustness test of models 1 through 5 dropping from the analysis all seventy-seven years in which the state fought more than one civil war. Keep in mind that the number of panels (states) stays the same; only years with overlapping civil wars are dropped from the sample. The results are almost exactly the same as in Table 7.1 for every single model. The coefficient on State Repression, our primary variable of interest increases in every single model. A couple of variables that were marginally significant fell out of significance.

It is important to view Density in conjunction with the results that were found for the Dispersion variable. For example, if we found that Density declined in the presence of harsh repression, this could simply mean that insurgency activity as a whole declines as repression
increases and logically this would be best reflected in the dominant locale for insurgency activity.

Table 7.2 Robustness Test: Dropping All Country Years with Multiple Ongoing Conflicts, 1981-1999

<table>
<thead>
<tr>
<th>Variable</th>
<th>Model 1 DV: Magnitude (Total number of attacks each year)</th>
<th>Model 2 DV: Dispersion (Cities housing attacks) War Type: Ethnic or Ideological</th>
<th>Model 3 DV: Dispersion (Cities housing attacks) War Type: Revolution or Territorial</th>
<th>Model 4 DV: Density (Percentage of total attacks in the dominant locale) War Type: Ethnic or Ideological</th>
<th>Model 5 DV: Density (Percentage of total attacks in the dominant locale) War Type: Revolution or Territorial</th>
</tr>
</thead>
<tbody>
<tr>
<td>State Repression</td>
<td>0.159</td>
<td>0.158</td>
<td>0.169</td>
<td>-0.048</td>
<td>-0.053</td>
</tr>
<tr>
<td></td>
<td>(0.034)**</td>
<td>(0.030)**</td>
<td>(0.033)**</td>
<td>(0.020)**</td>
<td>(0.020)**</td>
</tr>
<tr>
<td>Military Size</td>
<td>-0.000</td>
<td>-0.001</td>
<td>-0.000</td>
<td>0.000</td>
<td>0.000</td>
</tr>
<tr>
<td></td>
<td>(0.001)</td>
<td>(0.001)</td>
<td>(0.001)</td>
<td>(0.000)</td>
<td>(0.000)</td>
</tr>
<tr>
<td>Military Spending</td>
<td>0.000</td>
<td>0.000</td>
<td>0.000</td>
<td>0.000</td>
<td>0.000</td>
</tr>
<tr>
<td></td>
<td>(0.000)</td>
<td>(0.000)</td>
<td>(0.000)</td>
<td>(0.000)</td>
<td>(0.000)</td>
</tr>
<tr>
<td>GDP pc</td>
<td>0.618</td>
<td>0.538</td>
<td>0.533</td>
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<td>-0.189</td>
</tr>
<tr>
<td></td>
<td>(0.107)**</td>
<td>(0.116)**</td>
<td>(0.103)**</td>
<td>(0.065)**</td>
<td>(0.064)**</td>
</tr>
<tr>
<td>Duration</td>
<td>0.001</td>
<td>0.006</td>
<td>-0.004</td>
<td>-0.011</td>
<td>-0.013</td>
</tr>
<tr>
<td></td>
<td>(0.012)</td>
<td>(0.011)</td>
<td>(0.011)</td>
<td>(0.007)</td>
<td>(0.006)**</td>
</tr>
<tr>
<td>Ethnic fract</td>
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<td>-0.549</td>
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<tr>
<td></td>
<td>(0.331)</td>
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<td>(0.295)</td>
<td>(0.275)</td>
<td>(0.230)**</td>
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<td>(0.003)</td>
<td>(0.002)</td>
<td>(0.002)</td>
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<tr>
<td>Population (ln)</td>
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<td>0.222</td>
<td>0.321</td>
<td>-0.110</td>
<td>-0.094</td>
</tr>
<tr>
<td></td>
<td>(0.114)</td>
<td>(0.105)**</td>
<td>(0.098)**</td>
<td>(0.067)</td>
<td>(0.074)</td>
</tr>
<tr>
<td>Minority Rule</td>
<td>-1.138</td>
<td>-0.979</td>
<td>-1.198</td>
<td>-0.249</td>
<td>-0.274</td>
</tr>
<tr>
<td></td>
<td>(0.341)**</td>
<td>(0.330)**</td>
<td>(0.422)**</td>
<td>(0.202)</td>
<td>(0.200)</td>
</tr>
<tr>
<td>Polity Index</td>
<td>0.049</td>
<td>0.053</td>
<td>0.073</td>
<td>0.002</td>
<td>0.004</td>
</tr>
<tr>
<td></td>
<td>(0.013)**</td>
<td>(0.012)**</td>
<td>(0.012)**</td>
<td>(0.008)</td>
<td>(0.008)</td>
</tr>
<tr>
<td>Distance (ln)</td>
<td>-1.158</td>
<td>-1.015</td>
<td>-0.003</td>
<td>0.023</td>
<td>0.038</td>
</tr>
<tr>
<td></td>
<td>(0.060)**</td>
<td>(0.058)</td>
<td>(0.076)</td>
<td>(0.036)</td>
<td>(0.038)</td>
</tr>
<tr>
<td>Ethnic conflict</td>
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<td>-0.461</td>
<td>-0.001</td>
<td>-0.001</td>
<td>-0.001</td>
</tr>
<tr>
<td></td>
<td>(0.311)</td>
<td>(0.313)</td>
<td>(0.170)</td>
<td>(0.170)</td>
<td>(0.170)</td>
</tr>
<tr>
<td>Deaths (1000s)</td>
<td>0.013</td>
<td>0.013</td>
<td>0.009</td>
<td>-0.000</td>
<td>-0.001</td>
</tr>
<tr>
<td></td>
<td>(0.010)</td>
<td>(0.010)</td>
<td>(0.011)</td>
<td>(0.004)</td>
<td>(0.004)</td>
</tr>
<tr>
<td>Revolution</td>
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<td>0.990</td>
<td>0.144</td>
<td>0.144</td>
<td>0.144</td>
</tr>
<tr>
<td></td>
<td>(0.215)**</td>
<td>(0.215)**</td>
<td>(0.127)</td>
<td>(0.127)</td>
<td>(0.127)</td>
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<td>Constant</td>
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<td>(1.142)**</td>
<td>(1.203)**</td>
<td>(1.235)**</td>
<td>(0.773)**</td>
<td>(0.833)**</td>
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<tr>
<td>R2</td>
<td>.55</td>
<td>.58</td>
<td>.64</td>
<td>.87</td>
<td>.87</td>
</tr>
</tbody>
</table>

Standard errors in parentheses; ** significant at 5%; *** significant at 1%

We already know, however, that higher levels of state repression are strongly associated with a
greater magnitude and dispersal of insurgency activity. The results for Density give us a closer look at the causal mechanism. The results show that when insurgents face harsh repressive violence from the state, insurgency activity does not decrease but the amount of activity in any one place decreases. The simplest explanation for the pattern of results seen in models 1 - 5 is that insurgents, in response to state violence, simply move their operations to other parts of the state where levels of repression are lower.
CONCLUSION

Explaining how insurgencies grow despite the efforts of a much more powerful government to curb their growth has been the general purpose of this project. Applying this question to different temporal phases of insurgencies and different levels of analysis has produced some consistent findings. In this concluding section I summarize what I consider to be the most important findings of the project. Considering each part of the project independently and as parts of a whole, I see six conclusions that merit the most attention: (1) why states repress if it is so often counterproductive; (2) the consequences of state repression inside and outside of civil war contexts; (3) the importance of focusing on selection processes and opportunity in understanding insurgency escalation; (4) the differences in the causes of state repression outside and inside of civil war contexts; (5) bi-directionality and feedback loops in repression and insurgency growth, and lastly; (6) solving anomalies and advancing theory in civil war research.

1. Why do States Engage in Harsh Repressive Violence if it is Counterproductive?

Numerous recent qualitative works on civil war and counterinsurgency discussed in chapter 1 have concluded that state repression is more often counterproductive than productive. I believe that the results from this project show this as well while, more importantly, also showing the primary mechanisms involved in turning physical integrity abuses into physical insurgency expansion. The immediate question becomes: why do state’s repress if it is so often counterproductive? The results from this project suggest several possible explanations. The primary reason states repress when it’s counterproductive is that you first have to repress before you can evaluate its productiveness and by that time, I argue, a
dialectical bi-directional feedback loop has been established that makes insurgency expansion and in turn the expansion of repression almost inevitable. This is where Kalyvas’ (2006) mechanism of fragmentation and variation in repressive violence at the local level and my mechanism of repression driven contagion intersect. By and large, Kalyvas’ sees violence inside civil war, as the result of a decentralized process, operating mostly at the local level, in the field, in the village, where the agents of repression are local police or locally stationed soldiers. Driven by physical fragmentation and system overlap, repressive violence is produced, less by the policies and dictums originating from the capital and more by a decentralized, mechanistic, organizational process model whereby local people are detained, tortured, and killed due to the flows of daily intelligence.

The initial efforts to crush the insurgency disperse its members and operations putting the bulk of counterinsurgency efforts in the hands of local authorities. Local authorities are, on average, less professional than trained military personnel. While the dispersion of an insurgency expands the magnitude of governmental repression by putting counterinsurgency efforts in the hands of numerous local authorities throughout the state, the dispersion decreases the odds that any one insurgent in particular will become the victim of repression. We found in chapter 7 that state repression decreases the density of insurgency operations in the dominant insurgency locale. If the state goes on to apply this productive tactic that reduced insurgency activity in the dominant locale to other areas, a higher number of non-insurgents will necessarily be targeted since these new areas have a lower per-capita insurgent population. Stated differently, as the density of an insurgency declines and the state continues to cast the
same size net of repression violence in hopes of capturing insurgents, the proportion of insurgents caught relative to innocents will continuously decrease.

The dispersion of insurgencies and the demands it puts upon the state creates the structural foundation for guerrilla warfare. Since the state’s power resides in its centralized resources, dispersion generates the philosophical essence of guerrilla warfare: using the state’s strength against it. Dispersion creates a win-win situation for insurgents; if they disperse to new areas and the government chooses not to pursue, then the insurgents benefit. If the military follows, it is forced to spread out its own forces, commensurate with the density of insurgent forces. This fulfils a second dictum of guerrilla warfare: conduct surprise attacks on small isolated bands of government troops on insurgent’s turf.

A second explanation for why military and state leaders repress when it is usually counterproductive is that the immediate effects of harsh repression give the illusion that repression is productive. A recent article by Lyall (2009) illustrates this point well. Lyall (2009) found that Chechnyan villages that were shelled by the Russian military, witnessed a 24 percent decline in insurgency attacks compared to control villages that were not shelled. Lyall also found that insurgency activity did not increase in control villages. Lyall concludes that states engage in indiscriminate violence because it can be productive. It is likely that Russian military leaders have reached the same conclusion based on their pre-shelling and post-shelling analyses. The findings from chapter 7 of this project certainly bolster Lyall’s finding that harsh repression will have the immediate, empirically verifiable effect, of diminishing insurgency activity in targeted locales. The difference, of course, is that I find a less immediate and less empirically verifiable effect of insurgency operations being redistributed to other areas of the
Like Lyall, I found that harsh repression is indeed productive at diminishing insurgency attacks in some locations. While the results from chapter 7 show a general cross-national trend of spatial contagion, causality is never homogenous across cases; Chechnya might be one of those cases. Nevertheless, according to the Global Terrorism Database, the number of insurgency attacks in the Chechnyan region increased 316 percent from the end of 1999 to the end of 2000, the year that Lyall’s study begins. If the shelling began in the year 2000, then its immediate impact appears to be counterproductive. If shelling did not begin in 2000, then 2000 appears to represent a natural “peak” of insurgency activity, and thus, a gradual forthcoming decline might be expected. From the end of 2000 to the end of 2001, the number of attacks in the Chechnyan region did decline by 56 percent, providing evidence for the productivity of repressive violence. That is, until the number of attacks rose again by 31 percent from the end of 2001 to the end of 2002. In 2003, the number of attacks declined by 29 percent, returning to roughly the same levels as in 2001. Thus, the number of insurgency attacks in the region after three years of shelling was roughly the same as in 2001. Regardless of the final verdict on the Chechnyan case, or its generalizability, the details of case support my overall point in that the more immediate effects of repressive violence can often times be interpreted as productive; the counterproductive effects will almost always be more difficult to monitor.

2. The Consequences of State Repression Outside and Inside of Civil War Contexts

I have argued in this study that insurgencies, by and large, feed off the behavior of the incumbent government they are fighting, rather than being sustained by their own successful policies or motivations. Hence, what unites rebellions, revolutions, revolts, insurgencies, and civil wars as a subject of study, is that the origins of the rebel group have little to do with the
extent and speed of their militarization, their recruitment success, their membership size, the strength of their socio-political support system, their ability to expand their operations geographically or the overall severity of the conflict. Researchers with detailed knowledge of particular cases no doubt find that the historical origins of the insurgencies they study cannot be easily captured with cross-national macro-socioeconomic data. For instance, the Huk rebellion in the Philippines, one of the largest insurgencies since World War I (over 12,000 armed rebels), had its origins in the minute details of the tenancy system of wet-rice agriculture in Central Luzon (Kerkvliet 2002). The National Peasants Union, which later became the Hukbalahap, had two very modest grievances: a slightly higher share of the rice harvest (around a 10% increase) and interest free loans of rice in-between harvests to ensure tenants a subsistence floor.

The Huk rebellion presents two general puzzles about insurgency growth -- one about participatory expansion and one about geographical expansion -- which have not been adequately addressed in a cross-national fashion. First, given the modest and idiosyncratic nature of their grievances along with the lack of any explicit connection between their grievances and regime change, how did the Huk’s membership base grow so large? Second, how did the conflict spread beyond the highly localized parameters of the grievances that ignited it? Kerkvliet’s interviews with the villagers in Central Luzon consistently produced the same explanation for participation in the Huk rebellion: “Most peasants who joined or supported the HMB wanted to protect themselves and their families against repression and abuses...” (1977:207). If we accept Kerkvliet’s assertion that victimization from state repressive
violence was the primary cause of the Huk insurgency, then it is reasonable to expect that if the state diminished its violence, the Huks would demobilize.

This is precisely what we see towards the end of the Huk rebellion. The state experienced a large turnover in its leadership, state violence decreased, and support for the Huks began to diminish. As a wife of a former Huk rebel told Kerkvliet in 1970, “Once the landlords and government showed they would stop abusing us, we [in the rebellion] were ready to put aside our guns too…” (1977:207). The primary reason for the waning and then ending of the Huk rebellion was Ramon Magsaysay, secretary of defense from 1950-1953. Magsaysay stopped soldier abuses, did away with the civilian guards which were the most repressive units and created an amnesty program for Huk rebels. Once Huk rebels had the opportunity to stop fighting without fear of state violence, Huk guerrillas began to defect, seek amnesty, and return to civilian life. Once the government showed a credible commitment to stop its abuses and offer a way out and Huk soldiers saw that those seeking amnesty were not being killed or jailed (as in previous offers of amnesty from the government) the Huk rebellion dissolved rapidly.

The same two puzzles can be found in the growth of the Shining Path insurgency in Peru. Abimael Guzman’s small group of Marxist-Leninist students coalesced around the university in Ayacucho working with local peasants in the mountains for nearly two decades before they militarized. They “went violent” in 1980 in a very limited capacity of providing a local order that the government in Lima was failing to provide. Sendero’s initial acts of coercive force, “…punishing corrupt local officials or philandering husbands, trying and killing cattle thieves…” provided a basic social order that was greeted with considerable enthusiasm by the local peasant communities. It was not until the military arrived in Ayacucho in 1983 that
Sendero became something more. “The initial two years of random military repression in 1983 and 1984...,” argues Strong (1992:138), “...gave rise to the formal birth of Abimael Guzman’s People’s Guerrilla Army.” Created in March 1983, Sendero’s new guerrilla wing “responded to the dirty war in kind” leading eventually to the “…concerted militarization of the party on all levels, bringing the principal, regional, and local forces into play” (1992:139). By the end of the civil war in Peru, Sendero had increased its membership ranks to nearly 5000 active troops and insurgency activity encompassed roughly 75 percent of the state’s territory.

Comparatively speaking, insurgency growth, both in membership and geographical range, is ubiquitous, yet its causal mechanisms are largely untheorized and untested. Drawing from several cross-national, large-N analyses on participatory growth and the spatial expansion of insurgencies, along with illustrative case evidence from several civil conflicts, this project begins to fill this gap by showing how state repression creates contagion effects that allow insurgency organizations to expand in ways that would otherwise not have been possible. The repressiveness of the state, particularly, in the years preceding the civil war, is far more important than previously recognized. Repression was found to be the single most important causal factor in whether a low-intensity conflict environment generated a future civil war, as far in advance as 7 years. This strongly suggests that the foundations of sustainable insurgencies, the town-to-town social networks that become the political cadre in support of the military wing, take many years to build. One of the more important empirical relationships demonstrated among the three studies is that, while some insurgencies facing harsh repression will lack the necessary antecedents for growth, insurgencies inside non-repressive or mildly-repressive regimes generally do not grow beyond a few hundred members. Harsh state
repression may not be a sufficient condition for insurgency growth but it is close to being a necessary one.

In addition to being a significant causal factor in the development of civil war, pre-war levels of repression also have large direct effects on the size of insurgency organizations once a civil war begins. Indeed, the strongest explanatory variable in predicting the membership size of insurgencies is how repressive the government was five years before the conflict even began. Harshly repressive states that experience a civil war five years in the future will face insurgencies roughly 18,000 members larger than insurgencies that had less repressive incubation periods. This finding points strongly to the importance of the socio-political support system in sustaining an insurgency and suggests that much of it is built before the civil war begins. Moreover, state repressive violence appears to be a necessary ingredient in building such a support base. The number of troops a rebel organization can call upon during a civil war depends on the environment the organization was developing in - five, six, even 7 years - before the conflict began.

3. The Importance of Focusing on Selection Processes and Opportunity in Understanding Insurgency Escalation

Introducing the concept of an insurgency opportunity spell uncovered several important and interesting findings. In Part I, I created a dataset of insurgency opportunity spells, where the state is facing one or more violent oppositional groups. Using the lowest possible threshold for inclusion to ensure that every state experiencing oppositional group violence would be included (i.e., a single attack by a single oppositional group on a government target), I generated a sample of states facing real opportunities for insurgency escalation. While some
regions may receive disproportionate attention by coders operating in the United States (e.g., Europe, Central America), while other regions will be less noticeable (e.g., Africa), it is unlikely, however, that every single attack in a country, whatever the region, would be missed over a 19 year period. For this reason my threshold for inclusion in the risk set for insurgency escalation was a single attack. After examining state behavior during these spells, I predicted which opportunity spells were more likely to generate a civil war in the future. I found that the conditions that predict which states will experience oppositional group violence are very different than those which predict which insurgency will grow into a civil war.

In chapter 1 I found that the majority of states around the world faced some degree of violent opposition between 1981 and 1999. Using the lowest possible requirements for having an opportunity spell, I found that weak states did not experience more oppositional violence than other states. In fact, they experienced less. The global distribution of opportunity spells in no way displayed a pattern consistent with a weak state syndrome. Logically, the probability of civil war developing should depend to a large extent on the frequency of opportunity. Stated differently, one should expect the most actualities of an event where the most possibilities exist for the event. If this is a reasonable conjecture, which I believe it is, weak states should experience fewer civil wars than other states. Instead, almost every civil war that developed was in a weak state. I found that what sets weak states apart from the majority of states around the world was their behavioral response to oppositional violence.

While Davenport (2009), and others, have argued that most states respond to internal threats with repression, I found that the overwhelming majority of states facing oppositional group violence maintained respectable human rights conditions. Having examined the
harshness of state repression outside and inside of civil war while controlling for insurgency severity with either insurgency attacks, battle deaths, or both, I found weak support for the proposition that states respond to quantitative indicators of severity. In chapter 2, I found a weak relationship between state repressive behavior outside of civil war and the number of oppositional group attacks suffered by the state. In chapter 4, the findings of a regression analysis of state repressive behavior inside of civil war also did not reveal a strong relationship between the number of insurgency attacks and levels of state repression. In general, the findings suggest that civil wars are associated with greater levels of repression, not because the threats inherent in civil war generate harsh repression, but rather because harsh repression generates civil war.

4. The Differences in the Causes of State Repression Outside and Inside of Civil War Contexts

This project examined the level of state repression of physical integrity rights as the dependent variable among two samples of states: one sample outside of civil war and a second sample made up of only civil war years. I found that the determinants of state repression differed considerably between nations outside of civil war and those inside civil war. In chapter 2, I examined the state’s behavioral response selecting on 116 periods of oppositional group violence outside of civil war and found that attributes of state capacity carried the most explanatory power. For states outside of civil war, the decision to repress appears to be rooted more in political and economic development than in any other factors. Among a group of states experiencing oppositional group violence, but not civil war, economically underdeveloped, mountainous, non-democratic states, with ethnically marginalized populations had repression scores nearly five points higher on the nine point CIRI physical integrity index than dissimilar
states. Proxies for the level of social and military control within the state, such as the size of the national military, the number of insurgency attacks, or the level of ethnic fractionalization, were not important predictors of how state’s would respond. We might say that the correlates of state repressive behavior, outside of civil war contexts, appear to be more introspective rather than based on external events, interactions, or ongoing conditions.

As for states fighting ongoing civil wars, the evidence suggests that repressive behavior is not just a behavior carried forward from previous years spent outside of civil war. Rather than a result of bureaucratic inertia, state repression inside civil war should be seen as a more fluid response to changing territorial dynamics. States with larger militaries, rebel areas closer to the capital, larger conflict zones, a larger number of cities experiencing insurgency attacks, and ethically fragmented populations, had repression scores over four points higher than other states. In sum, the initial decision to repress, outside of civil war, is rooted in the lack of democratic accountability and a lack of alternative options stemming from regime weakness, but once a civil war develops, state repressive violence begins a process of bi-directional feedback loops where repression leads to the spatial expansion of the insurgency which in turn expands and decentralizes state repression throughout the state.

5. Bi-Directionality and Feedback Loops in Repression and Insurgency Growth

This project has sought to follow the interplay of state behavior and insurgency growth beginning with the very first attack upon a government target in a country through civil war to the date of conflict termination. The motivation for this project was the desire to study civil war as a process rather than an event or even a series of events. Instead I have tried to follow interrelated and endogenous relationships over an extended period of time. I have put forth
evidence that the variation among states in the harshness of their responses to insurgency depends, to a large extent, on the territorial dynamics of the war, especially the overall size of the conflict. Examining the causes of variation in the harshness of repression employed by states across 89 civil wars, I find that the level of territorial contestation carries more explanatory power than politically based explanations, such as a lack of democratic accountability, state capacity or even the severity of the conflict. Larger conflicts tend to be associated with more state repression.

The question, one might argue, is whether states respond to insurgency growth with greater levels of repression, or whether greater levels of repression lead to insurgency growth. I argue that state repression starts the process of insurgency growth and that insurgency growth cannot take place without it, although the overall ongoing relationship, between state repression and insurgency growth, is best understood as one large bi-directional feedback loop which is fed by many smaller feedback loops. Attempting to flee repressive areas, insurgency operations are dispersed, which in turn, expands state repressive violence into areas of the nation where it did not exist before; in their efforts to curb the recent expansion of insurgency activity, repressive states further catalyze the territorial diffusion of insurgency. In chapter 5, I argued that state repression causes forced migration from highly repressive areas of the state to areas of lower repression. This leads to the dispersal of fresh rebel recruits to new areas of the state, which in turn leads to the dispersal of state repression. Authorities perceiving this migration as an attempt to spread the insurgency to their city or town, respond with repressive violence. Thus, forced migration transplants the defensive mobilization dynamic to other areas of the state.
I test this theory by showing that higher levels of state repression are significantly associated with more dispersed insurgency activity. While military leaders might be inclined to interpret this dispersal effect as having put the insurgents “on the run,” it is important to note that overall insurgency activity does not decrease in the presence of harsh repressive violence. Rather, the more repressive the state is at year (t), the more insurgency attacks we can expect to see the next year (t+1). In other words, harsh repression does not decrease insurgency activity but merely ships it to other areas of the state. Since the purpose or underlying cause of forced migration is to flee state violence, migrants will presumably migrate to areas of the state where repression is lower. The fact that an area has low levels of repression strongly suggests that this area is either (a) isolated from state control, (b) not a current area of insurgency activity, or (c) the population of the area is not considered, by the state, to be sympathetic to the insurgency. Thus, forced migration transplants insurgency activity to areas of the state where it would have been far less likely to expand to naturally.

Harsh repressive violence by states is associated with more insurgency attacks, in more cities but, interestingly, fewer attacks in any one city. In essence, the findings for the density of insurgency activity in chapter 7 show the specific causal mechanism of insurgency expansion at work. Insurgencies expand by shifting active operating groups or cells to other cities. The number of attacks either remains the same or slightly increases while the number of attacks in any one place diminishes. From a tactical standpoint, I cannot think of any better way for a group of insurgents to continue insurgency activity while minimizing the risk from state repressive violence other than spreading the government’s coercive forces, a finite resource, out over an increasingly larger geographical space. In sum, repressive violence is associated
with more insurgency attacks, in more cities throughout the state, but fewer attacks in any one location. Thus, repression and contagion spreads the insurgency over a greater geographical area resulting in a greater amount of contested territory within the state producing greater amounts of fragmented social and territorial control throughout the state. This in turn produces the next feedback loop, which ties my findings on the mechanism of insurgency expansion to Kalyvas’ work on differential rates of violence within and across civil wars.

Repression causes the geographical expansion of insurgencies, which, in turn, generates more and more targets for the state to repress via denunciations from informants in areas of overlapping territorial contestation (Kalyvas 2006). Kalyvas (2006:12-13) offers what might be called a bureaucratic or organizational model of state violence, where the severity of state repression depends on the number of targets the military is given from informants in areas of fragmented social control (where supporters of both the government and the rebels cohabitate). In chapter 5 I find larger conflicts associated with more repression than smaller conflicts. While it could be argued that larger conflicts represent a greater threat to the state and therefore generate a greater repressive response, there is little evidence to support such a claim. Conflict size is significant even when controlling for the number of insurgency attacks and the number of battle deaths per year. More importantly, the number of insurgency attacks, probably the most visual indicator of the scope of rebel operations within a state, is not statistically significant. While the number of insurgency attacks is not a significant predictor of how repressive a state is during civil war, the number of cities that house insurgency attacks is statistically significant. This suggests further that larger conflicts are associated with more repression not because they are more severe but because they are more dispersed, a condition
that increases the amount of fragmented territorial control within the state yielding a larger number of denunciations.

More denunciations mean more people are detained. Using a mixture of qualitative and quantitative evidence from studies involving detainment, interrogation and torture in chapter 5 I argue that as the number of people detained by a state increases, the percentage of detainees that are not insurgents also increases. Since states torture to overcome the natural incentives of detainees to withhold information, and interrogators often have no way to tell the difference between a non-insurgent who has no valuable intelligence to give and an actual insurgent who has a strong incentive to withhold his intelligence, torture is likely to be inflicted, to the extent it is sanctioned by state authorities, on the innocent and guilty alike.

Widespread torture produces two additional feedback mechanisms: First, torture yields false confessions and false denunciations that exponentially increase the state’s list of targets as each innocent detainee provides more names to interrogators. At facilities where the number of detainees is extraordinary, such as the S21 death camp in Phnom Penh, this feedback loop has been documented. Second, torture yields false confessions and false denunciations that eventually lead to many detainees being released from confinement for a lack of evidence. The military, perceiving this as a judiciary that is undermining their work takes sentencing into their own hands, increasing the frequency of killings and disappearances. Berger (1992) witnessed this feedback loop unfold in the Peruvian villages he observed.

6. Solving Anomalies and Advancing Civil War Theory

In the introduction to this project, I described three major anomalies in the civil war literature, arguing that a theoretical approach and broad base of evidence would be put forth
that would resolve these puzzles in a way that was consistent with the major findings in the civil war literature. Now we are in a position to evaluate that claim. The first anomaly involved the role of ethnic fragmentation in the civil war escalation process. Hegre and Sambanis (2006) found seven different operationalizations of ethnic fragmentation significant in predicting civil conflict when the dependent variable included minor conflicts. When the same model was used to predict only major civil wars (more than 1000 battle deaths) none of the seven were significant. This pattern implied that many civil wars, for some unknown reason, grow out of minor conflicts not located in ethnically fragmented states, despite the fact that the best predictor of minor conflicts was ethnic fragmentation. Using Most and Starr’s (1993) opportunity and willingness framework, chapters 1 and 2 show that the answer to this anomaly involves selection effects and the response of the state.

In chapter 2, the analysis began with the quest to define a risk set for insurgency escalation by identifying which states, outside of civil war contexts, experienced violent attacks by known oppositional groups. The most consistent group of variables from that analysis (The Risk Set) were those involving ethnic demographics or those tapping into potential ethnic conflict. I found that ethnically polarized societies with higher degrees of ethnic marginalization experienced the most severe episodes of oppositional group violence, with 23 more attacks per year than other states. The risk of attack was not greater in the poorest societies under autocratic rule but in wealthier, more democratic, societies. This is persuasive evidence that the “ethnic competition/modernization” model of political violence (Jalali and Lipset 1992) might be the most widely applicable model for predicting the severity of oppositional group violence globally. Narrowing the sample to only those countries
experiencing a spell of oppositional group violence, I attempted to predict which periods would produce a civil war; ethnic fragmentation was not significant. Like Fearon and Laitin (2003) and Hegre and Sambanis (2006), I found ethnic fragmentation to be unimportant in determining which insurgencies grow into major civil wars.

The second stage analysis (The States Response) in chapter 2 provides the missing link to understanding this puzzle. Ethnic fragmentation is associated with more minor insurgencies yet lower instances of civil war because ethnic fragmentation is not an important predictor of which states will respond to oppositional violence with repression. Most minor insurgencies may be rooted in ethno-political violence, but fewer of these make it into the treatment group (repressive environments) where they have a chance to grow into civil war. This is further evidence that the underlying causes of rebellion are less important to the future growth of that rebellion than the response of the state.

A second anomaly involves mountainous terrain and insurgency growth. Mountainous terrain appears to be significant in predicting major civil wars but not significant when minor conflicts are included (Hegre and Sambanis 2006). In addition, it has been shown that conflict zones are usually less mountainous than the countries in which they occur (Buhaug and Lujala 2005) and higher levels of mountainous terrain in a certain geographic area makes the presence of a civil war there less likely (Buhaug and Rod 2006). Many of the findings in this project add to the significance of this anomaly. In chapter 6, I compared the membership sizes of rebel groups in every civil war from 1981-2005 and find no advantage to be had by operating in a state with less or more mountainous terrain. In chapter 7, I find that geographical expansion has nothing to do with geography; mountainous terrain does not seem to contribute in any
meaningful way to the production of insurgency. Yet, many studies find civil war onset correlated with mountainous terrain.

The second analysis in chapter 2 provides the explanation for these anomalous findings. States with larger amounts of mountainous terrain are significantly more likely to respond to oppositional group violence with repression. Mountainous terrain is unimportant in predicting the onset of minor conflicts because it does not play a substantial role in insurgency growth at any stage of escalation, even within ongoing civil wars. Rebel groups are no larger, and their operating ranges are no larger where there is an abundance of mountainous terrain. Mountainous terrain is a significant predictor of major civil wars because it is highly correlated (p-value = 0.000) with state repressive violence which is the most important contributor to insurgency growth. This is why Buhaug and Lujala (2005) find that the amount of mountainous terrain in a country does influence conflict dynamics even when there is no mountainous terrain near the conflict itself. The relationship between mountainous terrain and conflict dynamics is not spurious; mountainous terrain impacts the behavior of the state (most likely by contributing to poverty and social fragmentation) which in turn impacts conflict dynamics.

The last anomaly involves poverty and insurgency growth. This is arguably the most important anomaly, and the most controversial, since the two dominant theoretical approaches in the civil war onset literature theorize insurgency as primarily the output of poverty induced processes. In chapter 2, in the first stage analysis (Defining the Risk Set), I find that poverty is unrelated to the production of insurgency opportunities; poorer states experience less oppositional group violence than wealthier states. Regardless of poverty’s influence on opportunity or even on the risk of civil war onset, one of the strictest tests of the theory that
poverty provides intrinsic benefits to the production of insurgency would involve examining variation in the sizes of insurgencies as a function of national income. If poverty allows nascent insurgencies to survive and prosper into major civil wars, that trend should not abruptly stop at civil war onset. By all means, we should see the largest insurgencies inside the poorest states. In chapter 6, we find that this is not the case; poverty is not strongly predictive of insurgency size.

The non-finding regarding poverty and insurgency growth is not the result of a lack of variation in levels of economic development among civil war states. Although states housing civil wars will be a sample of disproportionately poor states (because poverty selects for repression which allows insurgency growth), there is sufficient variation in incomes. In the Sambanis (2004) civil war dataset, 59 percent of all onsets (1945-1999) occurred in country-years where income per capita ranged from 82 to 999 dollars per year (using the Penn World Table 6.2; Heston, Summers, and Aten 2006). Thirty-seven percent of civil war onsets took place inside country-years where income per capita ranged from 1000 dollars a year to 4,999 dollars a year. Around 4 percent of civil war onsets took place in country-years where income per capita was between 5000 and 19,999 dollars per year. Insurgencies where the average person earned less than 2 dollars per day were no larger or no smaller than in countries where average incomes were 100 percent, 200 percent or even 500 percent higher than this amount. In chapter 7, I examined the magnitude of insurgency activity within the state and the density and dispersal rates of insurgency activity. Lower incomes were associated with fewer insurgency attacks and the relationship was highly significant. Lower incomes were also significantly associated with a lower density and higher dispersal rate of insurgency activity within the state.
In other words, poverty means insurgency activity will be confined to a smaller number of cities and more attacks in one dominant city.

If poverty is unrelated to insurgency growth, why is it the most robust variable in the civil war onset literature? The results in chapter 2 show that poverty, along with several other important variables in the onset literature (population, mountainous terrain, anocracy), are paramount in terms of their impact on how the state responds to oppositional group violence outside of civil war contexts. In the last stage analysis in chapter 2 (The Consequences), we see that after controlling for state repressive behavior, GDP per capita does not reach statistical significance when predicting the onset of civil war. The totality of evidence from this study, conducted at the cross-national level and the sub-national level of analyses, strongly suggests that the significance and robustness of poverty in the civil war literature results from the fact that poverty has consistently been predicting, not civil war, but highly repressive states, which because of their human rights abuses are the only regimes to produce a sociopolitical support system large enough to sustain a major civil war.


Alan Heston, Robert Summers and Bettina Aten. 2006. Penn World Table Version 6.2, Center for International Comparisons of Production, Income and Prices at the University of Pennsylvania.


