

Military-Strategic Updating in Civil Conflict

By

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*To Tod and Copper*

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## ABSTRACT

### Military-Strategic Updating in Civil Conflict

Does military strategy matter in deciding success or failure during civil war? The qualitative literature on civil war and military history would suggest that it does, but quantitative analyses of this question are limited in both theoretical and empirical scope. In this dissertation, I set out a theory of military-strategic updating, arguing that it is not just the choice of strategy that matters, but how actors adapt their strategy over the course of the war. Conflict actors have to pick effective strategies to achieve their military goals, but they also have to be able to update their military strategy over time. Actors that are unable to recognize the need for a new military strategy, or that are unable to quickly and effectively adapt to a changing battlefield environment, are likely to see less success in the short run, and are more likely to see a bad outcome to the conflict as a whole.

I test the short-run implications of this theory on novel event data from the Second Intifada in Israel and the Palestinian territories, and on the Troubles in Northern Ireland. I find little systematic evidence suggesting that insurgents and states engage in incremental, short-term adaptation to each other's behavior. However, I find some evidence that military-strategic change may take the form of abrupt shifts in behavior, suggesting that this type of updating may be more akin to a threshold effect, in which sufficient information gathered over time eventually translates into a significant and long-lasting shift in actor behavior. I test the long-run implications of this theory on a cross-national set of conflicts on the African continent. I find more support for my theory in this case, suggesting that actors capable of rapid response to shifts in their opponents' behavior are likely to survive longer, even though they may not be more likely to win outright.

# Chapter 1

## Military-strategic updating in conflict

Following Angolan independence and the disintegration of a treaty between the major parties involved in the anti-colonial movement, a major conflict broke out in 1975 pitting the new government against one of the largest anti-colonial militant groups, the National Union for the Total Independence of Angola (UNITA). The war lasted over two decades, killing tens of thousands as UNITA and government forces traded control of large swathes of territory in central Angola. This level of intense conflict was made possible by UNITA receiving considerable aid from South Africa and the United States, who provided resources, funding, and (in the case of South Africa) direct military assistance to the insurgents. The Angolan government was supported by the Soviet Union with arms and training, and for a little while, by direct military assistance from Cuban forces Weigert [2011]. By the 1980s, both sides fielded large armed forces and significant amounts of military technology: this conflict actually saw some of the largest tank battles outside of World War II as both sides fought over strategic locations and resources.

However, as the conflict dragged on into the 1990s, political shifts caused South Africa and the US to withdraw their support. Without inflows of weapons and funding from its overseas supporters, UNITA found itself isolated and weakened. During this period, state forces turned the tide against UNITA insurgents, retaking significant swathes of territory previously controlled by the rebels. In 1999, after losing major strongholds to

government offensives in southeastern Angola, UNITA made a significant change to its military strategy. Instead of fighting to control the territorial and strategic gains it had made previously, leaders announced over radio, the group would return to the jungle in southern Angola, and go back to its roots as a guerrilla movement. Locally, tactics changed to focus on hit-and-run operations against isolated villages and government patrols; at the theater level, the zone of UNITA operations expanded dramatically to cover thirteen of Angola's eighteen provinces [Weigert, 2011, 144-45].

This led to a temporary upswing in UNITA's fortunes, as the group was able to avoid major confrontations with major government forces and carry out a number of successful operations across the country. However, the government soon reorganized its own military campaign to counter this guerrilla strategy, utilizing a combination of heavy bombing raids and ground-level denial operations to corral UNITA and its leadership into a smaller and smaller region of the state. By 2002, a combination of casualties, lack of equipment, and desertion and loss of morale had severely depleted UNITA's fighting strength, and its leadership was constantly on the run from encroaching government forces. In February Jonas Savimbi, UNITA's longtime leader, was killed, and UNITA surrendered shortly thereafter.

By shifting to a non-conventional, highly mobile strategy that de-emphasized large-scale clashes with military forces and instead focused on strikes at isolated and undefended locations, UNITA was able to temporarily revitalize its wartime efforts and survive a few years longer as a fighting force. However, competent adaptation to this change by Angolan state forces, combined with deteriorating military capabilities, were enough to ensure that UNITA would not see a military or political victory. Even though it did not achieve its primary goals, however, UNITA's tenacity as a fighting force helped it maintain military and political relevance during this period and even after the official end of hostilities: many of the group's key political issues were adopted by the government during the peace process, significant numbers of former fighters and leaders were folded into the state's armed forces, and the organization itself became a major political party in the Angolan parliament.

## 1.1 Puzzle

Why do weak actors win wars? Why do many asymmetric conflicts last so long? Theories of conflict generally try to answer these questions by looking at *ex ante* observable factors: the balance of military capability between combatants, the salience of the issue at stake, the costs each actor is likely willing to pay for victory, and so on. However, these theories still struggle to explain the durations and outcomes of non-conventional conflicts. One potential explanation for this discrepancy is that current approaches ignore the dynamics of military strategy and adaptation during these conflicts. During conflict, states and insurgents alike struggle to understand their military and political environment in order to adapt to changing conflict dynamics over time. In cases in which insurgents rapidly and effectively update their military-strategic approach relative to that of the state, these actors can fight more effectively than their material capability would suggest. By ‘punching above their weight class’ on the battlefield, insurgents can last longer in a fight and achieve better outcomes from fighting than current theories would predict.

In this dissertation, I construct a model of intra-conflict military-strategic updating. This adaptation model draws on existing models of pre-war and intra-war bargaining, extending these ideas to explain conflict outcomes as a function of relative insurgent capability to quickly and effectively adapt their military strategies and fight more effectively against stronger but poorly-functioning state forces. The past half-century has produced significant scholarship that supports a static model of conflict behavior, particularly in the case of state military forces, but there is a growing body of evidence suggesting that an adaptation-based model of conflict updating fits the historical record better in many cases, in the case of states as well as insurgent groups. This analysis examines military-strategic dynamics in a framework that (1) includes both insurgent and state forces, and (2) measures actor behavior and choices in such a way that we can compare patterns of behavior for state and insurgent actors across different conflicts.

In this chapter, I approach the issue of military-strategic adaptation in three steps. First, I review the existing literature on military strategy, and show that the way in which a group fights impacts conflict outcomes, apart from *ex ante* factors such as the issue at

stake or the relative capability of the actors. Second, I review the literature on military decision-making, comparing two strains of literature with very different views on how actors respond to changes in their environment during conflict. Third, I create a theoretical model of conflict updating that treats conflict actors as rational adaptors, allowing me to explicitly test whether this adaptation model can help explain both short-term military dynamics and long-term conflict outcomes.

## 1.2 Literature review

### 1.2.1 Bargaining theories of conflict

Recent theories of armed conflict conceptualize conflict as a process of bargaining and costly signaling between two or more armed actors over time [Wagner, 2000, Powell, 2004]. These models depart significantly from a large body of previous theory, which envisions conflict as a one-off interaction similar to a costly lottery [Fearon, 1995, Powell, 1999], in which both actors fight and one wins with some probability generally linked to the balance of capabilities between the two actors. In these one-off models, uncertainty about relative capabilities is resolved by conflict, which eliminates uncertainty by giving the victory to the stronger party with some higher probability. These models were created in an attempt to explain the existence of inefficient conflict, envisioning conflict itself as a breakdown in the bargaining process: conflict occurs when both sides are unable to agree over the distribution of some stake, and as long as this stake outweighs the expected costs of fighting, actors will pay this cost to achieve their desired outcome [Fearon, 1995]. However, these models struggle to explain the dynamics of actor behavior during conflict, as they treat the entire process of fighting as a probabilistic, one-time event [Wagner, 2000].

Repeated-interaction models, by contrast, treat conflict as an extension of the bargaining process [Smith, 1998, Filson and Werner, 2002]. If bargaining at the negotiating table fails, actors will engage in organized violence to uncover hidden information about each other's capabilities and willingness to fight. The core idea of these models is that repeated bouts of fighting reveal information. This information can pertain to the costs of fighting

[Powell, 2004], the balance of capabilities between fighters [Smith and Stam, 2003], or the ability of each side to exact future costs on each other and how these costs weigh against the willingness to continue fighting Slantchev [2003]. After each round of fighting, both actors update their beliefs about the world, trying to identify their ‘true’ chances of victory and how this affects the anticipated outcome of future fighting. As both actors observe the outcome of repeated battles, their beliefs about the world change, and a settlement becomes possible. Victory is still a function of relative capability: these models tend to assume that the stronger group will tend to win any given engagement [Filson and Werner, 2007], and so will win the overall conflict.

Smith and Stam [2003] conceptualize the conflict-bargaining process as an ongoing fight over a number of key objectives, which they label ‘forts’. Two actors fight over these forts, and as a result of conflict at every time step, there is a chance for any given fort to change hands from one group to the other. The division of forts between the two actors can serve as a signal for which group is winning by achieving its short-term battlefield objectives. As one group seizes territory, achieves key political goals, and deals damage to its opponent’s fighting capability, both actors gain credible information about the true balance of power. Moreover, an actor’s control of forts can also affect the chances of winning future battles: victory can become easier and more likely the more of these key objectives a given group controls. This means that (1) stronger actors are still more likely to achieve better conflict outcomes, as they are more likely to win individual battles, and (2) local victory has a snowballing effect: as an actor achieves more battlefield victories, they increase their chances of future victories as well [Filson and Werner, 2007].

However, these elegant models often have trouble explaining conflict dynamics and outcomes, particularly in messy non-conventional wars. The information-revealing mechanisms elucidated by previous theorists [Powell, 2004, Lewis and Schultz, 2003] do not seem to track well with some observed patterns in civil wars. For example: how much information was revealed over the decades-long Sudanese Civil War? Why did it take so long to reveal enough information for the conflict to grind to a halt? On a more general note, for that matter, is there even an ex-ante available metric of what constitutes ‘enough’

information for two actors to come to an agreement? These models struggle with the significant — and growing — disparity between actor strength and conflict victory. Not only are strong actors not always victorious in non-conventional wars; they are actually more likely to lose, according to some metrics [Arreguin-Toft, 2005, Sullivan, 2007, Wang and Ray, 1994]. Weaker actors are, for reasons that these models cannot explain, highly effective at fighting substate conflicts. While these theoretic models are elegant and have some evidence of their empirical tractability in large-scale interstate wars, there are many cases of civil conflict in which they have real trouble predicting duration and outcomes.

Some theorists have attempted to deal with this by theorizing additional mechanisms to explain conflict duration and outcome. Some of these mechanisms are game-theoretic: for example, potential commitment problems can make credible bargaining difficult, even when both parties have accurate information about the balance of power [Leventoğlu and Slantchev, 2007]. When actors cannot trust that a peace agreement will last, or fear future changes in the balance of power that could lead to defeat in the next conflict, they are likely to continue fighting in the present rather than make peace and hope for the best. Other mechanisms substantive: a weak group that is fighting for a more salient stake, such as territorial sovereignty, is more committed to a conflict than a stronger group that is seeking a lower-salience stake such as local policy change. In these cases, a weaker group can outlast its opponent, as it only needs to fight enough to exhaust the stronger group's public support for the war [Sullivan, 2008]. Another large body of work, which serves as a jumping-off point for this theory, looks at the way actors fight in conflicts — their military strategy.

## Military strategy

Bargaining models of conflict, though helpful in organizing our expectations about how wars begin and end, have little to say about how non-material factors like military strategy affect the duration and outcome of conflict. These game-theoretic models of conflict bargaining can only include so much information from the real world before they become empirically intractable. This is not necessarily a problem, but is simply a feature of this type of theoretically rigorous modeling technique. One issue with these models that stems

from their simplicity is that the actual updating processes they rely on are very simplistic: actors fight for limited rounds [Filson and Werner, 2002], battlefield victory is random or probabilistic and unchanging over time [Smith and Stam, 2003], and learning is a monotonic process in which actors are continually growing closer to convergence, meaning that short-term setbacks are assumed to have no long-term effects [Filson and Werner, 2007]. These simplifications are necessary for theoretical tractability, but a substantive (rather than game-theoretic) model of conflict processes can create a more complicated but perhaps more empirically accurate picture of conflict by relaxing such assumptions.

However, moving away from the simplicity of game-theoretic measures should not imply relaxing the rigor of theoretical mechanisms. Relaxing such assumptions but relying on measures like ‘issue salience’ [Sullivan, 2007] can be difficult. While intuitive measures of salience exist, the actual commitment an actor has to its stated goals can be very difficult to reliably identify before or even during a conflict. Actors engage in a great deal of cheap talk both before and during conflict [Fearon, 1995, Kydd, 2003], and this type of information can include both political demands and the ideology and beliefs of actors themselves. Consider the case of the Lord’s Resistance Army, whose stated goals include the establishment of a religious but democratic government and representation of the Acholi ethnic group [Van Acker, 2004] but whose actions largely involve looting, predation and violence against the Acholi themselves; their actions, to outside observers, belie their political claims. Relying on this type of measure can result in theoretic models of conflict processes that are primarily useful *ex post*, rather than trying to make ‘live-action’ predictions about conflict. A final issue with relying on factors like salience is that, even when we try to construct credible *ex ante* measures of salience based on factors like territory and ethnonational identity [Sullivan, 2007], salience alone is not an adequate explanation for conflict outcomes. Issue salience can be thought of as a necessary, but not sufficient reason for battlefield success; even highly committed actors cannot succeed if they have no arms, manpower, or resources to continue a campaign of organized violence.

Previous work on conflict-as-bargaining has served as a useful theoretical scaffolding to bring in an important explanatory factor: military strategy. For the purposes of this

analysis, I define strategy in war as a set of rules, whether formal or informal, that govern how they deploy military resources during conflict. These rules govern how actors use the limited manpower, finances, and technology at their disposal. It is important to note that under this definition, strategy is linked to means, not ends: these are rules outlining effective resource management in pursuit of some stated goal. An efficient ruleset is one that maximizes an actor's military efficacy against the opponent's military resources and strategy. This means that military strategy is an intervening factor between actor capability and conflict outcomes; good strategies maximize the efficacy of an actor's existing military capabilities.

Most work on military-strategic behavior as an input predicting some kind of conflict output is somewhat specific, estimating the efficacy (variously defined) of specific types of violent behavior. Most commonly studied are controversial strategic choices such as aerial bombing [Pape, 1996, Horowitz and Reiter, 2001] suicide terrorism [Pape, 2003, Hoffman, 2003] or targeting of civilians [Hultman, 2007, Humphreys and Weinstein, 2006, Wood, 2010]. In recent years, new advances in data collection and theoretical innovation have also focused on the spatial characteristics of conflict, analyzing where violence is likely to break out and the patterns of spatial diffusion that violence can take once conflict begins [c.f. Schutte and Weidmann, 2011]. However, A military-strategic analysis of conflict necessitates some type of theoretic framework. These analyses provide depth and empirical rigor to answering specific questions, but due to their highly focused nature, they are less useful when trying to construct a more comprehensive theory of military-strategic behavior and adaptation. However, there have been few efforts to bring these elements together in the context of a broader theory of military-strategic behavior. How an actor engages in violence, where they choose to spend their resources on violent acts, and who they target for violence are all different sides of the same decision-making process — these are all facets of military strategy, and as such they should be treated as parts of a whole rather than disparate behavioral patterns.

The classic military-strategic archetype for civil conflict is the superiority of a guerrilla strategy when fighting a numerically superior enemy, which can allow weaker forces to

rapidly engage and defeat small or unprepared sections of the stronger force while evading detection and engagement themselves. However, guerrilla strategies are not unique to civil war: the unconventional strategy adopted by Spanish resistance fighters against Napoleon's forces in the early years of the 19th century inspired Clausewitz to write somewhat extensively about small wars and guerrilla operations [Clausewitz, 1976, Heuser, 2010]. Additionally, civil wars do not necessarily involve traditional guerrilla warfare as described by Clausewitz and later revolutionary theorists such as Mao [1961], Guevara [2002], and Marighella [1971].

Since the end of the Cold War, insurgencies have been characterized by greater variation in the capability ratio between rebels and state forces, as states lack the backing of superpowers interested in local stability and insurgent forces have easier access to black-market arms and resources [Kalyvas and Balcells, 2010]. Finally, the very concept of 'guerrilla warfare' as a decontextualized ideal is a very difficult concept to tie down: the tactics, technologies, and force disparities usually associated with classic guerrilla wars are present to some extent in virtually every war, making it hard to specifically define these terms to begin with [Smith, 2003]. As such, the conflation of insurgency and 'guerrilla' strategy is increasingly less accurate in the modern age.

A few works have attempted to generate general theories of military-strategic behavior. Stam lays out a broad typology describes military strategies in interstate warfare, classifying military behavior as primarily 'maneuver, attrition, and punishment' [Stam, 1998]. Maneuver warfare relies on high-mobility, high-technology strike groups which can maneuver around stationary or slower-moving enemy forces to strike at high-value targets behind enemy lines. This type of warfare relies heavily on technology such as concentrations of armored forces and long-range attack aircraft, as well as highly developed command and control capabilities to effectively coordinate complex assault plans. Attrition warfare involves the mobilization of large military forces, often relatively low-tech military technology, and direct engagement with enemy military concentrations. This type of strategy attempts to grind down and destroy enemy forces directly, forcing a victory through sheer casualty counts. It is important to note that this definition of 'attrition' is

unique to Stam's typology: in other substantive treatments of military strategy, attrition can also refer to strategies, both conventional and non-conventional, that try to wear down the political will of the opponent over long periods of time [c.f. Bartholomees Jr, 2010]. Punishment strategies are most commonly seen in asymmetric warfare, making them the most easily applicable to sub-state violence. This type of strategy involves indirect damage to the morale or command structure of enemy forces, and emphasizes minimization of friendly casualties. Punishment can involve guerrilla warfare by weaker actors, as well as indiscriminately violent military operations by state forces seeking to destroy evasive insurgent fighters and deter civilian supporters [Stam, 1998].

Overall, this typology does not lend itself well to describing the military behavior of non-state actors — nearly all insurgent campaigns fall, at one time or another, into the ‘punishment’ category. This is largely because Stam’s typology correlates very closely with material capabilities, both manpower and command/control ability. Insurgent actors generally do not have the manpower to engage in attrition warfare, and lack the advanced military technology to engage in maneuver warfare. As such, this type of classification system is of limited use for describing and explaining variation in substate warfare.

Some work has attempted to create a typology of strategy for intra-state conflict, describing insurgency and counter-insurgency strategies in the same terms. This typology classifies strategies in a two-dimensional space as ‘direct versus indirect’ and ‘strong versus weak’ [Arreguin-Toft, 2005]. Direct and indirect strategies, in this typology, look very different when carried out by different types of actors: for example, an indirect strategy by a weak actor is described as guerrilla warfare, while a strong actor’s indirect strategic approach is labeled ‘barbarism’ and involves the indiscriminate and purposeful targeting of civilians in order to eliminate insurgent fighters. Both scholars recognize that strategy makes a difference; some types of military strategy are better-suited for certain types of actors, and work better against certain strategies chosen by an opponent. For example, Stam [1998] points out the repeated success that maneuver warfare has had against attrition strategies — although, given the high level of military technology, C&C capabilities, and training required to field maneuver operations, it is not clear how much

of this superiority is due to resources and military training. Likewise, Arreguin-Toft [2005] argues that weaker actors only have a chance if they attempt indirect forms of warfare: weaker actors seeking to engage a directly-attacking opponent with a direct defense are likely to be quickly defeated. However, an indirect defense when employed against a direct offense strongly resembles the traditional form of guerrilla warfare against a stronger but slower enemy, and is likely to be more fruitful for the weaker group over the long term.

Within these inter- and intra-national conflict frameworks, military strategy has been used to explain conflict duration and conflict outcome, both between states [Mearsheimer, 1983, Bennett and Stam, 1998] and in civil conflicts [Balcells and Kalyvas, 2014]. Military strategy has also been used as an outcome variable, with studies predicting the adoption of a given military strategy as a function of domestic politics, regime type, and other state-level factors [Reiter and Meek, 1999]. However, the implicit assumption that strategy is static or nearly static over the course of a war — that is, that military strategies can only be changed in peacetime — still holds over all the literature surveyed. This is both a theoretic and empirical limitation. Some of this work explicitly recognizes that strategic adaptation occurs: “Over time, states can adapt to each other’s strategy, and so if a strategic advantage does not lead to victory quickly, it is even less likely to do so as time passes” [Reiter and Stam, 1998, p. 356]. However, empirical analysis of military strategy and its effects almost never includes actual discussion of strategic change or updating. Due to the broad categories with which these works measure strategy, it is difficult to identify real-world variation in the way states and non-state actors prosecute military campaigns.

### **1.2.2 Strategic updating**

Despite a general recognition that military-strategic updating is an important factor in determining the combat strength of a military organization, only a few analyses have attempted to describe and explain this type of learning process. Some examples are the adoption over time, and subsequent effects, of new military technology [Pape, 1996, Horowitz and Reiter, 2001, Horowitz, 2010], or how states engage in high-level military-strategic updating over long periods of both peace and war [Murray, 2009]. While they are important, these analyses have little to say about the process of short-term adaptation

in the face of battlefield information.

Some more recent work has explicitly studied how groups adapt and change their military strategies over time. This work stresses not only the importance of picking ‘good’ strategies in war, but also the importance of successful adaptation of military strategies over time. Actors in conflict respond to each other constantly, as each actor tries to learn what the other is doing and develop a strategy that is maximally effective at countering the opponent’s aims. Johnson [2009] explains intra-conflict adaptation by conceptualizing warfare as a mutually evolutionary process of strategic innovation. Actors must adapt and innovate as they fight wars, or risk being defeated by a more successful strategy adopted by their opponent. In this way, military conflict is analogous to any other competition in the natural world: organisms, organizations, and individuals who can adapt more quickly to their competitive surroundings will survive and flourish, while those who cannot turn into (often literal) evolutionary ‘dead ends’.

Some qualitative case studies have also discussed this type of intra-war strategic adaptation in case study analysis, examining individual military forces and operations. Several recent works have focused on efforts by the United States and Great Britain as they have attempted to effectively combat the non-conventional strategies adopted by insurgents in Iraq and Afghanistan during the last decade. These analyses are particularly interesting in that they analyze the behaviors of large, highly institutionalized military actors — powerful states — engaged in peace-keeping operations in remote areas. In these conflicts, the stakes were relatively low: “[British] defense chiefs and civilian policymakers did not fear outright defeat in Helmand” [Farrell, 2010, p. 18]. Even for state militaries with rigidly defined modes of operation fighting relatively low-intensity, low-stakes conflicts, battlefield adaptation was perceived to be necessary to successfully completing their mission [Farrell, 2010, Hultman, 2012]. Johnson [2009] analyzes the rapid innovation and adaptation displayed by the same Iraqi insurgents, arguing that these weaker actors had high levels of motivation and opportunity for military-strategic change in order to survive.

Outside of the political-scientific literature, the importance of military adaptation during conflict has been a fairly common subject in military analysis and defense studies

[c.f. Nagl, 2002] that stress the importance of military learning and adaptation in the context of counterinsurgency. The general finding from these studies is that changes in military strategy are generally an attempt to fix a problem; an actor whose current strategy is enjoying success has little or no incentive to spend time and resources seeking out a better strategy [Gartner, 1999]. However, these works are limited in their scope and (in some senses) by their methodology. Case study analysis of conflicts or campaigns during conflicts are useful test cases for analyzing behavior and building theory, but these strategic analyses tend to limit themselves solely to individual qualitative analyses.

Although not all works frame these changes in behavior in the same context of military strategy, this strain of research identifies an important aspect of conflict. Groups faced with an unsustainable military situation do not necessarily surrender, but often change the way in which they conduct operations in order to regain an advantage or at least gain some breathing room. It is also clear that changing military strategy is generally done in response to duress, rather than a simple or costless shift in behavior. Changes in military strategy therefore represent an attempt for one actor in a conflict to ‘even the odds’ by giving themselves an advantage in local-level military engagements. What is lacking is a broad theoretic framework that allows scholars to study military-strategic updating between conflicts and over time with the same sets of assumptions and analytic tools.

### **1.2.3 Identifying a gap in the literature**

Given that previous work recognizes (at least offhand) that military-strategic adaptation can take place during a conflict, it is slightly surprising that there have been so few analyses of this phenomenon. I argue that there are two reasons for this lacuna. The first is simply a lack of good data: until very recently, conflict data was aggregated to the conflict or conflict-year level, simply indicating whether a given dyad of actors was ‘in conflict’. This has left little room to study adaptation, since there is little behavioral data to use. The second reason is more important, and has to do with theories of organization and their application to military bodies. While the conflict and peace-scientific scholarship has done little to directly test the presence or absence of intra-conflict updating, there is a significant body of literature originating in business and sociology that focuses on how

difficult it is for organizations — whether social, political, business or military — to adapt their behavior in response to changes in their environment.

When applied to military organizations, this body of literature suggests that adaptation during war is in most cases a non-starter. Scholars in this field analyze military actors (both state and non-state) as organizations with divisions of labor, internal regulations and procedures, and bureaucratic specialization. These organizations have multiple decision-making actors, some of whom may have different sets of information or even different incentives to make decisions. Bureaucratic analyses of military organizations treat short-term adaptation as an extremely rare phenomenon, arguing that there are too many factors impeding any major behavioral shifts for military adaptation to occur in most circumstances.

Effectively carrying out a given strategy on the battlefield requires a high degree of coordination, obedience, and discipline. However, the very factors that allow effective prosecution of war tend to be antithetical to the factors that allow organizations to easily adapt [Murray, 2009]. Feld [1959] takes a dim view of the ability of military organizations to update their behavior, arguing that the bureaucratic structure of professional militaries necessarily divorces those with decision-making power from direct contact with the battlefield. Bureaucracies prefer not to change; they prefer stability in which tasks can be carried out according to standardized protocols and procedures, and tend to strongly resist attempts by outside actors to change these behaviors. Unwanted informational inputs tend to be argued away or simply ignored until they become too serious to ignore further [Allison and Zelikow, 1971]. The inertia of bureaucratic management increases with the levels and complexity of the military bureaucracy, meaning that highly professionalized militaries tend to be very good at doing one specific task at a time, but have a high level of difficulty translating battlefield information into new military strategy. Steering an organization requires adapting to input from the outside world. In a military context, this is why modern militaries no longer rely on muskets and bayonet charges (or just very large rocks) to win battles. However, bureaucratic literature suggests that this process is slow and painstaking.

Even if military organizations do actively monitor battlefield information for indicators of success or failure, this does not mean that strategic adaptation necessarily takes place. Military decision-makers understand that some level of failure and loss of inevitable when an overall plan succeeds, which further complicates the way they interpret incoming information from the battlefield [Feld, 1959, pp. 15-16]. Even if the information is consistent, changing strategies is costly for an organization, in terms of both time and resources. Bureaucratic elements must be reconfigured, logistics must be reworked, new SOPs must be developed, and so on [Mintzberg, 1978]. Once set in motion, any organization tends to move forward with its current strategy, and the more bureaucratic inertia it holds, the more difficult it is to stop. This is why the choice of a given strategy is extremely painstaking, and deviation from that strategy can require a massive amount of information from the field to make the costs of strategic change worthwhile: “A tenet of military discipline is that if the original plan is sound nothing occurring in the course of its execution should in any way affect the determination to carry it out. The more professional a military organization regards itself, the more strictly it is apt to adhere to this rule of conduct” [Feld, 1959, p. 21]. Strategic updating is not something that military organizations take lightly. It is an investment of time, effort, and resources that is only undertaken when the alternative is catastrophic failure.

In addition, most of this literature focuses exclusively on state military organizations, which have three organizational factors differentiating them from non-state actors. First, state militaries are *persistent*: they exist prior to conflict breaking out, and (usually regardless of who wins the conflict) they will continue to exist in a fairly unchanged condition after conflict. This means that a state military has a very long time to build internal institutions, cultures, and practices, which are very difficult to change even in the face of overwhelming evidence that they are not productive [Soeters, 1986, Earl, 1984]. One of the most obvious examples of this phenomenon is the ‘cult of the offensive’ that Evera [1984] blames for the catastrophic chain of events that led to the First World War. Second, state militaries are *hierarchical*, with clearly defined roles, responsibilities, and informational flows. This means that decision-making relies on the efficient flow of

information up the command chain and communication back down the chain. Actors along this chain are not without their own biases, beliefs, and preferences, all of which can affect the type of information that makes it through [Chandler, 1969]. When either of these flows are disrupted or biased, it becomes much harder for the organization to change behavior accurately and efficiently. Third, state militaries are subject to *civilian management*, albeit to a varying extent across regimes. Civilian decision-makers do not have the same set of incentives as military leaders [Kier, 1995]: when domestic political concerns clash with military decision-making, domestic politics wins even at the expense of military efficacy.

Furthermore, much of the scholarship dealing with organizational structure is theoretically limited and hard to generalize, often attributing military decision-making to fuzzy factors like the competence of individual leaders [Murray, 2009]. While some recent work [c.f. Farrell, 2010, Serena, 2011] has discussed the role of organizational structure in affecting military updating, this work is primarily descriptive and limited to single-unit case studies, limiting its ability to generalize to a greater population. Other works draw on theories of organizational decision-making from other fields, looking at militaries as organizations with bureaucracies, internal politics, and relations with other organizations in a greater government network [Allison and Zelikow, 1982]. These more rigorous theoretical approaches to military decision-making are useful in explaining the relationship between changes in the environment and military outcomes; however, they have one key limitation in that they are designed to explain the processes undertaken by state militaries in state governments.

This gap is twofold. First, there is a disconnect between two strains of literature. One consists of very limited case-study analyses of conflicts or even individual locations or campaigns, showing that in some cases both states and insurgents can engage in rapid, effective strategic updating. The second strain focuses on military actors as organizations, arguing that these groups find it difficult or impossible to quickly change their fighting method. These bodies of work do not address one another directly, leaving the issue unresolved. Second, there is no coherent framework that exists to compare insurgent and

state decision-making side by side; nearly all of the studies reviewed here look at single conflicts, organizations, or time periods.

I build a detailed theory of behavioral military-strategic adaptation during conflict that applies equally to states and insurgent forces. This rational-adaptor theory provides me with a set of general expectations about how actors adapt to each others' behavior during conflict. This research design is generalizable across multiple types of actors, conflicts, and time periods. In the following three chapters, I engage in a set of tests looking for evidence of actor adaptation during conflict. If the rational-adaptor model holds, I should find specific, significant behavioral changes over the course of conflict, as actors find themselves in unfavorable positions and change their behavior in an attempt to achieve battlefield success. This analysis represents an important step in our understanding of military-strategic behavior in civil conflict. If we observe mutual patterns of adaptation and updating during conflict on the part of both insurgents and states, the rational-adaptor model may provide a useful foundation for future analyses and forecasting efforts in the context of civil conflict.

## 1.3 Theory

### 1.3.1 Strategic updating during conflict bargaining

Looking at actor behavior during conflict can help bridge gaps in the standard conflict-as-bargaining literature. Conflicts can last a long time, even when one actor is weak, when that actor is able to effectively use the resources it has available. If actors do update their behavior during conflict, then this is a dynamic process: as actors learn more from fighting, they gain a better understanding of the relative balance of capabilities. Instead of simply accepting a settlement based on their perception of losing the conflict, a weaker group can transition to a new form of war-fighting that is more appropriate to the new balance of power and the strategy employed by the opponent. An insurgent group who has unsuccessfully attempted a large-scale, conventional conflict may learn that it does not have the power to win such a war; instead of surrendering, it can instead transition to a less conventional, irregular form of fighting which leverages its relative mobility and

local informational advantage to potentially extend the conflict long enough to politically exhaust its opponent. In other words, more information about the balance of capabilities does not necessarily spell an end to fighting. As strategies change, both actors have to adapt and find effective ways of continuing to engage in warfare: a state's military strategy that is highly effective against a territorial, conventionally armed insurgent force may prove to be less effective at detecting and engaging a highly mobile insurgent that employs non-conventional technologies of violence.

The inclusion of military-strategic updating complicates traditional conflict-as-bargaining models somewhat, by allowing the relative success of each actor (at least in the short term) to vary as a function of military strategy. An actor that is currently losing the war may not be doomed to ultimate failure; instead, the losing group can attempt to reverse its military fortunes by changing the way in which it fights. This process of updating can explain why military dynamics such as casualty rates, conflict intensity, and the location and type of violence can change significantly over the course of a conflict in a way that random-walk bargaining models cannot explain as well.

However, the mere presence of military-strategic adaptation is not necessarily enough to bridge the gap between *ex ante* capability measures and conflict outcomes. If both sides are able to rapidly and effectively adapt to each other's behavior, then the effect of military strategy essentially cancels out: when both actors fight effectively 100% of the time, the stronger group is still more likely to win. The question, then, is: why are smaller actors able to spend more time fighting at a relative advantage? What is it about these actors that makes them more effective at military adaptation — or what is it about stronger actors like states and peacekeeping forces that makes them less capable of effectively learning while fighting? Insurgent forces are able to adopt less-conventional forms of combat such as highly mobile guerrilla operations, or urban insurgency and terrorist tactics.

This means that they are able to cope with short-term military losses by ‘going underground’, moving their bases of operations, or significantly shifting the type of violence and targets of violence. States do not have as much leeway in choosing new military strategies. More than insurgencies, which often have highly committed bases

of support, states have to consider the political costs (both domestic and international) of adopting certain strategies — carpet-bombing entire provinces may be effective in wiping out elusive insurgents, but it is unlikely to lead to good international publicity or domestic popularity. Additionally, states face logistic limits in how they can operate: for example, if local intelligence is limited, a targeted low-level campaign against insurgents is very difficult. As such, states seeking to quickly end conflict have to weigh the potential costs of a drawn-out but low-intensity war versus the fallout of a high-intensity, hopefully shorter military campaign. The result is that insurgent actors who face fewer structural and political obstacles to military-strategic adaptation can spend more time fighting at a relative advantage. This does not mean that weaker actors are likely to win conventional military victories, but it does mean that they survive longer and achieve better outcomes than would be expected given traditional conflict-as-bargaining models that focus on factors like material capability.

Although I do not propose a new game-theoretic model of strategic updating, this theory of intra-conflict strategic updating sits at the nexus of these two bodies of scholarship. By bringing military strategy into the round-by-round framework of conflict bargaining, I can better explain why weak actors (1) do not lose wars as much as we might expect, and (2) even when losing, often hold on and drag wars out longer than we might expect. By building on the simplified framework that conflict is a bargaining process in which information is revealed through fighting, I can better explain how actors can update their beliefs about capability ratios and the likelihood of victory through the process of fighting. By allowing actors to update their military strategy over the course of conflict, I can better explain why ex ante measures of both material capability and military strategy do not always lead to the predicted patterns of conflict duration and conflict outcome.

This work attempts to fill this gap in our understanding of civil war violence in two steps. First, I identify some general rules that can be used to describe military strategy. Strategies are described as sets of behavioral rules or patterns that armed actors (both states and non-state actors) adopt when undertaking long-term organized violence. These rules govern, in a broad sense, how military resources are allocated during a military

campaign: where troops and weapons are employed, what type of tactics and technology are used, and who or what constitutes the targets of violence. How an actor uses its resources, described on these three dimensions, comprises that group's military strategy. Second, I lay out a set of mechanisms that cause actors to change their military strategies over time, as new information is revealed that changes their perceptions about the world around them and their own likelihood of achieving their political and military goals. Finally, I analyze political and logistical obstacles facing states and insurgents that make it more difficult to quickly and effectively update their military strategies during conflict.

### **1.3.2 Rational adaptors and strategic updating**

The degree to which an actor can successfully (1) identify the true state of the world as it pertains to the ongoing military contest, and (2) quickly and effectively adapt its military approach to changing circumstances, can have a powerful effect on the duration and outcome of conflict. Actors that are unable to effectively adapt their military strategy to engage the opponent, whether due to a lack of information or an inability to change, are likely to fare worse than their raw military power would suggest. Weak actors who can fight effectively against stronger powers and adapt to changing circumstances can fight longer and harder than existing theories would expect, and can achieve greater political concessions when fighting ends. The focus on capability as an explanation of conflict outcomes relies on the assumption that all actors involved in war quickly and efficiently adapt their military-strategic approach to changes in their circumstances and the actions of one another.

In other words, this theory assumes rationality on the part of the actors involved. Just as in pre-war bargaining models, actors are assumed to be operating in an environment of limited information where talk is cheap. During conflict, information is revealed through battlefield outcomes. Actors know little about each others' capability, willingness to fight, and ability to win until they pay the cost of uncovering this information through fighting. As these actors learn more about their own power and likelihood of long-term success relative to each other, they update their own behavior: not just whether they continue fighting, but how they fight.

When armed actors (both state and non-state actors) are faced with information that suggests they are losing a conflict, *and* they believe this trend can be reversed through changing the way in which they fight, they are likely to do so rather than immediately surrender or continue in the same patterns of behavior. The way an actor fights — its military strategy — is important in both the short- and long-term. Conflict is expensive: actors have to spend manpower and resources to achieve both short-term goals (seizing a politically important city or controlling resource-rich territory) and long-term goals (exacting enough costs to make the enemy negotiate a favorable peace). Because conflict is expensive, actors want to spend their resources efficiently and maximize their ‘return on investment’ by fighting in such a way that they maximize their efficacy against the opponent. For both state and non-state actors, this means the use of an effective military strategy.

Weak actors succeed when they utilize effective strategies against their opponents. Strong actors fail when they are unable to utilize effective strategies against their opponents. This means that conflicts where a weaker group can systematically adapt to the strategy used by its stronger opponent are likely to end better for the weaker group. Furthermore, this theory argues that strategies are not static; both sides are in a constant process of assessing the current and projected state of the war, and working to adapt their behavior to more effectively deal with the threats posed by one another. Both sides adapt to one another’s behavior during the conflict, attempting to develop a strategy that will be militarily and politically effective given the behavior of the opponent.

Actors that can adapt more quickly spend more time fighting at an advantage. This means that in cases where there is an asymmetry of updating capability between sides, military strategy can — at least to a certain extent — moderate the effects of raw capability on the outcome of conflict. Although both sides face constraints on the range of strategic approaches available to them when fighting a war, states may find it more difficult to choose — and stick with — military strategies that are highly effective against insurgent forces, especially those employing highly non-conventional or ‘terrorist’ strategies. Even a strong state, if it is politically or militarily incapable of sustaining an effective strategy

against elusive insurgent forces, will have a hard time defeating the insurgent movement. Conflicts are unlikely to end until the stronger actor can find a military strategy that allows it to leverage its capability advantage against the weaker; until this happens, weaker groups can fight more effectively than their raw capability would expect. This ability to adapt and counter the behavior of opponents in war provides a new set of explanatory factors that can help bridge the gap between existing theory and observed conflict outcomes.

### **1.3.3 Defining strategy and updating**

Before moving further into a theory of strategic updating, it is important to define key terms. I define military strategies as sets of formal or informal rules that govern how an actor uses the military resources at its command to pursue some military or political goal. However, some additional work has to be done to operationalize these terms and differentiate types of strategy from one another. I argue that military strategy can be adequately defined from an observational standpoint by looking at three dimensions of violence: type, target, and location. The type of violence actors engage in refers to the technology and tactics that prevail at individual violent clashes, and can be divided loosely into direct versus indirect force. Direct force involves face-to-face clashes and direct-fire weapons. Indirect force involves tactics and technologies that remove the perpetrator from direct risk. IEDs and aerial bombings describe a very different type of engagement strategy than large-scale formation maneuvering. The targets that actors select for violence are categorized as military versus civilian. Actors that target civilians for violence are acting on a different set of rules than those that devote their resources against enemy fighters. Finally, the locations at which actors deploy military resources are also informative. Locations can vary in political, economic, and territorial value, as well as in their distance from points of strategic interest such as state capitals. Insurgent groups that target large cities for attacks are functionally different from those that engage in rural operations or border skirmishes.

The identification of strategy by measuring type/target/location of violence relies on observed event data to describe the rulesets both sides use during conflict. By observing a conflict actor's behavior over time, I can both describe its current strategy and identify

significant shifts in that strategy. Changes in strategy can be detected by tracking changes in these three dimensions over time. This makes it possible to look at the speed, degree, and persistence of changes in these dimensions over time. An actor that shifts from urban to rural operations over the course of several months, and continues to operate in the periphery for the next several years, has clearly made significant changes to its military strategy. It now operates in a different part of the state, focusing on different targets which require the development of different tools and tactics to successfully engage.

This functionalist conception of strategy relies on a significant assumption: that all (or at least most) of the violent engagements an actor participates in are of its own volition — in other words, that an actor selects into all violent events in accordance with its strategy. There are definitely cases where this is not strictly true: forces in the field can suffer surprise attacks, or be forced to fight in environments where they would prefer not to operate. However, I would argue that this assumption is not an over-simplification: as has been stated many times, ‘it takes two to tango’. Military forces only fight where they have been deployed. This means that every time we observe a clash between actors, both sides have chosen to deploy military resources to that location. For one — or even both — of these actors, it may not be the ideal time or location to fight; they may not even expect to win. However, fighting still takes place because both sides deemed it to be a better outcome than unilaterally withdrawing or refraining from committing troops. In other words, fighting is a conscious choice, and as such, the vast majority of conflict events should reflect the decision rules of each side’s military strategy.

### **1.3.4 Mechanisms of military-strategic updating**

Actors engaged in conflict prefer to use military strategies that are best fitted to the true balance of capabilities between combatants. At various points an actor may assess that it is using a sub-optimal military strategy. This situation can arise because an actor has miscalculated its own capabilities vis-a-vis those of its opponent, or the efficacy of its current strategy matched against the strategy of its opponent. When an actor perceives that it is using a sub-optimal military strategy, it has a strong incentive to change its approach to war-fighting in order to maximize its combat efficacy, and therefore its chances

of achieving military and political goals through violence.

Actors receive a torrent of information that is noisy at best and contradictory at worst. These actors have to use this messy, inconsistent information to inform their assessments of ‘how the conflict is going’, and how this affects the expected outcome of the conflict as a whole. When actors interpret these indicators to mean that the conflict is not going well, they have to decide whether to (1) continue in their current course of action, (2) alter their behavior in the hopes of achieving a different outcome, or (3) cease fighting altogether and give up.

Actors are more likely to change their behavior when they perceive that their current course of action is unsustainable or unlikely to lead to a good outcome. Updating behavior is commonly observed in individuals and actors, in both experimental settings and observational studies. Fiol and Lyles [1985] for a survey of this literature, and Ahlstrom et al. [2009] for a more recent discussion of military history as metaphor for organizational learning. In an uncertain world, organizations — from military bodies to companies to non-profit actors — create some kind of strategy or ruleset that they use to allocate resources and make decisions with the aim of achieving some kind of goal. The range of potential strategies is virtually unlimited; for this reason, actors tend to satisfice, working to find a strategy that is ‘good enough’ and persisting in this mode of operation until and unless new information emerges showing that the strategy is not working. Actors’ beliefs about the world are not static; instead, low-information actors can use new data as it is received to change their beliefs about the world, and in so doing, change their beliefs about the best course of action to achieve their goals.

### **1.3.5 Learning and mistakes during conflict**

Even rational adaptors can make bad choices if they do not have good information to work with. During conflict, actors gather information and learn about the true state of the world through fighting. This means that ‘bad’ strategic choices and delays in adjustments to ‘good’ behaviors are real possibilities, especially in the early stages of conflict. This is not to say that actors are irrational in a game-theoretic sense; it is important to recognize that conflict is a chaotic, low-information environment, and both states and insurgents

face significant uncertainty when engaging in violent conflict. Information is most scarce at the beginning of a conflict, meaning that many actors come into a conflict using a sub-optimal strategy that has to change for success to be possible.

Both weak and strong actors in conflict can choose poor strategies in the early stages of the war. Consider the early stages of the conflicts in Iraq and Afghanistan; despite being obviously, wildly outgunned, both the Taliban and Saddam Hussein's government forces mounted a conventional campaign of direct combat in the early stages of the war [Andres et al., 2006, Biddle, 2004, Woods et al., 2006]. These actors quickly lost the conventional stage of the war, and moved into a more fluid guerrilla strategy that allowed them to survive and maintain military relevancy for years afterward. Clearly, some type of learning took place, despite what appear to be very poor starting strategies in both conflicts.

In these cases, the degree to which both sides are likely to adopt any given strategy as a conflict breaks out is due largely to their estimation of the balance of capabilities between combatants. Insurgents do not have perfect information on state military capabilities, just as states considering entering war do not have perfect information of each other's capabilities. More than this, however, insurgents do not have perfect information about how willing the state is to fight, or what level of resources it is likely to devote to combat. Civil wars, even more than international wars, are not 'total wars' in the classic sense. Many civil wars occur in specific regions of states, outside of which life can largely continue as usual. States are also often unwilling to bring every resource to bear on eliminating insurgent forces; these resources are spread thin between providing public and semi-public goods to keep the rest of the population in line, providing security forces to the rest of the state, and guarding against international threats and challengers who may try to take advantage of internal strife. The actual degree to which a state is willing and able to bring its resources to bear on fighting insurgents can often only be found out through the process of fighting.

Insurgents also make forecasts about the actions of the civilian population. Most insurgent actors are relatively unknown prior to armed conflict, and one of the short-term goals of insurgent violence is to gain attention for its cause and draw recruits and resources

from the civilian population. However, the actual level of popular support an insurgent group can draw is also quite uncertain; many movements have overestimated the level of public support they will receive, and have suffered the consequences — see for example the abortive 1832 June Rebellion in France, immortalized in Victor Hugo’s novel (and the musical by the same name) *Les Misérables*, in which Republican rebels attempted to foment insurrection in Paris, only to be left alone at their barricades when the population failed to rise in their support against king Louis-Philippe. Insurgent actors can miscalculate the strength of the state, the willingness of the state to bring its full resources to bear, and the likelihood of mass public support for their cause once fighting begins. This information can only be gathered once fighting actually breaks out. Once insurgents are able to update their information about the strength and willingness of the state to fight, as well as the level of public support they are likely to receive through fighting, they are able to adjust their military strategy to reflect this new information.

States can also miscalculate when assessing insurgent forces. Insurgent military strength is unknown until and unless they mobilize, meaning that states face real difficulties in estimating actual insurgent military capability before conflict breaks out. Even once conflict begins, it is still difficult for states to accurately assess factors like manpower and resources: many insurgent movements are comprised largely of part-time fighters who are difficult to identify or engage when out of battle, and even during mobilization, do not wear uniforms or other identifying insignia. As such, uncertainty about insurgent strength can be a major problem even after conflict breaks out. Actors can dramatically underestimate the fighting capability of their opponents. This was seen in the early years of the US Civil War as the Union found itself up against a stronger threat than originally anticipated. Murray (2009) quotes General Ulysses S Grant in his memoirs of the early conflict:

“Up to the Battle of Shiloh, I, as well as thousands of other citizens, believed that the rebellion against the Government would collapse suddenly and soon, if a decisive victory could be gained over any of its armies. Donelson and Henry were such victories. An army of more than 21,000 men was captured and destroyed. Bowling Green, Columbus and

Hickman, Kentucky, fell in consequence, and Clarkesville and Nashville, Tennessee, the last two with an immense amount of stores, also fell into our hands. The Tennessee and Cumberland rivers from their mouths to the head of navigation, were secured. But when Confederate armies were collected which not only attempted to hold a line further south, from Memphis to Chattanooga, Knoxville and on to the Atlantic but assumed the offensive and made such a gallant effort to regain what had been lost, then, indeed, I gave up all idea of saving the union except by complete conquest” [Grant, 2006, p. 368] quoted in Murray [2009, 46].

States are also uncertain about the resolve and willingness of insurgents to fight. This is not to say that states are uncertain as to whether or not insurgents will fight, but rather that states are unsure of the losses that insurgent forces are willing to tolerate, and the outcomes they are potentially willing to accept. Regimes have been surprised before, usually unpleasantly, by insurgent forces who were able to sustain staggering losses and setbacks without surrendering or negotiating. The most famous example of this faulty assessment of loss acceptance is the Vietnam War, during which several hundred thousand Vietnamese fighters and civilians were killed before forcing the United States into an internationally embarrassing compromise that essentially amounted to political defeat.

States — like insurgents — do not have perfect information about the future actions of the civilian population. States struggle when a larger-than-expected portion of the population supports an ongoing insurgency, whether because the latent hostility towards the government was high to begin with, or because the insurgent movement was able to successfully frame itself as a viable alternative to state government. The success of Mao Zedong’s insurgent movement against Chiang Kai-Shek in China was in large part due to its ability to foster relations with the local rural population, who overwhelmingly supported the Communist movement against Chiang’s highly unpopular government.

Finally, both states and insurgents can be caught off-guard by developments on the battlefield. Even in cases where capabilities are known on both sides, things can go horribly wrong and lead to unexpected, large-scale losses that can impede future war-fighting efforts. Consider that the vast majority of history’s ‘decisive battles’, especially in the modern

era, were not labeled as such *ex ante* — one or both sides were caught off-guard, or otherwise fell victim to unexpected events. Clausewitz discusses in some depth the idea of ‘friction’ as a major element of uncertainty in warfare [Clausewitz, 1976, p. 119]. The overwhelming complexity of large-scale military operations means that minor setbacks or failures can trigger large-scale reactions that can result in catastrophic problems for military forces. Both states and insurgents can miscalculate their own strength, the strength of their opponent, and the reaction of the civilian population to the outbreak and continuation of hostilities. This is not the same as saying that these actors are not rational in a game-theoretic sense; instead, I claim that both types of group act in an essentially rational manner, by using information generated through fighting to update and reassess these initial conceptions about what the world looks like.

### **1.3.6 Military-strategic updating and conflict termination**

I argue that military strategy affects the outcome of conflict in two ways. First, the relative ability of the weaker actor to adapt and fight effectively will affect the duration of conflict. Weak insurgent groups that can effectively change their approach to fighting and counter the efforts of the state can live to fight another day, even if they are unlikely to achieve overall military victory. This is how Johnson [2009] explains the longevity of insurgent movements in Iraq: these groups have the ability to quickly find effective military strategies due to their fluid structures, long-lasting combat deployments, and high-attrition environments. Bad strategies are fail quickly, and ineffective fighters do not last long; this means that the movement as a whole was able to quickly and effectively ‘evolve’ new strategic counters to coalition peacekeeping efforts.

Given the dynamic nature of strategic adaptation, making *ex ante* predictions of conflict duration is difficult. However, studying patterns of short-term behavior during conflict may provide insight on when conflicts are likely to end. I argue that conflicts are more likely to end when the weaker actor is unable to shift the balance of military-strategic interaction in its favor — in other words, when one side can no longer adapt to the strategy chosen by the other. When the weaker actor cannot effectively blunt the force used by the stronger actor, neither victory nor escape is possible, and surrender becomes much

more likely. Returning to the case of Angola, this situation arose when UNITA, having lost significant military power, changed to a mobile guerrilla strategy. When the Angolan state was able to effectively counter this strategy through a mix of indiscriminate violence and area denial through large-scale ground operations, there were no remaining effective strategies available to the insurgent group — they were still losing manpower and civilian support through government violence, and were being slowly corralled into smaller and smaller areas of operations. At this point, the Angolan state was able to bring its superior military capability to bear, and since UNITA could not adapt to effectively fight this type of war, it was not able to continue.

In contrast, conflicts are less likely to end when the stronger actor is unable to find a strategy that is effective against the strategy chosen by the weaker side. In the following chapter, I discuss this scenario in the case of Israel and the Palestinian Territories. Palestinian insurgents, though significantly weaker, were able to carry out a sustained campaign of violence against Israeli targets. Israel's range of retaliatory options are limited significantly by the political costs associated with large-scale violence: even limited incursions into the Gaza Strip tend to result in destruction and loss of civilian life, which is broadcast around the world and subjects Israel to outcry both abroad and at home. Faced with these costs, Israel has been unable to pursue the type of massive and sustained effort required to bring the Palestinian insurgency to an end through purely military means, and the conflict has continued.

When weak insurgents can successfully adapt to state military strategies and states are unable to respond effectively, conflicts are likely to last much longer and end on more favorable terms to the insurgents. However, if and when states are able to develop effective counter-strategies to those used by insurgents, conflicts are likely to end more quickly. When states can effectively bring their superior capabilities to bear, they tend to be successful, but until that point insurgents can survive and even thrive while inflicting damage to the state over time.

### 1.3.7 Rational adaptation to changing circumstances

Actors are likely to change their strategies when battlefield information suggests that their own strategies are not working, or that they may fail soon as a result of changes in opponent behavior. When updating their military strategy, actors not only look at the past, using recent data to assess current performance, but also make estimates about the future, gauging the likely effects of an opponent's new strategy.

Actors use currently-available data to estimate their current military success. Casualties have been a key predictor of military behavior in both qualitative and quantitative accounts of conflict due to their importance to armed actors and their relative availability to scholars analyzing conflict dynamics and outcomes. The logic of casualties as a predictor of conflict dynamics is simple. Killing people and breaking things in conflict has two effects — one informational, and one material. First, inflicting losses upon an enemy force provides information about the relative balance of capabilities. A weaker group that has overestimated its strength will find out, through defeat and loss on the battlefield, that its power relative to that of its opponent is less than expected [Powell, 2004, Goemans, 2000, Smith and Stam, 2004]. Both actors learn from observing casualty trends, using this information to update their mutual beliefs about the state of the world, and eventually, converge upon an agreement that reflects these more accurate beliefs.

Second, inflicting casualties means the killing of actual people, and the breaking of actual things. The ‘power to hurt’ is crucial for states and non-state actors alike — even in cases where both sides have perfect information about relative capabilities [Slantchey, 2003, 2004]. Inflicting casualties on an opponent can bring conflict closer to a settlement, both by increasing costs — potentially over the opponent’s threshold of willingness to bear such costs — and by decreasing their actual military capacity to retaliate. In this way, casualties have a direct effect on conflict dynamics: the heavier the losses endured by one side, and the weaker their capability to inflict losses on their opponent, the less tenable a continuation of conflict appears.

Casualties may not be the best metric of success or failure in all cases. Evidence suggests that some actors are highly casualty-acceptant [Sullivan, 2007]; for example,

during the Vietnam War the North Vietnamese forces took heavy losses over a long period of time, but continued fighting against an overwhelmingly better armed and organized force. In fact, focusing on casualty counts as a metrics of military success was one of the downfalls of the United States during this conflict; the US Army consciously chose to rely on enemy death counts as a metric of success, and was repeatedly caught off-guard by the Vietnamese' failure to give in or surrender despite rising losses [Gartner, 1999]. By focusing solely on enemy deaths to measure success, the United States forces were unable to perceive the reality of a losing war.

However, this does not mean that casualties are meaningless. There may be situations in which raw casualty counts do not matter, but the relative rates of losses do; Johnson [2009] points out that during the course of Vietnam, the actual ratio between US and Vietnamese losses showed no clear trend in any direction, regardless of changes in the count and rate of losses on both sides. Viewed in this light, it appears that relative losses were telling a different story than focusing only on enemy deaths. It is not that casualties were meaningless, but it may be that the US was not using casualty information in an informed manner. I argue that when looking for a single metric to measure success, casualties are the best available instrument because they directly relate to the material and human power of an actor. Actors can be bereft of territory, political capital, or (willing) civilian supporters and keep fighting; only the loss of life and capability directly affect the ability to carry on with a war. As such, focusing casualties is, if not a perfect metric, than at least the best available one.

In the large-scale empirical literature, there is evidence that casualties affect conflict dynamics and conflict outcomes, but it is not clear exactly how this relationship works. Some scholarship has attempted to draw a direct link between casualties and outcomes, using casualties as a direct measure for the ability and willingness of a state to continue fighting an unsuccessful war. Overall casualty counts, and casualties as a ratio of overall population, are not significantly linked to the duration or outcome of major interstate conflicts; however, limited evidence suggests that short-term trends, such as dramatic increases in the rate of losses or the ratio of casualties between two sides, can precipitate

conflict termination [Klingberg, 1966]. Broader analysis of dyadic conflicts suggests that the level of casualties suffered by a given armed group does make victory less likely in the aggregate [Slantchev, 2004, Quackenbush, 2015], implying that casualties are associated with military success or failure. Meso-level analyses of conflict muddy the waters on this issue even further: disaggregating from the conflict-year to the individual battle level of analysis, [Ramsay, 2008] finds little evidence for a clear-cut relationship between casualty counts or trends and conflict termination. In the aggregate, it is unclear that there is a direct relationship between casualties and conflict termination or outcome.

However, the relationship between casualties and outcomes may not be as clear-cut as determining victory versus loss, or precipitating surrender by one side. Casualties are theorized to affect conflict dynamics indirectly in a variety of ways. In many cases, casualties are a proxy for political costs. Political leaders — particularly when discussing states — can be held accountable for the costs of engaging in conflict. Highly expensive or drawn-out wars can lead to severe penalties in public support for the war, and for the leaders perceived as responsible for engaging in the war to begin with. Both cumulative and marginal trends in casualties have been shown to have a significant effect on popular support for conflict in the United States [Gartner and Segura, 1998, Gartner, 2008]. This relationship may be conditional, however: some evidence suggests that the political impact of casualties varies depending on the nature of the parties involved, as well as the nature of the conflict. Wealthy, democratic, post-industrial societies are more sensitive to loss of life, and are therefore less willing overall to pay human costs to achieve political goals through violence [Devine, 1997, Luttwak, 1996, Schörnig and Lembcke, 2006]. Note, however, that this is not the same as a refusal or aversion to using high levels of violence overall; democracies tend to fight harder in the conflicts they engage in [Reiter and Stam, 1998] and can even be willing to inflict higher levels of destruction and indiscriminate violence in efforts to protect their own forces [Levy, 2010]. Autocratic states may be more willing to suffer larger costs in conflict, as leaders are not directly accountable to the public for conflict management and casualty counts [Filson and Werner, 2004]. However, the nature of conflict matters as well: armed actors, both states and non-state actors, are much more

willing to accept casualties when they are fighting for highly salient goals such as the liberation of what they perceived to be homeland or sovereign territory [Sullivan, 2007].

Another way of studying the relationship between casualties and conflict dynamics is by examining changes in actor behavior in short-term response to casualties. Recent micro-level analysis of civil conflict provides strong evidence that casualties matter to both states and insurgents. In line with the idea of casualties as both providing information on capabilities and directly affecting the balance of power, these theories propose that actors who are taking heavy losses during conflict update their beliefs about their own strength relative to the opponent, and their likelihood of victory if the current course of events continues. Insurgents who are taking higher levels of casualties from fighting are more likely to begin preying directly on the civilian population [Hultman, 2012, Wood, 2014]. This allows the insurgent group to remain a relevant political and military force by pressuring the state with threats of further attacks on civilian targets, undermining public support for conflict and state morale. States, particularly those that are more sensitive to casualties, often try to proactively avoid friendly losses through the use of high technology and overwhelming force [Levy, 2010]. There is clear evidence that casualties matter to insurgent actors, but it does not necessarily lead to the simple casualties → surrender relationship expected by most theories relating casualties to conflict dynamics.

This indirect relationship between casualties and behavior is key to a theory of military-strategic updating. As an actor inflicts and endures casualties, it (1) uncovers further information about its opponent and the balance of capabilities between the two actors, and (2) estimates changes in its ability, and that of its opponent, to effectively inflict and endure future damage. Casualties therefore serve as an effective metric of military success or failure, in that they provide credible information about the likelihood of achieving a better or worse outcome to the overall conflict. As the group updates its perceptions about the balance of capabilities between itself and its opponent, it also re-evaluates the efficacy of its current military approach. The rules an actor uses about where, how, and against whom to use violence are all part of the same decision-making process that I term ‘military strategy’.

### 1.3.8 State behavioral updating

Both states and non-state actors engage in military-strategic updating, but there are a number of important distinctions between state actors and non-state actors. During civil conflict, the constraints facing insurgent forces are very different from those facing governments, and as such, the range of options states versus insurgents may have in the same situation can vary considerably. In general, states escalate the use of force when they perceive they are losing; insurgents are more likely to de-escalate and switch to less-conventional forms of violence.

States rarely use their full military force against insurgents, especially in the early stages of conflict, for two reasons. High-intensity warfare is expensive, both financially and politically. Mobilizing the state military requires tremendous outlay of public funds which could be used to satisfy domestic unrest instead [Sullivan, 2008]. Politically, highly intense military strategies, especially for certain types of states, may prove to be unsustainable. Prolonged campaigns of mass violence provoke international condemnation and domestic unrest; as such, even a militarily successful strategy may be too politically risky for a state to carry out [Levy, 2010]. Second, logistic concerns make some more politically desirable strategies untenable, at least in the short run. Local informational advantage means that insurgents are difficult to find and kill, and states with little perceived legitimacy or power in an insurgent-supporting area are unlikely to meet with the type of cooperation required for a targeted campaign. However, this calculus can change as conflict reveals more information about the state of the world. As states face opponents that are more intractable or more dangerous than initially expected, they are likely to escalate the type and level of violence they use to eliminate this threat.

Full mobilization is expensive, and the use of some types of weapons and tactics can also be very costly from a political standpoint. The use of indiscriminate weaponry and tactics may be effective in killing insurgents, but it also comes with major risks: the accidental or purposeful targeting of civilians comes with significant political costs [Valentino et al., 2004]. Escalation by the state generally takes two forms. First, the state will commit more forces overall to the destruction of the insurgent movement. By committing more

resources — both material and manpower — to the conflict, the state can attempt to bring the conflict to a close via military means more quickly, or at least force insurgents to the bargaining table to achieve a more advantageous outcome. This can mean increased mobilization or recruitment of regular military forces, as well as the raising or support of paramilitary organizations to fight insurgents locally [Carey et al., 2015]. It can also mean the escalated use of highly destructive military technologies, and the adoption of more violent tactics overall. For states, more so than insurgents, the type of weapon and the type of target go hand in hand. This is because states often have difficulty locating and identifying actual insurgent fighters and supporters. Larger-scale weaponry and tactics, such as artillery strikes and aerial bombing campaigns, often hit civilians as well, who may be actual supporters of the insurgency or just in the wrong place at the wrong time. Greater reliance on indiscriminate violence, and tactics that target the civilian population to deter support for an insurgency, are adopted with the goal of ending a conflict more quickly, but are also correlated with higher levels of civilian casualties.

Violence against civilians, regardless of the type of weaponry used, has been studied intensely in literature on both international and civil conflict. There is little consensus on how effective it is to consciously target civilians [c.f. Condra and Shapiro, 2012, Toft and Zhukov, 2012]. In interstate war, large-scale violence against civilians through tactics such as strategic bombing seem to have a counterintuitive effect, hardening resolve among the population and increasing support for continuing conflict [Pape, 1996, Horowitz and Reiter, 2001]. States targeting civilians indiscriminately, or using large-scale violence that catches civilians in the crossfire, are likely to see international outcry and domestic backlash, which can be particularly damaging to leaders of democratic states [Hills, 2004, Merom, 2003]. Public support — the ‘hearts and minds’ of the civilian population — is crucial to successful counterinsurgency, and large-scale violence against civilians can erode support quickly and drastically. Purposeful targeting of civilians, according to this literature, is in the long term unproductive at best and dangerous at worst. Militarily, this means that states face difficult decisions in their military-strategic options. The use of overwhelming force in a civil conflict may lead to military success. However, if this success is not swift

and decisive, the state risks losing the support of its own population. Further evidence of this trade-off is shown when insurgent forces actually try to provoke indiscriminate violence from the state, specifically planning to use the casualties and destruction from this violence as a recruiting tool to grow their own support base.

The challenge facing states is to balance the short-term costs (both material and political) of high-intensity violence versus the long-term costs of fighting a low-intensity war that could drag on for years or even decades. Engaging in high-intensity conventional military operations to root out an insurgent group may be militarily successful, but the political downside is potentially enormous. However, the costs of high-intensity violence may be worth it in the short run, if it can bring conflict to a rapid halt. Violence against civilians is instrumental, not an end unto itself. While violence against civilians is occasionally due to personal vendetta [Kalyvas, 2009] or criminality [Mueller, 2004, Manekin, 2013] it is generally conscious military-strategic choice made by combatants. For example, some evidence suggests that in some cases, indiscriminate violence is effective as a punishment mechanism to dissuade civilians from giving aid and shelter to insurgents in the local population [Arreguin-Toft, 2005, Lyall, 2009]. This is especially common in conflicts where some ethnic, religious, or linguistic cleavage differentiates likely supporters of the insurgency from supporters of the government [Fjelde and Hultman, 2013]. Social and moral barriers against purposely targeting civilians during war, which are relatively common (at least in wealthy industrialized states) can break down in the face of security threats [Conrad and Ritter, 2013]. The commonly-cited Maoist doctrine that insurgents should be stealthy fish swimming in a ‘sea’ of civilians is also known by states, who often in turn attempt to ‘drain the sea’ by killing or driving away civilians who are potential insurgent supporters, with occasional success [Valentino et al., 2004]. Both states and insurgents commonly target civilians during conflict, suggesting that this tool is used despite its political dangers on both sides [Hultman, 2012, Valentino, 2014].

When armed conflict breaks out, states have a strong incentive to end the conflict as quickly as possible and as decisively as possible. For a state, having an ongoing civil conflict is dangerous for many reasons. The very presence of conflict signals that the state is not

strong enough to deter violence, sending a signal of potential weakness that gets stronger over time as the state is unable to fully crush the insurgency. This signal can be received by other disaffected actors within the state, who may take up arms on their own to press for political concessions while the state is otherwise engaged [Bormann and Hammond, 2015]. This means that for many states, the choice is to pay the short-run costs of intense violence, which may erode their own political legitimacy, or face the long-term danger of new security threats arising while old threats persist. Highly intense civil conflicts tend to end quickly and decisively, generally with one side suffering significantly heavier losses [Balcells and Kalyvas, 2014]. This suggests that if they believe they can win by paying higher up-front costs in manpower, overall violence, and (potentially) political legitimacy, states have an incentive to escalate and try to end conflict a faster [Valentino et al., 2004, Clancy and Crossett, 2007]. The end of conflict means that states are (1) no longer paying active-duty military personnel and incurring casualties; and (2) no longer sending a potential signal of weakness to other states and potential domestic insurgents. States will often initially try to extinguish insurgencies using the minimum level of force required; if this fails, escalation to large-scale violence becomes more likely.

States that are under internal or external pressure to quickly eliminate domestic insurgency are more likely not just to escalate overall levels of force, but also to rely on more destructive and indiscriminate methods of violence. Tactics like air strikes and bombing or artillery bombardment can be employed against suspected insurgent bases, even when those bases are located in or near population settlements; purposeful large-scale targeting of civilians can be used to try and cleanse an area of insurgents by ‘draining the sea’ by killing or driving away their potential or actual supporters so as to deny an insurgent movement shelter and resupply in the local vicinity. These actions can lead to severe costs down the line, from international condemnation to loss of support at home; however, they can be seen as the only way to effectively deal with an insurgent movement in the short term. This exact pattern has been seen over the past four years in the Syrian civil conflict; as the war has dragged on, the Syrian government has become more and more willing to rely heavily on air strikes, artillery bombardment of cities, and even the repeated

use of chemical weaponry against suspected insurgents and their civilian supporters. This is a case where prolonged escalation in the level, type, and targets of violence was seen as a strategic necessity to counter unexpectedly powerful insurgent movements.

Returning briefly to the US Civil War, this principle of large-scale punishment was eventually adopted by the Union during the campaigns of General William Sherman, who embarked on a campaign of massive destruction in the Southern states during the latter half of the war. This type of ‘hard war’ [Murray, 2009, 47] was not morally or politically desirable to the North, but was deemed necessary in order to fully subdue the rebellion:

“The government of the United States has in North Alabama any and all rights which they choose to enforce in war — to take their lives [those of the inhabitants], their lands, their everything — because they cannot deny that war exists there, and war is simply power unconstrained by constitution or compact. If they want eternal war, well and good; we accept the issue and will dispossess them and [put] our friends in their place... To those who submit to the rightful law and authority all gentleness and forbearance but to the petulant and persistent secessionist, why, death is mercy and the quicker he or she is disposed of the better. [Satan] and the rebellious saints of Heaven were allowed a continuous existence in hell merely to swell their just punishment” ([Scott et al., 1901, p. 47] quoted in Murray [2009, 280-281])).

However, even in situations where highly intense violence is deemed necessary, the political and material costs of intense warfare limit its use over time, even within the same conflict. Paradoxically, a military campaign can become politically counterproductive at the peak of its strategic success; as soon as the perceived need for high levels of violence is gone, the violence itself becomes undesirable and not worth its cost in political legitimacy and material expenditures to continue [Conrad and Moore, 2010]. As a military campaign succeeds, pressure mounts to remove soldiers from danger; to change budgetary priorities to social spending and non-military purposes; and move from military conquest to peace-keeping operations. A lessening of insurgent violence as a result of a successful military campaign, in other words, may lower the perceived legitimacy and usefulness of that military strategy. Over time, as the perceived severity of the security threat diminishes,

states are likely to lower the intensity and scope of violent activities in order to avoid paying high costs for diminishing returns.

### **1.3.9 Insurgent behavioral updating**

Insurgents also adapt their military strategy during conflict, but the logic of strategic change facing insurgents is different from that facing states for several reasons, especially those that affect insurgents' perceptions of military efficacy versus the government. Actors that find themselves unable to take or hold territory, or actors that are unable to successfully challenge the state military directly, are both more likely to seek new strategic options that change the nature of the armed conflict in their favor. As discussed previously, states often respond to insurgent successes by escalating the level and scope of military force they employ. Insurgents generally do not have this option: these actors fight as hard as they can from the beginning, as they generally face a severe imbalance of capabilities relative to the government. However, insurgent actors enjoy a local informational advantage over the state — because they do not rely on the resources of a delineated geographic territory and do not field standing, uniformed armies, insurgents can fight a type of mobile war that may be less militarily damaging to the state but allows for greater chances at survival in the long run. When battlefield information reveals that insurgent actors are losing a war, they are more likely to adopt a less direct, less intense form of military strategy.

Actors that control territory can credibly promise protection from government retaliation in return for cooperation from civilians [Lilja, 2009]. This means that insurgent capability is a self-feeding cycle: actors that are strong enough to control territory can effectively extract rents from the local population, which in turn strengthens the group's fighting capability. Insurgents can benefit from controlling territory, but their very existence is not tied to territorial control. This means that insurgents can continue armed struggle without controlling clearly defined territory. However, state survival depends on exerting political, military, and social influence over some defined swathe of land. This means that giving up territorial control is not an option for the state, regardless of how badly the war is going; when a state abandons its territory, it by definition ceases to exist. The weaker link between group identity and sovereign territory is a key factor in

explaining how insurgents react to new battlefield information, and why their reactions are different from those of states. However, insurgents that are too weak to successfully fight the government over territorial control are limited in the range of tools they have to extract resources from the local population [De la Calle and Sánchez-Cuenca, 2011].

Insurgents who are facing losses in territory and lives from state military campaigns can (1) become too weak to maintain existing control over territory, and/or (2) be defeated in their attempts to take and hold new territory. In these cases, actors may be forced to use more violently coercive methods to extract resources from civilians. Actors prefer to use selective violence to extract resources and punish defection [Kalyvas, 2009], but to engage in this type of violence they need to exert some level of control over the local population. When this is impossible, actors resort to indiscriminate violence to raise their reputation and capture resources [Weinstein, 2007]. One commonly cited example of this behavior is the Revolutionary United Force (RUF) in Sierra Leone. The RUF was able to exert military and political control over distinct regions of Sierra Leone. However, the group later faced military setbacks that weakened its ability to take and hold territory, and transitioned to operate as a highly mobile guerrilla force with no distinct territorial holdings [c.f. Truth et al., 2004, 3A, 88]. These transitions correlated with a dramatic increase in violence directed at civilians. Furthermore, this change in behavior was viewed (by leaders and fighters in the RUF) as rational and effective: the higher the visibility and shock value of RUF violence, the greater their reputation as a fighting force to be feared, and the less resistance they could expect to see from civilians [Truth et al., 2004, 3A].

For weak actors in particular, violence against the civilian population is instrumental in two ways. First, as discussed above, it is one of the few tools these actors have to extract necessary resources from a civilian population that is not always willing to cooperate [Wood, 2010, Raleigh, 2012]. Second, weak actors need a way to stay relevant. An actor that is too weak to effectively engage the state in a military campaign is unable to point to its own political and military victories to raise its reputation and strengthen its position at the bargaining table. However, even weak actors are able to target civilians with relative reliability [Valentino et al., 2004, Downes, 2011]. By attacking civilians,

insurgent actors can exact indirect costs on the government [Hultman, 2007, 2009]. This type of violence sends a credible signal that the state is incapable of protecting its civilian population, lowering morale and potentially willingness to support the government [Pape, 2003]. As with states, insurgents can occasionally differentiate between ‘types’ of civilians by separating potential supporters from potential opponents in conflicts of identity such as ethnic or ethno-religious warfare.

When insurgent actors can identify potential enemies through simple heuristics such as ethnicity, language, or religion, they can deal direct damage to the state by targeting these civilians for violence [Fjelde and Hultman, 2013]. Much of the literature on terrorism during civil conflict deals with this type of issue: insurgent actors fighting a conflict of identity, if they are unable to effectively engage the state military, are likely to move their attention to civilians who are potential supporters of the state. Identity-based tactics aside, violence against non-military targets can also directly damage the state politically and financially. Creating a dangerous environment in a ‘war zone’ causes many civilians to flee: these civilians cease to be working, tax-paying citizens, and instead become internally displaced people (IDPs) who need support and protection by the government. Targeting infrastructure such as bridges, power plants, and commercial hubs directly damages the state’s revenue base. These tactics — purposely targeting civilians and non-military targets to damage the government — are often referred to as terrorism. An important note here is that terroristic behavior can be perpetrated by any militant group; terrorism is a tactic, not an identity, and occurs to a greater or lesser extent in many conflicts by many actors [De la Calle and Sánchez-Cuenca, 2011].

So, why do insurgents not target civilians immediately, and even focus on dealing damage to civilian targets to the exclusion of fighting the state directly? There are two reasons why civilian targeting is ‘second best’ as a military strategy, and is only adopted extensively as a contingency. First, violence is not a particularly efficient method of resource extraction. It takes armed fighters from the front(s) where they could be engaging the enemy directly, pursuing military and political goals more directly. Overall, reliance on targeting civilians rather than state military forces is a difficult choice for insurgent actors.

Fighting the government directly is risky and possibly disastrous, especially for insurgent actors that face a significant material disadvantage. However, the potential rewards are much greater: victories against government forces, even if they do not lead directly to an overall military victory for insurgents, send a strong signal to potential recruits and the population at large that the insurgent group is a viable alternative to the state, at least at the local level. This signal is not affected by the backlash that is associated with large-scale violence against civilians, meaning that military victories against the government are of high value to insurgents seeking to maximize their support base, their military power, and ultimately their returns on investment from fighting in the first place.

The literature strongly suggests that insurgent actors do not innately prefer to target civilians; as with states, the decision to attack innocents is a strategic and instrumental one. The tendency to attack civilians is linked strongly to the relative capability of the insurgent, and to insurgent battlefield success. Insurgents enjoying military success are more likely to devote their resources to engaging the state directly. When the insurgent is strong relative to the state, direct contests of violence against the state are faster and more likely to end in a decisive victory for one side or another [Balcells and Kalyvas, 2014]; this means that actors that are confident in their ability to achieve their goals against the state directly are more likely to spend their time and effort fighting soldiers, not killing civilians [Wood, 2010]. This means that ‘weakness’ is not merely an imbalance of capability. A materially weaker group can still experience local military success due to effective use of geographic shelter (both physical and human), inept state command structures, or underestimation on part of the government. I argue that changes in how insurgent actors treat civilians are not due just to ex ante measures of capability, but (in line with more recent scholarship) can be traced to short-term changes in the military fortunes of these actors. It is not just weak actors that are more likely to be violent towards civilians; it is any group that perceives it has no better options in the short run.

When the chances of success from a conventional military campaign are low or nonexistent, insurgent actors have a choice: leave the conflict altogether, or survive and stay relevant through whatever means are necessary. In these cases, insurgents who do not

want to leave the conflict are likely to turn to military strategies that emphasize direct targeting of civilians. Attacking civilians can lead to long-term deprivation as civilians flee the war zone, as well as short-term increases in defection and noncooperation by the local population. However, violence against civilians means insurgent survival: when there are no better alternatives, many actors will prey on civilians because it is the only way to remain relevant in the military and political bargaining process of conflict.

Additionally, insurgents are (in the vast majority of cases) significantly weaker than states in terms of their resource base. States have the organizational structure to engage in geographically or demographically ‘selective repression’: directing high levels of repressive violence at an actor or actors of civilians who are deemed likely supporters of insurgent actors, while maintaining relatively gentler control over the rest of the population to gather tax revenues and other forms of rent that pay for the war. Insurgents, on the other hand, often rely totally (or near-totally) on the willing or coerced cooperation of the civilian population. This means that insurgents also face a quandary in how they treat civilians: too much violence and they risk betrayal or backlash, but too little violence and the civilian population will side with the government out of fear.

When faced with military setbacks, insurgents are also likely to change the location of where they engage in violence. A major theme in more recent work on civil war violence is the creation of meaningful ways of describing locations and differentiating them from one another when trying to explain where, why, and how events occur. A common geographic descriptor is the relational location of a given point on the map: a location’s position relative to other politically, socially, or militarily important places. For example, we can describe a location by measuring its distance to the state’s capital city, or to the nearest international border. These relational measurements are useful in that they serve as proxies for unobserved spatial features such as the ability of states to effectively project force within their borders. Capital cities are the ‘nerve centers’ of states [Quackenbush, 2015]. Political leaders, major government institutions, and communications networks are based in the capital, meaning that the lines of supply and communications between state leaders and state military forces are shorter close to the capital city. This is essentially an

argument about projection of military force, traditionally an area studied by international conflict scholars [Boulding, 1962, Webb, 2007]. However, for many states that are weak enough to experience civil conflict in the first place - meaning those who are incapable or unwilling to deter dissidents without major violence - the ability to project force varies within borders, not just internationally. These states face difficulties projecting force into isolated areas, usually because the state itself has trouble penetrating into these areas.

Isolated locations are likely to experience less state presence overall. State presence can take many forms, including physical infrastructure (roads, power lines), public goods (healthcare provision, schooling), and political presence (tax collection, crime prevention). Where state presence is lower, cooperation with state forces can also be lower. Citizens who do not have regular, institutionalized contact with state government are less likely to trust or actively support the state because they have not been socialized to do so; lack of government integration also means that informal or parallel systems of public goods provision, commerce, and security arise to meet local needs, meaning that the state has less of a foothold in obtaining information and resources from the local population. This lack of presence poses two types of problem for government forces.

First, militaries require physical infrastructure to operate well. Lines of communication and transportation have to be kept open and protected; fighters and military technology have to be fed, maintained, and serviced. In isolated areas where the government has little local infrastructure, the state faces difficulties supplying its fighters, maintaining communication lines between command and the front line, and even simply moving large numbers of fighters and machines from one point to another. An extreme example of this problem is the United States in Vietnam; even as a global superpower fighting a weak and low-tech enemy, the US faced tremendous logistical and organizational problems simply moving its military forces to the combat zone; once they got there, local communication, supply, and transportation became even more complicated due to the rough terrain and lack of infrastructure usable by a major international fighting force.

Second, states face a major problem gathering useful information in isolated areas. Locally sourced information is tremendously important when conducting counter-insurgency

operations, since local residents are likely to have useful intelligence on insurgent identities, movement patterns, bases, and so on. However, in regions with little state presence, it can be difficult for the state to find and convince (or coerce) the local population to provide this type of information. For example, Russian counter-insurgency forces in Chechnya were constantly frustrated by a lack of cooperation by the local population because the locally-based insurgents could easily find and punish civilians who were suspected defectors. The local insurgents had the ability to make effective, selective, and credible threats against defection, because they were highly integrated with the local population. Because the Russian state had little presence in these areas, Russian forces could not effectively make these types of specific threats and promises, and so (in the early years) instead relied on indiscriminate violence to punish noncooperative locals and (hopefully) kill or dislodge local but unknown insurgent fighters. It was not until Russia was able to deploy locally-recruited counterinsurgent fighters that it was able to effectively engage with Chechyan militias [Lyall, 2010]. Weaker insurgents who cannot effectively face state forces in direct combat are not only less likely to target military forces overall; they are also less likely to engage in any violence in areas where the state's military presence is relatively strong [Buhaug, 2010]. Instead, weaker insurgents, or those who are facing short-term military defeats, are likely to move away from areas of state strength: in this case, that means moving out into more isolated areas where the state's reach is less strong.

Why don't all insurgents operate in the periphery? If local victory is likely in areas where the state is weak, we might expect that insurgents of all stripes would operate primarily in these locations. The reason that insurgents might prefer, *ceteris paribus*, to operate in the political interior of a state rather than isolated or far-flung regions is that successful acts of violence in these locations, regardless of target, are likely to be more visible and more important in strengthening their bargaining position. Given that insurgents are nearly always weaker than states, especially in the early stages of a civil conflict, their goals are often to maximize the indirect impact of violence. While marching on the capital city and overthrowing the state may be a pleasant thought, it is simply not possible for most insurgent actors. However, successful acts of violence

perpetrated in or around major cities can have a tremendous impact on the visibility, recruiting capability, and perceived power of an insurgent group for two reasons.

Insurgents also face choices in the type of weapons and military technology they rely upon. Here, too, there is an obvious imbalance between insurgent forces and those of the state. Even weak states (in some ways, especially weak states) spend significant portions of their budget on military armaments. This means that state forces have access to high-tech weaponry such as armored vehicles, artillery, and transport and fighting aircraft. Insurgents, by contrast, often have to make do with much smaller-scale arms. However, the modern era has significantly leveled the technological playing field in some areas. The miniaturization and concentration of explosives has allowed insurgents from Palestine to Sri Lanka to field powerful, hard-to-detect bombs and incendiary devices. The spread of communications technology in the form of cell phones and the Internet has allowed these devices to become increasingly sophisticated and easy to operate from cover or safe distances. Small arms have become more powerful and much more widely available, from reliable automatic weapons to shoulder-mounted missiles and light artillery. This means that while insurgents still generally face a technological imbalance vis-a-vis the state, they still generally have some variety in the set of military options available.

The use of direct-fire weapons and direct engagement with enemy forces is most commonly associated with a conventional war: two armed actors meet each other in close proximity, and engage in direct, targeted violence until losses force one side to quit the field. This type of action is relatively decisive: losses are high, usually highest on the losing side, and the outcome is relatively clear to see in terms of bodies and territorial control. Indirect-fire weapons and indirect tactics, however, are much safer for the weaker party. Instead of launching a coordinated assault on a military base, insurgents can attempt to drive a vehicle-mounted IED into an undefended area; they can launch a barrage of mortar or light artillery rounds from a safe distance; or they can wait for patrols to leave and use remote-detected explosives or improvised land mines to inflict casualties against a less well-defended enemy force.

When faced with overwhelming state force, insurgents are likely to rely more heavily on

indirect-fire weapons and tactics. Note that the use of this technology is not the same as the choice to systematically target civilians versus state forces; indirect-fire and direct-fire weapons alike can be used to target both civilians and enemy military personnel. It is true that these technologies often go hand-in-hand with choice of target (see for example the Palestinian campaign of suicide bombing during the Second Intifada in the early 2000s), but the mechanisms themselves are distinct from one another. The Irish Republican Army, for most of its most intense years of fighting, claimed to exclusively target military and police personnel for bombing attacks - whether or not this was actually true is up for debate, but the vast majority of IRA-inflicted bombing casualties were in fact security forces.

Why do insurgents not focus exclusively on the use of indirect-fire weapons? They are safer, arguably more destructive in terms of loss of life and property damage, highly visible to both civilian and media observers, and often require a relatively low outlay of resources to obtain and use. There are two caveats to the use of indirect-fire weapons, both of which affect the calculus of risk versus reward for insurgent forces. First, indirect-fire weapons are spectacular but not always effective. IEDs, mortars, rockets, and so on can potentially cause large-scale damage, but they most often do not; this is because they tend to miss, misfire, backfire, and otherwise fail to do the right job at the right time. Not only does this mean that the goal of violence — to inflict losses and damage on the enemy by the destruction or elimination of some target — goes unmet, but it can cause significant backlash as well. As discussed above, the use of violence against civilians is tricky. Targeting civilians for violence can lead to an upswell of resistance against both insurgent and state forces, which can be much more dangerous for weaker insurgents who rely on the cooperation or coercion of local civilians to survive. Even when actors do not consciously target civilians, the use of inaccurate or unreliable indirect-fire weapons means that civilians may be accidentally hit or caught in the area of destruction. This can lead to rapid shifts in public opinion against insurgent actors, particularly in cases where the actors claim to be trying to avoid civilian casualties. In these cases, civilian deaths are not just signs of insurgent malignancy; they are signs of incompetency as well, which is

doubly damaging to the level of public support for an insurgent movement.

Second, indirect-fire weapons alone do not win military conflicts. Due in large part to their low accuracy and reliability, they do not exact a significant toll in resources and personnel compared to a campaign of direct fighting against the state. Instead, they are used to wage long-term, low-intensity campaigns designed to undermine morale and the will to fight. The successful use of indirect-fire weapons against some state or state-aligned target can provide credibility to an actor's claims to stand against the state, and it can undermine support for the government that is unable to successfully fend off these attacks, but indirect violence alone will not lead to military victory.

Even in cases where insurgents choose to target civilians for violence, it can be difficult to achieve goals through the use of indirect-fire weaponry. Looking again at Palestinian insurgents, the long-running campaign of rocket attacks against civilian targets in the Israeli Negev has done little to directly forward the military aims of Hamas or any other group, as they are so inaccurate and unreliable that they only rarely cause casualties or even significant property damage. However, gains from this type of violence can be counter-intuitive: the Israeli government has repeatedly used rocket attacks as a pretext for military incursions into the Gaza Strip, and Palestinian insurgents in turn use these incursions and the destruction they inflict as a recruiting tool to gain and motivate new fighters. However, this has not necessarily led to a long-term increase in support for Palestinian insurgent actors to the point where larger-scale mobilization has occurred in the Palestinian territories, or political concessions have been gained from the Israeli state.

Indirect-fire weapons, like targeting civilians or fighting in areas where the state is weak, are the weapon of choice for insurgents who are not just weak in terms of capability, but who are faced with no better options. Actors that would otherwise lose a conventional or high-intensity war, but do not want to give up or quit the fight, are likely to transition to a form of conflict that is highly indirect: instead of engaging government forces directly, they transition to a campaign of bombing, mining, shelling, and other forms of violence that do not expose insurgent fighters to direct risk, even if this means using a form of violence that is less likely to lead to rapid resolution or military victory.

Insurgents, like states, have a preferred strategy when they enter a conflict. Like states, their preferred strategy generally involves fighting as efficiently as possible. Whether their goals involve taking over government, creating a politically autonomous homeland, or simply increasing political power for the group they represent, insurgent forces will devote an ‘appropriate’ amount of force. However, for insurgents this calculation of ‘appropriate’ force is quite different from states. If an insurgent group can strike hard and quickly, it accomplishes several goals. First and foremost, it may gain its ultimate goal right away; an unstable government may topple, and a stable government may come to the bargaining table before incurring more losses. Second, even if the group’s primary goals are not accomplished in the first bouts of fighting, it shows that they are a serious threat. By doing so, the group can send a signal to potential supporters or allies that it can stand effectively against the government; ideally, this will attract new recruits from other disaffected areas of society to fight as well, creating a self-sustaining organization of violence.

Actors faced with steadfast government resistance, and taking heavier losses than expected, find themselves in a strategic quandary. Fighting a long-term conventional war is rarely a successful solution for insurgents, and winning a war of attrition is nearly impossible. Therefore, when faced with government forces that refuse to yield in the first throes of conflict, and facing a civilian population that is not mobilizing at a satisfactory rate to swell their ranks, insurgents are likely to change their strategy to one that is significantly less conventional.

By changing to a less conventional strategy, insurgent actors essentially hope to bleed out their opponent over time. Slantchev [2004] quotes US government reports generated out of Vietnam that assess the Vietnamese strategy: the goal was to minimize Vietnamese casualties, but do enough damage to force the US to escalate their military involvement. The more involved the US became in the Vietnam war, and the longer the conflict dragged on without clear victory, the greater the erosion of US domestic support for the war ([Karnow, 1983, 519], quoted in [Slantchev, 2003, 130]). By fighting a long-running, low-intensity conflict, Vietnamese insurgents were able to minimize their own casualties, thus preserving their fighting capability, while inflicting enough casualties on US military forces

to undermine their political will to fight.

### 1.3.10 Decision-making and unitary actors

In the previous sections, I have consistently used the terms ‘state’ and ‘insurgent’ when discussing the key actors involved in conflict. This theory explicitly assumes that for a given conflict, there is *one* state actor and *one* insurgent actor, and that these actors are unitary in their goals and decision-making processes. In other words, any given conflict involves two unitary actors, each of whom rely on one overarching military strategy at a time. Needless to say, this is far removed from reality in many conflicts. Multiple factions are common in the messy and chaotic environment of civil conflict, as multiple groups mobilize to pursue their own goals; furthermore, the weak states most likely to experience civil conflict are also often more likely to experience factionalization within their own forces.

Overall, in many cases this is a more defensible assumption for states rather than insurgent groups. States are by their very nature hierarchical structures with centralized decision-making, to the point where a lack of clear centralized leadership is a key sign of a failed or failing state. While locally disaggregated decision-making does exist in military hierarchies, and indeed is often seen as desirable [c.f. Fitzsimmons, 2012, Ghosh and Lee, 2000], state forces still take orders from central command and act in general accordance with an overarching military doctrine. As such, the assumption that the state is a unitary military actor with members that act according to a centralized strategy is generally not far from the truth.

For insurgent forces, however, this is often a much more heroic assumption. A single civil conflict may spawn dozens of different splinter groups, copycats, rivals, and other minor actors pursuing their own goals [Bormann and Hammond, 2015]; even a single larger, more cohesive group may disaggregate decision-making responsibility to the local level, either explicitly by adopting a cell-based organizational structure, or implicitly through a lack of centralized C&C capabilities.

However, this is a simplifying assumption I make — to a greater or lesser extent — in the following chapters. I make this assumption for both theoretical and pragmatic reasons.

Theoretically, while it is true that civil conflicts often involve multiple insurgent groups, there are reasons to expect that more coordination and control exists than might meet the eye. Even in the chaos of civil conflict, insurgent groups exert some level of control over their members; indeed, a base level of internal cohesion is required just to survive against an often stronger state opponent. For example, consider the Revolutionary United Front (RUF) in Sierra Leone. The conflict in Sierra Leone is a prime example of complete societal breakdown; RUF fighters, state military forces, and civilian self-defense forces fought each other at various levels of intensity for over ten years. The RUF engaged in widespread acts of terrorism and violence against civilians Richards [1996], actions which are theoretically linked to lack of cohesion and centralized control [Weinstein, 2007]. It operated across a large stretch of thick jungle terrain, attacking targets of opportunity and evading detection and engagement by the state [Richards, 2003]. In other words, the RUF was far from a traditional military group in both its structure and its activities.

However, this does not mean the RUF was not internally organized. Interviews with former fighters describe a system of radio command based out of the RUF's hidden headquarters, the 'Zogoda', that allowed leader Foday Sankoh to regularly contact his forces, issue orders, announce major operations, and otherwise set doctrine for his various fighting groups around the country [Peters and Richards, 1998]. Many former fighters claim that the RUF's treatment of civilians, characterized by brutal violence and theft, was not necessarily the actions of drug-addled criminal gangs, but a doctrine set by Sankoh. Clearly, this is not a cut-and-dry example of centralized control; communications linkages between leadership and fighting cells does not necessarily indicate a hierarchical, centralized command system. However, the RUF is a useful illustration: even in the middle of a chaotic, multi-sided civil conflict, insurgent leadership was able to exert significant levels of control over how, where, and when their forces engaged in violence.

There also exist cases where information-sharing can actually be detrimental; actors who know 'too much' can be captured, interrogated, and even 'flipped' to informants by state forces. In these cases, insurgent groups can adopt horizontal or cell-based organizational structures. The strength of these structures is that they are robust to the

removal (capture or death) of any given actor in the organization, or even whole cells; the downside is that coordination within and between cells can be difficult due to the barriers to information-sharing. In these cases, can the unitary actor assumption still hold?

I argue that it can, although again with some caveats. Cell-based organizations are still organizations; they have common interests and goals, despite their lack of day-to-day contact, and setting the methods for reaching these goals still requires coordination and centralized decision-making. While day-to-day operations may be performed independently, cells in a distributed organization still require periodic contact to receive orders and information from decision-makers. In Chapter 3 of this dissertation, I examine the strategies adopted by Republican paramilitary groups during the Troubles in Northern Ireland. The largest group involved in this conflict was the Provisional Irish Republican Army, or PIRA, which was responsible for the vast majority of violent events during the three-plus decades of violence. For much of this period, the PIRA was set up as a cell-based system, in which local groups (termed ‘Active Service Units’, or ASUs) were responsible for carrying on the political struggle in their own neighborhood, town, or county [Drake, 1991].

However, significant decision-making power was centralized within the PIRA’s Army Council, an elected group of activists who met semi-regularly to decide on issues of military and political strategy. While this system was ‘decentralized’ in that there was no single leader, the Army Council was responsible for directing PIRA operations, which was communicated throughout the various cellular entities. These decisions could be quite granular in nature, down to planning and target selection for individual, particularly high-profile attacks [Horgan and Taylor, 1997]. The PIRA is not a perfect or ideal-type case, but it does provide additional real-world support for this assumption of unitary insurgent actors. Even in a cellular organization carrying out a highly nonconventional military strategy over a period of decades, central planning and coordination played a major role.

I argue that the unitary-actor assumption is not a tremendous stretch when examining individual insurgent groups, even in the low-information environment of civil conflict.

In the common case where multiple insurgent groups are engaged in conflict, however, this assumption becomes more difficult to defend. The two conflicts I analyze in the next two chapters both involve multiple insurgent groups. In the Palestinian Territories, Fatah's Tanzim fighters operated in the same conflict space as Hamas, Islamic Jihad, and the People's Liberation Front of Palestine. In Northern Ireland, the Provisional Irish Republican Army bore the brunt of fighting, but other factions such as the Real Irish Republican Army and the Irish National Liberation Army also engaged in significant acts of violence. In both cases, insurgent groups engaged in conflict against the same state, but inter-factional competition could and did erupt into violence. When multiple actors, each presumably with different goals, capabilities, and incentive structures, are present, how can one defend the decision to roll them all up under one 'insurgent' label? There are two reasons why I believe that this assumption can *in general* be worth making, and in the *specific cases* I study in the next two chapters, is worth defending.

The first reason that this assumption can be safe to make is that the presence of multiple groups does not necessarily mean (1) constant competition or (2) lack of coordination between groups. For example, while the major Palestinian insurgent groups active during the Second Intifada competed with one another for media attention and popular support [Clauset et al., 2010], they also regularly coordinated their activities. During this period, fighters from multiple groups engaged IDF forces during incursions in cities like Gaza and Nablus; when going on the offensive, it was not uncommon for two or more groups to collaborate in the planning and execution of terrorist attacks within Israel [Jones, 2007, Hunsicker, 2006]. Some groups were also interconnected at the individual level: one highly active group, the Popular Resistance Committees (PRC) was largely comprise of current and former members of different insurgent groups. Likewise, many individual fighters, including senior insurgents, were members of more than one insurgent group simultaneously. Overall, not only did these groups coordinate their military actions (albeit sometimes more closely than others) but lines of separation were blurred by fighters with multiple affiliations. This does not paint a picture of multiple groups operating in total independence of one another. As such, while I recognize that these groups often differed

in their views of how to prosecute the military campaign against the Israeli state, I argue that the loss of information generated by aggregating their actions does not outweigh the analytic traction gained by doing so.

The second reason for making the unitary-actor assumption in this analysis is that my theory generalizes across insurgent groups. The core logic of my theory is that, even in cases where insurgent groups do not necessarily coordinate with one another, they should respond in roughly the same way to changes in the conflict environment. In other words, it is not necessary for Hamas and Islamic Jihad to share information and coordinate with each other for them to respond to changes in IDF strategy, nor is it necessary for the PIRA and INLA to coordinate with one another to respond to British violence. My theory assumes that there are general patterns in how insurgent groups and state forces respond to one another; while these patterns of behavior are certainly not uniform across all insurgents, or even across all states, I assume that the basic action/reaction patterns between these actors are the same. The insurgent groups in the cases I study operated in the same geographic areas, had access to many of the same sources of funding and supply, and drew on the same demographics for support and recruitment in their struggle against the state. In these cases, it is not a tremendous leap of faith to combine the actions of different insurgent groups together when analyzing their responses to state aggression or other forms of battlefield information.

All this being said: there are certainly cases, both within and between conflicts, where disaggregating actions by actor or group is necessary or beneficial to analysis. In Chapter 4, I engage in a cross-sectional analysis of African civil conflicts in the past two decades. In many of these conflicts, dozens of insurgent groups spring up, many of which have fundamentally different goals or means of carrying out violence. In these cases (which I discuss in more detail in Chapter 4) I engage in selective disaggregation, separating out the main actors involved in a given conflict episode rather than simply aggregating every non-state group into the same ‘insurgent’ label. Overall, the unitary-actor assumption is one that I find defensible in some cases, but untenable or unhelpful in others.

## 1.4 General Hypotheses

The theory laid out in this chapter gives rise to several general, testable hypotheses regarding state and insurgent military decision-making. While these hypotheses can be modified or tailored to specific conflict cases, they provide a basic framework of expectations for how insurgents and states react to changes in the conflict environment over time.

The first set of hypotheses have to do with the type and timing of military-strategic change. As discussed in this chapter, both states and insurgents respond to new information from the battlefield, both in the form of success/failure metrics (such as casualties) and in direct observation of changes in the opponent's strategy. In general, I expect that:

H1 Conflict actors are more likely to change their military strategy when casualty ratios favor the opponent.

H1 Conflict actors are more likely to change their military strategy following a change in military strategy by their opponent.

H1 An increase in the level of civilian targeting by a conflict actor is more likely following an increase in the level of civilian targeting by their opponent.

H1 As states adopt more indiscriminate or high-intensity military strategies, insurgents are more likely to adopt less conventional or lower-intensity military strategies.

Chapters 2 and 3 build on this framework for the specific cases of Israel/Palestine and Northern Ireland, showing how the general expectations laid out in my theory can be tailored to the unique dynamics of individual conflict cases. These tests focus on the short-term dynamics of conflict updating, analyzing the week-to-week and month-to-month behavior of conflict actors.

My theory suggests that the ability to quickly and effectively update military strategy is linked to longer-term conflict outcomes as well as short-term dynamics. My second set of hypotheses have to do with the links between military-strategic updating and conflict termination and outcome.

H2 Insurgents are more likely to avoid defeat in conflict when they can update their military strategy more quickly than states.

H2 Conflicts are likely to last longer when insurgents can update their military strategy more quickly than states.

H2 The longer it has been since an actor updated its strategy, the more likely a conflict is to come to an end.

Chapter 4 applies these hypotheses to a cross-sectional data set of conflict episodes covering 45 conflict episodes in 27 African countries over the 18-year period between 1997 and 2015. In particular, I expect that actors who can quickly update their strategy are likely to survive longer and achieve better conflict outcomes. I also expect that the lack of military-strategic updating on both sides is a predictor of conflict conclusion: the winning side has no need to update their strategy, while the losing side is unable or unwilling to engage in further updating, so conflict should come to an end relatively quickly.

## 1.5 Conclusion

Why do materially weak insurgents succeed? Why do so many asymmetric conflicts last longer than we might expect? Our current set of theoretical models generally frame conflict as either (1) a one-off test of strength, in which the stronger party wins, or (2) an ongoing process of violent bargaining that serves to uncover the true balance of capability — again, leading to victory by the stronger party. Neither of these models does a satisfactory job of explaining the duration and outcome of civil conflict. I argue that weaker actors can ‘punch above their weight’ by (1) employing an effective military strategy, and (2) updating their military strategy based on information from the battlefield. Insurgents who can fight at a military-strategic advantage can avoid destruction and inflict greater damage on the state. These groups enjoy more short-term battlefield success and, ultimately, a greater chance of surviving and achieving a better outcome at the end of a conflict.

In the following chapters, I analyze the mechanics of military-strategic updating and its effect on the duration and outcome of conflict. In Chapter 2, I conduct an in-depth

case study of the Israeli-Palestinian conflict, focusing on the Second Intifada period from 2001 through 2004. I use an original data set of over 17,000 violent events during this period to examine the type, target, and location of violence employed by both Palestinian insurgents and the Israeli military, analyzing patterns in how both sides respond to the others' actions over the course of the conflict.

In Chapter 3, I conduct a similar analysis on the Troubles in Northern Ireland over a 32-year period from 1972 through 2004. Using georeferenced data on a comprehensive data set of over 3,000 deaths attributed to political violence during the Troubles, I analyze the type, target, and location of violence employed by Republican and Unionist paramilitary groups, as well as the British state. Here, I look again at patterns of mutual military-strategic updating, and how changes in the strategies of violence employed by state and non-state actors correlate with one another over a much longer period.

In Chapter 4, I move from single cases to a cross-national analysis. Here, I use event data covering 15 years of conflict on the African continent, constructing measures of military strategy for some 45 episodes of civil conflict during this time period. In this chapter, I move from the mechanics of short-term updating to the longer-term effects of military-strategic updating on the duration and outcome of conflict. Using a larger sample of conflicts allows me to compare actors, analyzing whether the ability to quickly and effectively update military strategy has a discernible effect on how long conflicts last and whether insurgents are able to achieve their goals — or, at the very least, survive against a stronger state.

# Chapter 2

## Strategic Adaptation in the Second Intifada

### 2.1 Introduction

In September of 2000, Israeli opposition leader (and later Prime Minister) Ariel Sharon made a public visit to the Temple Mount, a location in Jerusalem containing holy sites of both Judaism and Islam. Massive protests broke out after Sharon's visit, which was interpreted as a deliberate provocation and claim of Israeli supremacy by Palestinian leadership, and were violently suppressed by Israeli security forces [Pressman, 2006a]. This pattern continued for months, with increasingly violent Palestinian protests met with increasing levels of Israeli force, until the conflict metastasized into direct use of military force on both sides. Violence would continue for four years, through the first months of 2005, and end with a shaky ceasefire between Israel and the Palestinian Territories.

Why the Temple Mount visit sparked this massive upswing in violence is still a matter of some debate. Israeli authorities have accused Yasser Arafat, then leader of the Palestinian Authority, of planning and orchestrating a surge in militant violence intended to force the Israeli government to uproot its presence in the Palestinian Territories. Palestinian leadership was displeased with the breakdown of the Camp David talks earlier that year [Pressman, 2006b], and may have been seeking a motivating factor to justify a more violent approach. Palestinian leaders accused the Israeli government of forcing the issue through blatant provocation, as well as unnecessarily violent tactics against Palestinian protestors,

which caused many injuries and several deaths in the first few weeks.

The Intifada was at first a wave of public unrest: large crowds staged marches and protests in various parts of the Gaza Strip and West Bank protesting Israeli occupation. As these clashes increased in size and severity, the retaliation by the IDF grew as well, leading to an escalatory spiral of violence [MEP, 2001]. The conflict became more distinctly militarized in 2001, as Palestinian insurgent groups mobilized to launch a series of attacks on Israeli targets, both military and civilian. This period also saw significant adoption of suicide-bombing tactics by the Palestinian groups, targeting Israeli civilians to incite terror and damage Israeli morale [Moghadam, 2003]. This escalation continued through early 2002, when Israel launched ‘Operation Defensive Shield’, a major combined-arms incursion into the West Bank intended to break Palestinian resistance by seizing weapons caches and capturing or killing insurgent leaders.

The following year was characterized by widespread clashes in the Gaza Strip and in large cities and refugee camps in the West Bank. Palestinian militants continued to launch suicide bombers, rockets and mortars across the Israeli border, hoping to inflict enough political and morale damage to force more favorable negotiations. The IDF continued to launch combined-arms raids into the Palestinian territories to bulldoze buildings it claimed were being used by snipers, destroy weapons factories, and capture militants [Graham, 2008]. Israeli leadership also adopted a controversial policy of ‘targeted killings’ against known militant leaders [Gross, 2003]. However, suicide bombs continued to claim Israeli lives despite security measures and crackdowns. In late 2003, spurred largely by the threat of suicide bombing, the Israeli government began the West Bank Barrier project, erecting a series of walls, fences, and checkpoints designed to physically limit access between Israel and the West Bank territories.

Construction of the wall continued into 2004. By this point, attrition of militant leaders, increased Israeli military presence in the Palestinian territories, and the physical presence of the Barrier was making it harder for Palestinian groups to operate freely and coordinate further attacks into Israel. On the Israeli side, political exhaustion with the ongoing conflict was increasing, thanks in part to domestic and international media coverage of

death and destruction in the Palestinian Territories [Deprez and Raeymaeckers, 2010]. In mid-2004, prime minister Ariel Sharon declared a policy of unilateral disengagement from the Gaza Strip, removing all settlers and abandoning settlement blocs along the Mediterranean Sea.

In conjunction with protective measures in the West Bank and disengagement in the Gaza Strip, military action against Palestinian militants continued. Hamas leader Sheikh Ahmed Yassin was killed by the IDF in March of 2004, severely weakening the organization's leadership structure. In November of the same year, Palestinian leader Yasser Arafat died, depriving the Palestinian people of a leader who had been central to their struggle for decades. In early 2003, the appointment of Mahmoud Abbas as Prime Minister installed a leader who emphasized the importance of a coordinated truce between Israel and the major Palestinian militant groups. In February of 2005, Israeli and Palestinian leaders met at the Sharm El-Sheikh Summit and solidified a truce between the Palestinian National Authority and the State of Israel. While violence never fully died down, this is widely considered to be the point where the Intifada period of violence came to a close.

The strategic challenge that has always faced Israel is how to deal with militants that blend in with the civilian population. The IDF has the firepower to easily overcome Palestinian forces in an open fight, which is why Hamas and other organizations almost never engage in large-scale confrontations with Israeli forces. This means that any IDF operation comes with a virtual guarantee of civilian casualties; higher levels of force will be more effective at killing insurgent operatives, but will also provoke greater international outcry and potentially mobilize further recruitment into these same militant groups. The strategic challenge Palestinian organizations like Fatah, Hamas, and Islamic Jihad is the inverse: how to effectively engage with a much more powerful opponent in a way that (1) allows these groups to maintain political and military relevancy, while (2) not provoking or succumbing to overwhelming military retaliation from the IDF.

## 2.2 Identifying strategic updating

My goal in this chapter is to identify when, how, and why Israeli and Palestinian groups changed their wartime behavior in order to fight each other more effectively. Through a detailed analysis of both sides' activities over this four-year period, I identify how both the IDF and the various Palestinian militant groups adapted to each other's military strategy. I am interested in the shifting patterns of violence that characterized this period of unrest: how and why both sides made the choices they did regarding how to attack one another, and what effect these choices had on the course of the conflict. Looking at the actions and reactions of both Israeli and Palestinian forces during this period may provide some important information about how violence can vary even in a highly unconventional, contained area like Israel and the Palestinian Territories. I derive and test several specific implications from my theory of rational wartime adaptation, and propose that specific patterns exist in how and why armed actors change their conflict behavior. I set out to show the existence and endogenous development of military-strategic shifts in the specific case of Israel and Palestine, and draw broader lessons about the behavior of states and insurgent groups. By doing so, I can provide evidence that this pattern of action-reaction in strategic innovation exists in conflict situations, and that with the right instruments, this type of behavior can be captured through quantitative empirical studies.

This analysis benefits our knowledge of civil conflict in two ways. First, the ability to explain and predict strategic shifts during conflict allows potential interveners to target locations and populations that may be at increased risk in the future. Both humanitarian efforts and military interventions can benefit from an informed picture of the present and the near future in terms of how groups are likely to employ technologies of violence, where violence is likely to increase, and what populations are at highest risk of violence. Furthermore, uncovering patterns of action and reaction in strategic adaptation can inform counterterrorism efforts by showing how unconventional forces respond, over both short and long term, to efforts by the state. This may provide tools and foundational knowledge as to how to better craft counterinsurgency methods in the future.

During the Second Intifada, both the Israeli state and the various Palestinian groups

struggled to develop a military strategy that would allow them to effectively pursue their political goals while preventing the opposing forces from doing the same. Hamas, Islamic Jihad, and other Palestinian insurgent groups innovated in their use and construction of bombs (both planted and human-borne), and developed indirect-fire weapons such as mortars and rockets. Besides the major strategic shift embodied by the Barrier itself, Israel developed programs of targeted strikes against known or suspected insurgent leaders in order to maximize damage to insurgent infrastructure while minimizing civilian casualties, and conducted major operations to physically change the combat landscape and deny Palestinian fighters cover and civilian surroundings near conflict hotspots.

This process of strategic innovation is highly endogenous. These actors did not develop their military strategy in isolation, but through a process of trial, error, learning, and estimation based on changing circumstances on the ground. A successful innovation by one side would redouble innovation efforts on the other side in attempts to counter this new strategy. I argue that changes in Israeli and Palestinian strategy intermingled over time, as both sides sought short-term military advantage over the other.

What is a ‘shift’ in military strategy? This is actually more difficult to identify in a quantitative framework than might be expected at first glance. Qualitative accounts are useful for identifying major changes in the flow of a conflict: for example, Operation Defensive Shield represents a major change in the level and type of force the IDF was willing and able to use against militants in the Palestinian Territories. However, many strategic innovations are harder to detect, especially without the benefit of historic hindsight. An armed conflict is a noisy, chaotic informational environment, and this goes double for civil conflicts that may involve multiple groups and blurred lines between civilian and military actors. This means that the empirical record of violence is also highly noisy, and identifying clear trends or change points in behavior over time can be very difficult without the use of sensitive instruments.

I take a quantitative, event-based approach to identifying shifts in military strategy. This approach allows me to identify shifts in military strategy by examining the behavior of the actors involved, rather than relying on historical accounts detailing the inner workings

of group decision-making during the conflict. This is not to say that this approach is identical to, or a replacement for, an in-depth qualitative analysis of the conflict. Instead, I argue that this approach is useful because it requires relatively minimal ‘classified’ information. As long as we can track events over time, it is possible to identify shifts in military-strategic planning through shifts in actor behavior. Because of this, a quantitative approach may be useful in cases where information is limited due to the recent or ongoing nature of a conflict, or for conflicts where extensive source material is not available to researchers. By conducting an event-based analysis of a well-known, highly documented conflict like the Second Intifada, I can give some indication as to its use in other cases.

## 2.3 Hypotheses

In this section, I lay out some specific expectations drawn from my theory of military-strategic updating during conflict. First, I expect that both sides will respond to information about the conflict as a whole. Specifically, I expect that both Israeli and Palestinian actors will track casualty trends, and respond when they perceive that the tide of the conflict is turning against them. In this analysis, I focus on casualty ratios rather than overall counts. I believe this to be more appropriate, given that for both sides some level of losses was deemed acceptable, or at least expected. However, both sides attempted to minimize their losses relative to one another. While both sides were willing and able to incur losses in order to achieve their political goals, a major shift in the rate of losses relative to the opponent serves as a strong signal that something has changed and that reassessment or change of strategy may be necessary.

Although previous work has examined fatality counts to identify action-reaction dynamics in the Second Intifada both between Israeli and Palestinian actors [Paserman and Jaeger, 2006] and within the various Palestinian factions themselves [Jaeger et al., 2010], I am looking at shifts in the casualty ratio rather than simple week-to-week values because this is more informative given the dynamics of this particular struggle. The casualty ratio throughout the Second Intifada was highly skewed towards the Palestinian side: during the entire period in this data set, 60-90% of dead and wounded each week were Palestinian. Clearly, Palestinian insurgent strategy has never depended on inflicting more casualties on

the Israeli population than the Palestinian population endures. However, looking at shifts in the casualty ratio from week to week may explain more shifts in Palestinian behavior.

Data on fatalities during the Second Intifada has already been gathered by multiple state and nonstate actors. However, I have also gathered figures on wounded on each side. This allows me to track trends in non-fatal casualties, and how each side responds to injuries short of death inflicted by the enemy. In this analysis, I combine dead and wounded into one overall casualty measure, producing a metric of Palestinian to Israeli casualties per week in terms of both dead and wounded, military and civilian personnel on both sides<sup>1</sup>. In future work, I plan to disaggregate wounded and dead on both sides to see how groups react to violence that causes damage (in the form of wounds or property destruction) but does not lead to fatalities.

From the theory of rational adaptation laid out in the previous chapter, I derive two specific hypotheses. On the Palestinian side, I expect that a shift in the casualty ratio regime in favor of the Israelis (an increase in the number of Palestinian dead and wounded for every Israeli casualty) sends a signal that the current strategy is ineffective. No Palestinian militants have hoped to defeat the IDF through direct attrition, but inflicting Israeli casualties is still crucial to their goal of increasing political costs on the Israeli government. If Palestinian militants are unable to carry out successful attacks on Israeli targets but are still incurring losses from Israeli incursions and strikes, they are unlikely to fulfill their long-term goals and risk losing political and military relevance. I expect that Palestinian militants will respond to a positive shift in the Palestinian/Israeli casualty ratio by increasing their reliance on attacks that target civilians. Successful attacks against civilian targets are more visible to observers both in Israel/Palestine and the outside world, and a group trying to maintain its relevance in the face of losses is more likely to emphasize

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<sup>1</sup>In gathering these data, I also made an effort to separate out civilian versus military casualties. This was possible in the case of Israeli casualties, where I am able to differentiate between civilians, military personnel, and non-military security personnel such as police forces. However, the sources I use for Palestinian casualties do not clearly differentiate between civilian and military casualties. This is likely due in part to the difficulty in separating Palestinian militants from civilians, given the non-conventional nature of the Palestinian campaign.

this type of target in order to show that it is a viable and politically relevant combatant.

On the Israeli side, I expect that a shift in the casualty ratio regime in favor of the Palestinians (an increase in the number of Israeli dead and wounded for every Palestinian casualty) sends a signal that the current strategy is ineffective: if the IDF are unable to deter Palestinian militant attacks that kill or wound Israeli soldiers and civilians, or unable to retaliate for previous attacks effectively, they are unlikely to ensure a long-term reduction in Palestinian violence. I expect that Israeli forces will respond to a negative shift in the Palestinian/Israeli casualty ratio by increasing the intensity of their operations in the Gaza Strip and West Bank. A surge in Israeli losses that is not accompanied by successful operations to kill or interdict Palestinian militants is likely to be met with an upswing in preventative security measures in areas where militants are based, in order to disrupt operations and kill key operatives.

H1.1 A shift in casualty ratios favorable to the Israeli side will lead to an increased reliance on Palestinian attacks on Israeli civilians per week.

H1.2 A shift in casualty ratios favorable to the Palestinian side will lead to an increase in the number of Israeli-initiated violent events per week.

However, both groups do not simply respond to updated information about casualty counts. These actors are strategic decision-makers, and as such, react to shifts in each other's behavior. More generally, I expect shifts in strategy to covary between the two sides. A significant military-strategic shift by one side should be met with a shift by the other side in fairly short order. These groups do not operate in isolation; they both spend significant time and energy monitoring, interpreting, and responding to updates in their opponents' behavior in order to deny them a military-strategic advantage. However, given that I anticipate that both sides engage in monitoring and adaptation over time, any instrument used to model actor behavior has to account for this endogeneity. Israeli behavior affects Palestinian behavior, which affects Israeli behavior... and so on.

My second set of hypotheses has to do with the technology used by both sides. I expect that Palestinian insurgents respond to greater project of Israeli military power in

the way that many insurgent forces do: by avoiding it, rather than fighting. Given the extremely confined nature of the Palestinian Territories, especially the Gaza Strip, this is not something that can be done by moving operations to the hinterland. Instead, I expect that as Palestinian militant forces come under greater military pressure from the IDF, even after controlling for the effect of casualties from this military escalation, they will be more likely to shift their local tactics and technologies of violence to better engage the IDF and Israeli civilians while minimizing their own risk.

First, I expect there to be a reciprocal effect between Palestinian attacks on civilians and Israeli use of force. The IDF cannot sustain long-term, high-intensity operations in the Palestinian territories, largely due to political costs both at home and abroad. As such, major force projection is generally short-term and linked to some clear cause that is being solved by large-scale use of violence. However, as militants focus their efforts on attacking Israeli civilians within the State of Israel, these opportunities come more frequently. For example, the most recent operations in the Gaza Strip have been framed as attempts to destroy terrorist infrastructure used to launch unguided Qassam rockets at civilian targets within Israel. This is a common theme: the targeting of Israeli civilians for indiscriminate violence by Palestinian militants is linked closely to the IDF's propensity for carrying out large-scale violence in Palestine.

When the IDF escalates its use of force in the Palestinian Territories, I expect that Palestinian militants will adopt indirect-fire military tactics and technologies to a greater extent. These technologies may be less effective at achieving the short-term goals of killing people and breaking things, but they also expose the operators to much lower risk of detection and retaliation. In an environment where IDF tanks are rolling through Rafah or Khan Yunis refugee camp, indirect-fire engagement may prove to be a safer option. When numerically and technologically weaker insurgents are faced with large-scale shows of force from the Israeli state, they are unlikely to engage directly and risk suffering major casualties. While armed resistance to Israeli raids is common, the brunt of violence is nearly always borne by civilians in the firing zone.

Second, I expect that increased use of large-scale force by the IDF will lead to a

greater emphasis on civilian targeting by Palestinian militants. Although some escalatory activities, such as armored raids into Palestinian cities and camps, may present more immediate military targets of opportunity, the higher risks involved with engaging a numerically and technologically superior force at close range may outweigh the greater opportunities. Under greater military pressure, insurgent forces are less likely to push back directly against a powerful state, and more likely to try and stay relevant by shifting their focus to softer targets instead. In the case of the various Palestinian militant groups engaged in conflict with the Israeli state, I expect that the same pattern will hold.

H2.1 An increase in Palestinian targeting of Israeli civilians will lead to an increase in Israeli use of heavy weaponry.

H2.2 An increase in Israeli use of heavy weaponry will lead to an increase in Palestinian use of indirect-fire weaponry.

H2.3 An increase in Israeli use of heavy weaponry will lead to an increase in Palestinian targeting of Israeli civilians.

The Second Intifada also saw significant variation in the location of violence on both sides. As discussed in Chapter 1, the type of location at which both states and insurgents choose to project force is a major factor in military strategy. Even in the close confines of Israel and the Palestinian Territories, the choice of where to attack was highly significant to both sides.

While Palestinian leadership lays claim to contiguous areas of land in the West Bank as well as the Gaza Strip, the administration of these regions is mixed. The Second Oslo Accords divided the West Bank of the Palestinian territories into three areas (A, B, and C) with each area under different levels of political and military control. Although this was meant to be a temporary measure until full Palestinian autonomy in the region was secured [States, 1995], the A/B/C division has remained for the past two decades. Area A is fully under Palestinian political and security administration. This area contains the largest cities and camps in the West Bank, and the bulk of the Palestinian population. Area B is under joint Palestinian/Israeli control in security matters, and contains over

400 towns and villages, many on the outskirts of larger cities in Area A. Area C is under full Israeli administration for both political and security issues. Area C contains a large number of small Palestinian villages, and most of the open space and natural resources (including water) as well as all Israeli settlements in the West Bank. The majority of area A is fully off-limits to Palestinian civilians, and Israeli security checkpoints and barricades on Palestinian thoroughfares were common during the period of the Intifada.

The choice to project violence against targets in these various zones deals with legal and military factors. As area A is legally under sole Palestinian jurisdiction, IDF incursions into major West Bank cities are in violation of the Oslo Accords. However, the security need to locate and destroy Palestinian insurgent cells may carry greater weight than the legal constraints of respecting Palestinian sovereignty. As such, I expect that as Israeli forces come to view Palestinian insurgents as a greater threat, Israel is more likely to launch preemptive or retaliatory campaigns into Palestinian jurisdiction, often engaging with insurgent fighters as well as official Palestinian administration forces.

For Palestinian insurgent forces, the barriers to operating in different areas are military-logistical, rather than legal. Operating in Area C or in Israel proper is naturally more difficult than operating in the Gaza strip or in areas A/B, due to the existing Israeli security structure. Area C contains a high concentration of road blocks, checkpoints, and other barriers to Palestinian movement, as well as long-term IDF presence to protect Israeli settlers. The Israeli border is closely guarded, with intense security checks at crossings and frequent border closures. Projecting force to these locations requires more planning, support, and sheer luck by Palestinian insurgents. However, the range of targets in area C and Israel are much more valuable for purposes of violence. A successful attack in Israel or against a settlement in area C can be expected to have a much higher impact on Israeli morale than an attack on IDF forces patrolling in areas A or B, or even against one of the embattled settlements in the Gaza strip.

As such, I would expect that Palestinian insurgents are more likely to launch attacks on targets in Israel and area C in circumstances where (1) targets of opportunity in Gaza and areas A and B are limited and (2) these groups have more local operating capability.

Both of these circumstances vanish when the IDF is present. IDF incursions into Gaza and areas A and B have two effects on the short-term decision-making of Palestinian insurgents. First, Israeli forces present a target of opportunity by simply being present in areas of relative Palestinian strength; patrolling soldiers and vehicles are ready targets for snipers or small, mobile forces that can engage the IDF and attempt to escape in the urban landscape. Second, the IDF is not passively waiting to be shot at, as these incursions are military in nature. IDF incursions into Gaza and areas A and B are designed to target and destroy Palestinian insurgent infrastructure. The damage incurred during these incursions make it more difficult for Palestinian groups to pay the costs required to project force past Israeli defenses at ‘softer’ targets in Israel and area C.

I expect that successful Palestinian incursions into Israel and area C in the West Bank are likely to be met with Israeli incursions into Gaza and areas A and B in the West Bank. Successful acts of violence on Israeli territory increase the perceived threat of Palestinian force, increasing Israeli incentives to engage in large-scale, technically illegal pacification campaigns in Palestinian territory. When the IDF is operating in Palestinian territory in Gaza and areas A and B in the West Bank, I expect to see a corresponding decrease in Palestinian attacks carried out on Israeli soil, and instead a short-term refocusing of violence on local targets as local IDF forces (1) present targets of local opportunity and (2) destroy infrastructure and kill fighters who would otherwise be utilized to carry out attacks on Israeli soil.

H3.1 An increase in Palestinian violence in Israeli-controlled territory will lead to an increase in Israeli violence in Palestinian-controlled territory.

H3.2 An increase in Israeli violence in Palestinian-controlled territory will lead to a decrease in Palestinian violence in Israeli-controlled territory.

In other words, the geographic patterns of violence should change based in part on Israeli perceptions of Palestinian threat, which are in turn linked to Palestinian ability to successfully carry out attacks on Israeli targets in Israel proper or on settlements in Israeli-controlled territory in the West Bank. As the perceived threat of Palestinian

insurgent increases, the Israeli government becomes more willing to pay the military and political costs of operating in Palestinian territory.

These individual elements of military-strategic behavior are worth investigating on their own. However, it is highly likely that there is significant covariance in the type, target, and location of violence on both sides. For example, Palestinian insurgents rarely carry out attacks against Israeli civilians within areas A and B of the West Bank – simply because Israeli civilians are legally barred from entering these regions to begin with. A combined measure that captures overall variation in military-strategic behavior on both sides may be useful in capturing general trends of action and response on both sides.

Therefore, I combine type, target, and location of violence into a single metric. For insurgents, this combined measure attempts to capture the degree to which the dominant use of violence in a given time period can be called ‘conventional’ versus ‘unconventional’ or ‘terroristic’. Conventional insurgent strategies are those that (1) utilize conventional arms, rather than indirect weapons such as IEDs or suicide bombs; (2) target enemy military forces, rather than civilians; and (3) focus operations in ‘friendly’ territory in an attempt to expel security forces or establish local autonomy. Unconventional strategies that more closely resemble terrorism are those that (1) utilize non-conventional technologies; (2) specifically target civilians for violence; and (3) focus operations behind IDF lines in cities or areas that are under Israeli control.

A combined measure of state strategy, in the case of Israel, falls on a spectrum from ‘targeted repression’ to ‘large-scale repression’. A strategy of targeted repression is one that (1) utilizes small arms and targeted weaponry; (2) targets militant fighters; and (3) focuses operations in areas where the Israeli state has legal authority to operate. An Israeli strategy of large-scale repression is one that (1) utilizes large-scale violence and indiscriminate weaponry; (2) targets civilians, either directly or through indiscriminate fire; and (3) focuses operations in Palestinian territory where Israel has no jurisdiction.

I expect that these two combined measures of military strategy covary with one another. As the intensity and scale of Israeli repressive violence escalates, I expect that Palestinian insurgents pursue lower-intensity strategies that can be described as terrorism. However,

as insurgents rely more heavily on terrorism, which by its nature is hard to predict and counter, the IDF is more likely to pursue severe means to preempt and punish Palestinian attacks. In other words, I expect to see a system that is escalatory in nature: greater IDF violence should lead to greater reliance on terrorism by Palestinian militants, which in turn will provoke even more severe retaliation from the Israeli government.

H4.1 An increase in reliance on large-scale repression by the IDF will lead to increased reliance on terrorist tactics by Palestinian insurgents.

H4.2 An increase in reliance on terrorism by Palestinian insurgents will lead to increased reliance on large-scale repression by the IDF.

Finally, I look for patterns of behavior that link short-term changes to long-term escalation or de-escalation in the level of fighting. What patterns of behavioral evolution are likely to precede the end of a conflict, or at least a lull in violence? Greater levels of IDF violence weaken Palestinian militants in the short run by destroying material, disrupting leadership, and killing fighters. However, the longer-term effect of these operations may be to increase militant targeting of civilians and reliance on indirect-fire weapons such as mortars, rockets, and bombs (both suicide and conventional): technologies that allow insurgents to project violence within Israel and at Israeli settlements while minimizing their risk of detection and confrontation. When successful, this type of Palestinian violence against civilians provides both incentive and political cover for greater Israeli use of force within the Palestinian territories, which reinforces the existing cycle.

My final set of expectations have to do with the de-escalation of violence. As Figure 2.1 shows, violence on both sides during the Second Intifada varied considerably, in terms of both intensity and strategy. Clearly, this pattern of escalation between Israeli use of heavy weaponry and Palestinian terrorist tactics is not monotonic or constant over time. How can we predict or explain downward trends in the level and type of violence over this four-year period?

My final set of hypotheses anticipate that there is a dampening effect of certain types of Israeli violence on Palestinian activity. History suggests that intensity of force

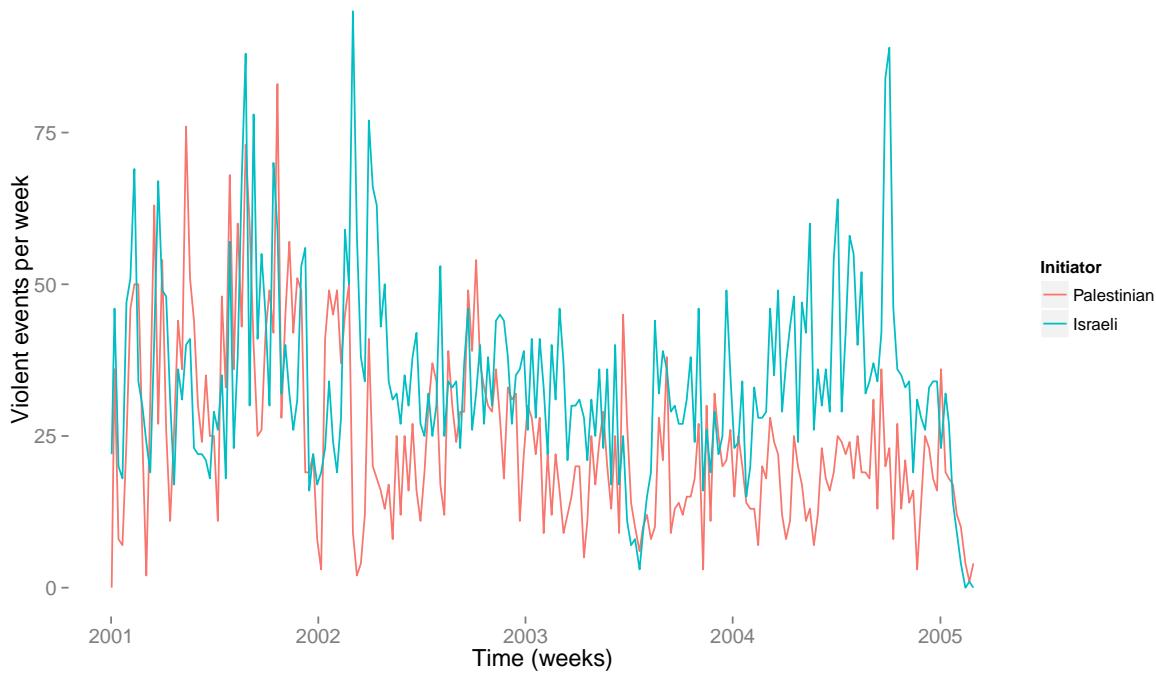


Figure 2.1: Weekly Israeli and Palestinian events during the Second Intifada

alone is not enough to win a counterinsurgency campaign; high levels of indiscriminate violence in particular have been proposed to actually make conflicts worse by estranging and enraging abused civilians. As the IDF gained better intelligence on the identities and activities of Palestinian militant leaders and cells, it was able to target them with greater emphasis versus larger-scale, indiscriminate actions directed at whole towns or neighborhoods designed to quell unrest in a large area. As such, greater emphasis on targeting militants and terrorists for violence should have a more effective quelling effect: successfully targeting actual fighters, as opposed to suspicious civilians or residential areas, should be more effective at directly reducing militant capability to launch new attacks. For the IDF, which operates in a politically fraught environment, targeting militants directly also means less to worry about in terms of political blowback and negative media attention from destroying residential neighborhoods or killing civilians.

Here, I expect a reciprocal effect that generates a negative cycle of violent activity and intensity. As the IDF is able to focus its efforts on targeting Palestinian militants and leaders with more accuracy, I expect that Palestinian activity will decrease. As this

combination of suppression and destruction weakens the material capabilities of Palestinian insurgents, they become less capable of launching attacks against either IDF personnel or Israeli civilians. As the level of Palestinian activity decreases in response to this suppressive action, the perceived threat to Israel decreases as well. With a lessening of threat perception, the legitimacy and need for large-scale operations into the Palestinian Territories decreases, leading to an emphasis on small raids to arrest suspected terrorists and surgical strikes against known militant leaders. I expect that as the intensity of Palestinian violence decreases, the IDF will emphasize selective violence against militants rather than large-scale violence in the Territories.

H5.1 An increase in direct Israeli targeting of Palestinian militants will lead to a decrease in Palestinian activity.

H5.2 A decrease in Palestinian violent activity will lead to a decrease in Israeli reliance on heavy weaponry.

## 2.4 Research Design

### 2.4.1 Data and Coding

To test these hypotheses about military-strategic updating, I use a set of roughly 17,500 event records describing conflict interactions between Palestinian and Israeli actors during the Second Intifada period. Specifically, I focus on a subset of roughly 12,400 events that involve the use of armed force or violence. I use these data to describe patterns in the behavior of the IDF and the various Palestinian militant organizations, and analyze covariance in these behaviors to identify the impact that a shift in Israeli strategy has on Palestinian strategy and vice-versa. These data describe actions taken by both Israelis and Palestinians during this time period, and cover a wide variety of violent interactions including firefights, air strikes, shellings, bombings, and others. In all cases, I was able to record the type of interaction that took place, the identities of the actors involved, the date and location of the event, characteristics of the interaction such as the type of weapons used, and the outcome of the interaction in terms of dead, wounded, and (where possible)

property damage. Each interaction is temporally referenced to the day, and geographically referenced to the city, town, or village in which it occurred.

Most events initiated by Israeli actors (the IDF, the Israeli government, or Israeli civilians) against Palestinian actors were gathered from weekly reports published by the Palestinian Center for Human Rights (PCHR). The PCHR has published these reports in English and Arabic every week since January of 2001. Reports describe individual incidents occurring within the Palestinian Territories, the vast majority of which are Israeli-initiated. These reports generally take the form of short (one to two sentences) descriptions of Israeli activities, a geographic reference denoting the village, camp, or neighborhood where the event took place, and any outcomes of note in terms of death, injury, or property damage. A sample report below is taken from January 2002:

*At approximately 05:00, Israeli forces encroached into Ramallah from three directions. They moved into Al-Ersal Street and three tanks and a number of military jeeps took position only 50m away from the office of the Palestinian President. Other tanks and military jeeps moved into Al-Tira area and took positions near the building of Ramallah First Group.*

Most events initiated by Palestinian actors (the Palestinian National Authority, various militant groups, or Palestinian civilians) against Israeli actors were gathered from daily news reports published by the Jerusalem Post. This newspaper is not the largest with an English-language component, but (importantly) it is the only English-language Israeli news source with significant archives available online. During the Second Intifada, the Jerusalem Post regularly published short accounts summarizing clashes between Palestinians and Israelis, both civilian and military. A pair of sample news clippings from the same period in January 2002 reads:

*Last night Palestinians fired three mortar rounds at an IDF post in Ganei Tal, Gush Katif. Earlier in the evening two anti-tank grenades were fired at an IDF post near the Egyptian border [...] There were no reports of Israeli casualties in the attacks.*

These reports are less standardized and often less in-depth than the PCHR format, but generally include the same type of information: location, date, interaction, parties

involved, and the type of arms or technology used. Because the Jerusalem Post does not publish the same type of specific reports as the PCHR, coding these data was a more involved process. Raw articles were first gathered using the LexisNexis online repository service, based on a keyword search designed to ping articles having to do with terrorism or violence<sup>2</sup>. The goal of this search was to cast a fairly wide net and gather as many stories dealing with Palestinian violence as possible.

The resulting corpus contains about 21,000 news articles from 2001-2005 that contained at least one word matching the keyword search. This data set was then subjected to automated text analysis in Python in order to identify duplicate stories, violent language taken out of context (for example, book and movie reviews or sports matches), and other false positives. The finalized set of raw data contains approximately 6,100 news articles that were identified as being most likely to contain ‘real’ records of violent interactions during the Second Intifada. This corpus was then hand-coded using the same set of variables and coding specifications as the PCHR weekly reports, identifying the location, technology, and key actors involved in each violent event.

Finally, records of fatalities on both Palestinian and Israeli sides were compared against the record of deaths compiled by B’Tselem, a well-known NGO in Israel dedicated to tracking violence and human rights abuses on both sides of the conflict. In the few cases where PCHR or JPost records differed from B’Tselem, the B’Tselem records were held as authoritative. The resulting data set consists of event records from both Palestinian and Israeli sources detailing a wide range of violent interactions between military/militant and civilian groups on both sides. The reports from PCHR and JPost listed above can be translated into the following event records including the parties involved, the type of interaction, the date and location, the technologies used, and the outcomes, as illustrated by Table 2.1:

After processing and de-duplication of events reported on by both sides, the final

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<sup>2</sup>Specifically, this search crawled the available stories on record for some form of the following word(s): *terror, attack, shoot, bomb, stab, rocket, missile, hit, kill, dead, die, injury, wound*.

Table 2.1: Event Records Example

| Date       | Location  | Actor1 | Actor2 | Interaction | Type       | Deaths | Injured |
|------------|-----------|--------|--------|-------------|------------|--------|---------|
| 2002-01-28 | Tulkarm   | IDF    | PalGov | Raid        | Heavy Arms | 0      | 0       |
| 2002-01-22 | Ganei Tal | PalGun | IDF    | Shelling    | Mortars    | 0      | 0       |
| 2002-01-22 | Gadid     | PalGun | IDF    | Shooting    | Small Arms | 0      | 0       |

data set contains 17,533 unique events in Israel and the Palestinian Territories over this four-year period. Roughly 10,000 events in this data set are sourced from PCHR, 6,000 from the Jerusalem Post, and 1,500 directly from B'Tselem.

The decision to use a Palestinian source to code Israeli-initiated events and an Israeli source to code Palestinian-initiated events was made out of equal parts necessity and opportunity. There are very few ‘complete’ sources of data on violent interactions in the Israeli-Palestinian conflict. Instead, Palestinian sources tend to report acts of Israeli violence, while Israeli sources tend to report acts of Palestinian violence. This means that focusing on only Israeli or Palestinian sources would have produced a data set with severe reporting imbalance. There is also the issue of bias. This conflict has been the focus of international reporting for decades, and both sides devote significant resources to ‘spinning’ actions and outcomes in their own favor.

I tried to select sources that were not overtly biased. The PCHR is a longstanding NGO in the Palestinian territories with significant international involvement and support, while the Jerusalem Post is one of the longest-running English-language newspapers in Israel. This data-gathering approach is certainly far from perfect, but it strikes a balance between coverage and potential bias that allows me to place significant trust in (1) the coverage of events overall and (2) the relative veracity of these event reports, especially those that record deaths during the conflict. By relying on both Israeli and Palestinian sources, I hope to let these potential biases balance each other out to some extent.

Figures 2.2 and 2.3 illustrate the type and distribution of violent events. Blue/triangular points are events of violence initiated by Palestinian militants or military personnel, and red/circular points are events of violence initiated by the IDF. Point size indicates the

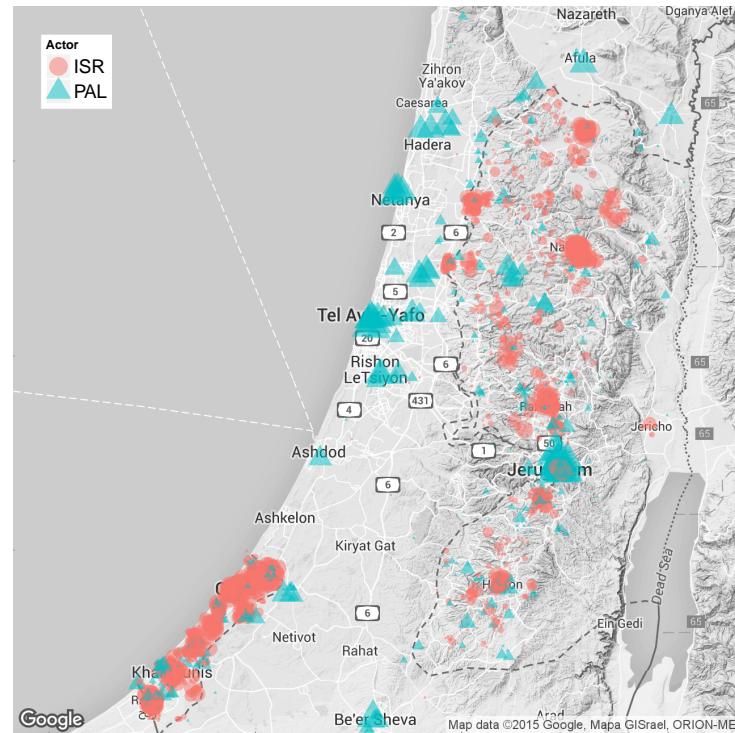
severity of the event in terms of total wounded and dead.

Mapping these events can provide some initial insights about how and where both sides used violence during this time. Figure 2.2 shows that in the southern arena, Palestinian violence was limited primarily to the Gaza Strip and nearby areas within mortar or rocket range — for example, note the clusters of shelling and shooting attacks around Sderot (east of Gaza City, just across the border from Beit Hanoun) and the former Gush Katif block of Israeli settlements in southwest Gaza on the coast of the Mediterranean. Northern and eastern Israel, sharing a much more porous border with the West Bank region, saw significantly more Palestinian violence, particularly bombings and shootings in large cities like Netanya, Hadera, and Tel Aviv.

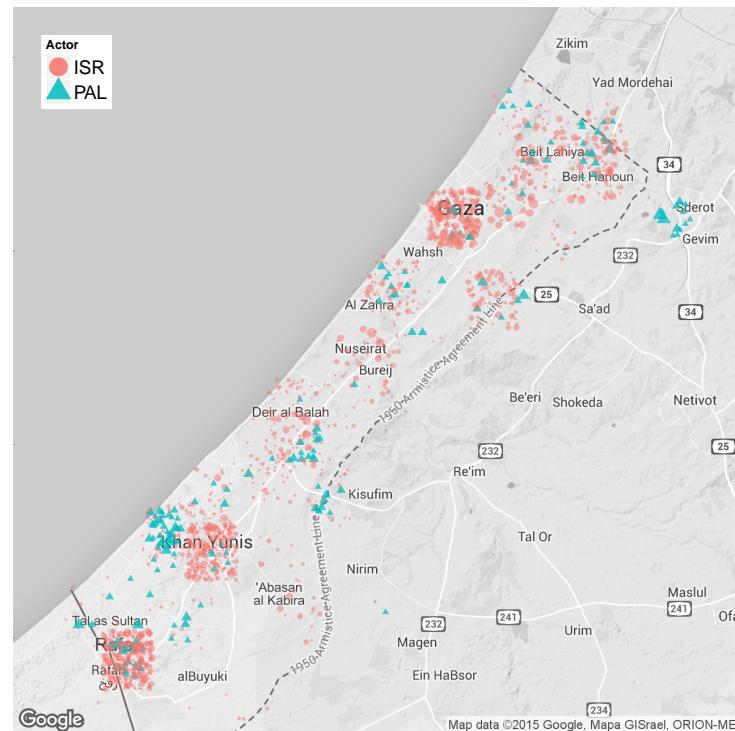
Figure 2.3 breaks down violence in the Gaza Strip by initiator and type of violence, aggregated by area. Here, we can see how not just the use of force, but the type of force used by both sides varies in space. The IDF carried out a fairly intensive campaign of air strikes against suspected militants and munitions factories in the Gaza Strip, focusing primarily on targets in Gaza City and the Khan Yunis refugee camp. However, the IDF was apparently more hesitant to commit ground forces deep within Gaza City; most major IDF operations were carried out in areas nearer the border (Beit Hanoun, Abasan) or in suppression operations in the Khan Yunis refugee camp. Palestinian violence in and near the Gaza Strip was (unsurprisingly) mostly directed at IDF patrols and incursions in the major cities and refugee camps, with the two notable exceptions of Sderot and Gush Katif noted in 2.2. Breaking down violence by the technology employed confirms that mortars and rockets were often used to attack otherwise-inaccessible targets outside Gaza, while light arms and improvised bombs were used to attack IDF occupation forces and fortified settlements within the Strip itself.

After generating these event data, I aggregated events on three dimensions to produce ‘behavioral profiles’ on each dimension for Palestinian and Israeli actors. These three dimensions correspond to the type or technology of violence, the targets of violence, and the location of violent events perpetrated by each side in a given time period.

It is worth noting that the range of technologies available to both sides differ in stark

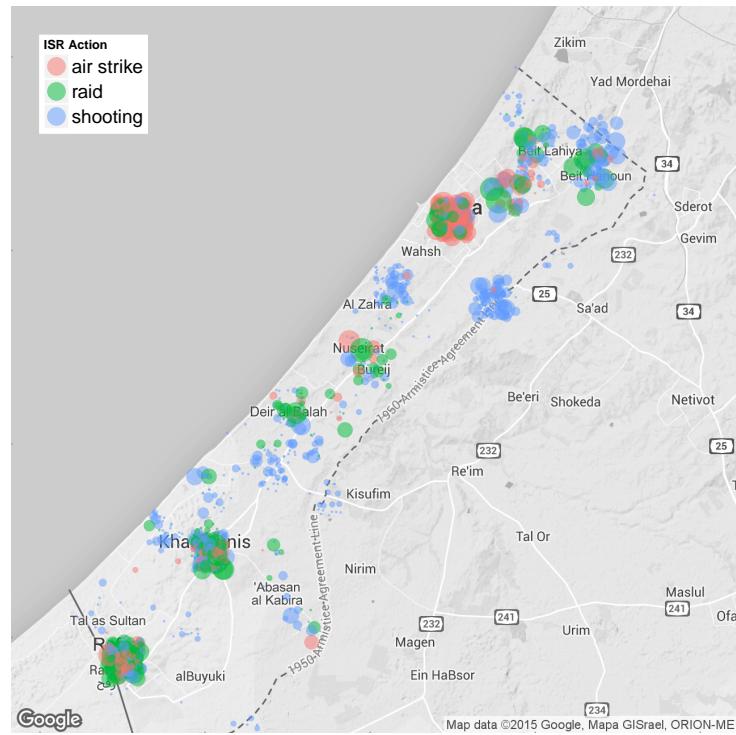


(a) Events in Israel and the Palestinian territories

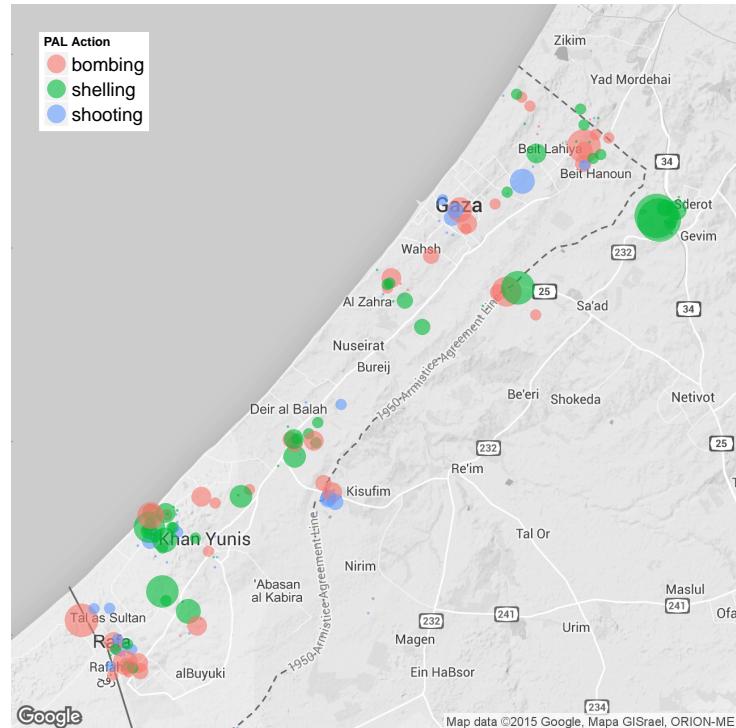


(b) Events in the Gaza Strip

Figure 2.2: Events and casualties during the Second Intifada



(a) Israeli-initiated events and casualties in the Gaza Strip



(b) Palestinian-initiated events and casualties in the Gaza Strip

Figure 2.3: Events in the Gaza Strip

ways. The IDF is one of the most technologically advanced militaries on earth, fielding significant armor, artillery, and aircraft capabilities. For the IDF, indirect-fire weapons are generally those that combine major destructive power with stand-off firing capabilities. These weapons include heavy ground forces such as tanks and artillery; they also include air-to-ground weapons fired from helicopters or fighter jets, and at several points in the conflict, shelling from naval vessels. These weapons all combine range, firepower, and a lack of risk to the operator with an increased chance of collateral damage. Heavy Israeli reliance on these technologies meant that the majority of Palestinian civilian casualties in the conflict were caused by these weapons.

The various Palestinian groups are significantly more limited in their selection of tools, relying primarily on small arms, improvised explosive devices, and homemade mortars and unguided rockets. However, the distinction is largely the same: indirect-fire weapons are safer to operate, but less accurate and less reliable at avoiding collateral damage than direct-fire weapons<sup>3</sup>. For example, the homemade mortars and rockets developed by Palestinian militants and used in increasing numbers in the latter part of the conflict were not adopted because they were highly effective at killing Israelis; they were chosen because (1) they could reach otherwise inaccessible targets such as Israeli cities across the Gaza border, and (2) they could be operated from out of LoS, allowing their operators a higher chance of firing and escaping before Israeli forces could locate them.

Similarly, I divide the target of violence into two categories: military and civilian. Military targets, obviously, are those that are clearly military in nature. For Palestinian militants, military targets include the various Israeli security forces, including both the IDF and the Border Police forces. For the IDF, military targets consist of Palestinian National Authority forces (Force 17 and the Presidential Guard) as well as the various militant groups mobilized in the Palestinian territories.

For the IDF, differentiating between militants and civilians was (and remains) a significant problem, as Palestinian gunmen often wear civilian clothing and blend in with

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<sup>3</sup>Whether Israeli and Palestinian forces differed in their concern for collateral damage is irrelevant here — the key division is whether discrimination is possible at all when using these weapons.

the overall population. In these cases, I code the target of Israeli action as being ‘military’ in nature if the interaction involves (1) reciprocal fire, meaning that Palestinian militants were clearly present, or (2) violence specifically directed toward a known target, such as targeted air strikes against Palestinian military leaders or suspected militants. Civilian targets can include individuals or groups, such as Israeli citizens targeted for suicide bombings; they can also include civilian structures or neighborhoods, such as areas of Rafah or farms near Beit Hanoun, targeted for bulldozing or demolishing by the IDF.

Finally, I classify violent events as occurring either in Palestinian-administered territory (the Gaza strip and areas A and B of the West Bank) or in Israeli-administered territory (Israel proper and area C of the West Bank). This is a meaningful distinction on both sides: within the Israeli government, there was real debate over the lengths to which the IDF should pursue pacification by operating within Gaza and Palestinian territory in the West Bank. Launching major operations into these areas, rather than attempting to apprehend or kill insurgents at border crossings or within Israel itself, was a contentious issue, both because it posed higher costs in terms of both Israeli and Palestinian lives and because it created greater legal backlash for the IDF. Not until Palestinian forces began to emphasize major attacks (such as suicide bombing) within Israel was there a significant shift towards large-scale operations within Gaza and areas A and B in the West Bank, with the goal of destroying insurgent infrastructure and disrupting leadership.

Modeling the co-evolution of military strategy is somewhat complicated from a statistical point of view, as this task is really two separate endeavors. First, I need to consolidate this disparate event data in some way that focuses on behavioral change over time. Second, I need a model that will link shifts in Palestinian behavior to shifts in Israeli behavior, and vice-versa, in a theoretically and statistically appropriate fashion.

Because this question is explicitly temporal, I began by converting the available set of conflict event data to time-series form. Moving from event-record data to time-series data necessitates aggregation by time period. In this case, I chose the week as the temporal unit of analysis.<sup>4</sup> Aggregating actor behavior to the week is useful for two reasons. First,

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<sup>4</sup>Although I argue that the week is a substantively and empirically logical choice, I understand that the

it allows for uncertainty and variation in the data being analyzed. Given that this is an incomplete representation of the day-to-day violence that took place during the Second Intifada, it is likely that looking at violence from day to day would produce significant noise from missing observations. Aggregating to the week helps smooth out these inconsistencies and get a better picture of what was ‘really’ going on in that time period.

Second, the week is a substantively and empirically useful time period. Substantively, the week is a natural unit of human time measurement. Empirically, examining actions and responses at the week level is a realistic choice for this conflict: the small size of the conflict theater, the emphasis on rapid reaction time by the IDF to Palestinian violence, and the flexible and loosely-organized nature of the Palestinian insurgent groups involved suggest that actions and reactions are likely to be fairly rapid, and may be missed at a higher level of aggregation such as the year.

One issue to be aware of when aggregating event data to a time-series format is that it is important to control for the overall level of activity as well as the type of activity. When looking at type and target of activity, my goal is to determine how heavily the Israeli or Palestinian forces relied on a given type of violence, or how often they decided to target civilians versus military forces for violence, in a given week. One way to do this would be to take the total number of events recorded in a given week. If the IDF launched five air strikes in the previous week, but 10 in the current week, this might be an indication of escalating reliance on this type of force. However, looking at the raw number of events per week may be misleading. If the IDF is broadly stepping up its level of activity in other areas, such as raids or small-arms patrols, then an increase in the number of air strikes may just be indicative of this increase in overall activity. While this is informative, it is not the same as tracking the relative weight placed on one type of violence versus another.

Instead, I look at the relative frequency of event types perpetrated by each actor in a given week. For example, if 25% of all violent events initiated by Palestinian militants were bombings last week, but 40% of events this week were bombings, would indicate an

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level of temporal aggregation can be an important factor in any time series analysis. Running robustness checks on all statistical models in this chapter at aggregation levels as low as the month and as high as the day produces substantively identical findings.

escalating reliance on bombings instead of small-arms fire. The relative-frequency approach is useful because it explicitly measures the degree to which a given actor distributed its limited resources toward violence in that time period. Given that neither the IDF nor the Palestinian militant groups have unlimited resources, any commitment of resources to violence means that those resources cannot be used anywhere else. Under conditions of restricted resources, looking at the degree to which an actor commits its resources to one type of violence versus is a more informative signal than simply looking at the number of events. I calculate this measure of relative frequency, by week, for (1) Israeli and Palestinian use of direct versus indirect military technologies, and (2) Israeli and Palestinian emphasis on violence directed at civilian versus military targets.

Aggregating by week distills this large set of geo- and time-referenced event data to two ‘streams’ of time-series information for each side. With this coding method, I measure two dimensions of behavior for both Israeli and Palestinian military (or militant) forces for each week from January of 2001 through February of 2005. In a given week, I can identify the degree to which both Israeli and Palestinian forces relied on selective/direct-fire technology versus indiscriminate/indirect-fire technology; and we know the degree to which both sides targeted civilian versus military forces for violence. Figure 2.4 below shows Israeli and Palestinian behavior over time, smoothed for illustration:

Figure 2.4a plots the IDF’s reliance on heavy arms raids into Palestinian territory over this four-year period. A visual assessment of these data shows trends that line up with known patterns of behavior during the Second Intifada. For example, a major shift in Israel strategy took place in the first quarter of 2002, when the IDF launched a major combined-arms operation into the West Bank, dubbed ‘Operation Defensive Shield’ (shown in the red shaded area in Figure 2.4a). The short-term goal was to disrupt and disable militant infrastructure in several major West bank cities and refugee camps, but Defensive Shield also represented a longer-term shift in IDF strategy away from small-scale retaliation and prevention within Israel’s borders and toward larger-scale, preemptive security measures including both ‘boots on the ground’ raids and targeted strikes with helicopters and fighter jets. Note also the major dip in Israeli combined-arms operations in

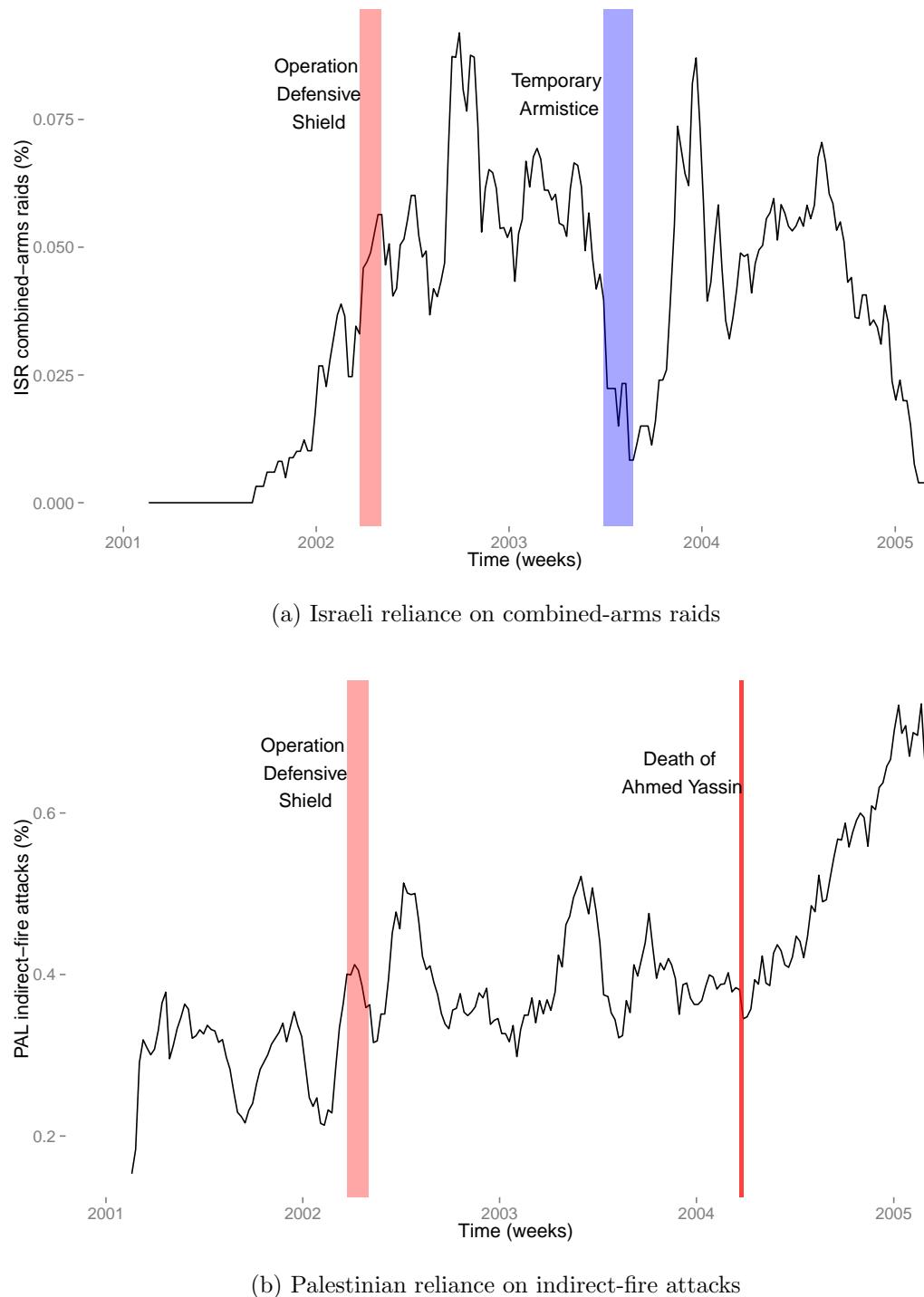


Figure 2.4: ISR and PAL reliance on technology over time

mid 2003 (shaded in blue in Figure 2.4a) during the unilateral ceasefire declared by Fatah, Hamas, and Islamic Jihad in late June. Major operations pick back up quickly, however, following an August 19th Hamas suicide bombing in Jerusalem that killed 23 civilians.

Figure 2.4b plots the various Palestinian factions' reliance on indirect-fire attacks on Israeli targets both inside and outside the Palestinian territories. Plotting these data over time shows two very interesting trends. First, there appears to be a phase shift of sorts in this series right around March of 2002 — right when the IDF stepped up its military operations through Operation Defensive Shield. This represents a distinct increase in Palestinian reliance on indirect-fire tactics from week to week for the next two years. However, in early 2004, this metric suddenly begins to increase dramatically week-over-week, and continues to do so until the ceasefire was signed to end major hostilities in mid-2005. Interestingly, one very significant event occurred right around this time: in March 2004, the IDF successfully assassinated Sheikh Ahmed Yassin. Yassin was the long-time leader of Hamas, one of the largest and most violent Palestinian militant groups, and a major figurehead for the militant wing of Palestinian politics. His assassination, followed a month later by the assassination of his successor Abdel Aziz al-Rantisi, marks the beginning of a major period of transition in which Palestinian violence becomes characterized by indirect forms of violence.

Finally, I create a combined measure of the type, technology, and location of violence each side employed from week to week. The approach I take to combining these metrics together is very simple, as it represents a first cut at this type of multivariate measure. Incidents are scored on a scale from 0 to 3, where 0 is ‘more conventional’ and 3 is ‘less conventional’. A single incident’s score is set at 0 initially, and increases by 1 for each of the following characteristics: indiscriminate or indirect-fire weaponry was utilized; violence was directed at civilians instead of military targets; and violence was projected into Israeli territory (for Palestinians) or Palestinian cities (for the IDF). As such, events with higher scores can be broadly described as being less ‘conventional’ than events with lower scores. The events of each week for each side are scored on this metric, and the mean score is extracted. This metric attempts to describe the overall violent behavior each side relied on

from week to week: the extent to which Palestinian fighters relied on unconventional forms of violence that could be described as terrorism, and the extent to which the IDF relied on large-scale military violence to prevent or retaliate for Palestinian attacks. Figure 2.5 visualizes these combined event ‘scores’ over time for both Israeli and Palestinian forces. This metric bears out the information contained in Figures 2.4a and 2.4b, as it shows that there is significant short-term variation in the overall approach to militarized violence that both sides engage in over this four-year period.

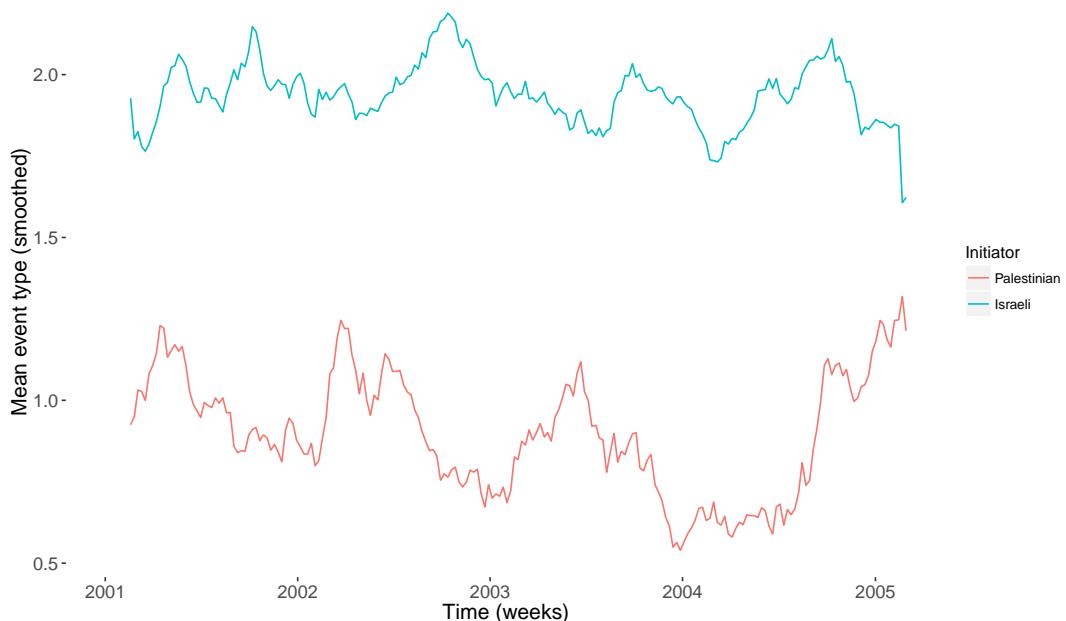


Figure 2.5: Combined scores of ISR and PAL events

These figures do not constitute an analysis in the statistical sense, and I do not claim that the events and time periods highlighted here are the only causal factors related to changes in both sides’ behavior over time. However, they do provide some preliminary evidence that (1) there are significant shifts in how both sides prosecuted the conflict at various points over this four-year period, and (2) these shifts can be associated with events that are highlighted as significant by qualitative and historical analysis. The goal here is not simply to reiterate previous findings or show well-known behavioral patterns, but to identify shifts like these in cases where we might not have the advantage of qualitative information and historical records. The goal of the next stage is to build a detection

system that can reliably capture significant and lasting shifts in group behavior, allowing me to make estimates and predictions in cases where we do not have known events to match up with peaks or valleys in time series data.

### 2.4.2 Modeling Approach

To test these hypotheses, I employ a set of vector autoregressive (VAR) time-series models. This statistical approach is well-suited to this research design for two reasons. First, and most obviously, this is time series data: because I am analyzing actor behavior over time, a model that can account for non-independent observations is important. Second, VAR models in particular are useful because they explicitly allow for endogenous relationships between multiple variables. In the case of these data, this feature is crucial: my hypotheses explicitly expect that relationships between Israeli and Palestinian behavioral shifts are highly endogenous as both actors react to one another's behavior. This means that if we want to estimate the effects of a shift in Israeli behavior on a shift in Palestinian behavior, we have to account for the reciprocal effects of a shift in Palestinian behavior on a shift in Israeli behavior.

These models have seen significant and increasing use in both political science and other forms of social science to study such processes as policy evolution [Enders and Sandler, 1993], protest and repression [Carey, 2006], and economic patterns [Blood and Phillips, 1995], as they are well suited for analyzing the short- and long-term effects of shocks over time. VAR models are somewhat similar to structural equation (SEQ) approaches to modeling endogenous outcomes. The advantage of the VAR approach in this framework is that it allows for modeling of the underlying structural process without making strong assumptions about what this structural process looks like. While this means some sacrifices in terms of potential accuracy, adopting a VAR approach also means making fewer assumptions about the exact or ‘true’ nature of the relationship between the different variables [Freeman et al., 1989]. Compared to SEQ modeling, VAR is a more useful tool when we relax the assumption that reality conforms exactly to our theorized structures and relationships.

The VAR model takes the following form [Freeman et al., 1989]:

$$\begin{aligned}
 y_t &= A(L)y_{t-1} + u_t & (2.1) \\
 A(L) &= A_1 + A_2L + A_3L^2 + \dots \\
 E(u_t) &= 0 \\
 E(u_t u_t') &= \Lambda \\
 E(u_t u_s') &= 0 \quad t \neq s \\
 E(y_t u_s') &= 0 \quad \text{for all } t < s
 \end{aligned}$$

“where  $y_t$  is an  $m \times 1$  vector of variables;  $A_i$  is a  $m \times m$  matrix of coefficients,  $i = 1, 2, \dots, j$ ;  $u_t$  is an  $m \times 1$  white noise vector;  $\Lambda$  is the  $m \times m$  variance-covariance matrix for  $u_t$ ;  $'$  denotes transpose;  $L$  is the lag operator; and  $E$  is the expectation operator” (844). The form of equation (2.1) is set by the scholar as with any other statistical model, by selecting a set of input and output variables. The advantage of VAR for this modeling purpose is that it does not require strong assumptions by the user about which variables and lags are exogenous or endogenous. Instead, the VAR approach treats all of the input variables as endogenous, unless explicitly specified by the practitioner. This means that a single VAR for a set of  $K$  variables consists of up to  $K$  sub-models, in which each of the endogenous output variables is regressed against lagged versions itself and the other  $K-1$  variables.

This framework allows for endogenous relationships between different input/output variables. However, it also complicates the interpretation of hypothesis tests. Instead of looking for statistically significant relationships between individual pairs of covariates, hypotheses are tested by analyzing the joint significance of multiple coefficients or blocks of coefficients [Freeman et al., 1989, p. 845].

One relatively straightforward way to test the relationship between specific input and output variables within a VAR framework is through simulation. Simulating a shock to one or more key input variables and visualizing the aggregate effects on the output variable of interest can show whether the overall relationship between input and output is statistically significant. This is most commonly done via analysis and visualization of an impulse-response function (IRF). IRF analysis is useful to visualize the estimated impact,

along with specified confidence intervals, of a shock in one variable on the future values of other variables in the model, as shown below in Figure 2.6.

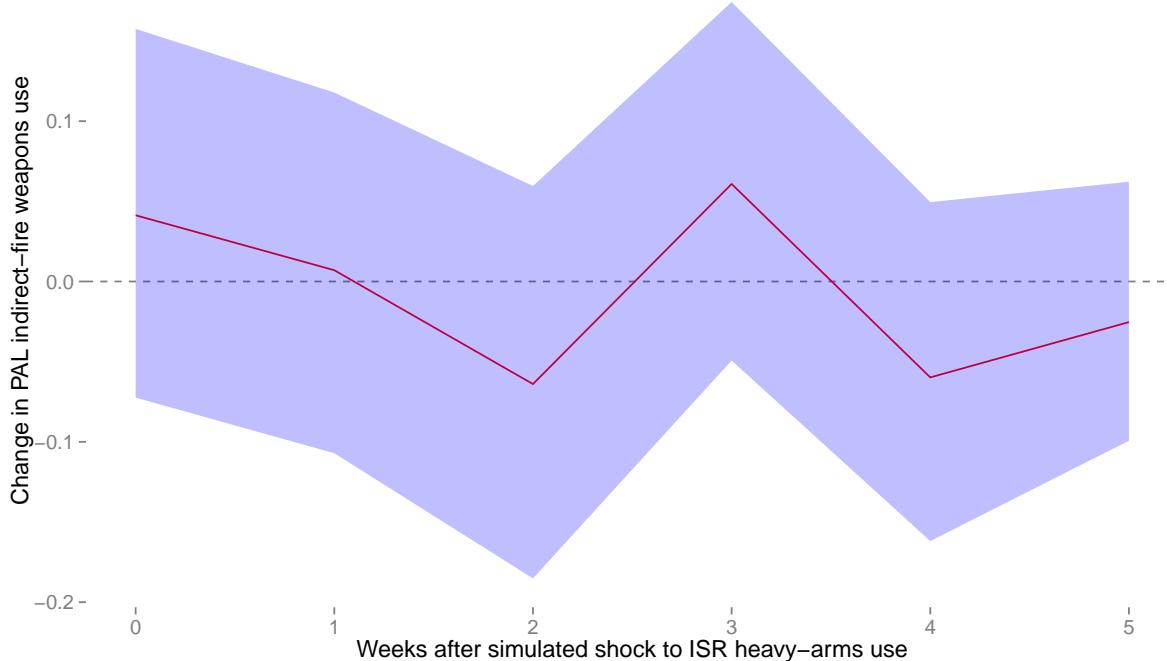


Figure 2.6: Example impulse-response function

Figure 2.6 visualizes one of the relationships hypothesized previously, showing the estimated effect of a sudden change in Israeli military decision-making — here, an increase in IDF reliance on heavy arms – and the expected response by Palestinian militant groups over the next five weeks. In this case (which I discuss in more detail further on) there does not appear to be a significant response by Palestinian fighters, as the simulated change in Palestinian behavior never exceeds zero with conventional levels of significance.

## 2.5 Results and Discussion

Table 2.2 shows the results of regressing Palestinian and Israeli behavior on previous trends in casualties as an exogenous regressor. These are autoregressive models with exogenous controls (ARIMAX), in this case the lagged casualty ratio between Israeli and Palestinian fighters and civilians. I specify casualties as exogenous because ultimately the number of dead and wounded is not directly controlled by either side. Both sides have

preferences about the level and type of casualties they would like to inflict or suffer, but these outcomes are not fully in their power to decide. Israeli helicopters trying to target a Hamas leader for assassination may mis-identify their target and kill a car full of civilians; a Palestinian suicide bomber may be apprehended at a border crossing or experience an equipment malfunction before reaching a Tel Aviv shopping mall. Casualties, as an outcome, are related to actor choices, but they are not fully in the control of either side. This means that they are the closest thing to a truly ‘exogenous’ variable present in this analysis, despite being directly related to both Palestinian and Israeli military efforts.

These models test whether casualty trends in the previous three weeks affect actor behavior in the current week, controlling for each actor’s previous behavior. If actors respond to shifts in the casualty dynamics of the conflict in the ways I expect, two relationships will hold. First, the degree to which Palestinian fighters target civilians should be positively correlated with the casualty ratio in the previous weeks. A positive shock to the casualty ratio indicates that Palestinian militants are being less successful in targeting Israeli soldiers and civilians for violence, indicating that they are struggling militarily; this should lead to a greater emphasis on targeting Israeli civilians for violence. Second, the pace and intensity of Israeli-initiated violent events should be negatively correlated with the casualty ratio in the previous weeks. A negative shock to the casualty ratio indicates that the IDF is not being effective at defending Israeli targets from Palestinian violence, providing impetus to step up its peacekeeping efforts.

While both Palestinian targeting of Israeli civilians and the frequency of Israeli-initiated violent events do display strong autocorrelation, casualty ratios do not have the hypothesized effect on either type of behavior. IDF violence seems to be entirely orthogonal to casualty ratios, as none of the lagged casualty terms have a significant relationship to violence in the current week. Palestinian targeting of Israeli civilians in the current week does have a significant effect to the previous week’s casualty ratio, but this relationship is actually in the opposite direction. It appears that Palestinian fighters focus more of their efforts on attacking Israeli civilians when the previous week saw a relatively high level of Palestinian success. However, this relationship is short-lived: after

Table 2.2: Casualties and Behavioral Change

|                                    | % PAL<br>Civ Targeting<br>(1) | # ISR<br>Event Intensity<br>(2) |
|------------------------------------|-------------------------------|---------------------------------|
| PAL civ targeting <sub>t-1</sub>   | 0.13*<br>( 0.07 )             |                                 |
| PAL civ targeting <sub>t-2</sub>   | 0.16***<br>( 0.07 )           |                                 |
| PAL civ targeting <sub>t-3</sub>   | 0.13*<br>( 0.07 )             |                                 |
| PAL civ targeting <sub>t-4</sub>   | 0.01<br>( 0.07 )              |                                 |
| ISR event intensity <sub>t-1</sub> |                               | 0.38***<br>( 0.07 )             |
| ISR event intensity <sub>t-2</sub> |                               | 0.21***<br>( 0.07 )             |
| ISR event intensity <sub>t-3</sub> |                               | -0.02<br>( 0.08 )               |
| ISR event intensity <sub>t-4</sub> |                               | -0.00<br>( 0.07 )               |
| Casualty ratio <sub>t-1</sub>      | -0.17***<br>( 0.07 )          | 0.04<br>( 0.06 )                |
| Casualty ratio <sub>t-2</sub>      | 0.08<br>( 0.07 )              | -0.09<br>( 0.07 )               |
| Casualty ratio <sub>t-3</sub>      | -0.05<br>( 0.07 )             | -0.00<br>( 0.06 )               |
| Casualty ratio <sub>t-4</sub>      | 0.02<br>( 0.07 )              | -0.06<br>( 0.07 )               |
| Intercept                          | 0.00<br>( 0.11 )              | -0.00<br>( 0.13 )               |
| Log-Likelihood                     | -294.63                       | -274.27                         |
| AIC                                | 607.25                        | 564.54                          |

*Note:*

\*p&lt;0.1; \*\*p&lt;0.05; \*\*\*p&lt;0.01

the first week, the casualty ratio ceases to provide any predictive power. In both cases, there are significant autocorrelative effects. Both Israeli and Palestinian behavior shows continuity over time, suggesting that at least in the short run, these patterns of behavior are relatively stable.

This first set of tests does not support Hypotheses 1.1 and 1.2. These findings are interesting, as this lack of significance may actually reflect the complex dynamics of irregular conflict and insurgency. Neither Israeli nor Palestinian forces seem to react to casualty dynamics in the way I expected, which is somewhat counterintuitive given recent findings on casualties and conflict dynamics [e.g., Wood, 2010]; this indicates that we may need to further break down different types of conflicts to better understand how the context of casualties on both sides affects the behavior of both states and insurgents.

Table 2.3 shows coefficient estimates from testing Hypotheses 2.1, 2.2, and 2.3. This set of VAR models attempts to identify a cyclical relationship between Israeli use of heavy weaponry and (1) Palestinian reliance on indirect-fire weapons as well as (2) Palestinian targeting of Israeli civilians rather than military personnel.

Here, too, I find unexpected but interesting results. Israeli reliance on heavy weaponry and indiscriminate-fire technology shows no systematic relationship to Palestinian military behavior in the previous weeks. However, it does show significant autocorrelation: IDF activity in the previous two weeks is by far the strongest predictor of activity in the current week. This would suggest that Israeli military decision-making is internally ‘sticky’ and is relatively more predictable over time. This is loosely in line with bureaucratic theories which expect military decision-making to be fairly well-insulated from the outside world: regardless of short-term trends in casualties or Palestinian behavior, IDF use of heavy arms follows a set plan of action.

By contrast, Palestinian activity is more unpredictable. Interestingly, in the endogenous VAR modeling approach neither Palestinian targeting of civilians nor reliance on indirect-fire technology displays the type of autocorrelation that characterizes Israeli behavior or Palestinian behavior under the standard ARIMAX in Model 1: last week’s actions have little or nothing to do with this week’s actions. There are a few relationships that may

Table 2.3: Civilian Targeting and Technology of Violence

|                                   | % PAL<br>Civ Targeting<br>(3) | % ISR<br>Heavy Weapons<br>(4) | % PAL<br>Indirect-Fire<br>(5) |
|-----------------------------------|-------------------------------|-------------------------------|-------------------------------|
| PAL civ targeting <sub>t-1</sub>  | 0.09<br>( 0.08 )              | 0.02<br>( 0.07 )              | 0.18**<br>( 0.07 )            |
| PAL civ targeting <sub>t-2</sub>  | 0.10<br>( 0.08 )              | -0.12<br>( 0.07 )             | 0.05<br>( 0.07 )              |
| PAL civ targeting <sub>t-3</sub>  | 0.13<br>( 0.08 )              | 0.04<br>( 0.07 )              | -0.07<br>( 0.07 )             |
| PAL civ targeting <sub>t-4</sub>  | 0.00<br>( 0.08 )              | -0.07<br>( 0.07 )             | -0.06<br>( 0.07 )             |
| ISR heavy weaponry <sub>t-1</sub> | 0.08<br>( 0.08 )              | 0.35***<br>( 0.07 )           | 0.01<br>( 0.07 )              |
| ISR heavy weaponry <sub>t-2</sub> | 0.01<br>( 0.09 )              | 0.15**<br>( 0.08 )            | 0.08<br>( 0.08 )              |
| ISR heavy weaponry <sub>t-3</sub> | -0.15*<br>( 0.09 )            | -0.04<br>( 0.08 )             | -0.05<br>( 0.08 )             |
| ISR heavy weaponry <sub>t-4</sub> | -0.06<br>( 0.08 )             | -0.07<br>( 0.07 )             | 0.04<br>( 0.07 )              |
| PAL indirect-fire <sub>t-1</sub>  | 0.08<br>( 0.10 )              | -0.07<br>( 0.08 )             | 0.03<br>( 0.08 )              |
| PAL indirect-fire <sub>t-2</sub>  | 0.09<br>( 0.09 )              | 0.12<br>( 0.08 )              | 0.11<br>( 0.08 )              |
| PAL indirect-fire <sub>t-3</sub>  | 0.01<br>( 0.09 )              | 0.10<br>( 0.08 )              | 0.08<br>( 0.08 )              |
| PAL indirect-fire <sub>t-4</sub>  | 0.03<br>( 0.09 )              | -0.06<br>( 0.08 )             | 0.05<br>( 0.08 )              |
| Casualty ratio <sub>t-1</sub>     | -0.16**<br>( 0.07 )           | 0.08<br>( 0.06 )              | -0.01*<br>( 0.00 )            |
| Casualty ratio <sub>t-2</sub>     | 0.10<br>( 0.07 )              | -0.09<br>( 0.06 )             | 0.12<br>( 0.06 )              |
| Casualty ratio <sub>t-3</sub>     | -0.02<br>( 0.08 )             | 0.01<br>( 0.07 )              | -0.09<br>( 0.07 )             |
| Casualty ratio <sub>t-4</sub>     | 0.05<br>( 0.08 )              | -0.08<br>( 0.07 )             | 0.09<br>( 0.07 )              |
| Constant term                     | 0.43<br>( 0.26 )              | 0.56**<br>( 0.23 )            | -0.65***<br>( 0.23 )          |
| Trend term                        | 0.00*<br>( 0.00 )             | -0.01**<br>( 0.00 )           | 0.01***<br>( 0.00 )           |
| Adjusted $R^2$                    | 0.09                          | 0.31                          | 0.28                          |
| Weekly Observations               | 209                           | 209                           | 209                           |

Note:

\*p&lt;0.1; \*\*p&lt;0.05; \*\*\*p&lt;0.01

be worth further exploration, particularly in Model 3 (Palestinian targeting of civilians). Model 3 shows that there are small but significant relationships with Israeli activity and with casualty ratios. Looking at Table 2.3 alone, it appears that an increase in Palestinian targeting of civilians in the current week can be explained in part by a decrease in Israeli reliance on heavy weapons in week  $t - 3$  (significant at  $p < 0.10$ ), as well as an increase in the ratio of Palestinian to Israeli dead and wounded in the previous week (significant at  $p < 0.05$ ). However, given the nature of VAR models, it is necessary to estimate impulse-response functions and analyze simulated shocks to identify whether this relationship holds in the overall model framework.



Figure 2.7: Impulse-response function of ISR heavy arms on PAL civilian targeting

As Figure 2.7 shows, the mixed signs on lagged Israeli behavior seem to balance one another out. While the effect of an increase in Israeli heavy-weapons use on Palestinian civilian targeting is positive, it never reaches conventional levels of significance, and in fact changes sign to negative (but still insignificant) within the next four weeks. It appears that despite the statistically significant relationship between the individual variables shown in Table 2.3, once we examine the relationship in the context of the full VAR model, there

is no support for Hypothesis 2.3.

Palestinian reliance on indirect-fire technology is even more difficult to predict in this modeling context. The only term in the VAR model set that is significant at traditional levels is the trend term, which indicates a slow but steady shift away from direct engagement and towards ‘safer’ forms of engagement over time. Recalling Figure 2.4b, these results make sense, especially in the last year or so of the conflict.

Overall, Hypotheses 2.1, 2.2, and 2.3 are not supported. However, this lack of findings is quite interesting in light of the context of the Israeli-Palestinian conflict. Palestinian militants, faced with overwhelming Israeli military opposition and sever constraints on movement that render them unable to mount a mobile or rural guerrilla action, have adopted a strategy that can be overall characterized as terroristic. While militants did engage IDF forces, both directly and indirectly through bombing and shelling attacks, a significant level of violence was directed at Israeli civilians both in Israel proper and in settlements in the Palestinian territories.

Table 2.4 shows coefficient estimates from testing Hypotheses 3.1 and 3.2. These models attempt to identify a cyclical relationship between Palestinian violence in Israeli-administered territory (Israel proper and West Bank area C) and Israeli violence in Palestinian-administrated territory (the Gaza strip and West Bank areas A and B).

Here, too, findings are not in accordance with my hypotheses. Hypothesis 3.1 states that there should be a positive relationship between recent events of Palestinian violence in Israeli-administered territory and IDF activity in Palestinian-administered territory. However, the VAR approach does not detect this type of relationship. Instead, the strongest predictors of where the IDF tends to use force from week to week are (1) previous IDF behavior and (2) recent Israeli-Palestinian casualty ratios. As the ratio of Palestinian to Israeli dead increases in the previous week, the IDF becomes slightly more likely to focus operations in Palestinian territory. However, this effect is short-lived, as can be seen by the correspondingly negative coefficient on the two-week-lagged casualty ratio. In other words, an increase in Palestinian casualties relative to Israeli casualties is associated with a ‘blip’ in Israeli attacks in the Gaza strip and West Bank areas A and B. I expect that

Table 2.4: Violence in Palestinian Territories and Israel

|                                    | % PAL<br>Attacks in area C<br>and Israel<br>(6) | % ISR<br>Attacks in area A/B<br>and Gaza Strip<br>(7) |
|------------------------------------|---|---|
| ISR area A/B events <sub>t-1</sub> | 0.05<br>( 0.06 )                                | 0.11<br>( 0.07 )                                      |
| ISR area A/B events <sub>t-2</sub> | -0.09<br>( 0.08 )                               | 0.20**<br>( 0.09 )                                    |
| ISR area A/B events <sub>t-3</sub> | -0.08<br>( 0.08 )                               | -0.07<br>( 0.09 )                                     |
| ISR area A/B events <sub>t-4</sub> | -0.09<br>( 0.08 )                               | 0.21**<br>( 0.09 )                                    |
| PAL area C events <sub>t-1</sub>   | 0.06<br>( 0.07 )                                | 0.03<br>( 0.08 )                                      |
| PAL area C events <sub>t-2</sub>   | -0.09<br>( 0.07 )                               | -0.07<br>( 0.08 )                                     |
| PAL area C events <sub>t-3</sub>   | 0.05<br>( 0.07 )                                | 0.01<br>( 0.08 )                                      |
| PAL area C events <sub>t-4</sub>   | -0.04<br>( 0.07 )                               | -0.01<br>( 0.08 )                                     |
| Casualty ratio <sub>t-1</sub>      | -0.01*<br>( 0.01 )                              | 0.01*<br>( 0.01 )                                     |
| Casualty ratio <sub>t-2</sub>      | 0.01<br>( 0.01 )                                | -0.01*<br>( 0.01 )                                    |
| Casualty ratio <sub>t-3</sub>      | 0.00<br>( 0.01 )                                | 0.00<br>( 0.01 )                                      |
| Casualty ratio <sub>t-4</sub>      | 0.00<br>( 0.01 )                                | -0.01<br>( 0.01 )                                     |
| Constant term                      | 0.43***<br>( 0.11 )                             | 0.48***<br>( 0.12 )                                   |
| Trend term                         | 0.00***<br>( 0.00 )                             | 0.00<br>( 0.00 )                                      |
| Adjusted $R^2$                     | 0.32  | 0.07  |
| Weekly Observations                | 209   | 209   |

*Note:*

\*p&lt;0.1; \*\*p&lt;0.05; \*\*\*p&lt;0.01

this is not a causal relationship; it is more likely that this correlation is due to the fact that many major Israeli operations in Palestinian territory were (1) associated with high Palestinian casualties and (2) more than one week in duration. This would lead to an association between casualties and Israeli operations in Palestinian territory, but not one that follows the causal logic laid out in this analysis.

Hypothesis 3.2 anticipates a negative relationship between recent events of Israeli violence in Palestinian-administered territory and current levels of Palestinian violence in Israeli-administered territory. Here, too, I find a pattern of behavior that is unexpected. Neither Israeli behavior nor lagged Palestinian behavior serves as a significant predictor of where Palestinian insurgents tend to use violence. Here, too, there is limited evidence that casualty ratios tend to be associated with Palestinian attacks on Israeli-administered territory: increased success by Palestinian insurgents (at least, as measured by casualty ratios) tends to be associated with higher levels of Palestinian operation in Israel and area C of the West Bank. In these cases, it may be the inverse of the relationship observed in Hypothesis 3.1: when the IDF is not engaged in raids on Palestinian cities, militants have more freedom to operate and launch attacks on Israeli targets, making them more likely to engage in this type of more costly but more profitable attack.

Overall, Hypotheses 3.1 and 3.2 are not supported. However, again there is an interesting difference between Palestinian and Israeli actors. IDF behavior does have a systematic autocorrelative element: future Israeli violence in Palestinian-administered territories can be predicted (to some extent) by looking at previous behavior. The geography of Palestinian violence, however, is unpredictable in this modeling context; it does not respond directly to changes in the location of Israeli violence, and is highly variable from week to week.

Combining these metrics of behavior – type, target, and location of violence – for both sides produces results that are roughly in line with the individual tests presented above. Hypothesis 4.1 states that as Israeli violence more generally takes the form of large-scale violent repression, Palestinian insurgents are likely to adopt less-conventional strategies that more closely resemble terrorism. Hypothesis 4.2 states that as Palestinian

violence more closely resembles terrorism, the response from the Israeli military will become more severe in scope and scale, more closely resembling large-scale violent repression. Taken together, the pattern of violence should be one of Israeli escalation and Palestinian de-escalation.

Table 2.5: Strategies of Terrorism and Repression

|                                | % PAL reliance on<br>'terrorism' strategy<br>(8) | % ISR reliance on<br>'repression' strategy<br>(9) |
|--------------------------------|--|---|
| PAL terrorism <sub>t-1</sub>   | 0.26***<br>( 0.07 )                              | 0.10<br>( 0.07 )                                  |
| PAL terrorism <sub>t-2</sub>   | 0.10<br>( 0.07 )                                 | -0.24***<br>( 0.08 )                              |
| PAL terrorism <sub>t-3</sub>   | 0.15**<br>( 0.08 )                               | 0.13*<br>( 0.08 )                                 |
| PAL terrorism <sub>t-4</sub>   | 0.03<br>( 0.07 )                                 | -0.11<br>( 0.07 )                                 |
| ISR repression <sub>t-1</sub>  | 0.12<br>( 0.07 )                                 | 0.10<br>( 0.07 )                                  |
| ISR repression <sub>t-1</sub>  | -0.06<br>( 0.09 )                                | 0.15*<br>( 0.09 )                                 |
| ISR repression <sub>t-1</sub>  | -0.04<br>( 0.08 )                                | 0.03<br>( 0.08 )                                  |
| ISR repression <sub>t-1</sub>  | -0.15*<br>( 0.08 )                               | 0.07<br>( 0.08 )                                  |
| Casualty ratio <sub>t-1</sub>  | -0.16**<br>( 0.07 )                              | 0.24***<br>( 0.07 )                               |
| Casualty ratio <sub>t-1</sub>  | 0.14**<br>( 0.07 )                               | -0.07<br>( 0.07 )                                 |
| Casualty ratio <sub>t-1</sub>  | -0.08<br>( 0.07 )                                | -0.02<br>( 0.07 )                                 |
| Casualty ratio <sub>t-1</sub>  | 0.10<br>( 0.07 )                                 | -0.15**<br>( 0.07 )                               |
| Constant term                  | 0.07<br>( 0.14 )                                 | 0.22<br>( 0.14 )                                  |
| Trend term                     | 0.00<br>( 0.00 )                                 | -0.00*<br>( 0.00 )                                |
| Adjusted <i>R</i> <sup>2</sup> | 0.17   | 0.12  |
| Weekly Observations            | 209  | 209   |

*Note:*

\*p&lt;0.1; \*\*p&lt;0.05; \*\*\*p&lt;0.01

Table 2.5 shows the results of testing Hypotheses 4.1 and 4.2. Here, some patterns

emerge that are not present when examining isolated aspects of Israeli and Palestinian behavior.

Lagged Israeli reliance on large-scale violence does show a slight relationship with current levels of Palestinian reliance on unconventional or terrorist forms of violence. A decrease in large-scale repressive violence in the previous month (a four-week lag) actually leads to a slight increase in current levels of Palestinian terrorism. While this is not in line with my initial expectation, with further thought this does make some sense. If terrorism is a strategy adopted by groups facing a stronger force, or groups who are perceiving that conflict is not going well, then this may be a sign that large-scale, indiscriminate operations in the Palestinian territories are not the best way to prevent future acts of violence. Instead, the negative sign on this coefficient suggests that an increase in the IDF's reliance on small-scale, selective strikes against known or suspected militants is more effective at damaging insurgent capacity, forcing Palestinian insurgents to rely more heavily on low-cost, indirect forms of violence such as terrorist attacks.

This idea is reinforced in Model 7, which estimates the relationship between previous levels of Palestinian terrorist violence and current levels of Israeli reliance on large-scale repressive violence. Here, I find a statistically significant and negative relationship between Palestinian terrorism and Israeli large-scale violence. An increase in Palestinian reliance on terrorist violence (indicating an increase in relative reliance on more conventional forms of violence) in the previous two weeks leads to an increase in IDF reliance on large-scale repressive violence in the current week. In other words, the more conventional a form Palestinian tactics take, the more intense and unrestrained the Israeli response is.

Figure 2.8 the results of IRF simulation of this relationship. Approximately two weeks after a simulated increase in Palestinian reliance on unconventional or terrorist tactics, there is a significant but short-lived decrease in the expected Israeli reliance on large-scale repressive violence. This is an interesting and unexpected finding that runs counter to my expectations. My hypotheses anticipate a cycle of Israeli escalation and Palestinian de-escalation, but instead the correlations seem to run in a different direction: the greater the Palestinian focus on using conventional weapons to attack Israeli military targets in

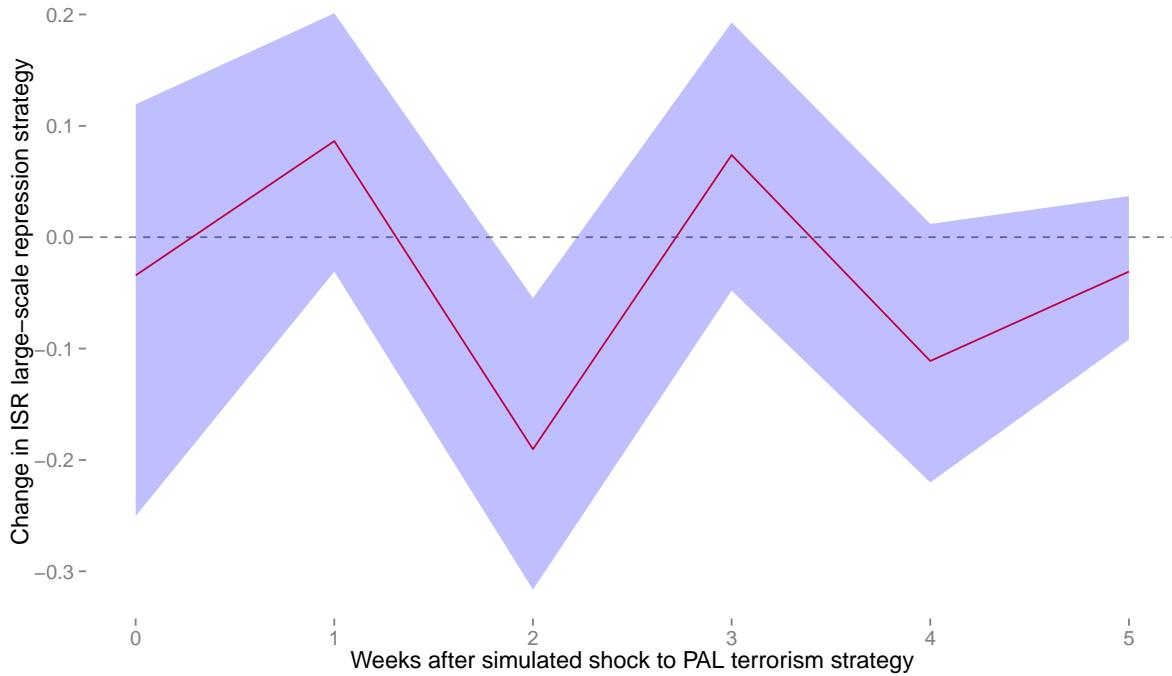


Figure 2.8: Impulse-response function of PAL terrorism tactics on ISR large-scale repression

the core Palestinian territories – a more conventional strategy in terms of technology, target, and location – the greater the subsequent Israeli reliance on heavy arms, civilian targeting, and operations in Palestinian cities. It may be that Palestinian violence against the IDF actually provokes a more severe long-term reaction, in terms of week to week violence, than the often-attempted but rarely successful attacks launched against civilian targets in Israel and the West Bank.

One additional note of interest in these findings is the significant relationship between recent casualty ratios and the strategies of violence employed by both Palestinian and Israeli forces. Once again, this is an interesting and counterintuitive finding. An increase in the conflict casualty ratio, as I measure it, indicates a shift in the rate of Palestinian to Israeli dead and wounded every week, indicating a worsening in the relative rate of Palestinian losses. Using casualty ratios as an indicator of short-term success, an increase in this value is worse for Palestinians and better for Israelis.

Here, I find a significant and negative relationship in Model 6 between casualty ratios in the previous week and Palestinian reliance on terrorist tactics in the current week. In

other words, as the ratio of Palestinian to Israeli casualties increases, Palestinian insurgent forces rely more heavily on conventional violence. This is not the effect I would have anticipated; again, if terrorism is a strategy adopted by losing forces, we should expect to see an increase in terrorist tactics following an increase in the casualty ratio. Likewise, Model 7 shows a significant and positive relationship between the previous week's casualty ratio and the current reliance on large-scale repressive violence by the IDF. This is also counterintuitive – it is unclear why an indication of Israeli success should lead to a greater reliance on large-scale violence.

Table 2.6 shows coefficient estimates for the second set of VAR models testing Hypotheses 5.1, 5.2, and 5.3. This set of models tests for a de-escalatory cycle of events between Israeli and Palestinian actors: successful targeting of militants by the IDF depresses Palestinian ability to launch attacks, which in turn reduces the need for, and legitimacy of, heavy-arms operations by the IDF.

This set of models also produces a set of unexpected but interesting results. Hypothesis 5.1 states that there should be a direct and negative relationship between Israeli targeting of Palestinian militants and overall levels of Palestinian violence from week to week. However, there is no evidence that this relationship exists. Instead, there is significant autocorrelation in Palestinian event intensity from week to week, suggesting that Israeli activity does not necessarily have a direct, short-term suppressive effect on Palestinian violence. This is interesting, especially given the lack of autocorrelation in the *type* and *target* of Palestinian violence from week to week. While overall levels of violence remain fairly stable in the short term, it appears that how violence is deployed, and the choice of targets at which violence is aimed, varies highly so as to avoid detection, prediction, and destruction.

Hypothesis 5.2 states that there should be a direct and negative relationship between Palestinian violence and Israeli reliance on light arms versus heavy arms or indiscriminately violent weapons. As Palestinian violence subsides, the IDF can respond in kind. Interestingly, the coefficient estimates in Model 12 do support this relationship: it appears that as the intensity of Palestinian aggression decreases, the proportion of Israeli-initiated

Table 2.6: Military Targeting and Intensity of Violence

|                                    | % ISR<br>Mil Targeting<br>(10) | # PAL<br>Event Intensity<br>(11) | % ISR<br>Light Arms<br>(12) |
|------------------------------------|--------------------------------|----------------------------------|-----------------------------|
| ISR mil targeting <sub>t-1</sub>   | 0.10<br>( 0.07 )               | 0.01<br>( 0.06 )                 | 0.06<br>( 0.07 )            |
| ISR mil targeting <sub>t-2</sub>   | 0.06<br>( 0.07 )               | -0.05<br>( 0.06 )                | 0.08<br>( 0.07 )            |
| ISR mil targeting <sub>t-3</sub>   | 0.12<br>( 0.07 )               | -0.05<br>( 0.06 )                | 0.03<br>( 0.08 )            |
| ISR mil targeting <sub>t-4</sub>   | 0.01<br>( 0.07 )               | -0.02<br>( 0.06 )                | -0.03<br>( 0.07 )           |
| PAL event intensity <sub>t-1</sub> | 0.13<br>( 0.09 )               | 0.30***<br>( 0.07 )              | -0.21***<br>( 0.08 )        |
| PAL event intensity <sub>t-2</sub> | 0.09<br>( 0.09 )               | 0.19**<br>( 0.07 )               | 0.05<br>( 0.08 )            |
| PAL event intensity <sub>t-3</sub> | -0.17*<br>( 0.09 )             | -0.13*<br>( 0.07 )               | 0.03<br>( 0.08 )            |
| PAL event intensity <sub>t-4</sub> | 0.03<br>( 0.09 )               | 0.01<br>( 0.07 )                 | -0.04<br>( 0.08 )           |
| ISR light arms <sub>t-1</sub>      | -0.2<br>( 0.08 )               | -0.02<br>( 0.07 )                | 0.15**<br>( 0.07 )          |
| ISR light arms <sub>t-2</sub>      | -0.08<br>( 0.08 )              | -0.08<br>( 0.07 )                | 0.29***<br>( 0.08 )         |
| ISR light arms <sub>t-3</sub>      | 0.05<br>( 0.10 )               | 0.03<br>( 0.08 )                 | 0.00<br>( 0.09 )            |
| ISR light arms <sub>t-4</sub>      | -0.01<br>( 0.09 )              | -0.07<br>( 0.07 )                | -0.09<br>( 0.08 )           |
| Casualty ratio <sub>t-1</sub>      | -0.00<br>( 0.07 )              | 0.05<br>( 0.06 )                 | -0.03<br>( 0.07 )           |
| Casualty ratio <sub>t-2</sub>      | -0.18**<br>( 0.07 )            | 0.06<br>( 0.06 )                 | 0.17**<br>( 0.07 )          |
| Casualty ratio <sub>t-3</sub>      | -0.05<br>( 0.08 )              | -0.03<br>( 0.06 )                | -0.00<br>( 0.07 )           |
| Casualty ratio <sub>t-4</sub>      | 0.14*<br>( 0.08 )              | 0.02<br>( 0.06 )                 | -0.11<br>( 0.07 )           |
| Constant term                      | 0.20<br>( 0.18 )               | 0.58***<br>( 0.15 )              | -0.18<br>( 0.16 )           |
| Trend term                         | -0.00<br>( 0.00 )              | -0.01***<br>( 0.00 )             | 0.00<br>( 0.00 )            |
| Adjusted <i>R</i> <sup>2</sup>     | 0.15                           | 0.40                             | 0.27                        |
| Weekly Observations                | 209                            | 209                              | 209                         |

*Note:*

\*p&lt;0.1; \*\*p&lt;0.05; \*\*\*p&lt;0.01

violence that relies on light arms (as opposed to heavy weaponry) increases. This suggests that some element of de-escalation is possible: as Palestinian attacks lose momentum, IDF forces adopt less indiscriminately violent tactics, even after controlling for past IDF behavior from week to week. However, again it is necessary to analyze impulse-response functions to assess whether this set of coefficient estimates translates into a discernible relationship in the full VAR framework.

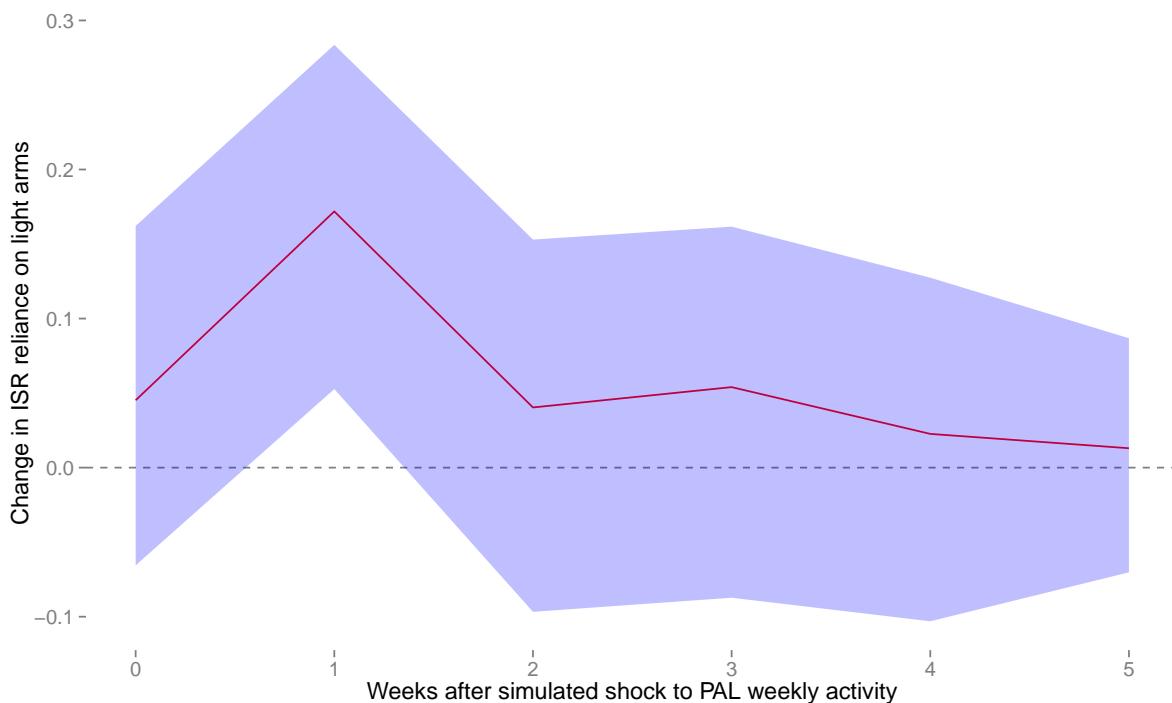


Figure 2.9: Impulse-response function of PAL weekly activity on ISR use of light arms

It appears that Hypothesis 5.2 is the only expectation that is confirmed by statistical analysis: after controlling for the other variables in this system of models, a decrease in Palestinian activity is linked to an increase (decrease) in IDF reliance on light arms (heavy weapons). Both the bivariate relationship identified in Table 2.4 and the fully specified impulse-response function in Figure 2.9 support this relationship, which may be useful in explaining de-escalation in the rate and type of violence during the Second Intifada.

It also falls in line with conventional military wisdom: when the state can force a ‘downshift’ in the rate of attacks by an insurgent, it becomes less profitable to engage in high-intensity, heavy-arms operations to catch a less-threatening, less-active opponent.

I argue that this relationship makes sense especially in the unconventional arena of the Second Intifada, because the level of force deployed by the IDF was (and still is) highly subject to both domestic and international political pressure. I would argue that this finding indicates that the Israeli government needs some level of provocation before the IDF is willing and able to deploy heavy arms. In periods that are defined by low levels of Palestinian violence, this level of force is much more difficult to justify, and so becomes less politically tenable even if its military efficacy remains high.

To summarize these various findings, I find little evidence that Israeli and Palestinian conflict behaviors covary in the short run over time – at least, not in the ways I originally anticipated. There are no significant findings that suggest the type or target of Israeli (Palestinian) violence in the previous weeks have a strong effect on the type or target of Palestinian (Israeli) violence in the future. In fact, particularly for Palestinian militants, violent behavior is nearly impossible to predict with these variables. The type, target, and location of Palestinian violence varies tremendously from week to week, to the point where looking at previous patterns of behavior on a given dimension provide no useful information about what will happen this week or next. While this does not support my theory of military adaptation, it does provide evidence for an interesting alternative: that within the geographic constraints of the occupied Gaza Strip and West Bank, even the period of intense organized violence that marked the Second Intifada was governed by a logic often referred to as ‘terrorism’. Weak and outnumbered Palestinian militants engaged both the IDF and Israeli civilians, but they did so in highly unpredictable ways: the type, target, and location of Palestinian attacks varied wildly from week to week, making them difficult to predict or counter.

Israeli behavior, by contrast, shows more signs of ‘stickiness’ over time. Israeli reliance on different types of armaments, the geographic patterns of sustained IDF violence, and the type of target selected for attack can be predicted in large part by looking at IDF behavior in previous weeks. In addition, the few significant action/reaction relationships identified in these models suggests that it is generally the IDF that responds to shifts in Palestinian militant behavior, rather than vice-versa. This is particularly interesting

because it suggests a fundamentally different decision-making process underlying Israeli versus Palestinian military action and adaptation. As Palestinian militants engage in less violence — whether due to successful IDF military suppression or due to political factors — the IDF relies less on heavy arms like tanks and fighter aircraft, and more on light arms in its efforts against Palestinian militants. This suggests that there is some logic of proportionality in the IDF response to Palestinian violence, which (at least in part) supports Israeli governmental claims to the same effect.

## 2.6 Threshold Detection in Actor Behavior

Even if the type of week-to-week adaptation I expected does not exist in this conflict, this may not be the same as assuming both actors ignored the type and target of violence each other relied on. There may be some type of ‘threshold effect’ present here, where minor variation in one actor’s behavior does not provoke a reaction by the other, but major changes are identified and reacted to. This type of threshold effect is difficult to identify and predict, but as a preliminary analysis, I look at actor behavior before and after key points in the conflict. I begin by focusing on a time period identified by historians as a major change point in the nature of the conflict: Operation Defensive Shield.

Defensive Shield was the first major combined-arms operation conducted by the IDF in the Palestinian territories during the Second Intifada. It was prompted by an increase in the frequency and severity of Palestinian suicide bombings in early 2002. Not only did the rate of attacks increase significantly over previous months, the effects were more deadly: in two suicide attacks (The Cafe Moment bombing in Jerusalem and the Park Hotel bombing in Netanya), some 41 Israeli civilians were killed and nearly 200 were wounded. These operations had a major effect on Israeli morale, and provoked a much more severe reaction from the IDF than had been previously thought necessary or permissible. In late March, the Israeli government declared that a major counter-terrorism offensive was necessary, and called up 30,000 reserve IDF troops to active duty.

Defensive Shield targeted several Palestinian cities and refugee camps that the Israeli government had designated as breeding grounds for terrorists. Simultaneous raids involving large numbers of troops supported by heavy armor and attack helicopters were carried out

in Jenin, Nablus, Tulkarm, Bethlehem, and Ramallah. This represented a major escalation in Israeli force projection, and in most regions was met with active resistance by Palestinian militants. Over the next several weeks, the IDF crushed Palestinian resistance in these areas — at least temporarily — by destroying infrastructure, killing militant fighters, and arresting suspected militants by the hundreds. The IDF suffered some 200 casualties (30 killed and 140 wounded), while Palestinian estimates of dead and wounded vary widely between sources but hover in the mid-hundreds of deaths and over one thousand wounded. The IDF also arrested over 7,000 Palestinians suspected of supporting terrorism, although many were quickly released.

The question here is whether Operation Defensive Shield represented a significant and lasting shift in the type of conflict both sides were fighting. Defensive Shield was certainly disruptive, especially in the short term, and it was the first major use of force by the IDF during this four-year period. However, it is not clear if this operation was a unique (or at least a rare) type of event brought on by special circumstances, or if it actually represented a sea change in Israeli strategy. I expect that this operation represented a lasting change in the level and type of force the IDF was willing to use in combating Palestinian attacks. I also expect that this action by the Israelis provoked a change in Palestinian strategy for two reasons. First, Defensive Shield had a significant impact on the fighting capability of Palestinian militants: major weapons caches were destroyed, fortified positions were bulldozed or bombed, and trained fighters were killed or arrested. Second, militant leadership was certainly aware after this point that the IDF was now able and willing to use major force in the Palestinian Territories — the events of Defensive Shield suggest that this signal would have been very hard to miss. This represented a significant change in the combat environment, one that required significant adjustment in Palestinian strategy to accommodate.

If Defensive Shield does in fact represent a significant shift in the way both Palestinian and Israeli actors fought, these changes should be detectable using the data I have gathered. Statistical analysis should identify Defensive Shield as a major change point in the conflict: actors should behave differently before versus after this major evolution in Israeli strategy.

If this difference can be detected, it poses an interesting and new set of questions about actor updating during conflict: if strategic change is driven by ‘significant events’ like Operation Defensive Shield, how do we know if an event is, in fact, significant enough to drive change?

To test for this expected difference, I rely on the same sets of time-series data, focusing on two streams of behavior. First, I look at Israeli reliance on heavy arms. This is a logical behavior to analyze, as Defensive Shield represented the first major combined-arms operation conducted by the IDF during the conflict. On the Palestinian side, I analyze militant reliance on indirect-fire weapons. If ODF represents a significant escalation of Israeli force, the only way to succeed for Palestinian militants in these new circumstances would be to avoid direct engagement with the IDF and instead concentrate more heavily on lower-risk forms of violence.

I use change-point (CP) analysis to identify whether/when there are statistically significant shifts in Israeli and Palestinian behavior. CP analysis is a method of analyzing time series data to identify shifts in the underlying data-generating process that produced that information. This allows the practitioner to divide a given time series into a set of ‘regimes’ characterized by constant or near-constant mean within regimes and statistically significant shifts in mean from one regime to the next. This is a way to take a noisy, chaotic set of time series data and extract time points where meaningful shifts occur. By detecting periods in which significant shifts in Palestinian and Israeli behavior occur, I can identify where and how each group’s military-strategic behavior changed over time.

I use a non-parametric algorithm known as energy-divisive change point analysis. This method uses repeated resampling to identify individual points in time that are most statistically probable locations for change points. This approach has three characteristics that make it an excellent tool for this case. First, it is entirely inductive: there is no prior information entered into the identification process. In this case, it helps protect against issues of data-mining to find a change point in the location at which I hypothesize it should be found. Second, it can identify multiple change points in a single time series, using information criteria as a rule for penalizing the number of points located. Third, it

identifies  $p$ -values for each likely change point located, allowing for a minimum threshold — in this case,  $p < 0.001$  — of significance to identify a change point.

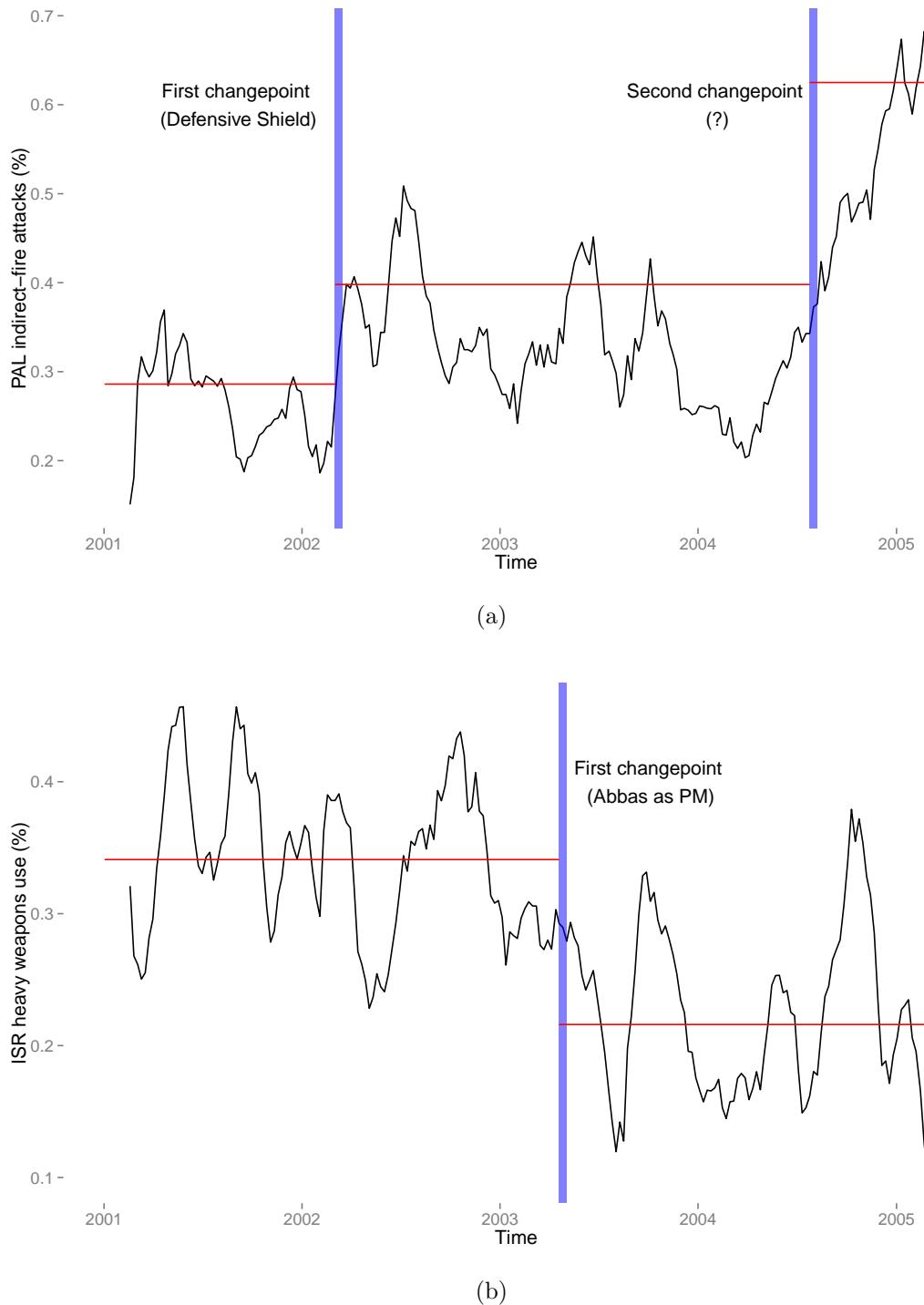


Figure 2.10: Change points in Israeli and Palestinian behavior

Figure 2.10 shows the result of two sets of CP analysis for Israeli and Palestinian behavior. Results are somewhat mixed, but provide some interesting food for thought. First, there does appear to be a significant shift in Palestinian reliance on indirect-fire arms, and a significant regime change occurs in early March of 2002 — right when Operation Defensive Shield was launched. It does appear that Defensive Shield signified a major change in the course of the conflict for Palestinian militants. Interestingly, there is a second significant change point identified in this process, located in late July of 2004. This showcases one downside of inductive CP analysis: statistically, this has been identified as a significant change point in Palestinian reliance on indirect-fire arms, but the historical record of the Second Intifada does not record any events of the same significance as Operation Defensive Shield that occurred in this week, or even this month. Violence certainly took place — a Palestinian suicide bomber killed 10 Israeli civilians, and several militant leaders were killed by Israeli air strikes — but nothing of major import. However, visualizing Palestinian activity over time, it does appear that this week falls in the early stages of a significant increase in indirect-fire weapons use by Palestinian militants, a trend that continued until the end of the conflict in early 2005.

Second, CP analysis does find a significant shift in Israeli military behavior, but this change point is not located at any point near Operation Defensive Shield. While ODF did represent a significant escalation in IDF violence, it appears that this does not show up in the measure I use here of counts/proportions of violent events per week. Instead, this shift occurs in the same week as the appointment of Palestinian Prime Minister Mahmoud Abbas. This finding is very interesting, as it strongly suggests that the kind of action/reaction dynamic I hypothesize encompasses more than just military action. Abbas ran on a peace platform, and was explicitly backed by the Israeli government in his bid for power. It appears that this support was more than just words — Abbas' ascension to the PM position is associated with a significant and lasting reduction in Israeli use of heavy arms. This arguably reinforces the findings suggested in my previous analysis. While Israeli military strategy — at least as identified along this one dimension — does shift significantly over the course of the conflict, the timing of this shift suggests that it

was largely driven by internal and political factors. Israel strongly supported Abbas, a pro-peace candidate for PM, and it would have looked bad for IDF tanks and aircraft to be launching major raids into Palestinian territory right after his appointment to leadership. These findings are hardly conclusive, but they provide an interesting counterpoint to the previous set of models analyzing actor updating behavior.

## 2.7 Conclusions

This research approach integrates novel data on the Israeli-Palestinian conflict with an innovative coding and modeling process to try and identify something that is very tricky *ex ante*: when, how, and why actors engaged in conflict will make significant changes to the way they fight. My theory of rational conflict adaptation suggests that the type, target, and location of violence chosen by one actor will be noticed, processed, and responded to by the other actor: in other words, that changes in the behavior of actor A will be met with changes in the behavior of actor B, and vice-versa. I test my hypotheses using a mixture of standard autoregressive time-series models and vector-autoregressive (VAR) model combinations, looking for systematic linkages between Israeli and Palestinian behavior over the course of the Second Intifada.

I do not find the linkages I expected to see between Israeli and Palestinian violent behavior in a week-to-week framework. In the majority of my VAR models, neither the type, target, or location of violence that either side engages in seems to have a significant impact on the military-strategic behavior of the other party. However, these findings suggest that something more complex may be at work here. My models repeatedly show that Palestinian violence is harder to predict from week to week, making it extremely difficult to predict the type or target of violence. This may be a purposeful choice: materially weak actors benefit from a strategy of violence that makes their behavior hard for an opponent to explain or predict. IDF violence shows less variability, suggesting that Israeli leadership does not respond to short-term variation in patterns of Palestinian violence.

However, there is some evidence that the strategies of both Israeli and Palestinian forces changed over time. Changepoint analysis reveals significant phase shifts in the

military approaches of both sides. On the Palestinian side, continued IDF pressure led to a significant move away from conventional tactics involving direct confrontation with Israeli forces, and an increased reliance on indirect-fire weaponry and assaults on Israeli civilians and settlements in Gaza and the West Bank. This shift in focus was accompanied by a decrease in the overall intensity of Palestinian violence, likely due to a combination of attrition and erosion of political support for the intifada – particularly in the last six months of violence before formal accords were reached. On the Israeli side, the initial year of violence saw a major shift away from preventative violence in Israel and area C of the West Bank, and much more retaliatory and preventative violence launched into the Gaza Strip and areas A and B of the West Bank. Although this shift in the geographic focus of IDF violence remained through the end of the conflict, as the overall intensity of Palestinian attacks decreased, the IDF came to rely more on light arms rather than large-scale or indiscriminate forms of violence.

This approach does suggest that there were measurable changes to Palestinian insurgent behavior leading up to the end of conflict. In the months leading up to the 2005 signing of the ceasefire, Palestinian militants shifted to a strategy of violence that relied overwhelmingly on low-cost, indirect-fire weaponry. While it may be a stretch to claim that this was a leading indicator of the erosion of Palestinian fighting capability or willpower, this is one finding that is in line with my overall expectations: as the conflict progressed and it became clear that the Intifada was losing popular support, insurgents became much more likely to engage in lower-intensity violence rather than directly confront the much stronger IDF forces. However, it is still unclear whether or how this change could be predicted; the VAR models used in this chapter do not show a systematic pattern of action and response that links IDF behavior or Palestinian losses to this behavioral change.

There is some evidence that major developments both on and off the battlefield can provoke significant readjustment of military strategy. On the Palestinian side, Operation Defensive Shield put militants back on their heels in a way from which they never recovered. Instead, militants quickly shifted their military strategy to rely much more heavily on indirect-fire weapons that (1) could reach targets in Israel, such as rockets and mortars,

and (2) exchanged accuracy and destructive power for operator safety. Rather than directly engage with IDF forces, militants came to rely more heavily on tactics that minimized risk. Given the overwhelming scholastic focus on suicide tactics employed by Palestinian groups during this time, this is a very interesting finding: when not engaged in purposefully suicidal violence, militants were quite interested in living to fight another day.

Israeli behavior during this same time suggests that actors in conflict respond to developments in the opponents' political arena as well as — or instead of, in this case — the military arena. Mahmoud Abbas' appointment as Prime Minister was a major political development in the Second Intifada. For the Israelis, this represented a major step towards peace: appointing a pro-peace leader was symbolic of Palestinian resolve weakening, and would have real political impact as Abbas would throw his power behind peace negotiations. A decrease in IDF heavy-arms operations may signify either (1) a good faith effort on the part of the Israelis so as not to threaten the domestic legitimacy of their preferred candidate, or (2) a pragmatic reduction in offensive operations as a result of decreasing Palestinian violence, as Abbas' domestic policies led to a shift in Palestinian behavior. I suspect that both explanations are, to an extent, correct. There is a significant decrease in Palestinian attacks starting in the months leading up to Abbas' appointment, and one of the only significant findings resulting from my earlier hypothesis tests suggests that the IDF moved away from heavy-arms operations as Palestinian attacks per week decreased.

It also points out a potential weakness in this event-based analysis of conflict behavior. For the IDF, Operation Defensive Shield did represent a change in military behavior, but this impact is not necessarily reflected in weekly event counts and proportions. The magnitude of ODF in terms of destruction and loss of life may not be accurately represented when the operation is reduced to a weekly count of conflict events. Further analysis may prove useful in identifying the ‘real’ impact of Operation Defensive Shield on the dynamics of conflict during the Second Intifada.

More broadly, my findings provide new information on the intra-conflict behavior of both state and insurgent actors. Two findings in particular are directly applicable to the

topic of counterinsurgency. First, I find significant evidence that, at least in week-to-week behavior, insurgents and states have fundamentally different decision-making processes. This is particularly interesting when contrasted with previous works that qualitatively analyze states and insurgents separately, and come away with expectations that states are static or slow to learn while insurgents can do so much more rapidly. In my analysis, I do not find evidence for this difference in adaptive capacity: instead, it appears that neither insurgents nor states engage in much on-the-fly adaptation, whether in direct response to one another's conflict behavior or in response to shifts in relative casualty rates.

Second, the fact that some shifts do occur, and that these shifts correlate with 'important' developments both on and off the battlefield, is a very interesting avenue for future study. The immediate question, naturally, is how to identify and predict this type of 'important' event. Defensive Shield was not the only major operation launched by the IDF during the Second Intifada, and there were many other important political developments besides Mahmoud Abbas' appointment during this period as well. It is likely that identifying and analyzing likely changepoints in actor behavior takes a mixture of quantitative analysis and qualitative expertise, and suggests that while quantitative analysis of behavioral trends is a useful starting point, some level of substantive knowledge is essential to identifying when, and more importantly *why* these changes occur.

S

# Chapter 3

## Northern Ireland

### 3.1 Introduction

During the Troubles, conflict between Republican paramilitaries (especially the Provisional Irish Republican Army, or PIRA), unionist paramilitaries, and the British state was characterized by consistent and pervasive strategic updating. Over the nearly thirty years of relatively high-intensity conflict, both Republican and British forces engaged in a constant pattern of assessment and adaptation, characterized by ebbs and flows in the type, location, and targets of violence over time. Substantive and historical accounts of the Troubles tend to divide the period between 1969 and 1994 into three or four broad periods. The exact boundaries of where one period ends and another begins are sometimes fuzzy, but overall these periods are characterized by distinctive patterns of behavior, both on and off the battlefield, by both the PIRA and the British government.

The first period of conflict, from 1968 through 1974, was characterized by high levels of violence, focusing in the large cities of Belfast and Derry. Republican paramilitary groups, particularly the PIRA (an offshoot of the older IRA movement), engaged local security forces directly, barricading streets and firing on police officers. The goal was to rapidly create an untenable security situation, forcing concessions from the British government in return for peace. Instead, Britain launched Operation Motorman in 1972, shipping over 30,000 British Army personnel to Northern Ireland in an attempt to keep the peace through an overwhelming security presence. While intense fighting persisted for the next

two years, the sheer level of force projected by the British forced a reevaluation of overall PIRA strategy.

Through the early 1980s, the PIRA buckled down to what was referred to as the ‘long war’ [Ni Aolain, 2000]. Rather than engaging the British Army and local police forces in direct action, the PIRA moved to an indirect method of warfare. In an attempt to foil British intelligence gathering, PIRAs command structure was reorganized as a cellular system, in which small ‘active service units’ (ASUs) operated independently and had little contact with one another [Jarman, 2004]. These units operated in many rural areas, particularly those with friendly populations and rough terrain like County Armagh, and engaged in acts of sabotage, assassinations, and indirect attacks on security personnel who were off-duty or on patrol. During this period, the British responded to the reduction in overall levels of PIRA violence by stepping down their own level of militarization, turning more day-to-day operations over to the local security forces [Jarman, 2004]. The approach of ‘Ulsterisation’ attempted to engage more directly with the minority Catholic population, in an attempt to erode PIRA support at home. However, British attempts to win over the Catholic population were often met with significant resistance from the local Protestant power structure, which was threatened by efforts to increase Catholic representation .

By the early 1980s, it was clear that Ulsterisation was perceived as a failure by both sides. The PIRA, frustrated by their failure to effect significant political gains over more than a decade of fighting, and enduring slow degradation of their military capabilities through police raids and defensive measures by the British Army, sought a re-escalation of the conflict [Finegan, 2014]. While there was still a clear perception that the war would never be won in a purely military sense, PIRA leadership believed that a shock of intense violence, mostly in the rural areas of East Tyrone and Armagh, could create a ‘liberated zone’ in the southern territories where British and RUC personnel could not venture without significant risk [Ni Aolain, 2000]. During this period, Republicans were able to strike a deal with Omar Gaddafis regime in Libya, receiving large shipments of assault rifles, military-grade explosives, and shoulder-mounted rocket launchers. This increase in capability helped open new opportunities and tactics for PIRA violence, and this was

reflected in a significant upswing in large-scale bombing attacks as well as direct assaults on security personnel and security bases.

This shift towards high militarization provoked a proportional response from the British government, who delegated more responsibility to the Army and the SAS [Finegan, 2014]. This period was characterized by significant increases in both offensive and defensive operations by the British. Defensively, bases were hardened against bombing and rocket attacks, and personnel were equipped with more body armor and armored vehicles. This was also the period when the system of watchtowers were built in County Armagh: large, permanent observation stations with armor capable of resisting small arms and rocket fire, which were used to guard the porous border with the Irish Republic. Offensively, the SAS was given significantly more leeway to engage in traditional ‘counterinsurgency’ operations, meaning direct-fire incidents became both more common and more deadly. Taking advantage of intelligence provided by double agents within the PIRA, the SAS and British military engaged in a system of ambushes, in which PIRA members were lured into fortified positions and arrested or killed.

While it was not common knowledge at the time, declassified documents show that this stepped-up campaign of mutual violence played a powerful role in bringing the PIRA to the negotiating table [Dingley, 2008]. By this point, many senior PIRA activists, invaluable for their experience and expertise, had been captured or killed; the rate of recruitment, compared to the rate of attrition, was simply not high enough to maintain a high level of combat effectiveness. This did not mean that violence was a thing of the past; while the overall level of PIRA activity decreased, and violence became less direct and more scattered, this was also the period in which Britain was attacked directly by PIRA bombers, putting the costs of violence directly before the British people. Along with this violence, however, by the late 1980s and early 1990s PIRA leadership had begun secret talks with the British government to agree on ceasefire terms. By 1994, the signing of the Good Friday agreement signaled, if not an end, then a massive reduction in PIRA violence in Northern Ireland.

## 3.2 Identifying strategic updating

Many accounts of the Troubles rightly emphasize the role of political factors in governing both PIRA action and British reaction, both on and off the battlefield. It is true that this conflict was fought in a highly politicized and publicized arena, and that this environment had a powerful effect on the patterns of violence during this time. The British government had little desire and less incentive to carry out a large-scale military campaign in Northern Ireland [Colin McInnes, 1997]; the political costs of crushing the Republican movement with pure military force, let alone the price in manpower and resources of defeating an insurgent movement with widespread popular support, were far too high.

This means that the British military involvement was characterized by a (relatively) very high level of restraint. These were still British citizens, subject to British legal protection, and British and Irish media alike closely covered the conflict from a very early stage, meaning that conventional counter-insurgency strategy was not acceptable in this conflict [Kennedy-Pipe and McInnes, 1997]. Additionally, the British forces worked directly alongside the local police force, the Royal Ulster Constabulary (RUC). Despite Britain enduring more casualties during the Troubles than the entirety of its involvement in the recent campaigns in Iraq and Afghanistan combined [Edwards, 2011], military response to Republican violence was still (relatively) highly constrained by the potential costs of indiscriminate violence. A major military campaign to crush Republican groups, with the inevitable civilian casualties it would bring, represented an unacceptable political cost to the British state.

In addition to these factors, the entire political system of governance in Northern Ireland made for additional complications in dealing with the Republican movement. The quasi-autonomous Northern Irish Parliament (Stormont) held little legitimacy either for the Catholic community in Northern Ireland or for the British Parliament, but still made British direct management of the conflict more difficult because Stormont was both democratically elected and loyal to Great Britain [Kennedy-Pipe and McInnes, 1997]. In fact, during the early months of the Troubles, the British Army was largely welcomed by the Catholic community, as they were perceived to be less biased against Catholics than

the local police forces [Thornton, 2007].

Political factors played a major role in the dynamics of political struggle during the Troubles. However, the military aspect of the conflict is impossible to ignore. While the British government waged both public and private campaigns to bring about negotiations with the various Republican movements, bringing them to the table required significant levels of military force. Despite the relatively low number of overall casualties in the conflict, the level of resources devoted to quelling PIRA violence was enormous; at the peak of violence in the early 1970s, the British military had over 30,000 forces occupying Northern Ireland and engaged in active ‘peace-keeping’ duties [CAI]. On the subject of casualties, as well: despite the low number of overall deaths due to Republican, British, and Loyalist violence, bear in mind that Northern Ireland is not a large or populous territory, and so looking at absolute numbers of casualties is not informative: “scaled proportionally, had the conflict taken place in Great Britain, it would have claimed 100,000 lives, or 500,000 in the United States” [Edwards, 2011, pos.1484]. Additionally, looking at deaths alone does not tell the full story; survey evidence shows that nearly a third of the population of Northern Ireland, whether Catholic or Protestant, had someone close to them victimized by violence during this period [Edwards, 2011].

The British Army and the local Northern Irish security force, the RUC, both engaged in long-term militarized security actions against Republican groups, primarily the PIRA. Pro-Union paramilitary groups also engaged in significant violence: in fact, more deaths are attributed to Unionist paramilitaries than the British Army and the RUC combined during this time. While Republican groups like the PIRA framed their long-term struggle as against the British state, day-to-day violence often took place between paramilitary groups, or was directed by one group against suspected supporters of the other group. Most commonly, civilian targets were chosen based on religious affiliation. Protestants largely supported the continuing unionization of Northern Ireland in Great Britain, while Catholics largely supported an independent state. These cleavages were not exact — religious affiliation was never a perfect mapping onto either militarization or views on independence — but they were used as a heuristic in the information-poor environment of

the conflict.

Unionist paramilitary groups were not bound by any rules of war, meaning that (for better or worse) they had a greater range of action compared to the British security state. British forces were held to a relatively high standard of behavior by both the media and the public, who generally did not approve of widespread violence against British citizens. However, Unionist paramilitaries, operating as they did outside the law, were capable of a greater degree of violence against a wider range of targets, with the same goal of quashing Republican support. In addition, Unionist paramilitaries were made up of local citizens, who lived and worked in the same areas in which they fought. This added a distinctly personal note to the conflict between paramilitaries, particularly regarding retaliatory violence. An attack on one side, or its supporters (or civilians it claimed to represent) had to be met with violence against the other side, or supporters, or civilians of the wrong religion [Rolston, 2005]. This led to a parallel conflict that took place largely in Belfast and Derry, where Protestants and Catholics lived side-by-side. Violence here often had more to do with retaliation and turf control and less to do with the overall political struggle for freedom. However, again, I expect that patterns of learning and updating between these groups should remain fundamentally the same.

In some of the following hypotheses, I make a strong simplifying assumption, but one that is backed up to a great extent by the historical evidence. I explicitly lump in the British Army and the RUC in with the various Unionist paramilitary groups as a greater ‘Unionist’ coalition. I realize that does not fully reflect the reality of this conflict: the British government condemned the existence and actions of Unionist paramilitaries, who operated outside the law and engaged in murder and assault. However, there are three reasons why I argue this assumption is beneficial for this analysis.

First, although the British Army and the local RUC security forces were nominally unbiased, quite often this was not the case. Despite attempts to promote equal representation in the RUC, during the bulk of the Troubles this security force was overwhelmingly Protestant [CAI]. The fact that most Protestants aligned themselves with Unionist sentiment meant that the official security forces responsible for keeping public order during

this time period were biased heavily against Republicanism and against Catholic civilians suspected of supporting or sheltering Republican fighters. As such, the RUC was not a functional third party trying to keep the peace in a time of sectarian violence. While the British Army regiments in Northern Ireland may have carried less individual-level bias, being made up of citizens from other parts of the United Kingdom, their job was largely the capture or killing of Republican fighters; keeping Catholic civilians safe from Unionist violence was often a secondary concern.

Second, both state and non-state groups shared the same goal: combating Republican violence in Northern Ireland and, in the longer term, suppressing the broader Republican movement for independence and increased representation of the Catholic community in the political arena. The means these groups used varied considerably, based on their own motivations and restrictions, but the fundamental goal was the same.

Third, although the British government publicly condemned the extralegal violence of Unionist groups, records uncovered after the bulk of fighting subsided show a startling level of cooperation between the British security forces and Unionist militias. Not only did these groups evade prosecution and conviction for criminal activities, in many cases they were even fed intelligence directly from the British Army and the RUC. This is a strong claim, but one that is supported by multiple analyses of declassified information from the Troubles [Rolston, 2005, Dillon, 1999, Holroyd and Brubridge, 1989, White, 1999]. In other words, Loyalist groups were often — if not directly supported — at least ignored or provided with information by the British government during this period.

British forces, local security, and Unionist militias often had the same short-term and long-term goals, shared similar attitudes and priorities regarding Catholic and Protestant civilians in Northern Ireland, and even cooperated (sometimes secretly) with one another. This does not mean that these groups should always be lumped in with one another, or that there were no incidents of British or Northern Irish security making serious attempts to stop Unionist violence. However, I argue that due to these factors, the loss of precision resulting from making such a broad assumption is worth the gain in modeling and explanatory power. By lumping the various groups involved in the conflict under broad headings of

‘Unionist’ and ‘Republican’, I can generate and test broader hypotheses that allow me to better model the overall dynamics of the conflict with maximum degrees of freedom and explanatory power.

### 3.3 Hypotheses

The cities of Belfast and Derry were a world apart from the broader campaign of resistance waged in the countryside. In the first spasms of violence in 1970 and 1971, violence was limited largely to these large urban areas, as Republican fighters attempted to carve out zones of control (with various success) in the hopes of making these areas too dangerous for British forces and the RUC to effectively police [Dingley, 2008]. Catholic neighborhoods in these urban battlescape provided the PIRA with friendly human terrain in which to evade detection and capture, leading to initial success in this aim. Republican fighters barricaded neighborhoods in Belfast and Derry, creating ‘no-go’ areas that were virtually inaccessible to security forces. RUC and British troops did not enter these areas, seeking to avoid sparking further violence; the result was that the PIRA and other groups could use these zones as safe havens to plan and launch attacks.

One of the most significant shifts by British security forces during the Troubles was the launch of Operation MOTORMAN in 1972. This represented a major escalation in the intensity and militarization of the conflict, as the British government dramatically increased the number of troops stationed in Northern Ireland and relaxed the rules of engagement for Army troops [Bennett, 2010]. One of the ways MOTORMAN shifted the battlefield for Republican fighters was the removal of the no-go areas in Belfast and Derry. MOTORMAN escalated the number of troops stationed in the two cities, and mounted large-scale military operations involving mass ‘clearing’ of homes and neighborhoods. Search-and-destroy missions eliminated major Republican weapons caches, and thousands of civilians were detained or prosecuted. This period also saw significant breakdown in relations between British security and the Catholic community; after having been seen initially as impartial peacekeepers, the escalation of violence via MOTORMAN set British soldiers firmly on the side of the enemy in the eyes of many Catholic civilians in Belfast and Derry [Bennett, 2010]. However, the overwhelming power of the British Army in the

area meant that Republican fighters had to find alternative ways of carrying out violence.

Geographically, MOTORMAN represented a move by the British Army towards direct engagement and away from the lower-intensity role of containment and separation it had previously pursued. The elimination of no-go zones sent the message that the British Army held full jurisdiction in Northern Ireland, and that Republican fighters would not be permitted to set up areas of autonomy within Catholic-dominated neighborhoods. The advent of more intensive urban anti-insurgent tactics by the British government made carrying out a long-term, high-intensity urban campaign much more difficult. Widespread search-and-destroy missions caused major public unrest, but were effective in the short term in capturing insurgents and locating and destroying weapons caches. As this high-intensity counter-insurgency campaign came to be the norm in Belfast and Derry, the PIRA switched its own strategic approach — from a short-term, high-intensity campaign of violence designed to drive the British out, to a long-term, low-intensity fight (the ‘long war’) designed to raise the costs of occupying Northern Ireland over a longer time period [Dingley, 2008]. This perception was shared by British command in Northern Ireland, forecasting the shift in Republican strategy: “The tactics of the IRA may change as a result of Op MOTORMAN and attacks on the Security Forces in the rural areas could increase; the increased use of command detonated mines is all too likely” [Edwards, 2011, pos. 676].

The PIRA discovered quickly that despite local condemnation, the British military was more than strong enough to succeed in direct urban clashes. As such, I expect that Republican fighters reacted by relocating forces when the British and RUC focused their efforts on peacekeeping in urban areas. One way to measure this is by separating the Northern Irish battlefield into two geographic sections. One section is made up of the major urban centers of Belfast and Derry. These are by far the largest cities in Northern Ireland, and have historically been hotbeds of Republican sentiment. The second section is the more rural part of Northern Ireland, including smaller cities and villages. I anticipate a cycle of relocation in which an increase in UK success at detecting and destroying PIRA cells in Belfast and Derry is followed by an increased focus in Republican violence in the

countryside, attempting to draw British attention and resources away from embattled cells in the major cities. This pattern has been seen repeatedly in wars of occupation; for example, there was a ‘seesawing’ pattern of violence during the Algerian war of independence [Lowi, 2005] in which insurgents would increase activity in the countryside when French military forces were successful in hunting cells in Algiers.

By contrast, when operations in the countryside are successful, I expect Republican violence to increase in Belfast and Derry. However, I expect an increase in Republican violence in urban areas to have the additional effect of increasing Unionist militia violence in these same areas. Unionist paramilitary organizations operated primarily in Belfast and Derry, operating largely in a tit-for-tat war against Republicans and Catholics as a whole; styling themselves protectors of the Protestant majority population, these groups engaged in retaliatory violence against Republican fighters and civilians alike. Due to the rhetoric of retaliation these groups engaged in, I expect that an increase in Republican violence in Belfast and Derry will be met with a corresponding increase in the level of violence carried out by Unionist groups in the same region.

H1.1 As the UK increases its emphasis on violence in Belfast and Derry, Republican fighters will increase their emphasis on violence in the countryside.

H1.2 As Republican forces increase their emphasis on violence in Belfast and Derry, Unionist paramilitaries will increase their level of violence in Belfast and Derry.

Within the broader struggle for independence, there took place a vicious sectarian conflict, particularly in Belfast and Derry. Republican and Unionist paramilitaries in both cities engaged in widespread violence against each other; however, due to the small numbers of active fighters on both sides, and the highly unconventional nature of the conflict, civilians were often targeted for violence when ‘active’ enemies could not be found or could not be reached. Unionist fighters justified violence against Catholic civilians by linking Catholicism to Republicanism; by waging a campaign of terror against potential or suspected Republican sympathizers, who could be identified by their religious affiliation, these groups hoped to erode support for the IRA among the civilian population. While

the rhetoric of Republican groups generally eschewed purposeful targeting of Protestant civilians, retaliation killings were common: ‘one of them’ had to die to pay for the death of ‘one of us’ [Dillon, 1999].

This was most common in Belfast and Derry for two reasons. First, both areas were hotbeds of Republican violence, with relatively high concentrations of operating cells on both sides and relatively high intensity conflict. Second, both cities were highly segregated by religion: Catholics largely lived near Catholics, and Protestants near Protestants. That meant that militant groups motivated to sectarian violence could launch attacks at civilians or use indiscriminate forms of violence without worrying about accidentally wounding or killing a potentially ‘friendly’ civilian. In other words, these groups operated in a target-rich environment that lent itself well to this type of retaliatory, quasi-random violence. I anticipate that there is a set of patterns to this sectarian, retaliatory violence that took place alongside, and often interchangeably, with the broader military struggle for independence.

First, I expect to see clear evidence of retaliation at the larger strategic and tactical level. An increased emphasis on targeting Catholic civilians by Unionist militias should lead to a retaliatory increase in the targeting of Protestant civilians by Republican groups. While retaliatory killings did not supplant the Republican struggle against British occupation, they served the purpose of showing Republican power: murders of Catholic civilians had to be answered with violence, or else the local PIRA command would look too weak to protect its own supporters. I expect that the need for swift and visible retaliation is strong enough that it would draw resources that would otherwise be expended against British forces and the RUC. Therefore, I expect to see not just an increase in the number of killings, but an increase in the relative emphasis on Republican violence against civilian rather than military targets.

I expect to see a similar pattern on the Unionist side. These groups were created for the nominal purpose of protecting Protestant civilians and deterring Republican violence, but lacking the extensive resources, infrastructure, and intelligence of the British Army, they overwhelmingly targeted civilians for violence. I expect that the type of violence Unionists

engaged in varied in proportion to Republican violence. Neither side was unconditionally eager to use indiscriminate violence in an urban setting against civilians; the potential political costs were enormous, and likely to bring down severe retaliation from the British army on both sides. However, I anticipate that an increase in Republican targeting of Protestant civilians will lead to a corresponding increase in Unionist willingness to use indiscriminate violence. Unionist paramilitaries faced with an increase in PIRA targeting of Protestants felt a strong need to protect their own civilians and retaliate for Republican violence; launching large-scale indiscriminate attacks against Catholic targets such as pubs and meeting halls were a highly visible way to do this. As such, I expect that an increase in PIRA action against Protestant civilians will be met with not only an increase in Unionist activity against Catholic civilians, but also an increase in the likelihood of large-scale, indiscriminate violence being utilized.

H2.1 As Republican forces increase their reliance on civilian targeting, Unionist forces will increase their reliance on indiscriminate weaponry.

H2.2 As Unionist forces increase their reliance on indiscriminate weaponry, Republican forces will also increase their reliance on indiscriminate weaponry.

H2.3 As Unionist forces increase their reliance on indiscriminate weaponry, Republican forces will increase their reliance on civilian targeting.

My third set of hypotheses deals with the eventual draw-down in Republican violence. There are many reasons given in the historical accounts of the Troubles for the drawdown in the intensity of Republican violence and their eventual willingness to enter into serious peace negotiations. Many of these factors are not explicitly military: the infiltration of PIRA cells by British informers, the gradual erosion of support for Republican violence in the Catholic community, the success of political compromises, and so on. However, battlefield factors played a significant role as well. Better British intelligence, coupled with a willingness to engage in higher levels of deadly violence, severely weakened the PIRA's ability to launch large-scale attacks. The British Army and RUC were able to attrit PIRA

fighters at a higher rate than they could be replaced, leading to a gradual but inevitable draining of efficacy from the PIRA fighting force.

While the end of the Troubles came through political compromise, rather than military victory on either side, it was only possible once the PIRA had been sufficiently weakened through military action. As the British Army was able to gain better intelligence on PIRA activities and personnel, it was able to selectively strike at cells of fighters and kill or arrest important members. I expect that as this shift in British strategy had its effect, it forced a reduction in PIRA intensity, particularly in the type of large-scale strikes against British or RUC targets that had made headlines and shored up Republican support in the past. I expect that this strategy forced a shift in PIRA activity. An increasing lack of trained and experienced fighters, and an increased lack of trust in Republican leadership, would make large-scale coordinated attacks difficult to plan and execute. Instead, I anticipate that Republican cells would become more likely to launch attacks that were smaller and simpler in scope and technology, and more likely to engage in violence in areas where British power was weaker.

I anticipate that weakened Republican forces will eschew higher-value, higher-risk targets and areas where the British Military and RUC have a strong local presence. The penetration of the British security state, as well as the local RUC, varied significantly both in the urban centers of Belfast and Derry and in the rural areas of Northern Ireland. While mobility was not unlimited for PIRA cells, I expect that Republican fighters had the freedom of movement and/or the local organization to pick the regions in which they would engage in organized violence. As the organizational strength of the PIRA weakened, I expect that Republican fighters engaged in greater levels of deadly violence in areas without (or with a lower number of) permanent military or police facilities. These areas would have a lower level of permanent security presence, making it easier for Republican fighters to carry out successful attacks.

In turn, as the perceived threat of PIRA violence decreases, I expect there to be a decrease in the willingness of state forces to engage in high levels of deadly violence. Great Britain is a democratic state, and Northern Irish citizens, regardless of their Republican

leanings, are UK citizens; the entire duration of the Troubles has been marked by a forcible restraint on British military power in dealing with a violent insurrection. As such, force has been deployed in proportion to the perceived threat; as the perceived threat is reduced, the justification for high levels of force disappears as well. I expect these cycles to lead to a state in which Republican violence becomes increasingly uncommon as state forces become more successful in targeting and killing or arresting Republican fighters. As the pool of trained personnel shrinks, Republican forces are also more likely to change the type of violence in which they engage, relying more heavily on indirect forms of violence that lower the risk to personnel. I also expect that the location of Republican violence will change; a weakened PIRA will choose targets in areas where state power is lower, as measured by the concentration of British Army and RUC bases.

H3.1 As the rate of state kills of Republican fighters increases, the rate of Republican kills will decrease.

H3.2 As the rate of state kills of Republican fighters increases, Republican forces will increasingly operate in areas with less state military presence.

H3.3 As the rate of Republican kills decreases, the rate of state kills will decrease.

My fourth set of hypotheses deals with the local geographic elements of violence during the Troubles. Figure 3.1 shows that significant variation exists in the location and type of fatal violence during the Troubles. Figure 3.1 shows a map of Northern Ireland, with an overlay showing the location, type, and faction responsible for each of the roughly 3,300 fatalities that occurred during the conflict. Belfast is the area with the largest number of deaths during this period, with a much higher intensity of violence than any other city or area in Northern Ireland. Both Republican and Unionist paramilitary groups engaged in significant retaliatory violence, leading to a high level of casualties in and around Belfast. Derry (Londonderry), the second-largest city in Northern Ireland, was another stronghold of Republican militancy, with a great deal of fatal violence involving Republican and Unionist forces.

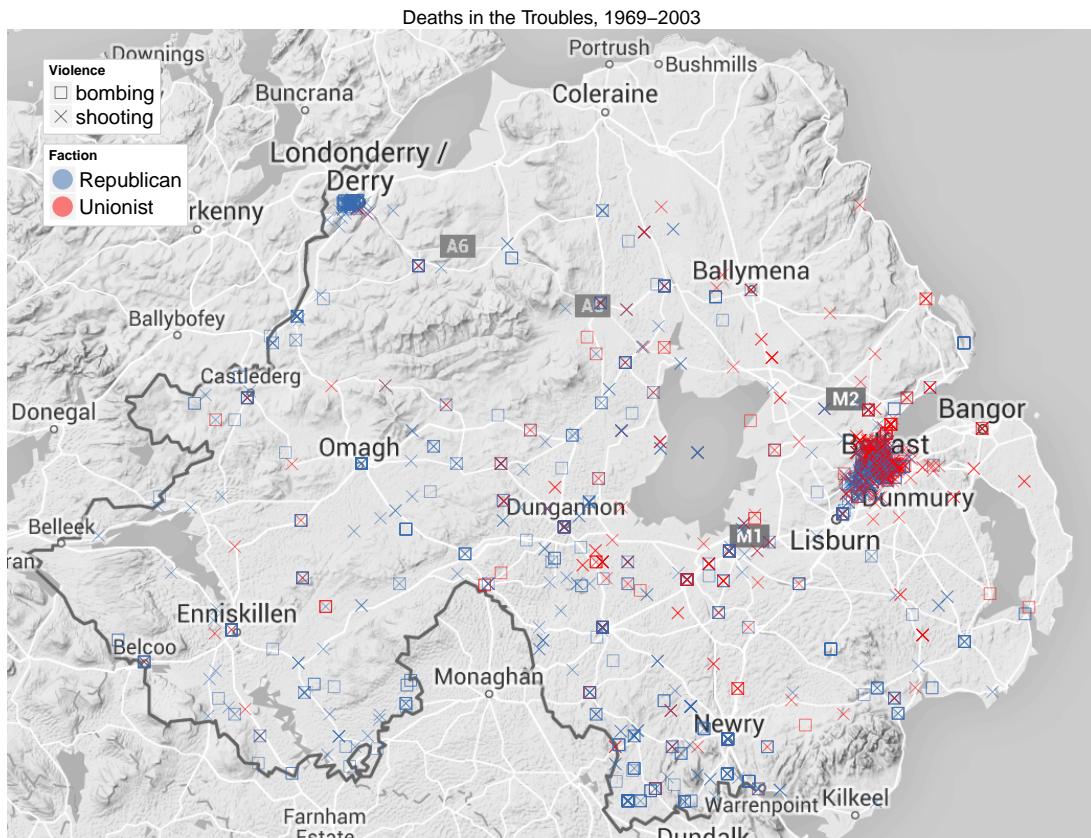


Figure 3.1: Violent deaths in the Troubles, by faction and type

Interestingly, violence in the rural regions of Northern Ireland shows different patterns for Unionist and Republican groups. Unionist violence concentrated primarily in Belfast and in the central region of Northern Ireland between Dungannon in the west, Newry in the south, and Ballymena in the north. It is not immediately clear why this geographic concentration exists, but it is likely due to the patterns of latent support for Unionist versus Republican causes. Unionist paramilitary groups never had the widespread infrastructure and communication that the PIRA established, fighting primarily in Belfast and the surrounding area.

This is largely due to the fact that rural areas in Northern Ireland, particularly in the south, were both more Catholic and more sympathetic to the Republican cause. In addition, Republican groups outside of Belfast and Derry rarely purposely targeted civilians for fatal violence, striving to target military or political personnel instead. The result

is that Unionist groups lacked (1) the underlying support of the local population, (2) the infrastructure to sustain long-term operations outside of a limited, fairly urbanized region, and (3) the short-term impetus to support or justify violence against Republican sympathizers.

By contrast, Republican violence took place all across the country — in line with PIRA rhetoric about fighting a long-term rural insurgency. The type of violence used (small arms versus explosives) shows little distinction in where each type of weapon was used by Republican fighters. Republican violence also concentrated in regions close to the Irish border, particularly in County Armagh south of Newry. PIRA fighters took advantage of the relatively open border with Ireland, launching attacks against security forces and then retreating across international lines to safety [Patterson, 2010]. This ability to retreat, coupled with higher levels of Republican support in the area, made County Armagh one of the deadliest battlefields of the Troubles outside of Belfast and Derry.

Interestingly, very little violence on either side took place in the north of Northern Ireland, near Coleraine and Bushmills. It is not immediately clear why these areas were comparatively peaceful, but may have to do with their relative isolation from the more fraught areas of the country. These areas are also far from safe havens for Republican fighters, being far away from international borders or large cities with sympathetic Catholic neighborhoods, making it more difficult for Republican forces to mount sustained operations in the area.

Figure 3.2 shows the distribution of Unionist and Republican fatal violence in the city of Belfast, the nexus of most violence during the Troubles. Killings in these areas were often retaliatory, as paramilitary groups on both sides engaged in cycles of revenge killings often, especially on the Unionist side, targeting civilians instead of enemy fighters. The goal on both sides was to demonstrate strength, showing that enemy violence would not go unanswered; this led to a cyclic pattern of killings on both sides.

Both sides engaged in high levels of violence in central Belfast. This area contains several neighborhoods that are close to one another but locally highly segregated by religious affiliation: highly Catholic neighborhoods (Ardoine, New Lodge, Falls and

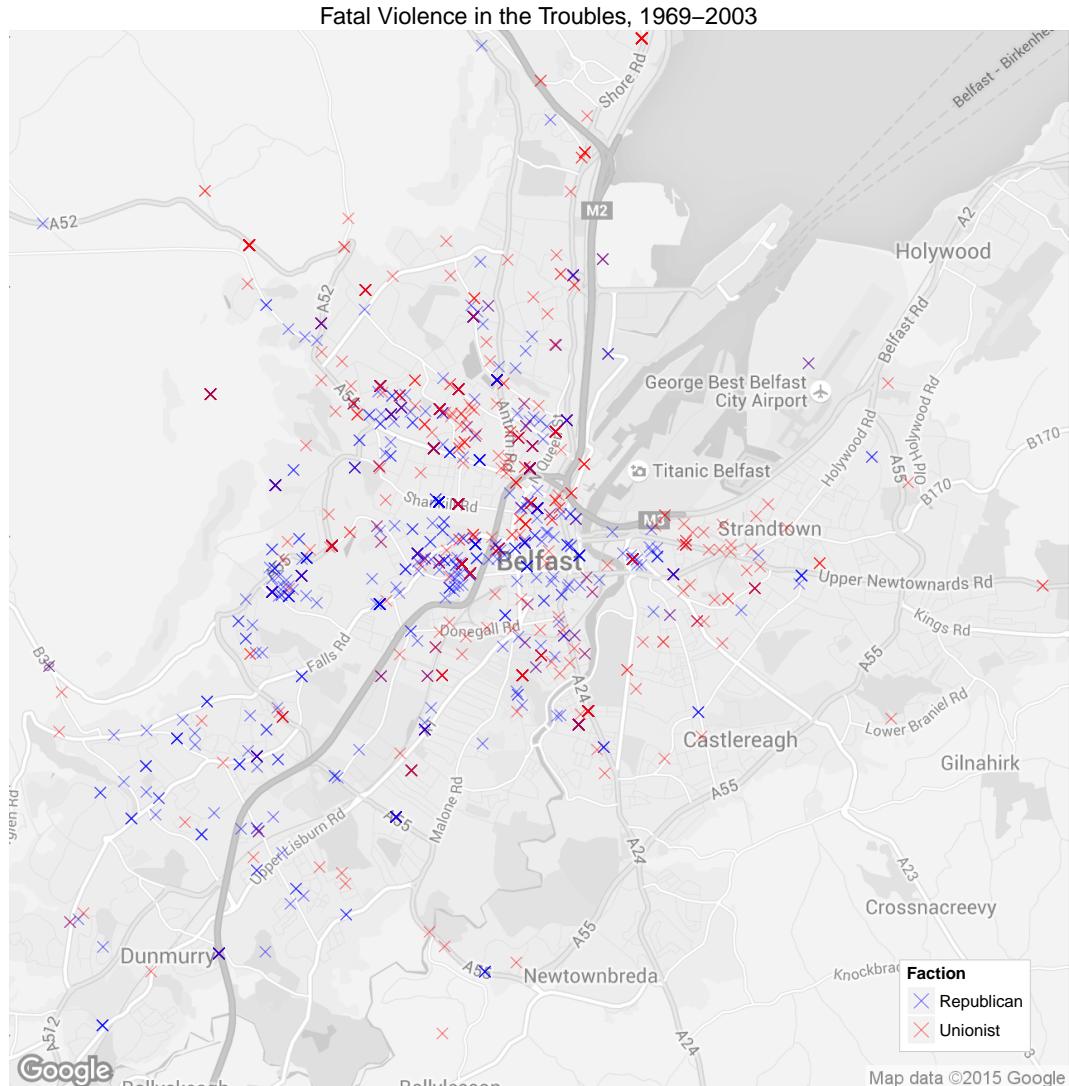


Figure 3.2: Violent deaths in Belfast, by faction

Clonard) abut highly Protestant neighborhoods (Shankill, Crumlin, Duncairn), leading to increased local tensions. Groups looking for easy targets for violence could travel a short distance under cover of night or in disguise, and find themselves in a target-rich environment in which any civilian could be safely assumed to be a potential supporter of the other side.

On the outskirts of Belfast, Republican and Unionist forces varied in their areas of operation. Unionist forces engaged in more fatal violence in northern and northwest Belfast, particularly along the Shore Road leading out of the city. Republican groups engaged in more violence in the southwest area of the city leading towards Dunmurry, between the Falls and Upper Lisbourne Roads. One explanation for this geographic variation that presents itself immediately is the distribution of religious groups. The areas along the Shore Road, making up the Fort William and Castleview neighborhoods, are overwhelmingly Protestant in their religious makeup. The neighborhoods of southwest Belfast such as Falls Park, Andersontown, and Ladybrook, on the other hand, are overwhelmingly Catholic. This suggests that in these regions, militants claiming to align themselves with the dominant religious demographic (Catholic/Republican in the southwest, Unionist/Protestant in the north) targeted local minorities for violence, and paramilitary groups claiming the support of the local minority group lacked the power to retaliate in that area.

Figures 3.1 and 3.2 show an interesting pattern of violence. Both within urban areas like Belfast and in rural areas outside of the main cities, one major predictor of where violence happens is religious demography. However, it is not as simple as the local proportion of Catholics to Protestants; rather, it appears to be linked to highly segregated local demographics. In other words, it is not simply the proportion of Catholic population in the neighborhood, district, or town; it is the proportion of Catholic population relative to the surrounding Protestant population.

In Figure 3.1, this helps explain the low levels of political violence taking place in the northernmost regions of Northern Ireland. These areas are highly segregated at the local and regional level: there are very few areas where highly Catholic populations live right alongside highly Protestant populations. In the south and west, however, populations

are significantly more mixed, and this may well be related to the distribution of violence. Republican killings are far more common in southern Armagh near the Irish border; these regions have highly Catholic areas that border mixed-demographic and largely Protestant areas. Likewise, the broader region surrounding Lough Neagh is majority Protestant, with several majority Catholic areas. Both of these regions see high levels of violence directed at the local minority (Catholic around Lough Neagh, Protestant in south Armagh) by paramilitary fighters aligning themselves with the local majority group.

In Figure 3.2, this pattern emerges again but at the local level. Eastern Belfast experienced very little violence during the Troubles. One explanation is that eastern Belfast is overwhelmingly Protestant, with very little Catholic population east of River Lagan. This demographic profile provided little opportunity or motivation for religious-aligned violence in the area. However, western Belfast sees a significantly more mixed population overall; highly Protestant neighborhoods abut highly Catholic neighborhoods, although there are more Protestants in the north and more Catholics in the south. In other words, these areas see greater local tension due to the proximity of competing groups; they also provide greater opportunities for violence, as groups with strong support in one area can project violence to another area, then retreat to a friendly neighborhood. Unsurprisingly, these areas saw the most intense violence during the entire period of the Troubles.

Figures 3.1 and 3.2 serve to reinforce previous findings about violence during the Troubles: religious demographics mattered, both at the local level (neighborhoods within cities) and at the broader level of villages and rural districts. Violence was more likely between Republican and Unionist groups in areas that (1) had a clear majority of one group and (2) were adjacent or near areas where the other group held a local majority. This holds true at the local level within Belfast, and also (although to a slightly lesser extent) in Northern Ireland as a whole.

However, these analyses are limited because they are cross-sectional in nature. Knowing the overall level of violence during decades of conflict and unrest is useful, but it may not tell the full story of what was going on from month to month and year to year. Using VAR

models can provide further information here by analyzing whether this demographic story holds true in a time-variant framework: does an increase in Republican violence in more Protestant areas lead to an increase in Unionist violence in more Catholic areas? I expect that this is the case; there is no reason to assume that the logic of local demographic and religious tension and support for either side should change over the course of the Troubles.

H4.1 As the level of Republican violence in Protestant areas increases, the level of Unionist violence in Catholic areas will increase.

H4.2 As the level of Unionist violence in Protestant areas increases, the level of Republican violence in Catholic areas will increase.

## 3.4 Research Design

### 3.4.1 Data and Coding

The data used in this analysis is based on two sets of data on deaths during the Troubles, gathered by Malcolm Sutton and Michael McKeown over the past three decades [CAI]. Both data sets records deaths due to politicized or sectarian violence linked to the overall struggle for Northern Irish independence. The two data sets cover different aspects of deaths: the Sutton index contains short vignettes describing the location and circumstances of death, for example, while the McKeown index lacks location but contains more systematically coded information about the type of weapon used, the political or religious affiliation of assailant and victim, and so on. The two data sets were manually joined by victim name and date of death, resulting in a combined list of roughly 3,300 deaths over this three-decade period. The data sets share a roughly 93% overlap; there are a few death records that exist solely in one data set, but this proportion is sufficiently small that I do not expect it to cause systematic bias or coverage issues in the combined data set.

The death records in the combined Sutton-McKeown data set were georeferenced by hand using the information contained in the Sutton vignettes. In Belfast and Derry, location data were usually specific enough that a death could be referenced to a specific address or street. Outside of these two cities, deaths were referenced either to village in

which they occurred, or in the case of rural deaths, to the nearest road address. As such, the spatial resolution of death records varies depending on whether the death took place in Belfast or Derry versus a smaller city or a rural village.

These data are notably limited in one regard: they only contain incidents of fatal violence. A more comprehensive analysis of the Troubles would require a similar data set on incidents of non-lethal violence; unfortunately, no such index exists, and time concerns in this analysis did not allow for such an index to be recreated from British and Northern Irish sources<sup>1</sup>. This poses a problem for analysis in two ways. First, it renders it impossible to answer questions about the overall intensity of the conflict on both sides: without knowing the raw number of events, it is more difficult to assess cause and effect in the number of fatalities. Second, and more disappointingly for the study of British strategy, it lacks any record of arrests and trials, which were a major tool used by the British Army and RUC to remove suspected and actual Republican militants from the conflict. Without data on arrests, all we have to analyze the cost of the conflict on the Republican side are fatalities.

However, these issues do not render these data useless. Fatalities were very important in this conflict, given that it existed (1) in a democratic, highly developed country and (2) before the cameras of mass media, both domestic and international. A death on either side had powerful effects, in terms of morale, optics, and perception of the conflict overall. That means that each observation in this data set carries significant weight in predicting actions and reactions, in a way that observations of non-lethal violence would not. Furthermore, this data set is unique in that it is very close to comprehensive, a very rare thing in conflict event data sets. Years of labor by two scholars, including rigorous examination of primary and secondary sources as well as interviews with relatives and friends of victims, has led to a data set that leaves very few deaths unrecorded. This means that not only is each observation likely to carry a high predictive weight, but there is little concern of bias or unobserved variance brought on by an unknown set of missing or unreported events. This

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<sup>1</sup>One potential source of information is the Northern Ireland Research Initiative (NIRI) based out of Louisiana State University [Loyle et al., 2014]. However, repeated requests to gain access to these data, or indeed verify the actual existence thereof, were unsuccessful on all fronts.

is extremely rare in disaggregated data sets, many of which rely on news media reports and are thus subject to several different forms of bias [Weidmann, 2016].

The result is a data set that is virtually unparalleled in its chronicling of fatalities during a period of severe violent unrest. Northern Ireland is unique in that it is an extremely well-documented political struggle. Because it took place in a highly developed, democratic state with a free media system, and because many combatants and their families are still alive, scholars have been able to effectively rely on primary sources for data about the date, location, and type of deaths due to the conflict.

The second original data set used in this analysis is the location and density of British Army and Ulster Constabulary bases. To my knowledge, this is the first attempt to create a unified, geo-referenced data set documenting active and formerly active security bases. These locations include 329 British Army barracks, headquarters, and garrisons; RUC barracks and command centers; and permanent watchtowers and surveillance centers. These data were gathered from a wide variety of sources, including Sinn Fein data [Fein, 2015a,b], CAIN records [CAI], and historic sources listing RUC and RIC (Royal Irish Constabulary, the precursor to the RUC) barracks around Northern Ireland [RIC].

Base locations were manually geo-referenced. Where street addresses were available, geo-referencing was done through address searching using Google Maps; in cases where an exact location was not known, often in small villages or when bases had been closed, Google Street View was used to manually scan streets and locate these locations. As such, locational information for these bases are accurate down to the street address or intersection<sup>2</sup>.

Finally, data were gathered on the proportion of Catholic and Protestant population at the electoral ward level, relying on the 2001 Northern Irish Census. There are 582 electoral wards in Northern Ireland, created to be roughly equal in population with a mean of 3,000 population per ward. This means that the geographic size of wards differ considerably: those in rural areas are much larger, in square kilometers, than those in

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<sup>2</sup>In addition, this data set also includes roughly 100 known locations of permanent checkpoints, roadblocks, and closed border crossings between Northern Ireland and the Republic of Ireland, but these data are not used in the current analysis.

major cities, which were often about the size of a small neighborhood. These wards are a small enough unit of spatial measurement to capture local-level variation in religious and socioeconomic demographics: 582 wards can capture more local-level variation than the 26 District Councils that make up the next level of aggregation in the Northern Irish political system. Electoral wards are also useful because they map well onto politically relevant cleavages, particularly in Belfast and Derry: the borders of different wards also mark the borders of historically and demographically distinct neighborhoods, such as the Protestant Shankill and Catholic Falls neighborhoods in Belfast.

One potential concern with these data is that they were gathered in 2001. While 2001 is well after the bulk of violence during the Troubles happened, long-term demographic analysis suggests that population mobility in Northern Ireland is slow to change; while populations have shifted somewhat in the 30+ years between the outbreak of the Troubles and the oldest available census data, there has been relatively slow movement or change in local demographics apart from an increase in local-level religious segregation [Compton, 1981]. This does suggest that wards that are highly segregated (very high Catholic or Protestant population) in 2001 may have been less so in 1971, but without reliable data from earlier census years, it is difficult to estimate the degree to which this is the case.

Overall, I do not expect this discrepancy to cause major bias in my results. Much of the demographic change in Northern Ireland (itself largely focused in Belfast and Derry) occurred in the first few years of the Troubles, and largely stabilized thereafter. This means that even if there are a few years in which the true religious demographics of Northern Ireland are significantly different than my records, the bulk of the timeline does largely reflect the data I have available from 2001. Second, while shifts in religious demographics did occur, they were by far more common in areas that were already majority-Protestant or majority-Catholic; the effects of the Troubles were to increase already-existing majority populations as threatened minorities sought shelter elsewhere. This means that discrepancies between older data and the 2001 census are differences of degree, rather than kind; it is not the case that majority-Catholic wards turned Protestant, or vice-versa, as the existing level of segregation prior to the Troubles was already quite

high and had been stable for decades beforehand.

### 3.4.2 Modeling Approach

To test my hypotheses, I employ the same family of vector autoregressive (VAR) models described in Chapter 2. The VAR framework allows me to analyze relatively short-term patterns of mutual action and reaction between the various groups involved in the Troubles. With these models, I analyze 29 years of fatalities data, from January 1970 through December 1998. This period covers the most intense years of conflict during the Troubles, starting with the initial uprisings in Belfast and Derry and ending with the signing of the Good Friday agreement in 1998. I aggregate these time-varying data into three-month (one calendar-year quarter) periods. Using a three-month period as the unit of temporal analysis means that the process I am trying to model through VAR is considerably slower in pace than the weekly processes I examine in the previous chapter. This is done for both theoretical and pragmatic reasons.

Theoretically, this is more appropriate given the processes I am trying to model: the pace of fatal violence during the bulk of the Troubles was much less intense compared to the spasm of violence that marked the Second Intifada. While unrest was overall quite high during this period (relative to peacetime), weeks or months could go by without major incident. This was due to both the small size of the groups involved – the PIRA never had more than a few hundred core combatant members – but also due to the fact that for both Republican and Unionist paramilitaries, operations often took weeks or months to plan and carry out. As such, it is realistic to assume a larger aggregation of time as the core temporal unit of analysis. The resulting data set contains 102 quarters of data. For each faction (Republican militants, Unionist militants, and British security forces) I record the mean type, target, and locational characteristics of fatalities perpetrated by each side each quarter. This data set is identical in structure to the data used to analyze Israeli-Palestinian interactions in Chapter 2, with values ranging from 0 to 1 and reflecting the proportion of weekly events in which violence involved a given type, target, or location.

### 3.5 Results and Discussion

My first hypothesis deals with the location of Republican and British deadly violence. I expect that as British security forces focus their efforts on suppressing Republican violence in the major cities of Belfast and Derry, Republican fighters are more likely to relocate to the more rural areas of Northern Ireland. British forces anticipated a shift in both the location and the type of Republican violence as a result of Republican forces being driven out of their strongholds in Belfast and Derry due to MOTORMAN. Furthermore, I anticipate a parallel pattern of violence in which efforts by Republican fighters to step up their operations in Belfast and Derry will be met with retaliatory violence by Unionist paramilitary groups in the same cities, as sectarian tensions increase.

Table 3.1 shows the results of VAR models testing Hypotheses 1.1 and 1.2. Models 1 through 3 show some interesting patterns. The location of Republican deadly violence is not significantly related directly to British activity, but Model 2 suggests there are some patterns of updating in where Republican forces choose to focus their efforts. There is a positive and significant relationship with Unionist paramilitary violence in Belfast and Derry in the previous six months; this suggests that Unionist violence in major cities may actually draw higher levels of Republican activity back to urban areas. The retaliatory logic of sectarian violence makes sense in this case; an increase in local Unionist violence is likely to draw retaliation from Republican forces in the same area.

Testing the significance of this relationship in the VAR context once again requires the use of impulse-response function simulations. Figure 3.3 shows the results of IMF simulation for a hypothetical positive shock to the level of British violence in Belfast and Derry. As shown here, there is a slight negative effect within about six months, as the level to which Republican fighters focus on urban violence slightly decreases. However, this effect attenuates over time, and never reaches conventional levels of statistical significance.

In addition, there is a positive relationship between Catholic casualty ratios and Republican operations in Belfast and Derry. An increase in the casualty ratio indicates that a greater number of Catholic or Republican citizens are dying relative to the number of Protestant or Unionist citizens killed in a given three-month period. If this is indeed

Table 3.1: Urban and Rural Violence

|  | % State Attacks in Belfast and Derry<br>(1) | % Unionist Attacks in Belfast and Derry<br>(2) | % Republican Attacks in Belfast and Derry<br>(3) |
|--|---|--|--|
| State urban violence <sub>t-1</sub>    | 0.33***<br>( 0.1 )                          | -0.19*<br>( 0.1 )                              | 0.03<br>( 0.11 )                                 |
| State urban violence <sub>t-2</sub>    | 0.15<br>( 0.1 )                             | 0.04<br>( 0.11 )                               | -0.06<br>( 0.11 )                                |
| Unionist urban violence <sub>t-1</sub> | 0.04<br>( 0.09 )                            | 0.21***<br>( 0.1 )                             | 0.03<br>( 0.1 )                                  |
| Unionist urban violence <sub>t-2</sub> | 0.12<br>( 0.08 )                            | -0.12<br>( 0.09 )                              | 0.21**<br>( 0.1 )                                |
| Repub urban violence <sub>t-1</sub>    | -0.05<br>( 0.09 )                           | -0.12<br>( 0.1 )                               | 0.11<br>( 0.1 )                                  |
| Repub urban violence <sub>t-2</sub>    | -0.04<br>( 0.09 )                           | -0.07<br>( 0.11 )                              | -0.01<br>( 0.1 )                                 |
| Catholic casualty ratio                | 0.08<br>( 0.09 )                            | -0.12<br>( 0.1 )                               | 0.19*<br>( 0.1 )                                 |
| Catholic casualty ratio <sub>t-1</sub> | -0.05<br>( 0.09 )                           | 0.11<br>( 0.1 )                                | 0.12<br>( 0.1 )                                  |
| Catholic casualty ratio <sub>t-2</sub> | 0<br>( 0.09 )                               | 0.07<br>( 0.1 )                                | 0.08<br>( 0.1 )                                  |
| Constant                               | 0.36**<br>( 0.2 )                           | 0.65***<br>( 0.21 )                            | 0.18<br>( 0.23 )                                 |
| Trend                                  | -0.01**<br>( 0 )                            | -0.01***<br>( 0 )                              | 0<br>( 0 )                                       |
| Adjusted $R^2$                         | 0.29  | 0.14   | 0.08   |
| Weekly Observations                    | 102   | 102  | 102  |

*Note:*

\*p&lt;0.1; \*\*p&lt;0.05; \*\*\*p&lt;0.01



Figure 3.3: Impulse-response function of State and Republican urban violence

an indicator of military success or failure for Republican forces, then it appears that Republican violence may have moved back to Belfast and Derry during periods when the overall conflict was not going well. The fact that this relationship does not hold at standard measures of statistical significance ( $p < 0.10$ ) does not provide a high level of confidence in this finding, but the logic does make sense. Belfast and Derry were Republican strongholds, densely-populated areas in which fighters could easily find shelter, make connections, and share information. During periods where the overall conflict was not going well, focusing their efforts in areas where they were already strong may have made sense.

Unionist paramilitary violence, on the other hand, shows no direct relationship to Republican activity. Examining the results of Model 3, it appears that Republican forces do respond to shifts in the location of Unionist violence (as shown in Model 2) but Unionist forces do not respond directly to shifts in the location of Republican violence. Instead, Unionist violence is relatively stable over time; Model 3 shows that there is a positive and significant relationship between the lagged location of Unionist violence in the previous quarter and the current location of Unionist violence. This means that the relative level

of Unionist paramilitary operations was more predictable in Belfast and Derry over time.

Of particular interest, however, is the relationship between the location of Unionist violence and the location of violence carried out by British security forces. There is a negative and significant ( $p < 0.10$ ) relationship between lagged British operations in Belfast and Derry in the previous quarter, and Unionist operations in the same urban areas. I interpret this as limited evidence that a substitution effect may exist between Unionist and British violence in Belfast and Derry. While this effect may not be particularly strong, it is somewhat in line with historic findings on the relationship between British security and Unionist paramilitary forces; as noted earlier, the British military not only failed to prosecute Unionist activities to the same extent as Republican activities, but in many cases was an active collaborator with Unionist paramilitaries, sharing intelligence and resources with these groups. As such, a substitution effect is not entirely unsurprising.

Overall, neither Hypothesis 1.1 or 1.2 are supported. The expected relationships between urban violence on the Republican, British, and Unionist sides do not materialize in these models. However, several interesting and unexpected results do come out of Models 1 through 3. While the direct relationships I hypothesize are not supported, it does appear that Republican and Unionist paramilitaries do respond to changes in their environment when allocating resources to fighting in the urban centers of Belfast and Derry versus the more rural countryside of Northern Ireland. Republican forces tend to respond to upticks in urban Unionist violence by increasing their own level of deadly attacks in the urban areas, responding to sectarian violence. Unionist paramilitaries, by contrast, do not seem to change their urban operations in response to shifts in the location of Republican violence. They do, however, seem to engage in some limited form of substitutive violence with British forces; in time periods where British security forces are less engaged in Belfast and Derry, Unionist violence in the same areas shows a tendency to increase.

Hypothesis set 2 deals with the type of violence employed, and the target of violence, of Republican and Unionist forces during the Troubles. I anticipate that there is a pattern of escalation in the type and target of violence between Republican and Unionist forces: the more one side relies on indiscriminate weaponry and civilian targeting, the more

the other side will do so as well. Substantive accounts of the Troubles corroborate this story: sectarian tensions led to high levels of retaliatory violence between Unionist and Republican paramilitary groups, especially in the highly segregated neighborhoods of West Belfast. While the Republican forces were better-known for their use of indiscriminate weaponry, particularly improvised bombs targeting both security forces and Protestant targets, Unionist militants also engaged in bomb-building and indiscriminate attacks, often on Catholic civilians. Table 3.2 shows the results of VAR models testing these hypotheses.

Table 3.2: Civilian Targeting and Indiscriminate Violence

|  | % Republican<br>attacks on<br>civilians<br>(4) | % Unionist<br>reliance on<br>indisc. violence<br>(5) | % Republican<br>reliance on<br>indisc. violence<br>(6) |
|--|--|--|--|
| Republican civilian targeting <sub>t-1</sub> | 0.12<br>( 0.10 )                               | 0.01<br>( 0.08 )                                     | -0.13<br>( 0.10 )                                      |
| Republican civilian targeting <sub>t-2</sub> | 0.06<br>( 0.11 )                               | 0.03<br>( 0.08 )                                     | -0.04<br>( 0.10 )                                      |
| Unionist civilian targeting <sub>t-1</sub>   | -0.01<br>( 0.13 )                              | 0.38 ***<br>( 0.10 )                                 | 0.12<br>( 0.12 )                                       |
| Unionist civilian targeting <sub>t-2</sub>   | 0.11<br>( 0.13 )                               | 0.10<br>( 0.10 )                                     | -0.12<br>( 0.12 )                                      |
| Republican indisc. weaponry <sub>t-1</sub>   | -0.15<br>( 0.11 )                              | 0.09<br>( 0.08 )                                     | 0.17 *<br>( 0.10 )                                     |
| Republican indisc. weaponry <sub>t-2</sub>   | -0.02<br>( 0.11 )                              | -0.19 **<br>( 0.08 )                                 | 0.12<br>( 0.10 )                                       |
| Catholic casualty ratio                      | 0.06<br>( 0.11 )                               | -0.02<br>( 0.08 )                                    | -0.28 **<br>( 0.10 )                                   |
| Catholic casualty ratio <sub>t-1</sub>       | -0.02<br>( 0.10 )                              | 0.17 **<br>( 0.08 )                                  | 0.10<br>( 0.10 )                                       |
| Catholic casualty ratio <sub>t-2</sub>       | -0.06<br>( 0.10 )                              | 0.14 *<br>( 0.08 )                                   | 0.10<br>( 0.10 )                                       |
| Constant                                     | 0.27<br>( 0.24 )                               | 0.52 **<br>( 0.18 )                                  | 0.33<br>( 0.22 )                                       |
| Trend  | 0.00<br>( 0.00 )                               | -0.01 **<br>( 0.00 )                                 | -0.01<br>( 0.00 )                                      |
| Adjusted R <sup>2</sup>                      | -0.01  | 0.45   | 0.12   |
| Weekly Observations                          | 102  | 102  | 102  |

Note:

\*p&lt;0.1; \*\*p&lt;0.05; \*\*\*p&lt;0.01

Model 5 tests Hypothesis 2.1, looking for a relationship between Republican violence

against civilians and Unionist reliance on indiscriminate technologies. However, this model does not provide evidence that Unionist behavior is predictable by looking at Republican violence against civilians. While Republican paramilitaries did kill Protestant civilians, both accidentally and purposefully, an uptick in Republican targeting of civilians does not seem to be met with a corresponding surge in the use of indiscriminate violence by Republican forces. However, Unionist strategy — insofar as it deals with the use of indiscriminate-fire weaponry — does show some degree of predictability over time. The current Unionist level of reliance on indiscriminate violence is best predicted by looking at the type of violence Unionist forces relied on in the previous quarter. This is indicative of a strategy that is more static, or at least more sticky over time, than the behavior of Republican paramilitary groups.

Interestingly, however, there is limited evidence that Unionist indiscriminate violence can be predicted by looking at previous patterns of Republican violence in the previous six months. However, this too is not a relationship I expected to find. While this relationship is statistically significant, it is in the opposite direction I expected: a decrease in Republican reliance on indiscriminate violence actually may lead to an increase in Unionist reliance on indiscriminate violence. This does not hold with the pattern of retaliation and mutual escalation I expected to find, in which both groups increased their level, type, and target of violence in response to one another. Instead, Unionist forces may take advantage of temporary lulls in the Republican bombing campaign to launch their own attacks; it is not immediately clear, however, that this reasoning holds in the historic literature.

On the other hand, Model 6 finds a similar result to Model 5 in that it suggests a degree of stickiness in the use of indiscriminate violence over time. Here, there is a positive and significant ( $p < 0.10$ ) relationship between lagged and current Republican reliance on indiscriminate weaponry, but with this level of statistical significance it is unclear without impulse-response simulation to determine whether this is accurate. Simulating the IRF in Figure 3.4 shows this in more detail.

In the multiply-endogenous structure of the VAR, there is not sufficient evidence to reject the null hypothesis of non-autocorrelation. This suggests that Republican

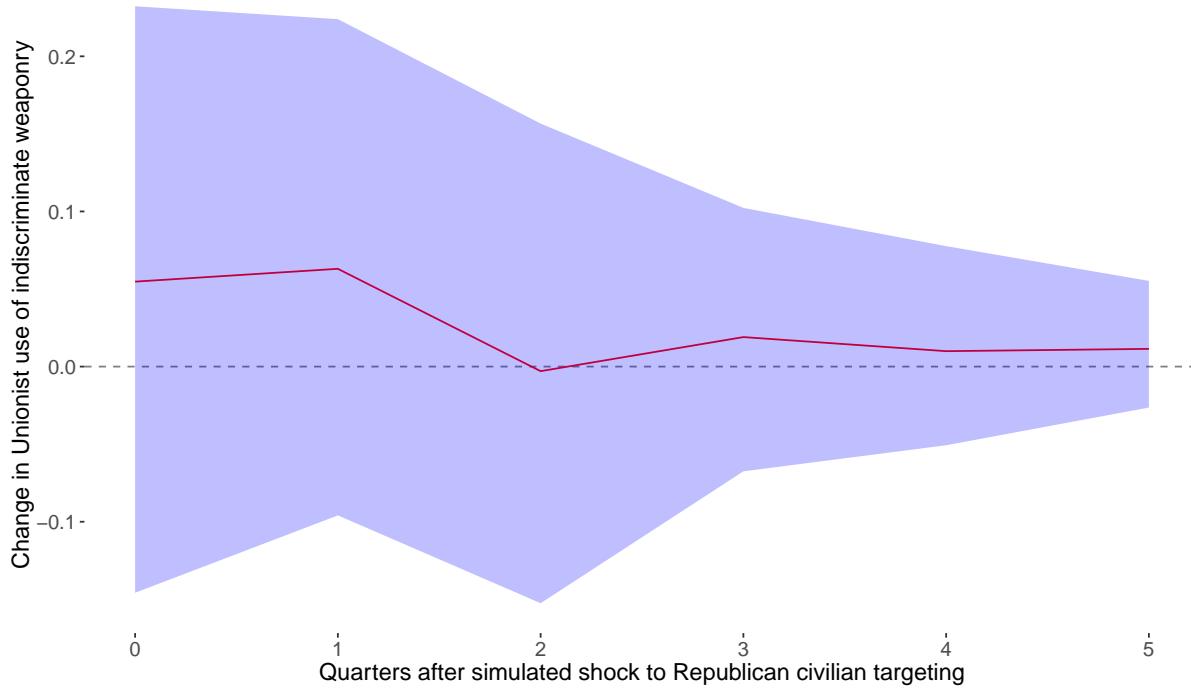


Figure 3.4: Impulse-response function of Republican attacks on civilians and Unionist indiscriminate violence

paramilitary forces do not behave as predictably — at least in terms of the weaponry they rely on — as Unionist paramilitary forces over this 30-year period. Similar to the previous chapter’s discussion of Palestinian insurgent activity, this is not entirely unexpected: both movements carried out non-conventional terror campaigns against stronger state groups, and explicitly relied on surprise and unpredictability to survive in the face of material disadvantage.

Interestingly, Model 6 does show a significant and negative relationship between Catholic casualty ratios and Republican reliance on indiscriminate violence. This finding is unexpected: a negative trend in casualty ratios indicates that Catholic/Republican losses are decreasing relative to Protestant/Unionist losses — in other words, from the perspective of casualties, it means that Republicans are succeeding in the short term. Given that my general theory expects militant groups to rely on indiscriminate-fire technologies when they are facing obstacles or losses on the battlefield, this is unexpected: by this indicator, Republican fighters are actually more likely to rely on indiscriminate violence when the

casualty count suggests that they are succeeding. This is a finding that is worth exploring in future analysis, as the reason for this relationship is not immediately clear.

Model 4 tests Hypothesis 2.3, looking for a relationship between Unionist reliance on indiscriminate weaponry and the tendency of Republican paramilitaries to target civilians for retaliation. If this pattern of escalation holds, there should be a systematic relationship between weapons use and civilian targeting between Unionist and Republican forces. I expect that as Unionist paramilitaries rely more heavily on large-scale or indiscriminate violence, Republican retaliation will become more intense, including targeting civilians for violence directly when Republican forces are unable to locate or engage with Unionist paramilitaries directly. The results of Model 4 do not support this hypothesis, however: Republican civilian targeting shows no systematic relationship with Unionist reliance on indiscriminate violence. This suggests that (at least with the data used here) Republican forces did not respond to deadly Unionist bombings by purposely shifting their strategy towards targeting Protestant civilians — at least not for fatal violence.

This is not to set up Republican forces as paragons of virtue or restraint, however; instead, it merely suggests that this particular relationship did not hold. This may be understood in light of the political and social framing of the Republican struggle: the PIRA and other Republican groups stressed repeatedly that they refrained from targeting civilians for violence as much as possible, and were quick to disavow or excuse attacks that targeted civilians. This was intended to increase the political legitimacy of the Republican movement, by making it clear that violence was targeted at British interlopers rather than Protestant neighbors; through this, Republican fighters and political allies hoped to increase the chances for successful negotiations with the British government, by claiming credibly to be fighting for all of Northern Ireland. As such, it may have been difficult for Republican forces to shift their strategy significantly towards one that relied heavily on targeting civilians, although sectarian retaliation was still a commonplace event in Belfast.

Overall, the VAR models used to test Hypothesis 2 do not provide evidence for my theory of strategic updating. Both Unionist and Republican paramilitary organizations do show some changes in their behavior over time, but only rarely are these responses correlated

with the actions of their enemies. Instead, particularly for Republican paramilitary groups, violent behavior in the current time period is very difficult to predict using both lagged Unionist and Republican violence, as well as casualty ratios in the Catholic community overall. Again, this is largely in line with a group that relies on unpredictable and unconventional forms of violence.

Table 3.3 shows the results of VAR models testing Hypotheses 3.1 and 3.2. Model 8 tests Hypothesis 3.1, analyzing the relationship between overall levels of fatal violence between British security forces and Republican paramilitary groups. Hypothesis 3.1 posits that successful attempts by British forces to target Republican fighters for fatal violence will have a dampening effect on subsequent Republican efforts towards violence. Here, the VAR modeling framework shows some interesting patterns in the evolution of Republican violence over time.

Model 8 shows that Republican fatal violence is highly autocorrelated over time. When taken in conjunction with the results of Models 1-6, this is particularly interesting: while the type and target of Republican violence varies widely from month to month and year to year, the overall level of Republican fatal violence is relatively stable over time. This is in line with the overall Republican strategy, which focused on applying constant pressure to the British state through various forms of violence and political protest in order to keep attention focused on the Republican cause: “If we could continue to inflict high British casualties and step up the sabotage campaign it would be difficult for them to bear the strain and drain on their economy, and no government could be expected to continue indefinitely in such a situation” [Smith and Neumann, 2005, p. 67]

However, Hypothesis 3.1 is not supported in this model. On the contrary: the relationship between British violence against Republican fighters and the overall level of violent activity by Republican forces is negative and significant. Looking only at the coefficients and *p*-values, this may mean that a period punctuated by heavy Republican losses is likely to be followed by an increased outburst of Republican violence in the following quarter. However, simulating the IRF in Figure 3.5 shows that the fully specified relationship may not be so clear-cut. British and Republican casualties seem to covary

Table 3.3: Casualties and Republican Target Selection

|  | State kills<br>of Repub. fighters<br>(7) | Republican<br>fatal attacks<br>(8) | BA base density<br>of Repub. targets<br>(9) |
|--|--|------------------------------------|---|
| State killing of Repub. fighters $_{t-1}$  | -0.06<br>( 0.12 )                        | -0.31 **<br>( 0.07 )               | -0.05<br>( 0.10 )                           |
| State killing of Repub. fighters $_{t-2}$  | 0.17<br>( 0.11 )                         | 0.03<br>( 0.10 )                   | 0.14<br>( 0.14 )                            |
| Republican fatal attacks $_{t-1}$          | 0.54 ***<br>( 0.13 )                     | 0.48 ***<br>( 0.10 )               | 0.08<br>( 0.14 )                            |
| Republican fatal attacks $_{t-2}$          | 0.00<br>( 0.14 )                         | 0.29 **<br>( 0.11 )                | -0.11<br>( 0.16 )                           |
| BA base density of Repub. targets $_{t-1}$ | 0.02<br>( 0.08 )                         | 0.04<br>( 0.12 )                   | -0.12<br>( 0.17 )                           |
| BA base density of Repub. targets $_{t-2}$ | 0.03<br>( 0.08 )                         | 0.06<br>( 0.00 )                   | -0.09<br>( 0.00 )                           |
| Catholic casualty ratio                    | 0.05<br>( 0.08 )                         | -0.05<br>( 0.07 )                  | 0.04<br>( 0.10 )                            |
| Catholic casualty ratio $_{t-1}$           | 0.13<br>( 0.08 )                         | 0.16 **<br>( 0.07 )                | -0.12<br>( 0.09 )                           |
| Catholic casualty ratio $_{t-2}$           | 0.04<br>( 0.08 )                         | 0.10<br>( 0.07 )                   | 0.23 **<br>( 0.10 )                         |
| Constant                                   | 0.18<br>( 0.20 )                         | 0.49 **<br>( 0.17 )                | 0.53 **<br>( 0.24 )                         |
| Trend                                      | 0.00<br>( 0.00 )                         | -0.01 **<br>( 0.00 )               | -0.01 **<br>( 0.01 )                        |
| Adjusted $R^2$                             | 0.39                                     | 0.55                               | 0.09  |
| Weekly Observations                        | 102                                      | 102                                | 102   |

*Note:*

\*p&lt;0.1; \*\*p&lt;0.05; \*\*\*p&lt;0.01

somewhat in the same time period; in the following quarters, Republican activity may decrease, but not in a way that is statistically detectable. On a similar note, Republican violence is also positively predicted by lagged casualty ratios in the Catholic/Republican community. As British forces kill greater numbers of Republican fighters, and as the overall balance of losses trends against the Catholic/Republican community, Republican groups seem more likely to fight harder. This is unexpected and runs counter to Hypothesis 3a.

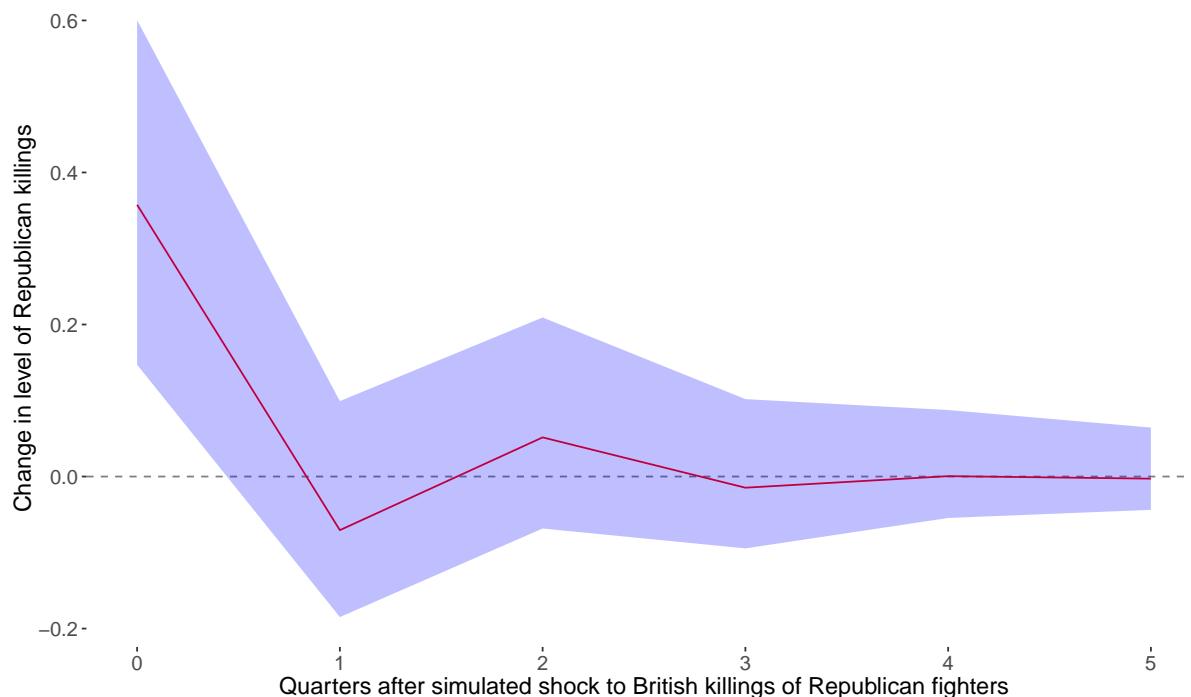


Figure 3.5: Impulse-response function of State killings of Republican fighters and Republican deadly attacks

Model 9 tests Hypothesis 3.2, analyzing the relationship between overall levels of deadly violence by British security forces and the level to which Republican groups target or avoid areas with high levels of British security for deadly violence. Significant findings here would provide evidence that Republican groups learn from losses and become more cautious, choosing to target areas with lower British security presence for violence despite their lower political, military, or economic value.

However, Hypothesis 3.2 is not supported in Model 9. The choice by Republican forces to target or avoid high-security areas is not predictable by either state levels of violence or

overall levels of violence by Republican forces in the previous six months. Interestingly, however, there is a positive and significant relationship between casualty ratios in the Catholic/Republican community and the type of location at which Republican groups engage in deadly violence. This seems to support the same broad finding as Model 8: as things get worse for Republican forces, at least as measured in overall casualty levels, these groups are more likely to fight hard not only do they engage in higher overall levels of deadly violence, but they engage in more violence in areas with high British security presence. It appears that Republican groups have some tendency to raise the stakes when conflict is going poorly.

Finally, Model 7 tests Hypothesis 3.1, looking for a direct link between deadly violence by Republican and the same by British security forces. If British security forces are interested in maintaining a ‘proportional’ response to Republican violence, it is likely that deadly violence targeting Republican fighters will decrease as the perceived need for deadly violence decreases. As Republican violence decreases, therefore, the need and justification for highly violent peacekeeping measures by British forces should decrease in turn, and the level of violence perpetrated by British forces should decrease. However, once again this does not seem to be the case. The VAR framework of Model 7 finds, in fact, that deadly violence by British security forces is very difficult to predict using Republican behavior. Neither Republican deadly violence, Catholic/Republican casualty ratios, or Republican targeting of high-security locations show a predictive relationship to deadly violence by British security forces.

This finding is surprising as well. The political rhetoric surrounding British peacekeeping stressed proportionality and responsiveness, especially given the sensitive nature of the Northern Ireland issue with the British electorate. As such, one might expect a more direct relationship between Republican and British violence: as the level of Republican violence decreases over time (whether due to political successes, erosion of local support, or successful suppression by British forces) one would expect British strategy to move in a less-violent direction as well.

Historical accounts of the Troubles do suggest that there were fairly clear ‘stages’ of

strategy employed by both British and Republican forces, and that these different strategic epochs had a strong effect on the level of violence employed by both sides [Ni Aolain, 2000, Kennedy-Pipe and McInnes, 1997, Smith and Neumann, 2005]. The historical record makes a strong case that these changes exist, and that changes in Republican strategy were mirrored closely by changes in British strategy, but the lack of clear results from VAR modeling suggests that these changes took the form of rare but significant shifts, rather than impulse/response functions measured in months or quarters. Overall, these patterns are not picked up by the VAR framework employed here.

Hypothesis set 4 looks for clear evidence of sectarian violence cycles between Republican/Catholic and Unionist/Protestant communities. The historic record suggests that violence during the Troubles, particularly in Belfast and Derry, often contained a religious-sectarian element that went well beyond the political struggle being carried out by the Republican paramilitary groups against the British state. Parallel to this struggle, tensions between the Catholic and Protestant communities in Belfast and Derry had been simmering for decades as well: the minority Catholic community faced systematic discrimination from the majority Protestant population for generations, and one element of Northern Irish freedom that motivated Republican violence was a greater level of freedom and participation for Catholics.

In Belfast and Derry, religious tension and self-selection of living space led to many areas that were nearly 100% Catholic or Protestant. This high level of local segregation meant that both Republican and Unionist paramilitary groups could seek shelter and aid from civilians they claimed to represent. It also meant that nearby areas containing citizens of a rival religion were often deemed fair targets for violence, particularly when groups were seeking retaliation for previous losses. I expect that overall, but particularly in Belfast and Derry, this relationship should take on a spatial aspect. When a Catholic group launches an attack in a Unionist group's Protestant 'backyard', I expect that there will be a corresponding increase in the likelihood of Unionist violence in a predominantly Catholic area. On a larger scale, I expect that as Unionist (Republican) groups increase their focus on attacking targets in Catholic (Protestant) areas, the other side will respond

in kind. Table 3.4 shows the results of VAR models testing Hypothesis set 4.

Table 3.4: Catholic Population and Target Selection

|   | % Catholic pop.<br>of areas targeted<br>by Republicans<br>(10) | % Catholic pop.<br>of areas targeted<br>by Unionists<br>(11) |
|---|--|--|
| % Catholic pop of Republican kills <sub>t-1</sub> | -0.02<br>( 0.10 )  | 0.09<br>( 0.11 )   |
| % Catholic pop of Republican kills <sub>t-2</sub> | 0.03<br>( 0.09 )   | 0.13<br>( 0.10 )   |
| % Catholic pop of Unionist kills <sub>t-1</sub>   | -0.09<br>( 0.09 )  | 0.10<br>( 0.10 )   |
| % Catholic pop of Unionist kills <sub>t-2</sub>   | -0.03<br>( 0.09 )  | 0.14<br>( 0.09 )   |
| Catholic casualty ratio                           | 0.25 **<br>( 0.09 )  | -0.02<br>( 0.10 )  |
| Catholic casualty ratio <sub>t-1</sub>            | 0.01<br>( 0.09 )   | -0.20 **<br>( 0.10 )   |
| Catholic casualty ratio <sub>t-2</sub>            | -0.15<br>( 0.09 )  | 0.05<br>( 0.10 )   |
| Constant  | 0.43 **<br>( 0.18 )  | -0.06<br>( 0.20 )  |
| Trend   | -0.01 **<br>( 0.00 )   | 0.00<br>( 0.00 )   |
| Adjusted $R^2$                                    | 0.07   | 0.03   |
| Weekly Observations                               | 102  | 102  |

*Note:*

\*p<0.1; \*\*p<0.05; \*\*\*p<0.01

Once again, I do not find direct support for this hypothesis. While anecdotal evidence and local patterns of retaliatory violence between Catholic and Protestant communities do exist, and sectarian violence is widely recognized to have played a role in the Troubles, it appears this relationship is not as straightforward from a geographic point of view. More violence in Protestant neighborhoods does not necessarily lead to more violence in Catholic neighborhoods, and vice versa.

However, there does seem to be some responsiveness to casualty ratios. While the action-reaction pattern of violence in these communities does not seem to hold, Model 10 does show a statistically significant and positive relationship between the Catholic casualty

ratio and the level to which Republicans focus violence in Protestant neighborhoods. In other words, as the casualty ratio moves against the Catholic community, Republican paramilitary groups are more likely to launch attacks against targets (military or civilian) in predominantly Protestant areas. The inverse relationship holds for Unionist forces. After periods in which the casualty ratio moves against the Protestant community, Unionist paramilitary groups are more likely to launch attacks against targets in predominantly Catholic areas. However, simulating the IRF for this latter relationship in Figure 3.6 shows that it may not be particularly robust. While

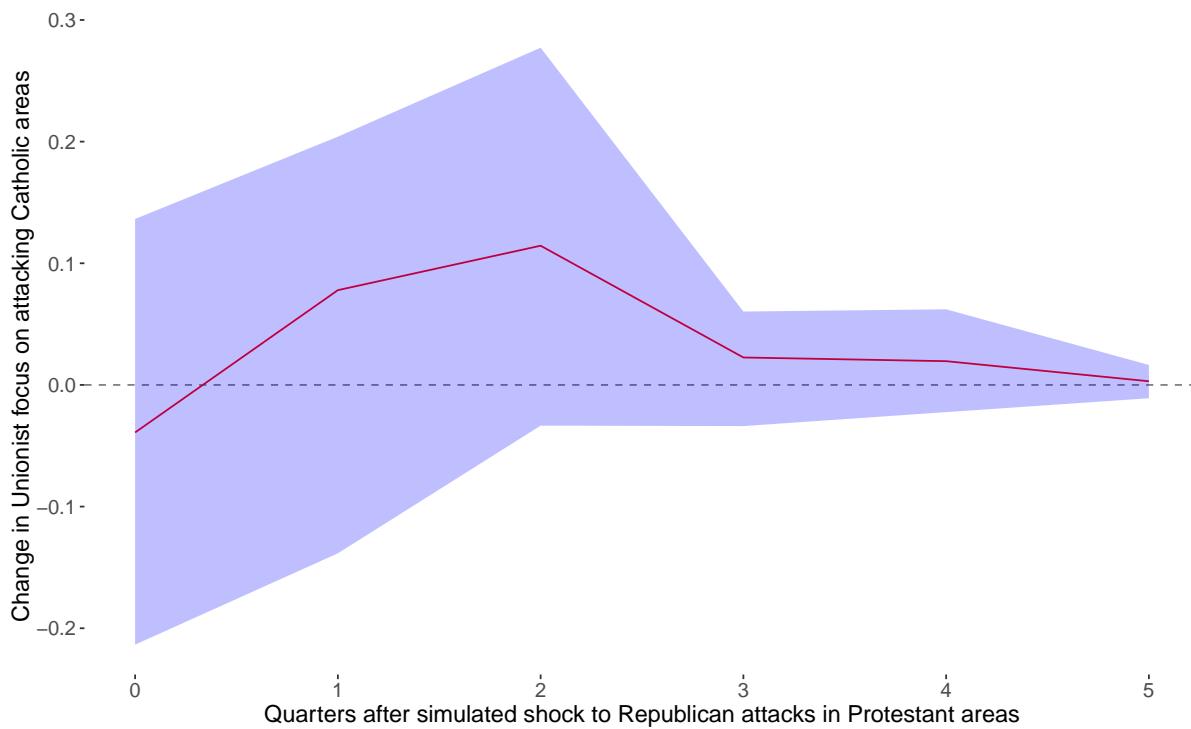


Figure 3.6: Impulse-response function of Catholic population of Republican and Unionist targeted areas

This finding is interesting because it does provide some limited support for the idea that groups change their behavior in response to near-term shifts in the tide of conflict. As Republican groups experience less success in their fight against Unionist forces and British security, they may need to make up this lost ground by attacking targets that balance accessibility and visibility. While the PIRA in particular repeatedly stressed that it was not fighting a sectarian war, it did engage in violence that was intended to shock and publicize

its goals to a predominantly Protestant audience. As such, making a greater effort to publicize its presence to the Protestant population would be easier in areas dominated by Protestants. However, these areas did not necessarily contain more ‘legitimate’ targets for Republican groups (British security bases or patrol routes, for example) meaning that an increase in violence in Protestant areas was likely an exigency brought on by sectarian retaliation, rather than part of the overall political struggle to free Northern Ireland from British rule.

Overall, testing Hypotheses 1 through 4 through the vector autoregressive framework does not provide sufficient evidence to reject the null in any case. It appears that if military-strategic adaptation took place between Republican fighters, Unionist paramilitary groups, and the British security apparatus, it did not take the form of a gradual, co-evolutionary process over time. In fact, in many cases there is no apparent relationship whatsoever between the actions of one group at time  $t$  and the actions of an enemy group at time  $t - 1$  in terms of how the opposing group used violence, who was targeted for violence, or the type of location at which the opposing group used violence. As discussed both here and in the previous chapter, this does make some sense on the side of non-state groups, particularly Republican paramilitary fighters. These groups fought at a distinct material disadvantage to state forces, often relying on home-made, outdated, or smuggled weaponry and rarely able to afford fighting pitched battles or engaging in conventional violence against government forces.

Instead, the PIRA and other Republican groups were forced to adopt non-conventional means of doing battle: not just the type of technology or tactics used over time, but an overall lack of predictability in their behavior. Groups fighting at a disadvantage cannot risk the stronger opponent predicting their behavior over time, lest they risk being detected, pinned down, and destroyed. Instead, Republican groups’ behavior from time period to time period is very difficult to predict in terms of where they strike, what type of violence they employ, or what group of people bear the brunt of violence.

There is some limited evidence of mutual responsiveness in the behavior of Republican and Unionist paramilitary groups. These groups fought something of a parallel conflict

to the broader politicized struggle for Northern Irish independence, although it certainly was linked to the broader struggle. Paramilitaries on both sides engaged in reciprocal violence against one another, launching attacks at both fighters and civilians of the ‘wrong’ religious sect who were often assumed to be active or passive supporters of the other side.

### 3.6 Threshold Detection in Actor Behavior

While my hypotheses of mutual adaptation are not supported, I still expect that some level of strategic adaptation occurred during this thirty-year struggle. If strategic shift is a more abrupt shift from one state to another, rather than a gradual shift over time, it may be that real-world changes are occurring that the VAR model cannot take into account. This is borne out by the work of military historians analyzing the Troubles, who have divided the struggle into a number of distinct strategic ‘epochs’ characterized by a certain type of behavior on the behalf of both Republican and state forces.

One example of this shift is the balance between urban and rural violence employed by Republican fighters and state forces. Hypothesis 1.1 anticipates a relationship between the location of British and Republican violence: an increase in urban violence by state forces (representing a focused campaign of suppression in the large cities of Belfast and Derry) should, I anticipate, lead to a decrease in urban violence by Republican forces, as fighters move their efforts to the less-guarded countryside to avoid detection and apprehension or killing by state forces.

Figure 3.7 shows trends in the mean of both British and Republican behavior over time, using the same method of change-point analysis described in the previous chapter. This figure shows several interesting findings that suggest that the ‘spirit’ of Hypothesis 1a might have some support, even if it does not show up in the VAR modeling framework. First, the trend of Republican violence over time lines up with historical records: initially, Republican forces fought a highly urbanized conflict largely confined to Belfast and Derry. Over time, as the overall Republican approach shifted toward a ‘long war’ of political attrition, their efforts moved from the highly-policed urban areas to rural regions where British security was spread thin. On the part of Republican fighters, the change is quite distinct and fairly abrupt: until the early 1980s, around half of all fatalities inflicted by

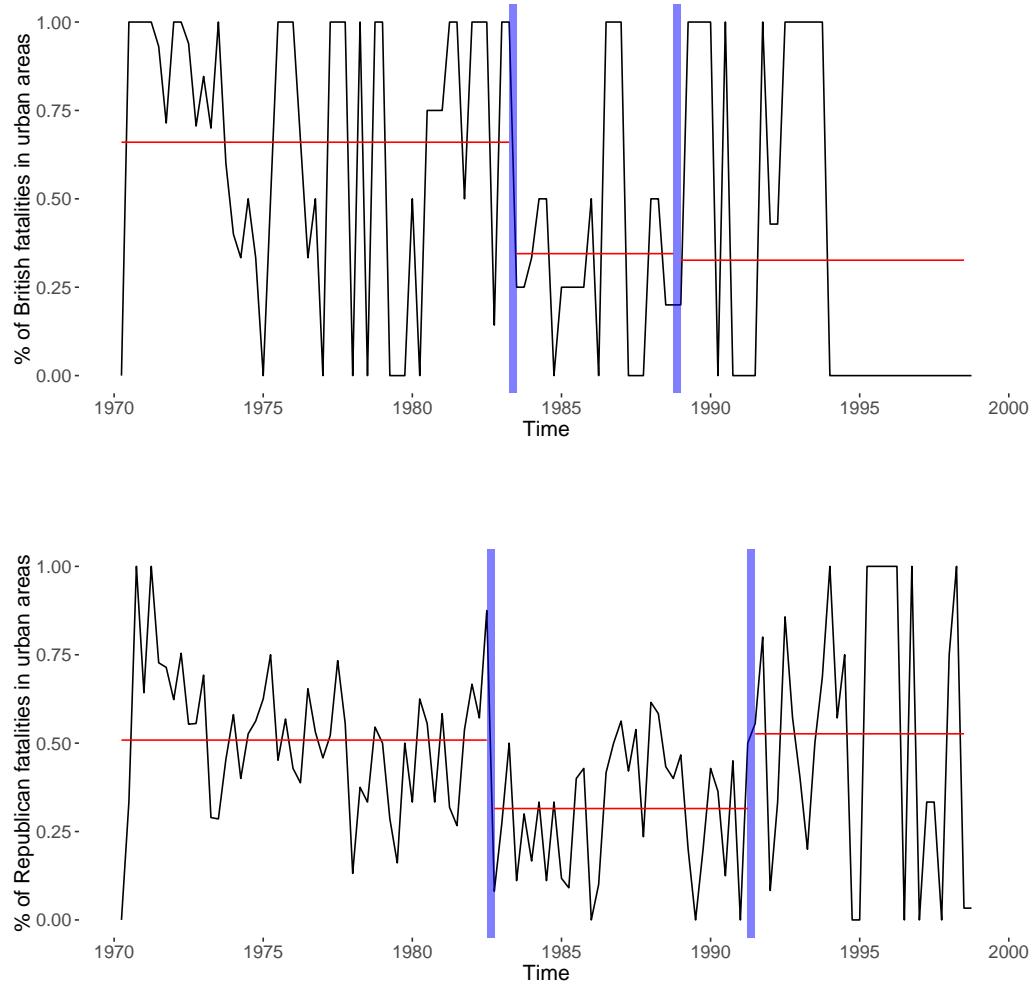


Figure 3.7: Proportion of State and Republican violence in Belfast and Derry

Republican groups took place in Belfast and Derry. In 1982-3, however, this proportion dropped to less than one-third, indicating a shift in the focus of fatal violence from rural to urban areas.

Interestingly, this is right around the same period when British security changed their own geographic focus on policing Republican efforts. Also in the early 1980s, the focus of British violence shifted from overwhelmingly taking place in Belfast and Derry (roughly two-thirds of all fatalities) to primarily taking place in rural areas, with just under two-thirds of fatalities now taking place outside the major cities. In other words, both parties shifted their efforts from pitched fighting in urban areas to a rural campaign: Republican fighters adopted a strategy of rural violence to reduce their own risk when facing stronger British forces, while British security moved their own efforts out of the cities in order to pursue Republican fighters now launching attacks at vulnerable or poorly-secured targets in the countryside.

So, is this shift borne out by the historical record, or is it more likely that this is a spurious correlation? While it is impossible to ‘prove’ that these shifts line up perfectly with historical accounts, multiple qualitative analyses of the Troubles indicate that a significant shift in both Republican and British strategy took place in the early 1980s. This period was marked by several shifts in the political and military incentives governing behavior on both sides. Republican forces were galvanized by the deaths of Bobby Sands and nine other prisoners during a hunger strike 1981, in what was seen as a brutal injustice by the British security system. This period also marked the intensification of an electoral/political process in parallel to the armed struggle, as the PIRA’s Sinn Feinn party began to contest local elections in Northern Ireland. The PIRA in particular believed that this was the right time to engage in a rural contest in the more friendly regions of East Tyrone and Armagh, somewhat similar to the attempts to create ‘no-go’ areas in Belfast and Derry. The goal of this operation was to create a more distinct territorial base for Republican operations in the countryside.

On the British side, the ‘Ulsterisation’ policy pursued through the 1970s by the British government was widely recognized to have failed by the early 1980s. While Operation

MOTORMAN had met with some success at quashing Republican violence in the major cities, the British government had failed to succeed in reconciling with the overall Catholic population, hampered in part by active resistance from the Protestant community. Political will on the British side, spurred by continuing and escalating unrest in Northern Ireland, was beginning to swing towards a more intense strategy of militarization, focusing more on directly engaging and killing Republican fighters and leveraging additional assets such as the SAS to conduct counterinsurgency operations.

The result was a shift in the tone and location of fighting between Republican and British forces. Republican fighters focused their efforts more distinctly on a rural campaign of violence, trying to carve out territory and weaken British power in an area where it already had difficulty making serious inroads. British authorities responded to this campaign by increasing the intensity and militarization of their own efforts, attempting to locate and destroy rural Republican cells using counterinsurgency methods rather than the policing-based attempts employed during the period of Ulsterisation.

Interestingly, a second shift in Republican violence is detected nearly a decade later in late 1991. After a period in which Republican forces focused the bulk of violence in rural areas, the early 1990s saw a distinct shift back to urban violence. As with the shift in the early 1980s, it is hard to point to a distinct event or events that may have triggered this shift. However, while it was not common knowledge at the time, declassified documents show that the campaign of militarization in the 1980s played a powerful role in bringing the PIRA to the negotiating table in the early 1990s. By this point, many senior PIRA activists, invaluable for their experience and expertise, had been captured or killed; the rate of recruitment, compared to the rate of attrition, was simply not high enough to maintain a high level of combat effectiveness. This did not mean that violence was a thing of the past; while the overall level of PIRA activity decreased, and violence became less direct and more scattered, this was also the period in which Britain was attacked directly by PIRA bombers, putting the costs of violence directly before the British people. Along with this violence, however, by the late 1980s and early 1990s PIRA leadership had begun secret talks with the British government to agree on ceasefire terms.

However, historical analysis points to the early 1990s as the point when the PIRA, weakened by the uptick in militarization by British forces since the 1980s, began to engage in serious peace negotiations with the British government. This period saw a downswing in overall Republican violence, but this change was particularly notable in rural areas.

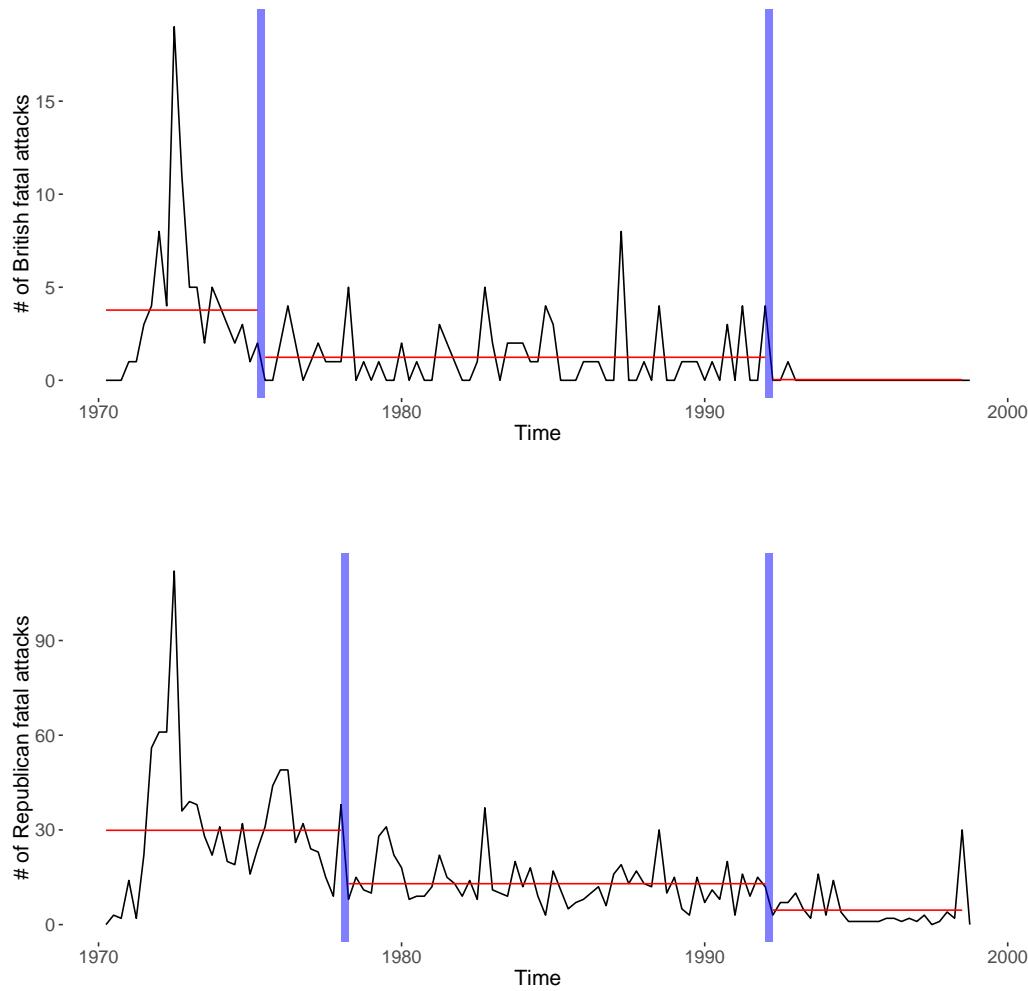


Figure 3.8: Level of fatal attacks by State and Republican forces

Figure 3.8 shows that the 1991-1992 period saw a distinct downswing in the mean number of Republican-caused fatalities per time period. Taken together, this shows that the early 1990s saw (1) a decrease in overall Republican violence, as well as (2) a shift in the geography of Republican violence away from the rural campaign and back to Belfast and Derry. One possible interpretation of these trends is that Republican forces lowered

their overall level of violence to show their commitment at the negotiating table, while continuing to engage in some level of high-visibility urban violence to show their continued viability as a military threat. By 1994, the signing of the Good Friday agreement signaled, if not an end, then a massive reduction in PIRA violence in Northern Ireland.

Overall, the patterns detected via change-point analysis line up well with the strategic shifts noted in the historical record. Both British and Republican forces changed their military behavior in a way that (1) is linked closely to real-world military and political incentives facing both sides and (2) can be detected using uninformed statistical analysis. This finding provides some support for the idea that military-strategic evolution may occur in distinct shifts or regime changes, rather than month-to-month or even year-to-year evolution in response to enemy behavior.

If nothing else, this chapter reiterates the ‘finding’ from the previous chapter: conflict behavior is difficult to explain or forecast in the short run. The type, target, and location of violence perpetrated by both state and non-state actors from week to week and from month to month can vary considerably, especially on the side of non-state actors carrying out violent campaigns against materially stronger states. Groups engaged in non-conventional or ‘terroristic’ campaigns rely on unpredictability for survival: allowing their behavior to be modeled or identified by the state is dangerous, if not fatal.

However, this does not mean that military actors behave randomly, or that they do not respond to changes in enemy behavior. The VAR models provide some evidence that, at least in some contexts, Republican forces responded to changes in their environment, in particular the recent trends in casualty ratios and the imbalance of deaths between the Catholic and Protestant sectarian communities. While these changes are not necessarily those I hypothesized, they do point to a pattern of updating that may be further explored in future work.

Finally, using change-point analysis provides some interesting preliminary results that point towards a different mechanism of strategic updating. These behavioral shifts are significant and abrupt, indicating a conscious refocusing of a military actor’s efforts. They also line up well with changes in the course of the conflict that have been previously

identified by historical analysis of the Troubles. If nothing else, these findings show that the quantitative, casualty-based approach I take to describing and explaining actor behavior is able to pick up on real-world variation in the way that these groups prosecuted their military campaigns.

# Chapter 4

## Strategic Updating and Conflict Duration and Outcome

### 4.1 Introduction

In Chapters 2 and 3, I focus primarily on testing two ideas. First, does military-strategic exist in such a way that we can detect and measure it using event data? Second, can we explain patterns in the timing and direction of military-strategic change by looking at action-reaction behavior between conflict actors? To answer both questions, I analyze two novel data sets on two well-documented conflicts: Israel and Palestine during the Second Intifada in the early 2000s, and the Troubles in Northern Ireland from 1970 through 2000. In answer to the first question, I find significant evidence that conflict actors do update their military strategy over the course of conflict, and that this can be measured by looking at summary statistics of the type, target, and location of violence employed by an actor over time. The changes I find are identified through non-parametric statistical methods that do not require *ex post* information, but line up well with qualitative accounts of strategic shifts at various points during conflict.

These findings provide evidence that military-strategic updating happens during war, which itself is a new finding in the conflict literature. Moreover, military-strategic updating happens even on the side of much stronger state actors, which even the limited theory available on the subject states should be significantly more rare than updating on the part of weaker but more nimble insurgent groups. The British and Israeli governments vastly

outmatched their opponents in terms of manpower, funding, and armaments; however, even in these cases, state forces showed distinct shifts in military strategy over time. This indicates that even the more powerful party in an asymmetric conflict has both motive and opportunity to update its fighting style to maximize military efficacy.

To answer my second question, I use a set of statistical tests to identify whether these changes occur incrementally and conjointly: if — and how — changes in the military-strategic behavior of an insurgent group corresponded to changes in the behavior of the state they were fighting against. In both the cases I study, I do not find strong evidence to support this idea. Short-term variation in insurgent and state behavior certainly exists, but these changes do not seem to be correlated in any systematic fashion. Larger, more abrupt or distinct shifts exist as well, but these changes seem to correspond to internal shifts or non-military outward impetus, not short-term variation in the behavior of one's opponent.

Instead, two patterns emerge. First, state behavior seems to be somewhat more predictable in the short run, or at least vary less from time period to time period. This may signify a higher level of organization and regularity within state forces (being able to stick to a defined mission plan) or simply a greater level of entropy present in a more hierarchically organized and bureaucratic state military organization. The result is that state military strategy tends to be relatively more predictable in the short term by looking at previous patterns of behavior.

Second, insurgent behavior seems to vary significantly in the short run (week to week or month to month) while showing little responsiveness to state behavior over time. This finding, which surfaced repeatedly in my hypothesis tests, was unexpected. However, from another perspective this makes sense — both the groups of insurgents studied here fought at a severe resource disadvantage to their opponents. When faced with this type of material imbalance, the only way to survive is to take the advantage of information. Behaving unpredictably in the short run as to where, when, and how attacks would occur allowed these groups to evade detection and capture and fight a longer unconventional war.

## 4.2 Strategic Change and Conflict Dynamics

In this chapter, I move from analyzing the existence and causes of military-strategic updating to identifying the effects of actor updating on the duration and the outcome of civil conflict. My theory states that actors who are more adaptable – meaning that they can quickly and effectively update their military strategy to fight effectively against their opponent – are likely to enjoy more short-term battlefield success, and I expect that this success should translate into more favorable conflict outcomes for these groups.

In the cases I analyze in the previous two chapters, I can leverage rich descriptive information about the type, target, and location of violence during these insurgency/counter-insurgency campaigns. In this chapter, I expand my analysis to include a larger and more coarse-grained cross-sectional data set of civil conflicts. In this large-N analysis, I examine how actors' intra-war behavior relates to the duration and outcome of conflict. This is necessary because here, I move from studying the behavior of individual actors or groups during conflict, and toward the conflict itself as the unit of analysis. Testing hypotheses about duration and termination require a larger data set of conflicts so that I can compare across countries, actors, and time periods.

During a conflict, both sides engage in military-strategic adaptation. This adaptation is due to both sides' responses to changes in the course of conflict. These changes can be observed (a shift in the casualty ratio over time) or estimated into the future (based on a change in the military strategy of the opponent). My theory suggests that military strategy has a real impact on an actor's success or failure in conflict. In line with previous work, I argue that both states and insurgents engage in an 'arms race' of strategy [Johnson, 2009], in which both sides attempt to adapt to each other's warfighting approach and come up with a strategy that will succeed against that chosen by the opponent. In other words, military-strategic updating is a way for groups to change the tide of combat to their advantage. However, this updating is not a frictionless process.

Shifts in the type, target, and location of violence — the dimensions I use to define military decision-making and therefore strategy — are costly decisions in terms of time, money, and military resources. This can be a major undertaking for both states and

insurgent groups. A significant change in the technology of violence a group uses requires fighters to be retrained in weapon use, effective tactics, and equipment maintenance - and in many cases, even weapon manufacturing. For groups with few 'full-time' fighters, or for those with little formal military training, this can be a major barrier to surmount. Even for states, technological reorientation is a significant undertaking. While state military forces have more formal training and experienced soldiers, the logistical challenge of adapting a large force to rely on different weapons and/or tactics requires dedicated effort and significant expenditures.

Changing the type of target at which an actor directs a campaign of violence can also be a challenge. Moving from a military-targeting to a civilian-targeting strategy often requires significant effort, not necessarily in the military arena (since civilians are, by nature, often much easier to target for violence) but in the political one. States can potentially suffer severe losses in legitimacy and public support when they adopt a strategy that kills non-fighters, as has been shown repeatedly in the counter-insurgency literature; killing non-fighters in an attempt to suppress dissent often has the exact opposite result, as the increased grievances of the population lead to greater levels of rebellion. For insurgent groups, optics are also very important in most cases. An insurgent group seeking to increase its support in the population has to be seen as taking real action against the state; those that target primarily civilians for violence risk sacrificing their legitimacy and the sympathies of the population, and being relegated to the role of a fringe terrorist group.

On the other hand, actors that try to adopt a strategy of targeting military personnel and installations also face real obstacles. States that work to adopt a strategy of only targeting civilians have to increase their intelligence-gathering efforts dramatically: finding and capturing or killing insurgents is very difficult when these fighters have no uniforms or other demarcations that easily distinguish them from civilians. When insurgents are operating out of 'friendly' territory, or where they have enough local power to intimidate potential informers, this task becomes even more difficult. Insurgents, on the other hand, have little trouble identifying state forces - the challenge is how to successfully engage those forces without incurring major losses. Insurgents that are materially weaker than

the state are often unable to effectively engage state forces directly; while opportunities may arise to attack military patrols, for example, insurgent groups are generally unwilling to risk clashing with the state in major engagements.

Finally, changing the physical location of violence can also pose major challenges. Fighters on both sides have to familiarize themselves with new territory. Logistical issues, particularly for insurgents, can also arise when these groups either have to (1) transport fighters and material a longer distance or over hostile terrain to reach new targets or (2) develop a base of support and information in a new location altogether. In addition, the tactics of successful engagement (and, for insurgents, disengagement) in rural areas is often very different from operating procedures in urban population centers. Both types of forces have to be retrained to operate in this new type of environment.

By this reasoning, I interpret an actor's shift in military strategy as an important signal that the environment has changed (or is estimated to change in the near future) and that the actor is responding to this signal of danger by investing in a different type of warfighting that it expects to be better suited to the new conflict environment. Major shifts in strategy along the axes of type, target, and location of violence are costly and difficult for both states and insurgents. As such, shifts of this significance contain meaningful information about the group's estimation of its military position. Looking at the timing and nature of these shifts should provide useful information about how well a group is doing at a given point in time, and how well they are projected to do in pursuing their military and political goals over the course of the conflict.

#### **4.2.1 Hypotheses**

If the process of mutual military updating can be described as an arms race, then I expect the more effective updater to be more likely to succeed. As described in Chapter 1, an actor that can rapidly and effectively respond to changes in its opponent's behavior can fight more effectively, even against a much stronger opponent, and can likely inflict enough damage to achieve a better overall outcome in peace negotiations. In this chapter, I set out several hypotheses that try to link military-strategic updating to conflict outcome.

My theory states that in conflict, the actor which can update their military strategy

more quickly in response to changes in the battlefield environment will fight at a relative advantage more often and for a longer period of time over the course of the conflict. In turn, an actor that can fight more effectively should be able to (1) inflict higher damage on its opponent's military forces, logistic support base, and morale; and (2) avoid taking unsustainable losses itself. Military strategy can minimize or exaggerate the effects of capability on the overall outcome of conflict. A weak group that is also bad at military-strategic updating, when facing a state with superior capability as well as superior information-processing and decision-making abilities in strategic updating, is likely to lose quickly. However, a relatively weak insurgent group can (to some extent) make up for its capability imbalance by quickly and effectively updating its military strategy so as to maximize its effectiveness in fighting the government. By doing so, this weak group may be able to stay relevant in the conflict for a longer period of time, and inflict significantly more damage on the state, potentially achieving a better outcome to the overall conflict.

Insurgents that can update more quickly and effectively than states should do better in conflict than their capability alone would indicate. If we conceive of an ideal-type 2x2 table between a strong party (the state) and a weak party (the insurgent) we can come up with a set of predicted outcomes regarding both the duration and the outcome of civil conflict.

Table 4.1: Insurgent updating and conflict outcome

|            | Fast Insurgent                    | Slow Insurgent                    |
|------------|-----------------------------------|-----------------------------------|
| Fast State | Short Conflict,<br>Insurgent loss | Short Conflict,<br>Insurgent loss |
| Slow State | Long Conflict,<br>Insurgent wins  | Short Conflict,<br>Insurgent loss |

As Table 4.1 illustrates, when the updating capability of both sides is equally matched (both actors update quickly, or both actors update slowly) in other words, if neither side

can gain a significant battlefield advantage by using a superior military strategy there should be no distortion of the translation between capabilities and outcomes. This outcome is identical to those predicted by current models of conflict termination, which rely heavily on ex-ante factors such as relative capability of the state, terrain, regime type, and other slow or unchanging elements affecting the battlefield environment.

When military-strategic updating is imbalanced, the advantage lies with the group that can more quickly and effectively update its strategy relative to the other. When the updating advantage lies with the state, conflict is likely to be even shorter, and is likely to end with the worst outcome for the insurgent. A group that is materially weaker and unable to fight effectively with the resources it has is likely to be defeated quickly and resoundingly, even compared to the equal-updating / capability-only model of conflict outcomes.

However, the most interesting predicted cell in this matrix is the case where a materially weaker insurgent movement can process information and make decisions about military strategy more quickly than its state opponent. When this occurs, I expect that the insurgent group will be able to ‘punch above its weight class’, fighting more effectively and for longer periods of time against a state government that is unable to identify and switch to a fighting strategy that allows it to bring its superior capability to bear against the insurgent.

H1.1 Conflicts are more likely to end in favor of insurgents when insurgents can update their military strategy more quickly than states.

Fast military-strategic updating also affects the duration of conflict. Insurgents who are fighting at a significant material disadvantage do not just seek to kill state military forces — they are also keenly interested in staying alive. A group with only hundreds or a few thousand trained fighters has to emphasize the minimization of friendly losses if it wants to stay relevant in a conflict. For insurgents, then, an effective strategy needs to balance doing damage to the opponent with avoiding friendly losses.

When insurgents and states have roughly equal military-strategic updating ability, the conflict should be relatively shorter. Weak insurgents that are unable to achieve a lasting

strategic advantage against the government are more likely to find themselves in a fight where the state can bring its material capabilities to full effect, and when this happens, insurgents are unlikely to do well. As such, I expect that in cases where insurgents and states have similar military-strategic updating abilities, conflicts are unlikely to last a long time. When insurgents are weaker and less capable of updating their military strategy relative to the state, conflicts should be even shorter. As stated earlier, when insurgents are not only weaker but less effective at using what capabilities they do have, they are unlikely to do well. When the state can bring superior capability fully to bear against a weaker insurgent opponent, conflicts are likely to be short and end decisively in favor of the state.

Finally, I expect that insurgent actors that are able to update their strategy more quickly in response to changes in the conflict environment should not only be able to fight more effectively but to survive for a longer period of time. Even insurgents that can use their military resources effectively against the government are still limited in the resources they have; weak insurgent groups rarely win fast, decisive military victories against much stronger state forces. Instead, I expect that conflicts last longer when weaker insurgents can fight more effectively; this gives insurgent groups more time to wear down state forces' resources and morale, and eventually achieve a better outcome as the state realizes that a military victory is unlikely.

H2.1 Conflicts are more likely to last a long time when insurgents can update their military strategy more quickly.

Taken together, Hypotheses 1 and 2 approach the question of conflict duration and outcome from a different angle. Duration and outcome have been studied in conjunction before by conflict scholars, who have found a correlation between conflict duration and outcome: the longer conflicts last, the less positive the outcome for the government [Cunningham et al., 2009]. I argue that this correlation may be due to this common mechanism of effective military strategy. While this idea has been roughly proxied for by looking at factors like insurgent control of peripheral territory [Cunningham et al., 2009], rough or forested terrain within the state [Fearon and Laitin, 2003], or broad qualitative

descriptions of the strategy adopted by insurgents over the course of the entire conflict [Kalyvas and Balcells, 2010], the measures I adopt here are better suited to getting at the underlying mechanism. Insurgent groups that can fight effectively and adapt to changes in their conflict environment are likely to survive longer and achieve better outcomes than those that cannot do so.

Obviously, military-strategic updating is not a guarantee of success. The question is worth asking: if strategic change is not guaranteed to result in success for the actor involved, why do any actors engage in this type of updating in the first place? In other words, why pay the high costs of significant military-strategic change if the war may be a lost cause regardless? In response, I would argue that even in dire straits strategic updating is not a guaranteed failure — just that it is unlikely to succeed. It is important to remember that even if major military-strategic change is not likely to result in a losing actor's return to military viability or dominance, even a small chance of success may be better than the perceived chances of success under the status quo. An actor that is losing badly enough that failure is guaranteed under the current circumstances is more willing to pay high short-term costs in the hope of a long-term payoff, even if the probability of that payoff is low.

It is possible that major military-strategic shifts can work for a beleaguered actor, at least in the short term. Consider the case of the Angolan Civil War, in which the major rebel group UNITA engaged in a major military-strategic reorientation in the late 1990s. UNITA abandoned their former territorial holdings in urban areas, retreated to the thick jungle areas of southeastern Angola, and pursued a highly nonconventional campaign against the government and its supporters. This shift in military strategy involved significant changes in the type of violence UNITA engaged in, the targets of this violence, and both the physical location of operations and the type of target selected for violence by UNITA.

This allowed UNITA to survive a few more years, and for part of this time, to turn the tide of the military campaign against the Angolan government. However, it was not enough to win the conflict, because the Angolan military updated its own strategy in turn.

In response to UNITA's move to the jungle and to a less-conventional form of warfare, the Angolan state changed its own doctrine, focusing on restricting UNITA's movement within southeastern Angola and disrupting the rebel group's operations. UNITA did not win the war — a few years later, the group surrendered and reintegrated into Angolan society as a political party. However, it can be argued that this strategic reorientation at least allowed the group to hang on longer, achieve some military successes, and possibly achieve slightly more favorable ceasefire terms than would have resulted if they had kept their previous military strategy intact.

The case of UNITA is a illustrative example, not an empirical argument, but I argue that it serves a purpose in outlining the difficult calculus of military-strategic updating that faces actors in a losing war. UNITA's leadership decided to gamble on a fundamentally different strategy, hoping that it would be enough to throw the Angolan government off-balance and effectively counter the current government strategy which it did, at least for a few years. In the long term, it was not enough to lead to overall victory, but it was enough to avert short-term disaster.

The reasoning here is similar to the concept of ‘gambling for resurrection’ that has been used to explain both economic and political behavior. This theory explains seemingly costly, long-shot behavior by key actors as, essentially, desperate attempts to buy time. Political leaders may launch unsuccessful wars abroad to deter domestic challengers at home [Downs and Rocke, 1994]; firms in deep financial danger may bid more aggressively for contracts, even accepting unfavorable outcomes [Calveras et al., 2004]; banks in debt may accept significantly higher levels of risk in making new loans [Jiménez et al., 2014]. In all these cases the reasoning is the same: actors in crisis choose a risky, potentially very costly course of action because (1) these costs will not come due until a future date and (2) the only alternative is total failure. When faced with catastrophe, actors will take action that otherwise would be deemed unacceptable, accepting high levels of risk to buy more time.

The reasoning is the same for both state and insurgent actors: the hope of staving off disaster is a powerful motivator, especially when disaster often involves the literal

destruction and potentially death of the decision-maker in question. By attempting to change their military-strategic course of action, even if this means repeatedly paying the cost to find a long-shot strategy that works against that of the opponent, insurgents and states are acting rationally in the face of catastrophe.

While Hypotheses 1 and 2 use the conflict episode as the unit of analysis, Hypothesis 3 adds a temporally dynamic element, predicting the expected remaining duration of the conflict based on the behavior of a given actor at a given point in time. If successful military-strategic updating gives a group an advantage, especially in the short run, we may be able to link updating behavior with the timing of conflict termination. In the case of insurgent groups in particular, military-strategic updating may be most effective at buying time — delaying the end of a conflict in order to gather more resources, wait for international aid, or continue to inflict costs on the state in hopes of sapping morale and achieving a non-military victory.

By contrast, actors that are unable to update their military strategy — whether due to internal inertial, resource limitations, or external pressure — may be unable to effectively engage their opponents on the battlefield. In these cases, I would anticipate that a lack of military-strategic updating should be associated with a faster termination. In other words, we may be able to predict conflict termination based on *lack* of military-strategic updating. When one or both actors are incapable or unwilling to change the way they fight, this is a sign that the conflict is unlikely to last much longer. Because I have temporally referenced data on actor behavior, I can frame this hypothesis as a hazard analysis, analyzing the short-term likelihood of conflict termination at any given point in time based on the recent behavior of the actors involved.

H3.1 The longer the elapsed time since an actor has most recently updated its military strategy, the higher the likelihood of a conflict episode terminating.

### 4.2.2 Research Design

As in the previous two chapters, I rely on event data to measure patterns in conflict actor behavior. By summarizing the type, target, and location of violence initiated by a given

actor over a given time period, I can identify periods when actor behavior is relatively stable. In these periods of stability, I argue that the actor's use of violence is governed by a roughly static set of decision-making rules, and I label these rules as a military strategy. Expanding from the individual cases I analyze in the previous two chapters, and moving from the actor-level to the conflict-level in my analysis, requires a significantly larger cross-national event data set. This allows me to compare actor behavior and patterns of military-strategic updating across different conflicts in different countries and time periods, allowing me to test my hypotheses at a larger scale and make more generalizable claims about conflict updating.

Unfortunately, there does not (yet) exist a cross-national data set of any usable size that includes data on the type, target, and location of violent events in civil conflict hence the need to construct novel data sets for analysis in the previous two chapters. Three large, well-known data sets exist that capture cross-national conflict events in detail: Uppsala University's Georeferenced Event Dataset or GED [Sundberg and Melander, 2013]; the Armed Conflict Location Event Dataset or ACLED [Raleigh et al., 2010], hosted at the University of Sussex; and the Integrated Crisis Event Warning System or ICEWS [Ward et al., 2012] and Phoenix [Pho, 2016 (accessed March 12, 2015) automated event-coding programs, sponsored by the Lockheed Martin Corporation and the Open Event Data Alliance, respectively. I treat these two data sets as combinable and interchangeable because both initiatives run on similar auto-coding programming platforms, and can be combined to create one ICEWS/Phoenix data set.

All three of these data sources capture large numbers of conflict events across multiple conflicts. ACLED records both fatal and non-fatal conflict interactions for some 25 African states between 1997 and 2015. UCDP-GED contains observations violence for a greater spatial and temporal range (Africa, the Middle East, and Asia between 1989 and 2014, and Europe from 2005 through 2014) but only records events of violence in which at least one person died. ICEWS (which is publically available from 1995 through mid-2014) and Phoenix (which is available from 2014 through the current day, as of May 2016) together boast the greatest range of events, covering not just conflict violence but a wide variety of

other events, on a global scale. However, these coding platforms trade this massive range for lower-level coding accuracy and source saturation, meaning that mis-coded events are relatively common and the number of events surrounding a specific event (such as a conflict in a small country) may be very low.

In addition, these data sources differ in the type of information they record about a given event. ACLED and GED both include georeferencing information about individual events, usually to the scale of the city, town, or village in which violence occurred. ICEWS/Phoenix do not record this information, only coding the date of a given event. However, ICEWS/Phoenix do attempt to code the technology and type of violence: this allows the user to distinguish between (for example) a small-scale firefight and a tank battle, or between an air strike and an artillery strike on a town. UCDP-GED does not record this information, noting only that a violent event occurred between two or more actors, although for some events, small vignettes are available describing the context of violence. ACLED falls somewhere in the middle: while it does not include explicit coding for the type of event or the technology used, it does differentiate between ‘battles’ (direct, mutual violence) and ‘remote violence’ which includes bombings, IEDs, missile and artillery strikes, and so on. While this category is not as clearly defined or recorded as the ICEWS/Phoenix data, it does provide useful information that GED does not contain.

All three data sets record the actors involved, allowing the user to distinguish between military and civilian targets of violence, but ICEWS/Phoenix generally record actors as very broad categories (for example, ‘rebels’ versus ‘civilians’) while ACLED and UCDP-GED both attempt to identify the specific groups involved in violence. Finally, ACLED and UCDP-GED record the outcome of violence in terms of casualties (0 or more in the case of ACLED, and 1 or more in the case of GED, as it only records fatal events), while ICEWS/Phoenix do not. However, UCDP-GED contains much more detailed information about casualties, not only differentiating between the various actors receiving casualties (state, insurgent, civilian) but also including various statistics reflecting the level of uncertainty about casualty estimates where multiple, conflicting reports are available. ACLED, by contrast, only includes the aggregate number of casualties resulting from a

given event, which is not particularly useful for my purposes here.

This frustratingly imperfect data-coverage overlap can be summed up in the table below. No large, cross-national event data set contains sufficient information to record and analyze all three axes of violence I use to measure military strategy. The question, therefore, is which of these imperfect data sets is relatively better suited to testing my hypotheses as well as possible.

| Data Source            | ACLED | GED | ICEWS/Phoenix |
|------------------------|-------|-----|---------------|
| Type of violence/event | No*   | No  | Yes           |
| Target of violence     | Yes   | Yes | Yes*          |
| Location of violence   | Yes   | Yes | No            |
| Outcome/Casualties     | No*   | Yes | No            |

I choose not to use the ICEWS/Phoenix data for two reasons. First, while these programs together boast the greatest overall event density (millions of events recorded over several decades of coverage), their coverage of civil conflict is less complete than either ACLED or GED. An all-purpose event tracking platform exchanges substantive focus for broad coverage by design: for ICEWS/Phoenix, this poses two problems. First, ICEWS/Phoenix rely solely on news reports, mostly from global wire services like Agence France-Presse and Reuters. This means that these data face potentially severe issues of reporting bias: if an incident is not reported in the (international, English-language) news, it does not appear in the dataset. This is a common criticism of media-based, cross-national event data, particularly when covering conflicts in developing nations, because in these cases the only sources of information are often international news agencies who tend to focus their reporting efforts in areas with high economic and demographic significance - usually the capital city. The result is that systematic bias against reporting events that occur in far-flung or low-populations areas of the state, and the result can be a distorted view of ‘what was happening, where’.

This issue of coverage bias is well-known in event data analysis [Weidmann, 2016], but it is particularly worrying for ICEWS/Phoenix because these programs rely only on media reports. This means that anything that is not recorded in the (international, English-language) news media does not make its way into the data set. By contrast, both UCDP-GED and ACLED draw on a wide range of sources including news media, NGO reports, historical works, and local government sources. Because UCDP-GED and ACLED are designed specifically to be conflict-tracking tools, they have much deeper source saturation and better coverage when examining wartime violence. ICEWS/Phoenix's coverage of these same conflict episodes is likely to be (1) more sparse overall and (2) much more vulnerable to geographic bias in terms of the type of events that are recorded. As such, while these data programs are useful for answering many other questions, they are not useful for my purposes here. For this reason, I decide against using the large, promising, but ultimately non-optimal ICEWS/Phoenix data sets for this research design.

This leaves the UCDP-GED and ACLED data sets, both of which set out to do fundamentally similar tasks. Out of these two programs, ACLED is a logical choice to use for this analysis for three reasons. First, ACLED has broader event coverage than UCDP-GED. One of ACLED's major strengths is that it includes both fatal and non-fatal incidents of violence. This means that a firefight or bombing incident that does not result in casualties will still be recorded in the data set. This is important because, particularly in low-level conflict, many violent interactions do not result in casualties. However, these interactions may be still important they represent decisions about resource allocation and risk acceptance on behalf of the actors involved, and as such contain useful information about the military strategies those actors were using. UCDP-GED, on the other hand, only records incidents in which at least one combat-related fatality occurs. While this is useful information, it is more helpful to have both fatal and non-fatal events.

Second, ACLED includes some limited data on the type or technology of violence used, while GED does not. While it is impossible to differentiate between specific types of tactics or technologies employed by conflict actors, the presence of a 'remote violence' category does provide some useful information in differentiating the general type of violence being

used. This allows me to attempt to measure all three axes of violence that I use to measure military strategy, which is very useful.

Third, and more importantly, UCDP-GED does not code which party initiated a given violent interaction. In the case of civil conflicts, UCDP-GED does not differentiate between initiator and target parties, instead always coding the state military (where applicable) as the first actor. It is worth noting that UCDP-GED does have small vignettes or descriptive notes attached to many event records, which may contain contextual information about which party is the most likely initiator; however, these are not universally informative (many events have no meaningful information attached) and since these records number in the tens of thousands, it is infeasible to look up each original data source and hand-code information about the initiating party and the type of violence.

Information on initiator and target parties is crucial to testing my hypotheses, however. Without this information, there is no way to analyze the type of violence each conflict actor focuses on in their military strategy, as it is impossible to tell which actor chose to initiate conflict at that time and place. ACLED, however, codes initiator and target actor wherever possible, making it very useful for my purposes. This information is key to separating out streams of actions made by states and insurgents, and as such, makes ACLED a significantly better choice. I argue that even given the greater spatial and temporal coverage provided by UCDP-GED, as well as the finer-grained data on casualty outcomes, ACLED is still a more appropriate choice: the combined factors of greater event coverage and initiator-target differentiation make ACLED a more suitable data set for a cross-national, conflict-level analysis of military-strategic updating.

The second data challenge in this analysis is identifying conflict episodes. The UCDP-PRIOR identifies armed conflicts as occurring between two primary parties, at least one of which must be the government of a state [Sundberg and Melander, 2013]. While other actors can officially or unofficially support or enter into an alliance with either primary party, conflicts are assumed to be primarily dyadic, in which the state is engaged in military struggle with one primary opponent. To build the data set I use to describe conflict actor data and test my hypotheses, I merge ACLED and the UCDP Conflict

Termination data set to identify a set of conflict episodes, actor dyads, and termination types. A ‘conflict episode’ in this analysis is coded by UCDP-PRIO as a “continuous period of active conflict years” [Kreutz, 2010], in which at least 25 deaths attributed to conflict are recorded each year. An episode ends when fewer than 25 deaths are recorded that year. This can be for different reasons, including a lapse in levels of violence (but not a resolution of the underlying conflict) as well as conflict termination through victory/defeat, peace negotiations, or other mechanisms.

The UCDP Conflict Termination data set codes how a given conflict episode ends. This is a 6-category outcome describing a range of different ways in which a conflict episode can terminate. Conflict episodes can end through negotiations (peace agreement or ceasefire), victory by the state or insurgent group, an ‘unofficial’ drop or cessation in hostilities between the warring parties, or (rarely) when either state or insurgent cease to exist through reorganization, splintering, or combining with other politically relevant actors. Coding conflict termination is more complicated than might be thought.

Many conflicts do not end decisively for one side or another, but instead end with negotiated settlements or ceasefires. For the purposes of this analysis, I build a similar research design to Cunningham et al. [2009], who use the lack of government victory as the primary outcome of interest. This is a useful simplification because for state governments, whose primary task is the preservation of the geographic and political entity they embody, outcomes short of victory mean (at best) a weakening of the regime and (at worst) its actual destruction. Furthermore, under a purely capability-based theory of conflict outcomes, one would expect government forces to win nearly all of the time, because they are nearly always stronger from a material standpoint than their insurgent foes.

In line with Cunningham et al. [2009], I collapse these six categories into a binary measure denoting whether or not the government clearly achieved its desired outcome. As the empirical record shows, insurgent groups are generally significantly weaker from a capability standpoint than the state government: as such, I would expect the ‘default’ outcome of conflict to be in favor of the state. The binary measure I use collapses these six categories into ‘government outcome’ (government victory or the disappearance of an

insurgent actor) and ‘insurgent outcome’ (peace agreement, ceasefire, cessation of violence, or outright insurgent victory). Note that this second category is not intended to indicate ‘insurgent victory’. Instead, it simply indicates the absence of clear victory by the state, indicating that the insurgent was able to effectively survive for some period of time and achieve an outcome other than being defeated by the state.

The resulting data set measures conflict dynamics at the conflict-episode level. It encompasses all conflict episodes found in the UCDP-PRIOR data set that fit the following characteristics:

1. Conflicts coded as civil wars in which the state government engaged in military conflict with one or more insurgent groups;
2. Conflicts that occurred on the African continent (ACLED currently contains data only on African states);
3. Conflicts that have terminated at some point as coded in the UCDP Conflict Termination data set;
4. Conflicts in which ACLED data is available for both state-initiated and insurgent-initiated violence.

These data comprise 45 conflict episodes in 27 African countries over the 18-year period between 1997 and 2015<sup>1</sup>.

As in the previous two chapters, I look at temporally aggregated metrics of how a given actor is behaving along different axes of violence. The temporal unit of analysis here is the conflict-month. The month is a logical choice in this case because many of these

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<sup>1</sup>In building this data set, coding irregularities and data coverage issues in ACLED meant that two conflicts present in UCDP-PRIOR were not included in my analysis. For two civil conflicts in Angola (the long-running struggle for governmental power involving the UNITA insurgency, as well as a campaign for territorial autonomy or independence for the enclave of Cabinda) irregularities in ACLED’s data process have coding every recorded event as having been initiated by the state government. Without correctly coded indicators of event initiation, I cannot analyze changes in both actors’ behavior, and so these cases were dropped.

conflicts are (1) intense, (2) long-lasting, and (3) subject to issues of data scarcity. This means that to identify meaningful trends and patterns in actor behavior, I need a level of temporal aggregation that is small enough to capture meaningful variation, but is large enough to encompass a large enough number of events to give me confidence that I am truly capturing the true pattern of behavior over time.

The month makes sense when doing a cross-national survey of conflicts in part because ACLED's large scope often means that its coverage of events in an individual conflict can be sparse. By contrast, the Israeli/Palestinian data I analyze in Chapter 2 contain over 15,000 unique interactions over a roughly 4-year period; while ACLED contains a larger number of total interactions in a data set that encompasses that includes 21 different conflicts, many of which were full-blown civil wars, this contrast indicates that ACLED's coverage of conflict dynamics is still more sparse than country- or conflict-specific data sets such as the data I gathered on Israel/Palestine. As such, a fine-grained measure like the weekly unit of aggregation is likely too small to pick up trends in these data. By contrast, the Northern Ireland data set, which I analyze at the three-month level of aggregation, contains roughly 3,000 unique (fatal) interactions over a 30-year period. ACLED's density of events falls somewhere in between these two data sets, and as such, I select a level of temporal aggregation that is in the middle as well.

In each conflict episode, I code each event as being initiated by either the state or the insurgent, where the ‘rebel’ actor combines the actions of all major, aligned insurgent groups involved in a given conflict. This is a bit of a challenge, as ACLED and UCDP-PRIOR use very different actor coding protocols. UCDP-PRIOR gives a list of one or more (usually fewer than four) insurgent actors denoting the primary allied groups engaged in conflict with the government [Cunningham et al., 2013]. ACLED takes a much more open-ended approach to coding the actors involved in conflict, including violent events involving splinter rebel groups, alternative names for the same rebel organization, different categories of civilians (for example, ‘rioters’ versus ‘protestors’), ethnic militias and self-defense forces, and many events simply attributed to ‘unidentified armed groups’. Likewise, state actors can include different military organizations within the same country (for example, army

versus presidential guard), police, anti-terrorism units, prison guards, and so on.

In the previous two chapters, I have made the somewhat heroic assumption that all insurgent groups could be treated as one aggregate actor. Here, this is simply unfeasible due to two factors. First, ACLED codes such a wide range of conflict actors that to combine them all under the umbrella of ‘rebel’ would be an impossible assumption choice to defend. Instead, I set out to identify which of ACLED’s insurgent actors could be coded as being ‘on the same side’, whether this took the form of a formal alliance or simply de facto conflict partners. To do this, I manually identified the major insurgent actors in each conflict, then subsetted ACLED’s actor set to only include insurgent groups which were (1) formally allied, (2) de facto allied, or (3) alternate names for the same group.

For example, the 2005-2010 Chadian Civil War is coded by UCDP-PRIOR as involving six major insurgent groups: the Unity Front for Democratic Change, (FUCD), the Rally of Democratic Forces (RAF), the Union of Forces for Democracy and Development (UFDD), the Union of Forces for Resistance (UFR), the National Alliance of Chad (ANC) and the PNFR, a group with no identifying information either in the UCDP-PRIOR conflict data set or in UCDP’s own conflict-actor identification data. These six groups worked together formally and informally, and as such could be combined into one ‘insurgent’ actor in my time-series analysis. However, ACLED’s actor data set codes over 60 unique actors in the same time period, no less than 50 of which are rebel groups and many of which lack identification beyond ‘[group] ethnic militia’ or ‘unidentified armed group’.

To deal with this, I gathered information on unique, named conflict actors in ACLED. For example, FUCD (the primary rebel actor in the conflict) appears in ACLED as both ‘FUCD’ and ‘UFDC’, due to translation issues from the French. In addition, FUCD itself was a conglomeration of several other insurgent groups, some of which such as the Foundation for Change, National Unity and Democracy (SCUD) are listed in ACLED as unique actors themselves. In cases like this, I include both alternate names for the same group (FUCD/UFDC) and subsidiary groups like SCUD as being part of the same aggregated insurgent actor.

Along with this manual merging of named conflict actors, I also employed two broad

rules governing non-named conflict actors. First, I do not include any events initiated by ethnic militias. Ethnic militias appear commonly in ACLED as both initiators and targets of violence, often between one another. Because they are identified specifically as representing the interests of a given ethnic group, rather than a political or religious cause linked to the broader civil conflict, I assume that they are not acting in conjunction with either the state or the insurgent, but either out of self-defense or in smaller-scale internecine conflict with other militias or groups. As such, it does not make sense to tie these groups in with either the state or insurgent. Second, I code ‘unidentified armed groups’ as being affiliated with the insurgent actor, as this is the best (if imperfect) estimate of their affiliation. These groups are not affiliated with the state government, and as such, it is a reasonable assumption that they are affiliated with the insurgent movement but were not identified as such by the reporting agency responsible for describing the event in question.

After this manual merging, the ‘insurgent’ actor in this data set represents the combined actions of any and all relevant, allied armed groups that (1) appear in the ACLED data set and (2) engage in conflict with the government. This allows me to capture as many relevant conflict events as possible, while avoiding data reliability issues brought on by simply assuming any action taken by a non-government actor is part of the same unified insurgent movement. The result, as in the previous two chapters, is a set of time-series data describing the actions of the state and the insurgent for each conflict episode. In each month, I calculate the location of violence (where each actor initiates violent interactions), the type of violence (the employment of direct or indirect weapons and technologies) and the target of violence (whether violence is directed at a military or civilian target).

To calculate the location of violence, I combine ACLED’s geocoding with CIESIN’s Global Rural-Urban Map of Population (GRUMP) v1.0 [CIESIN, 2005]. GRUMP is a grid-based measure that codes global satellite data to determine whether a given area is primarily rural or urban in nature. Because the events in UCDP-GED are geocoded, usually down to the village or region, this allows me to determine with reasonable accuracy how many of an actor’s initiated events take place in cities or large towns, as opposed to open ground or small villages. This coding allows me to calculate whether, in a given time

period, an actor relies primarily on urban or rural violence to achieve its military and political goals.

To calculate the target of violence, I take the percentage of an actor's initiated violent actions that involve military versus civilian targets. ACLED codes the target of violence as well as the initiator, which lets me determine the extent to which an actor's strategy relies primarily on targeting civilians or military personnel for violence. To calculate the type of violence, I take the percentage of an actor's initiated violent actions that fit what ACLED codes as 'remote violence'. For remote violence, ACLED notes that 'the main characteristic of an event is that a spatially removed group determines the time, place and victims of the attack' [ACL, p.10]. This can include remote-fire or time-activated weapons such as mortars, IEDs, and artillery, as well as 'stand-off' weapons such as aerial bombs or missiles.

To calculate changes in strategy, I rely on changepoint analysis, using the same methodological framework described in Chapters 2 and 3. This nonparametric approach allows me to 'organically' detect periods in which an actor's behavior, as measured by the location, type, and/or target of violence it engages in from month to month, experiences a statistically significant change. This allows me to identify periods in which some stimulus, either internal or external, leads an actor to implement some notable shift in the way it fights. Because I can calculate these changepoints for both states and rebels, I can track the pace at which both actors update, the timing of these updates relative to one another, and the overall number of changes that occur during the course of the conflict. These changepoints are the fundamental inputs I use to test my hypotheses.

### 4.3 Results and Discussion

To test Hypotheses 1-3, I examine the behavior of conflict actors and how it relates to the outcome of a given episode of conflict. In each of the 45 conflict episodes for which data is available, I look at the behavior of insurgents and state military forces from month to month along the axes of type, target, and location of violence.

For Hypothesis 1, my independent variable of interest is the mean time elapsed between a change in the rebel's (state's) strategy and a corresponding change in the strategy of the

state (rebel). I expect that conflict actors that update their strategy rapidly following a change in their opponent's behavior are displaying the ability to adapt to changes in the battlefield environment. The outcome of interest in this hypothesis is the termination of the conflict, as measured by the UCDP's conflict termination dataset. I anticipate that conflicts in which the insurgent displays faster updating behavior than the state, conflicts will end with a better outcome for the insurgent group than conflicts in which insurgents are slower or at parity with state updating speed.

Table 4.2 shows the cross-tabulation of insurgent updating and conflict outcome. Due to the very low number of observations available in this data set, I simplify this analysis by binarizing the insurgent updating variable: rather than measuring the overall speed with which the insurgent updates its strategy during conflict, I simply indicate whether the insurgent, on average, was faster in responding to changes in military strategy by the state than the state was to changes by the insurgent. In conflict episodes where the insurgent group can update its strategy more quickly than the state, I expect the insurgent to be more likely to avoid defeat.

Table 4.2: Cross-tabulating conflict outcomes and rebel updating

| Conflict Outcome      | State success | Rebel success | Total    |
|-----------------------|---------------|---------------|----------|
| Faster rebel updating | 6             | 4             | 10 (22%) |
| Faster state updating | 25            | 10            | 35 (78%) |
| Total                 | 31 (69%)      | 14 (31%)      | 45       |

Table 4.2 shows some interesting patterns. In cases where the rebel actor was faster at updating than the state, it avoided defeat 4 out of 10 times (41.67%). In cases where rebels were slower updaters relative to the state, they avoided defeat 9 of 24 times (37.5%), a difference of 4.17%. This seems to provide some indication that faster-updating rebels are likely to fight more effectively and avoid being defeated by the state. However, given the small sample size it is hard to say with confidence that the proportions of rebel successes under these different conditions truly exist. To better ascertain whether the mean conflict outcome correlates with faster military-strategic updating by rebels, I conduct a  $\chi^2$  test

for difference of proportions, summarized in Table 4.3.

Table 4.3:  $\chi^2$  test for difference in proportion

| Condition             | State victory | Rebel victory | <i>p</i> -value |
|-----------------------|---------------|---------------|-----------------|
| Faster rebel updating | 19%           | 29%           | 0.76            |

Testing this relationship shows that the difference in proportion is not statistically significant at conventional levels ( $p = 0.76$ ). Given the small sample size available, gathering more data is likely more necessary to identify whether or not a real difference exists. Overall, this does not support Hypothesis 1. The speed at which rebel forces update their military strategy relative to the state may have some effect on conflict outcome, but with these data it is impossible to determine this with statistical significance.

For Hypothesis 2, my independent variable is the same — relative speed of military-strategic updating — but my outcome is the length of a conflict episode, measured in months. This data is also drawn from the UCDP-PRIOR conflict database, which records the date to the month in which a military conflict comes to an end. Again, I expect that conflicts in which insurgents can update their strategy more quickly than the state are likely to last longer than conflicts in which insurgents are slower or at parity with state updating speed.

Here, I use the same binarized indicator of insurgent updating speed, measuring whether or not the insurgent (on average) updates its military strategy more quickly than the state. Again, under the constraints of low sample size, I am prevented from using more complex statistical instruments, so I rely on a *t*-test to identify whether conflict episodes in which insurgents update quickly tend to last longer than conflicts in which the state is a more effective updater. Table 4.4 shows the results of this test of significance.

Table 4.4: Sample *t*-test for difference of means: conflict duration

|                        | Faster Rebels | Faster State | <i>p</i> -value |
|------------------------|---------------|--------------|-----------------|
| Mean duration (months) | 69.5          | 28.0         | 0.10            |

This *t*-test shows some support for Hypothesis 2. Conflicts in which the insurgent is

more responsive in military-strategic updating than the state tend to last an average of 69.5 months, or about four and one-half years. Conflicts in which the state is the more responsive updater, on the other hand, last a mean of 28 months, or just over two years. This difference is quite sizable (over three years' difference) and significant at the  $p < 0.10$  level.

Given that these data are time-variant, a more appropriate test of Hypothesis 2 is a duration model. While sample size is still a concern for the robustness of any findings, given that my key covariates are dichotomous variables, a test with moderate power is still possible. My outcome is the estimated time at which a conflict episode will terminate. Another way of formulating this outcome of interest is the probability of termination, or hazard rate, at any given point in time. To estimate this probability, I employ a Cox proportional hazards duration model [Box-Steffensmeier and Jones, 2004]. This model estimates the hazard rate at any given time period: that is, the relative likelihood that an event will occur, in this case the end of a conflict episode. The model I employ predicts conflict termination using the binary indicator of rebel military-strategic updating, effectively splitting the sample of conflict episodes into two subgroups. Because these conflict episodes occur in the context of larger or longer-lasting conflicts, I implement robust standard errors by clustering observations along overall conflict ID.

Table 4.5: Rebel updating and conflict episode termination

|                       | Conflict episode<br>termination<br>(1) |
|-----------------------|--|
| Faster rebel updating | 0.41**                                 |
| Observations          | 45                                     |
| Adjusted $R^2$        | 0.13                                   |
| Log Likelihood        | −126.05                                |

*Note:* \* $p < 0.1$ ; \*\* $p < 0.05$ ; \*\*\* $p < 0.01$   
Coefficients presented as hazard ratios.

Table 4.5 shows the results of the Cox model. This further bolsters the initial findings from the t-test: conflicts where rebels are faster at updating their military strategy tend to last significantly longer than conflicts where states are faster updaters ( $p < 0.05$ ). While I emphasize yet again that sample-size issues limit the weight we can place on these findings, both a simple t-test and a more appropriate duration model provide evidence that military-strategic updating matters, at least as far as conflict duration is concerned.

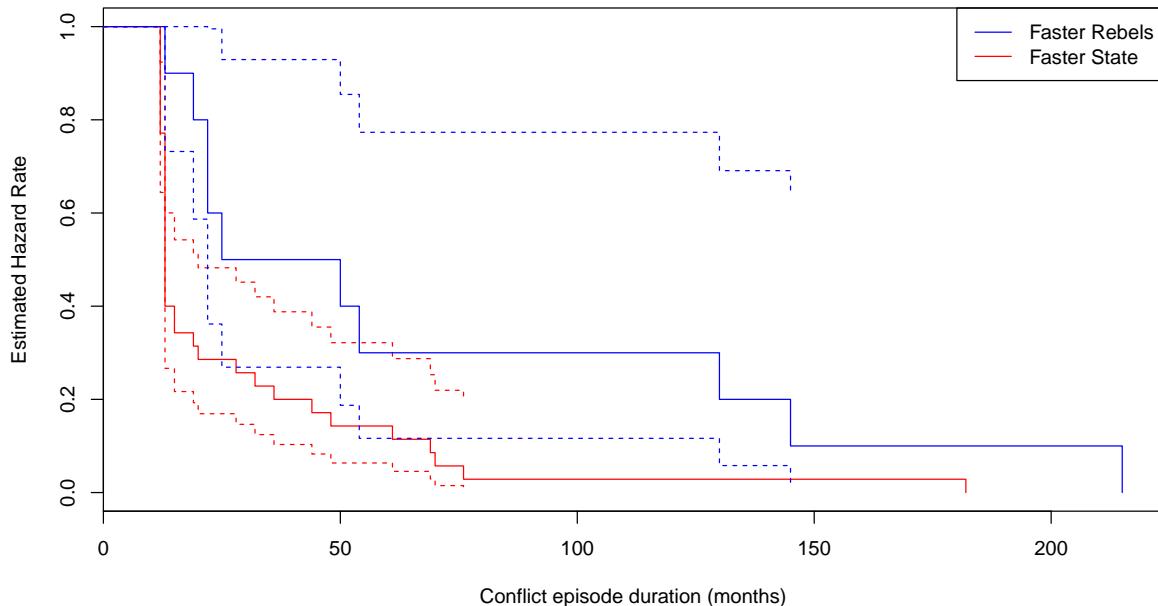


Figure 4.1: Estimated survival, by rebel updating speed

Figure 4.1 visualizes the estimated hazard rates over time based on the indicator of superior rebel updating. The median duration of conflicts with fast-updating rebels is 44 months, compared to 13 months for conflicts where rebels are slower relative to the government. The Cox model shows a positive and significant ( $p < 0.05$ ) effect for conflicts where rebels are faster military-strategic updaters relative to the government. One important note when employing the Cox model is the strong assumption that proportional hazards are constant – that is, that the effects of independent variables are proportional regardless of the point in time. Testing the PH assumption on scaled Schoenfeld residuals shows no evidence that the effects of rebel updating speed vary over time ( $p = 0.19$ ).

The findings from Hypotheses 1 and 2 together provide some evidence for the importance of rapid military adaptation. Insurgent groups have to be responsive to changing battlefield conditions in order to survive. If they operate at a strategic disadvantage relative to the state government, they are likely to be destroyed by a materially stronger opponent which is fighting on its own terms. Groups that can fight at a strategic advantage more of the time by updating their own strategy quickly in response to changes in government strategy are likely to do better in conflict, at least in the short run. However, the ability to quickly adapt to changes in opponent behavior does not guarantee victory.

In the short run (month to month or year to year) effective updating may help insurgents stay alive and avoid destruction. In the long run, it is not clear that effective military-strategic updating necessarily leads to a better outcome: even fast battlefield updaters may simply be delaying the inevitable in some cases. Consider again the example of the UNITA rebel movement and its struggle against the Angolan state: significant military-strategic changes in the late 1990s and early 2000s did help UNITA survive for several years in the face of a much more powerful state government – but it was not enough to bring about military victory, or in the long run, even to avoid defeat. This example illustrates the findings in Hypotheses 1 and 2: effective military-strategic updating may help an insurgent group extend its lifespan, but short-term survival and long-term victory are very different and even effective updaters may be defeated by a stronger state.

Finally, for Hypothesis 3 my independent variable is the elapsed time since a group most recently changed its military strategy. Here, I again use a Cox proportional hazard model with conflict-level clustering to analyze whether conflict episodes are likely to end soon after one group updates its strategy, or if the chances of a conflict episode ending increase with the length of time a group goes without updating. This model implements a gap-time approach, in which the hazard rate is re-estimated each interval as values of covariates change over time. Table 4.6 shows the results of this model.

Model 2 attempts to predict conflict episode termination as a function of elapsed time since the rebel's and government's most recent military-strategic shift. Implementing the simplest model – assuming there is no relationship between state and rebel updating

Table 4.6: Conflict episode duration as a function of elapsed time since updating

|                         | Conflict episode<br>termination<br>(2) |
|-------------------------|--|
| Time since state change | 1.01                                   |
| Time since rebel change | 0.99                                   |
| Observations            | 1,675                                  |
| R <sup>2</sup>          | 0.0003                                 |

*Note:*

\*p&lt;0.1; \*\*p&lt;0.05; \*\*\*p&lt;0.01

Relationships presented as hazard ratios.

behavior – I find no statistically significant relationship between the elapsed time since an actor's most recent military-strategic shift, and the end of a conflict episode. Under this model, major military-strategic shifts by either rebels or governments (or the lack thereof) do not seem to signal an impending end to a conflict, nor do they significantly extend the duration of conflict.

However, my theory clearly states that rebel and government updating are not independent of one another. If both sides are in fact engaged in a struggle to effectively identify and adapt to one another's behavior, then the dynamics of conflict should be measured as a function of both actors' choices taken in conjunction with one another. As such, I include the interaction between both sides' elapsed time since most recent update in Model 3, presented in Table 4.7. I anticipate that the relationship between rebel or government military-strategic shifting and conflict episode termination will vary based on the actions of the other party.

Table 4.7 shows some interesting patterns. First, at least when taken in isolation, the timing of both rebel and government military-strategic shifts are significantly associated with the termination of a conflict episode. The greater the elapsed time since a shift in rebel strategy ( $p < 0.01$ ) or government strategy ( $p < 0.10$ ), the higher the chances of the current conflict episode terminating. However, given that this is an interactive model, both these variables must be interpreted in light of the significant and negative interaction term

Table 4.7: Rebel updating and conflict episode duration

|                         | Conflict episode<br>duration<br>(2) |
|-------------------------|-------------------------------------|
| Time since state change | 1.01*                               |
| Time since rebel change | 1.05***                             |
| Rebel change time *     | 0.995***                            |
| State change time       |                                     |
| Observations            | 1,675                               |
| Adjusted $R^2$          | 0.005                               |

*Note:*

\* $p < 0.1$ ; \*\* $p < 0.05$ ; \*\*\* $p < 0.01$   
 Coefficients presented as hazard ratios.

( $p < 0.01$ ). The significance of the interaction term in this model leads me to conclude that when considering the relationship between one group's updating behavior and conflict episode termination, the other group's behavior must be taken into account.

To better understand the relationship between these variables in light of the interaction term, I visualize the marginal effects of the 'time since rebel update' covariate on the 'time since state update' covariate. Figure 4.2 shows the marginal effects of time elapsed since the most recent rebel strategic change at different simulated lags of most recent state strategic change.

As Figure 4.2 shows, a conflict episode is relatively more likely to end when both parties have recently updated their strategy. A conflict episode is less likely to end, however if both parties have gone a long time without updating their strategies. Interestingly, the hazard ratio is virtually identical when the state has not updated its strategy recently, but the insurgent has. In other words, once the interaction is taken into account, the effect of rebel updating time is driven largely by the time elapsed since the last military-strategic update by the state. The likelihood of a conflict episode terminating increases with the elapsed time since the most recent change in rebel military strategy when the state has recently updated its military strategy. However, when moderate time (roughly 6-12 months) has elapsed since the state has updated its military strategy, this relationship loses statistical

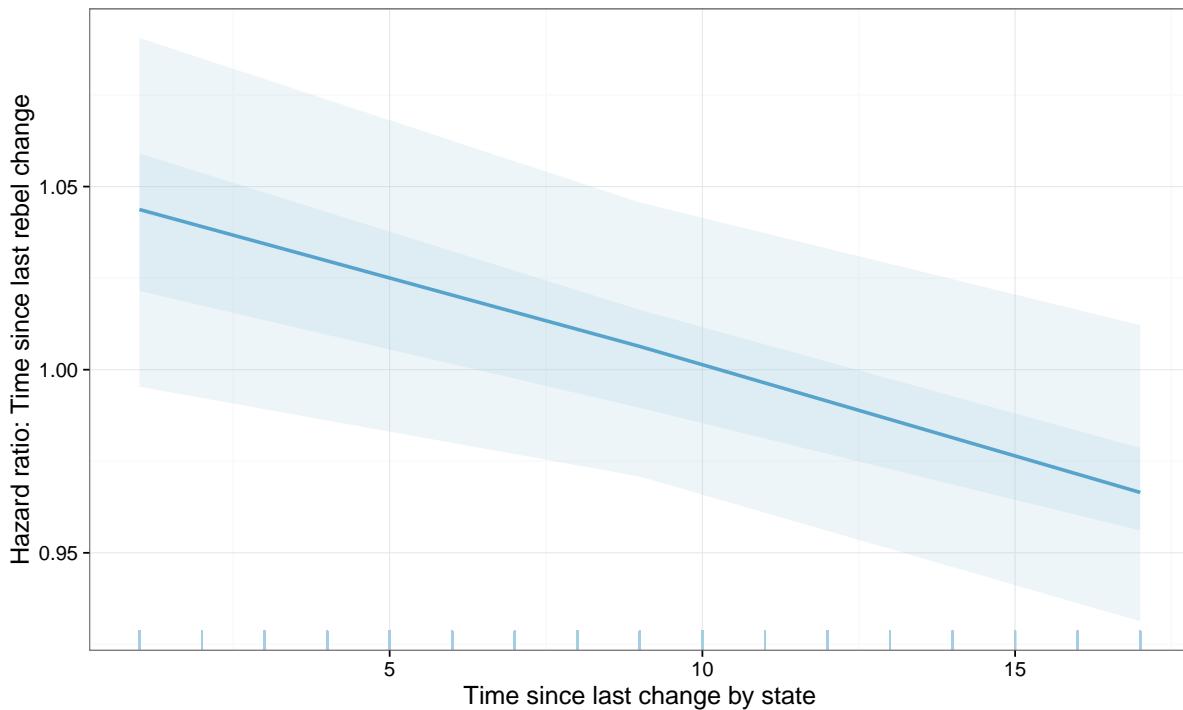


Figure 4.2: Marginal effects: time elapsed since most recent rebel military update

significance. As this time increases, the relationship actually changes sign and becomes significant again: now, the likelihood of conflict termination actually decreases the more time has elapsed since the most recent rebel strategic shift.

Substantively, this relationship makes sense with a little parsing. When the rebels are slow to update and the state is relatively faster (more time elapsed since rebel change, less time elapsed since state change), conflicts are more likely to terminate. Weaker rebels that are adept military-strategic updaters tend to survive longer, as they fight more effectively against the state. This is supported by the fact that when states are slow to update (more time elapsed since most recent change) conflicts tend to last longer – the rate of rebel military-strategic updating becomes less relevant, as strategic updating is less necessary in the short run.

It appears that in this model, the likelihood of a conflict episode terminating in a given month is largely driven by the elapsed time since the government's most recent strategic change. The longer it has been since the government has updated its military strategy, the greater the relative chances of the current conflict episode coming to an end. Elapsed

time since a rebel change, however, has a minimal effect on conflict termination likelihood once state updating time is taken into account.

This builds on the findings discussed previously. Testing Hypothesis 3, I find that in general, insurgents that can respond rapidly to state military-strategic shifts tend to survive longer, fighting wars that last longer than those involving slow updaters. Model 3 finds that conflict episodes are less likely to terminate in the months immediately following a shift in rebel strategy that is not matched by a shift in state strategy. These findings reinforce each other, painting a picture of conflict in which insurgents that can rapidly update their military strategy can stay one step ahead of a stronger state: by shifting the type, target, and location of violence, rebels can (at worst) avoid defeat and buy time, and (at best) achieve a military advantage over their opponent.

# Chapter 5

## Conclusions and Future Directions

In this final chapter, I summarize the findings (or lack thereof) from the previous three analyses, discuss potential factors that may lead to the pattern of non-significant relationships I identify, and set out a program for future refinement of this research design.

### 5.1 Short-term patterns in strategic updating

#### 5.1.1 Action and reaction during civil conflict

The analyses laid out in Chapters 2 and 3 are striking in their near-universal lack of significant findings in the directions I anticipate. While the qualitative evidence strongly suggests that both states and insurgents respond to changes in the conflict environment and update their military strategy over time, I find little or no evidence in these analyses that this change is incremental in the short run.

In the case of Israel and Palestine, I find little evidence that either side adapts their military strategy from week to week in response to changes in either the casualty ratio of the conflict or changes in their opponents' military strategy, at least not in any systemically identifiable way. There is limited evidence that a decrease in Palestinian activity is linked to an increase in IDF use of light arms, indicating a limited system of mutual de-escalation. As the rate of Palestinian violence slows, the response by the IDF is more selective and smaller-scale, rather than relying on heavy weaponry or combined-arms operations in the Palestinian territories. This pattern is also logical given the media-saturated environment of the Second Intifada. Under intense domestic and international scrutiny, the IDF may

desire to use ‘minimal sufficient force’ when possible; alternatively, this may indicate that the tendency to use heavy weaponry is checked by the necessity politically justifying large-scale violence.

In the case of Northern Ireland, I find a similar lack of relationship between Republican, Unionist, and British Army actions over time. I do find some limited evidence for mutual responsiveness between Republican and Unionist paramilitary violence: for example, there is a small but significant link between each side’s focus on attacking targets in the major cities of Belfast and Derry. These were both high-visibility, target-rich environments, and this finding is in line with the qualitative and historic literature on the Troubles that describes the ongoing pattern of retaliatory violence between Republican and Unionist groups in these cities. I also find a tenuous relationship between the overall level of fatal Republican violence and the level of fatal attacks by British peacekeepers: as in the case of Israel and Palestine, this points to a state desire to engage in (roughly) proportional violence. It appears that British forces were given more leeway (or more direct orders) to engage in ‘kill’ missions versus ‘capture’ missions in periods when Republican fatal violence was increasing.

One of the clearest findings in both cases (although not one I set out explicitly to uncover) is the variability over time in insurgent versus state activity. In the time-series models I use, both the Israeli and British military forces show significant autocorrelation in their military behavior. The type of violence used, the targets selected for violence, and the location at which violence was deployed were relatively ‘sticky’ over time, meaning that the previous time period’s activities could be used to predict the current period’s activities.

For insurgents in both conflicts, this was not the case. Both groups show high variation in the type, target, and location of violence from time point to time point. As I discuss in both chapters, this makes sense given the tactics of terrorism that insurgents in the Palestinian Territories and Northern Ireland employed. Outnumbered and outgunned, these groups relied on unpredictability to survive in the short run: if their previous actions could be used to forecast future actions, they could be identified and destroyed. This

may be one reason why I find so few short-term relationships between state and insurgent behavior in these cases: if the ‘meta-strategy’ employed by insurgents relies heavily on varying the type, target, and location of violence over time, it may be difficult to parse out any systematic changes in insurgent decision-making, at least in the short run.

Another potential reason I do not find incremental, short-term updating behavior may be that this is simply not how the updating process works. In Chapters 2 and 3, I also present the results of change-point analyses designed to empirically identify significant shifts in the patterns of insurgent and state behavior. These analyses suggest that military-strategic change does exist, and that the breakpoints identified by the algorithms I use often correlate with time periods identified in the qualitative literature on conflict.

For example, while I do not find support for the idea that the IDF responds to short-term trends in Palestinian insurgent violence, change-point analysis identifies a major shift in Israeli use of heavy arms in the same week that Mahmoud Abbas was elected President. This is particularly interesting because Abbas not only campaigned domestically on a pro-peace platform, but was also explicitly backed by the Israeli state as the best choice for Palestinian leadership. It is not a tremendous stretch to link this change in the IDF’s military strategy to the desire to support a pro-peace Palestinian politician: continuing major armored incursions into the Palestinian Territories would undermine Abbas’ regime and further weaken the internal Palestinian peace process.

One avenue for future research would be to treat these empirically-identified change points as outcomes for prediction, attempting to identify time periods when a significant and lasting shift in military strategy is more likely, as opposed to a short-term incremental change in behavior.

### **5.1.2 Endogeneity of strategy and indicators**

In both cases, I also do not find the expected relationships between casualty ratios and conflict behavior. Upon further reflection, it is likely that this is because casualty ratios are not always an appropriate indicator of success or failure in conflict. In Chapters 2 and 3, I make several assumptions about military strategy. One such assumption is that military success is always linked to the ratio of friendly to enemy casualties. Actors that

observe a high K/D ratio are succeeding; those that observe a low K/D ratio are failing. However, in Chapters 2 and 3 I fail to find systematic evidence suggesting that this is the case. The ratio of friendly to enemy casualties does not have an appreciable effect in the expected direction for either Palestinian or Northern Irish militants.

As a first step, this assumption is necessary. While I recognize that different military strategies have different short- and medium-term goals, the endogeneity problem arises when trying to use the same events data to elicit both strategy and goal. Holding one of these factors constant allows me to identify whether a single indicator of success (casualty ratios) has an impact on military strategy, and potentially (although I do not pursue the question here) what strategies are best suited to maximizing the ratio of enemy fatalities.

The issue in these chapters is that even though picking a single metric of strategic success may prove empirically useful, picking the ‘wrong’ metric for a given actor or a given strategy may muddy the analytic waters considerably. For example, it is likely that Palestinian insurgents are not as motivated by casualty *ratios* as they are by the overall level of casualties they are able to inflict on the IDF or on Israeli civilians. Since these groups often employ terrorism tactics, they may be most successful when carrying out large-scale, fatal attacks, regardless of the losses incurred while doing so.

Insurgents that face overwhelming military odds but can easily blend into the civilian population may in fact *prefer* a certain level of friendly casualties. Palestinian groups that carry out fatal attacks on Israeli targets fully expect a military response by the IDF. This military response often kills or injures civilians, but this does not necessarily mean Israeli success or Palestinian failure: in the medium-term, IDF violence is one of the most powerful recruiting tools available to Palestinian insurgent groups. As such, these groups may succeed when carrying out fatal attacks, and, as long as the core group of fighters can evade killing or capture, benefit when retaliatory violence causes additional suffering in the civilian population.

On the side of states, as well, merely looking at casualty ratios may be insufficient. States are definitely concerned with protecting the lives of their population from insurgent violence; allowing friendly casualties is a sign of state weakness and provides additional

fuel for anti-governmental mobilization. While friendly casualties may be a sign of failure, however, success may not be as simple as a body count. States seeking to quell domestic unrest — especially developed democracies like the UK and Israel — cannot simply engage in large-scale indiscriminate violence without suffering political costs.

If state violence against civilians provides insurgents with recruiting material, as it has in both the Second Intifada and the Troubles, then state success is linked to the ability to capture or kill insurgent fighters without collateral damage. As such, success for both Israeli and UK military forces may be the ratio of enemy combatants to civilians killed in state military operations.

In future work, I see two approaches to better identifying indicators of success or failure. One approach would be to relax the assumption I make in picking only casualty ratios as an indicator of success, and pick indicators inductively. I could select a range of potentially useful indicators, and observe which seem to correlate more strongly with an actor's likelihood of military-strategic change. This would allow me to link specific indicators to specific actors or strategies, and possibly identify a systematic relationship between certain types of military-strategic behavior and certain empirical indicators.

A second approach is deductive and better suited to the analysis of individual conflicts. This would involve combining quantitative event data with qualitative information on the nature of a given conflict and the actors involved. With better information about the material capabilities, area of operations, leadership structure, and political goals of a given actor, it would be possible to theorize a more useful metric of battlefield success or failure, and use this metric to predict the likelihood of military-strategic change at any given point in time.

## **5.2 Long-term patterns in strategic updating**

Turning to my second set of hypotheses, I do find some limited evidence that an actor's ability to quickly update its military strategy is linked to the duration and outcome of conflict, particularly for insurgent groups. While fast and effective updaters may not win more conflicts, they do seem to be better at staying alive: the ability to shift military strategy may make insurgent forces harder to detect, predict, and ultimately defeat by a

stronger but slower-moving government.

However, the robustness of these findings are limited in many ways. These data are limited in time and space, only covering African conflicts since 1997. More generally, any analysis of 45 observations should be accompanied by significant caveats about the robustness and generalizability of its findings. While the ACLED itself contains over 100,000 unique observations of conflict events, the subset of conflicts that meet my thresholds for analysis (event coverage, termination, time period, geographic location) is quite small. 45 conflict episodes across 27 conflicts is a useful subset for showing simple differences and trends, but will not provide the degrees of freedom necessary for a more involved multivariate analysis. As such, I view this analysis as a ‘first cut’ of sorts that suggests avenues for future data gathering as well as modeling.

The key challenge to these analysis simply the lack of suitable data available. In this chapter, I surveyed several cutting-edge event data generation programs (UCDP-GED, ICEWS/Phoenix, ACLED) and found that they were, at best, imperfectly suited for this type of analysis. While the generation of event data has been of terrific use to the study of conflict, these programs were designed to focus primarily on the ‘where and when’ of violence rather than the ‘what’ (the type of violence employed) or the ‘who’ (the initiator of a violent event). This is not an attack on the quality of these data programs, merely an observation on their suitability for this type of cross-national, temporal analysis of actor behavior.

A logical extension of these preliminary analyses is the generation of new data, or the recoding of existing event data sets to make them more suitable to spatial-temporal analyses of actor behavior. While this is not feasible in the scope of this dissertation, it is possible to do given existing data sets. In particular, additional coding of the information contained in the UCDP-GED event data set would make it possible to conduct this analysis on the GED data rather than the geographically and temporally more constrained ACLED data. UCDP-GED’s most recent iteration not only covers more space (Africa, Asia, Europe, and Latin America) but also more time (1989-2015 for Africa and Asia, 2004-2015 for Europe and Latin America). This would significantly increase the number

of conflicts available for analysis, both strengthening the robustness of my findings and allowing for more complex but more theoretically suitable statistical analysis.

Adapting the UCDP-GED for actor-based strategic analysis would be difficult but doable with time and research assistance. This is because the vast majority of UCDP-GED observations include a short vignette describing the event in question, and/or a hyperlink reference to the original data source. With these data, it would be possible to code the initiating and target actors involved in all 100,000+ observations in the UCDP-GED data set. With information on which actor initiated each event, I would be able to apply the coding scheme outlined in this chapter to UCDP-GED and extract patterns of behavior for both state and insurgent actors.

The next step in this research program is to use these preliminary findings as the basis for grant seeking. With resources to hire research staff, I would be able to gather enough data to double or triple the set of conflicts I analyze here. Specifically, re-analyzing and re-coding additional data from the raw documents used to create the UCDP-GED would allow me to add new variables to these existing data, which is a dramatically easier endeavor than recreating such a data set from scratch.

### 5.3 Conclusions

Does military strategy matter? The qualitative literature on military conflict would suggest that it does, but quantitative analyses of this question are limited in both theoretical and empirical scope. In this dissertation, I set out a theory of military-strategic updating claiming it is not just the choice of strategy that matters. Conflict actors have to pick effective strategies to achieve their military goals, but they also have to be able to update their military strategy over time. Actors that are unable to recognize the need for a new military strategy, or that are unable to quickly and effectively adapt to a changing battlefield environment, are likely to see less success in the short run, and are more likely to see a bad outcome to the conflict as a whole.

I test the short-run implications of this theory on novel event data from the Second Intifada in Israel and the Palestinian territories, and on the Troubles in Northern Ireland. I find little systematic evidence suggesting that insurgents and states engage in incremental,

short-term adaptation to each other's behavior. However, I find some evidence that military-strategic change may take the form of abrupt shifts in behavior, suggesting that this type of updating may be more akin to a threshold effect, in which sufficient information gathered over time eventually translates into a significant and long-lasting shift in actor behavior.

I test the long-run implications of this theory on a cross-national set of conflicts on the African continent. I find more support for my theory in this case, suggesting that actors capable of rapid response to shifts in their opponents' behavior are likely to survive longer, even though they may not be more likely to win outright.

I do not claim that the analyses presented here provide conclusive proof that military-strategic updating as I measure it affects the course of civil conflict. However, I argue that this analysis provides preliminary evidence that *something interesting is happening* during conflict that previous studies have failed to measure or account for. The military strategies used by actors do change over time, albeit not always in the ways I anticipate. Furthermore, the ability of these actors to engage in military-strategic updating does seem to have an impact on when and how conflicts end. While further research is definitely warranted to uncover more information about these patterns, this dissertation provides a theoretical and empirical foundation for future work in this area.

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