Assessing the Effectiveness of Leadership Decapitation in Counterinsurgency Campaigns

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Abstract

Is killing or capturing enemy leaders an effective military tactic? Previous research on interstate war and counterterrorism has suggested that targeting enemy leaders does not work. Drawing on newly collected data on counterinsurgency campaigns, new analysis on the effectiveness of leadership decapitation is presented in this paper. The results suggest that leadership decapitation is more effective than the conventional wisdom suggests. The paper contains three significant findings. First, campaigns are more likely to end quickly when counterinsurgents successfully target enemy leaders. Second, counterinsurgents who successfully capture or kill insurgent leaders are significantly more likely to defeat insurgencies than those who fail. Third, conflict intensity is more likely to decrease following successful leadership removals than after failed attempts. The implications of these results for academic research, military operations, and policy are explored in the conclusion.

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1 Introduction

Targeting insurgent and terrorist leaders is central to many states' counterterrorism policies, but does it work? Academic researchers have long been dismissive of leadership decapitation as a cause of military effectiveness. Although a growing body of international relations scholarship demonstrates the importance of leaders (e.g., Chiozza and Goemans 2004; Iqbal and Zorn 2008; Jones and Olken 2009; Cronin (2006)), social scientists have paid almost no attention to insurgent leaders in counterinsurgency campaigns. By focusing instead on structural variables that correlate with outcomes such as campaign success or levels of insurgent violence, schlolars have implicitly rejected the largely untested hypothesis that insurgent leaders can have a significant influence on key outcomes.

Most scholars who have written on leadership decapitation conclude that it is ineffective and that it can have counterproductive effects on military effectiveness (Pape 1996;2003; Hosmer 2001; Staniland 2005; Jordan 2009).[1] Drawing from his research on suicide terrorism, Robert Pape argues that leadership decapitation, "has met with meager success" (Pape (2003, 14)). Pape argues that leadership targeting is not likely to coerce adversaries because (1) it is hard to find individuals and kill them; (2) the death of leaders during war often brings less policy change than is expected; and (3) succession is unpredictable in many states, especially during war ((1996, 79-80)). Jenna Jordan (2009) extends these insights to the counterterrorism literature. Jordan, who recently published a leading study on leadership decapitation in counterterrorism in the journal *Security Studies*, argues that "going after the leader may strengthen a group's resolve, result in retaliatory attacks, increase public sympathy for the organization, or produce more lethal attacks." Jordan concludes that high-value targeting is "a misguided strategy" and that "we need to rethink current counterterrorism policies" (Jordan (2009,

[1] Pape (1996, 79-80) rejects Col. John Warden III's ((1992, 65)) assertion that leadership decapitation often has a decisive impact in coercive bombing campaigns.

753-755)).

This consensus is premature. Previous studies provide valuable theoretical insights that explain particular important historical cases, but our general knowledge about the effectiveness of leadership decapitation in war is limited due to a common set of methodological and conceptual challenges that include selection bias, limited empirical data, and insufficient attention to causal identification.[2] New analysis, with greater attention to research design, is needed to increase our understanding of the impact of leadership decapitation.[3] Such research is presented in this paper.

1.1 Identifying the Effects of Leadership Decapitation

My approach departs significantly from previous studies of counterinsurgency and counterterrorism. Whereas most previous studies are theory-driven and are focused on the fit of rival theoretical explanations to a small number of important historical cases, my approach is data-driven and is focused on identifying causal effects across large number of cases. I analyze a large number

[2]Recent studies have begun to account for these challenges, particularly in studies of how national leaders influence economic growth, democratization, and war. Jones and Olken ((2009), in their study of the assassination of national leaders, use a similar identification strategy to that used here, and find that changes in national leadership can increase the probability of war termination in high-intensity wars but can also increase the probability that new, low-intensity conflicts will break out. Price (2009) uses event history analysis to model how leadership decapitation affects the lifespan of terrorist organizations. Although this approach does not fully address the issue of unobserved heterogeneity across the sample of terrorist campaigns under examination, it offers a more precise measurement of the correlation between leadership decapitation and organizational decline than the ordinal measure in the descriptive statistics presented in (Jordan 2009).

[3]James D. Fearon and David D. Laitin make a similar argument in an unpublished manuscript on civil war termination. They write that "a . . . factor that comes out clearly from the random set of cases (that we examine) is that change in government or rebel leadership can influence war termination." However, they note the difficulties of identifying the causal effects of leadership removal: "Change in leadership can of course be endogenous to the war–indeed, changing the leadership of the other side is generally the point of the war!" Yet based on their interpretations of randomly-selected case studies, they conclude, "Still, in each case we think that the leadership change had an independent causal effect on termination as well." See Fearon and Laitin 2008, 39-42,

of cases in which governments attempted, successfully or unsuccessfully, to remove top insurgent leaders. In analyzing the success or failure of attempts to decapitate insurgencies, I use instances of failure as controls for cases of success. [4]

My research design thus resembles a natural experiment. The primary identifying assumption is that conditional on attempts to capture or kill insurgent leaders, the outcomes of decapitation attempts are plausibly exogenous (Jones and Olken 2009, 56). As I demonstrate below, although the *timing* of decapitation strikes is not plausibly exogenous–leaders are often targeted at key moments of campaigns when a government is more likely either to win or to lose–whether or not attempts to decapitate insurgencies are successful is plausibly exogenous.[5] The exogeneity of successful and failed bids to remove insurgent leaders is exploited to identify the causal effects of leadership decapitation.

1.2 Targeting Insurgent Leaders: "Near Misses" and "Bad Luck"

Accounts of "near misses" and "bad luck" are common in historical accounts of high-value targeting. The first known assassination plot authorized by U.S. officials against a non-state actor, for example, targeted Mexican rebel Pancho Villa during the 1916 Punitive Expedition. While the Expedition sought to eliminate Villa for the threat his bands' deadly raids posed to Americans border towns in Texas, New Mexico, and Arizona. Initial anti-Villa operations in northern Mexico were unsuccessful, as Villa's mobility and elusiveness made it difficult for American forces to locate him. After U.S. forces had repeatedly failed to find Villa, General John Pershing, who commanded the

[4]For a similar identification strategy of the impact of assassinations of national leaders on economic growth, regime type, and war, see Jones and Olken 2009.

[5]I present substantiary evidence below. To give a preview of this evidence, the data show that decapitation strikes result in failure more often than in success, and neither successful nor failed attempts appear to be correlated with observable variables.

Expedition, resorted to hiring a local team to infiltrate Villa's camp and kill the rebel leader. The team successfully penetrated the camp. One morning over breakfast, Pershing's operatives managed to slip poison into the rebel leader's coffee. Villa reportedly drank more than half of the laced cup of coffee but did not die. Villa lived to fight until he was assassinated in 1923 (Knott 1996, 171; quoted in Thomas 2000, 112).

The failed 1916 attempt to kill Villa foreshadowed the challenges that states would confront in dealing with unconventional threats in the future. Consider an example from the First Russo-Chechen War.

Bogged down in a bloody stalemate with Chechen separatists and Islamists, the Russian government sponsored multiple assassination plots against senior Chechen officials in 1994 and 1995. In July 1996, Russian intelligence officials were informed that Chechen leader Dzhokar Dudayev would be chauffeured to an upcoming conference in Moscow by a driver named Khamad Kurbanov. Armed with this information, Russian agents clandestinely developed an intricate plan to assassinate Dudayev during his trip to Moscow. The plan was put in motion weeks before the assassination was to take place. It began when Russian police made what appeared to be a routine stop Kurbanov's vehicle at a Russian-manned checkpoint inside of Chechnya. Kurbanov was briefly taken inside a nearby office for questioning. While Kurbanov was being questioned by the police, a team of Russian agents quickly planted explosives under the seats of his car. Once the explosives were in place, the Russians released him Kurbanov from questioning. For weeks before Dudayev's Moscow trip, Kurbanov unwittingly drove the explosive-packed vehicle without incident. Meanwhile, Russian operatives made preparations to detonate the explosives once they could confirm Dudayev's presence in the vehicle. However, shortly before he was to ride with Kurbanov, and without any apparent knowledge of the assassination plot, Dudayev's itinerary was changed and he was forced to ride with another driver. The plot was foiled, but had Dudayev's schedule not been changed unexpectedly, he almost certainly would have been killed. [6]

1.3 Preview of the Results

To execture the empirical strategy described above, I collected data on publicly reported attempts to decapitate insurgencies in campaigns that began between 1974 and 2003. The key identifying assumption of my empirical strategy is that the success and failure of attempts to decapitate insurgencies is, on average, exogenous from both observed and unobserved covariates. Accordingly, I restrict my analysis to "near miss" cases—instances where insurgent leaders narrowly escaped capture or death. For example, attempts in which insurgent leaders fled their base camps well ahead of counterinsurgent military operations are not included in the analysis. Since insurgent leaders were likely tipped off about impending operations before any meaningful attempt could take place, no credible assertion that these "failures" were plausibly exogenous can be made. This process resulted in a sample of 118 decapitation attempts against top insurgent leaders.[7] Out of these 118 attempts, a total of 46 (39 percent) were successful.

The empirical analysis strongly suggests that successfully removing insurgent leaders enhances military effectiveness in counterinsurgency. The results indicate that decapitating insurgencies (1) increases the chances of speedy war terminations; (2) enhances the probability of campaign outcomes that favor the counterinsurgent; (3) reduces the intensity of violent conflict; and (4) reduces insurgent-initiated incidents, such as armed attacks and kidnappings, though this last finding finds less support in the data than do the first three.

[6] One piece of evidence supporting the hypothesis that Dudayev was unaware of the assassination plot when he opted to ride with the other drive is that the explosives in Kurbanov's car were not found until weeks later. Kurbanov continued driving the vehicle during this period, apparently unaware of the cargo he was carrying on board. ITAR-TASS News Agency, "Dudayev's Aide Claims Assassination Attempt on Dudayev's Life", BBC Summary of World Broadcasts, April 14, 1995.

[7] A list of insurgent leadership removals can be found in Table 9.

1.4 Plan of the Paper

This paper proceeds as follows. First, I discuss my empirical approach. I describe the conceptualization and measurement of key variables and provide additional information on the leadership decapitation data that are used in the analysis. This section also contains an empirical assessment of the plausibility of my identification strategy. This assessment suggests that it is indeed plausible that, on average, successful attempts to decapitate insurgencies are exogenous conditional on an attempt taking place. I conclude this section by discussing my estimation strategy. Section 3 contains a detailed discussion of my main results. The results suggest that leadership decapitation does indeed have a causal effect. Section 4 probes these results, testing a rival interpretation of the results presented in Section 3. I conclude the paper by recapitulating the main findings and discussing their implications for policy.

2 Empirical Strategy

2.1 Operationalization

Most empirical analyses of "civil war" are based on data sets that pool together all conflicts that occurred within state borders and meet a designated battle death threshold. Although these studies have had a tremendous impact on our knowledge of macro-level trends, they are not designed to answer questions about particular types of political violence, such as insurgency, terrorism, or pro-state militia. To get leverage on these questions, scholars of political violence have recently developed theoretically-motivated concepts to guide data collection and analyses appropriate for these questions. Stathis N. Kalyvas, for example, develops a typology that suggests at least three distinct sub-types of warfare can occur within the broader category of civil war. He uses the concept of irregular warfare–a close analogue of asymmetric guerrilla war–as a point of departure in developing a theory of violence in civil war (Kalyvas 2006). Laia Balcells extends Kalyvas' conceptual framework to conventional civil wars, explaining why the symmetric and conventional aspects of warfare itself generate distinct predictions about patterns of violence. The evidence presented in her empirical examination of civilian victimization in the Spanish Civil War is consistent with this approach (Balcells 2009). Using these precepts of irregular warfare, Lyall and Wilson III (2009) and Johnston (2009b) compile new data sets restricted to counterinsurgency campaigns–as opposed to internal wars or civil conflicts–for analysis of success and failure in COIN.

In this study, I adopt a similar approach. Four criteria are used to identify appropriate campaigns for analysis. First, power must be asymmetric. Power asymmetries can be observed when the relative sophistication and lethality of counterinsurgent forces' weapons or other warfighting technologies are more sophisticated than those used by insurgents. Second, in irregular warfare, violent non-state actors must attempt to win civilian support from at least some segment of the population. Because it is impossible to directly observe militants' motivations and desires, militant organizations are coded as meeting this criterion if they implant themselves into the civilian population are likely to seek popular support. Because civilian betrayal can mean the demise of an insurgency, insurgencies that embed themselves within the population must attempt to win its support. Third, the militant organization must rely primarily on unconventional tactics. In these cases, rebel units must use violent tactics against government targets, such hit-and-run attacks and ambushes, while generally avoiding direct battle. The final operational criterion is a minimum one-month duration rule. These criteria help to ensure that all campaigns in the data set were sustained, asymmetric violent conflicts between organized military actors and not brief, disorganized bursts of violence or instances of rioting.

In all, 90 campaigns between 1974 and 2003 satisfied these criteria. Cases that did not meet these criteria are excluded from the analysis. The data set contains a total of 928 campaign-years and is large enough to identify statistical trends.[8]

2.2 Dependent Variables

To estimate the effect of leadership decapitation on counterinsurgency effectiveness, two sets of dependent variables are used. The first involves campaign outcomes. My first outcome variable is termination. Termination is measured as a dummy variable where "1" indicates that the campaign ended or dropped below a minimal threshold of violence in a given campaign-year. My second outcome variable is success. Success is also measured as a dummy where "1" indicates that a campaign ended on terms favorable to the counterinsurgent in a given campaign-year and "0" indicates that it did not. These variables are from the Correlates of War (COW) Project.[9]

The second set of dependent variables involves conflict dynamics and insurgent behavior. Two outcomes are again examined. Both come from the Global Terrorism Database. The first is conflict intensity. [10] This variable measures the total number of confirmed fatalities, by group, in each campaign-year. Since this variable includes government, civilian, and insurgent deaths, it is not a direct measure of insurgent violence. While we should expect that the conflict intensity variable will be noisy, it should provide insight into whether removing insurgent leaders has a causal effect on conflict escalation or de-escalation because successful and failed attempts should not be correlated with observed or unobserved variables. The second insurgent behavior variable

[8]I limit my analysis to campaigns between 1974 and 2003 because extending the analysis as far back as 1945–the standard "start date" of many civil war analyses–would likely produce undercounts of the "failure" variable because less information is available for this period.

[9]For campaigns not in the COW Project data, Lyall and Wilson's (2009) "Correlates of Insurgency" data set, the Global Terrorism Database or other secondary sources were consulted.

[10]See p. 21 of the GTD codebook for full documentation: `http://www.start.umd.edu/gtd/downloads/Codebook.pdf`.

is insurgent-initiated incidents. This variable measures the number of attacks and other violent incidents, by group, aggregated to the campaign-year level.[11] This variable serves as a proxy of insurgent groups' levels of activity and operational tempos. Together, the insurgent behavior variables enable me to test whether leadership removal increases or decreases both the lethality of conflict and the pace of insurgent activities.

2.3 Independent Variables

My primary independent variable is leadership decapitation. I define a "leader" as the most powerful figure in an insurgent organization. I limit my focus to top leaders; identifying the upper echelon, mid-tier, and low-level leaders in clandestine organizations is difficult, and focusing exlusively on top leaders is done to minimize measurement error. Top leaders are the most important members of insurgent organizations and the highest-priority targets of targeting operations, so this approach maximizes accuracy while directly maintaining a strict focus on the most pressing counterterrorism policy question.

For regressions where decapitation attempts are the units of analysis, the variable is a dummy coded "1" if the attempt was successful. For regressions in which propensity-score matching is used to separate the effects of success and failure, the campaign-year is the unit of analysis. Separate dummies for success and failure are both included on the right-hand side of the regression. An observation coded "1" on either or both of these variables indicates that a successful or failed attempt occurred during that campaign-year.

Before I collected the data necessary to execute my empirical strategy, my first task was to identify each organization's top leader or leaders. I created a list of insurgent leaders using data from the START Terrorist Organization

[11] See p. 42 of the GTD codebook for full documentation: `http://www.start.umd.edu/gtd/downloads/Codebook.pdf`.

Profiles (TOPS) database at the University of Maryland.[12] Once the top group leader or leaders were identified, data on leadership decapitation were collected from the Lexis-Nexis Academic Universe database.[13] Keyword searches were performed on the entire collection of English-language news sources. These searches returned results that were reviewed for information on leadership removals and attempted removals. Each attempt returned in the search results was then cross-validated by at least one additional source before being tagged for inclusion in the data set.

As mentioned above, my data on attempts to remove insurgent leaders are restricted to attempts in which the outcome was plausibly exogenous. Examples of plausibly exogenous decapitation events include attempts to remove leaders through assassination plots, in which bombs are planted or shots are fired; combat operations in which firefights or airstrikes directly target leaders' units; and raids or sweeps of leaders' compounds or camp areas. Rumored plots that never materialized, and operations in which leaders escaped before an attempt was made, are not plausibly exogenous and were excluded. For each attempt that satisfied these criteria, the date, location, attmpt type, and outcome was coded. After the data were filtered, 118 attempts could be documented. 46 of the 118 attempts (39%) resulted in the successful removal of a top-level insurgent leader.

2.4 Exogeneity

My primary identification assumption is that, conditional on a decapitation strike occurring, its outcome will be uncorrelated with the error terms of the regression equations used to estimate the effects of the strikes. To test this assumption, I put the independent variable on the left-hand side of the

[12]The TOPS database contains information on the top leader or leaders of almost all of the insurgencies in my sample. Leaders not identified in the TOPS database were coded using historical encyclopedias and secondary sources. See the database online at http://www.start.umd.edu/start/data_collections/tops/.

[13]Lexis-Nexis' website can be accessed at http://www.lexisnexis.com/.

regression and examine whether observable variables predict successes. The results are displayed in Table 1. In Column 1, the mean values of the variables are presented. These values are all taken from the year before decapitation strikes took place. The means of these variables in the year before failed attempts; Column 3 displays the differences in the means for successful and unsuccessful decapitation strikes; Column 4 presents the results of two-sided t-tests of the equality of these means.[14]

[TABLE 1 ABOUT HERE]

The table illustrates that the sample of successful and failed assassination attempts is balanced across key variables: regime type; gross domestic product (GDP) per capita (log); the total population of the counterinsurgent state; the conflict theater's average elevation in meters (log); and the distance from the counterinsurgent's capital city to the conflict theater.

The only variable for which the mean difference is statistically significant is the counterinsurgent nation's total population. The difference in means is significant at the 10 percent level (p-value = 0.07). This preliminary analysis demonstrates that I cannot reject the possibility that population and distance are confounding variables.[15]

This analysis is extended in Table 2, which displays marginal effects from probit regressions in which the success or failure of decapitation strikes is regressed on the variables examined in Table 2.[16] Specifically, I estimate the following equation:

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[14]These are results of t-tests that do not assume equal variance.

[15]However, this evidence is inconclusive; given that five variables are examined, it is not surprising that two specifications were statistically significant.

[16]The regressors are included either because they were statistically significant in Table 2 or because scholars have suggested they are key determinants of counterinsurgency effectiveness. These variables are used as controls in the regressions used to estimate the impact of successful decapitation strikes on counterinsurgency success presented in Section 4.

$$P(SUCCCESS\alpha) = \Phi(y_1 + y_2 X_a)$$

The probability of a successful leadership removal in a given campaign-year is only likely to be exogenous conditional on the number of decapitation strikes that occurred in the given campaign-year, so fixed effects for the number of the attempts to remove insurgent leaders are included in all specifications shown in Table 3. Decade fixed effects are also included in all of the regressions. Fixed effects for attempt type are included in some specifications to control for unobserved time-invariant differences between methods used to target leaders-–bombings, shootings, combat operations, and capture/raid operations. Fixed effects for the region of the world where each attempt took place-–Eastern Europe, Latin America, North Africa, the Middle East, and South Asia-–are also included in select specifications as indicated in the tables.[17]

[TABLE 2 ABOUT HERE]

The probit regressions displayed in Table 2 suggest that the success of decapitation strikes is indeed plausibly exogenous. Two of the estimates are statistically significant, but neither result appears to be robust. The first, shown in Column 1, suggests the national population of the counterinsurgent is significant at the five percent level (p-value = 0.011). This specification includes neither attempt type nor region fixed effects. This variable is not significant in any of the fixed effects specifications. The distance from the counterinsurgent's capital city to the conflict theater is also significant at

[17]Asia was dropped as a reference category. North America and Western Europe are also omitted because no decapitation attempts occurred in either region during the period under study.

the five percent level (p-value = 0.023) as shown in Column 3, yet it is not significant in any other specification. Finally, I tested whether the variables in Table 2 are jointly significant predictors of successful attempts. They are not: Depending on the combination of fixed effects that are included, the joint p-values of the variables range from 0.19 to 0.46. The unpredictability of success supports the plausibility of my identification assumption.

2.5 Identification

To identify the effect of leadership decapitation on counterinsurgency effectiveness, I exploit the inherent randomness in the success and failure of decapitation attempts. My estimation strategy is to use simple OLS regressions that take the following form

$$y_i = \beta SUCCESS_i + \gamma X_i + \varepsilon_i$$

where i indexes a campaign-year in which there is an decapitation attempt, y_i is the dependent variable (campaign termination; campaign outcome; insurgent violence; or insurgent attacks), $SUCCESS_i$ is a dummy variable equal to one if a leader is killed in that campaign-year and zero if the leader survives any attempts, and X is a vector of other regressors.

The key identification assumption is that conditional on observables, $SUCCESS_i$ is exogenous. Then, $E\left[\varepsilon|SUCCESS, X\right] = 0$, and the average treatment effect can be written as the following:

$$\beta = E[y|SUCCCESS = 1, X] - E[y|SUCCCESS = 0, X]$$

This shows that the estimates from the OLS regression equation written above identify the difference between successful and failed decapitation attempts. If the regressions reject that β is zero, then the outcomes of bids to decapitate insurgencies have a causal effect.

Since assassination attempts do not occur randomly, this empirical strategy cannot conclusively demonstrate whether an observed effect is caused by success, failure, or both success and failure. Addressing this question requires a different empirical strategy. My strategy is to use propensity-score matching to parse the effects of successful and unsuccessful leadership targeting. Matching is my preferred approach because it can ensure sample balance on observed covariates. To preview the results, it appears that most of the effects can be attributed to successful targeting.[18] Since matching cannot ensure balance on unobserved covariates, however, these results are admittedly more speculative than those presented in the next section.

3 Results

This section contains a presentation of the main results of my analysis. To identify the effects of leadership decapitation, both parametric and non-parametric specifications are considered. I begin by using OLS regression. Robust standard errors, adjusted for clustering at the campaign level, are reported.[19] All regressions include fixed effects for the number of attempts

[18]These results are discussed at length in Section 4.

[19]Robust standard errors account for possible serial correlation of the error term when more than one decapitation attempt occurred within a single campaign. Joshua D. Angrist shows that when the researcher's empirical strategy is to estimate causal effects rather than structural models, linear models such as OLS regressions are more efficient than non-linear models because structural models have to be converted into causal effects. Estimating causal effects is not fundamentally different when working with limited dependent variables; the main differences are the increased likelihood of interest in distributional outcomes and the inherent non-linearity of the conditional expectation functions for the dependent variables in models with covariates. In models without covariates, conventional OLS estimates capture both distributional effects and effects on means. Hence OLS is suitable for most of my analysis. See Angrist (2000, 29)) for more discussion of this.

that occurred in a campaign-year and for the decade during which each attempt occurred. I also include fixed effects specifications for the attempt type, i.e., the targeting method that was used, and the region in which each attempt took place. Fixed effects specifications are labeled in each table.

The results of nonparametric tests are also reported. Following Jones and Olken ((2009, 68)), I use Fisher exact tests. Fisher exact tests have exact small sample properties. They take the marginal distribution of each variable as given and calculate the probability that the observed association, or a tighter association, could be produced by chance (Fisher 1935; Jones and Olken 2009, 68). The exact probability of each permutation of the finite set of variables is calculated.

3.1 Campaign Outcomes

3.1.1 Termination

Table 3 displays estimates of leadership decapitation's effect on counterinsurgency campaign termination.

The dependent variable is campaign termination; it is a dummy coded "1" if the campaign ended in the year in which a decapitation strike took place. Campaign termination data were compiled from Lyall and Wilson III's (2009) "Correlates of Insurgency" data set and the PRIO-Uppsala Armed Conflict data set (Gleditsch et al. 2002). All regressions include fixed effects for the number of attempts in a campaign-year and and for the decade in which each attempt occurred. The first column presents the results without additional fixed effects; the second column includes fixed effects for attempt type; the third column includes fixed effects for region; the fourth column includes fixed effects for both attempt type and region.

[TABLE 3 ABOUT HERE]

In each regression, campaign termination was regressed on decapitation attempts' success or failure; the results are estimates of the average effect of successful decapitation strikes compared with failed attempts. The results displayed in Table 3 suggest that campaigns are more likely to end after successful attempts than after failed attempts. The estimate shown in Column 1 suggests that leadership decapitation increases the probability of war termination by 27 percentage points, with a standard error of 0.079. This result is significant at the one percent level. This result appears to be robust: the estimates displayed in Columns 2, 3, and 4 range from 0.249 to 0.290, and all of the specifications are significant at the one percent level. Also, the lower bound of the 95 percent confidence interval of the estimate is above zero in each specification. The results change little when attempt type or region fixed effects are included. These results are also robust to non-parametric modeling. In each of the non-parametric specifications, the results are significant at the one percent level.

3.1.2 Victory

Table 4 displays the average effect of leadership decapitation on counterinsurgency campaign success. The dependent variable is counterinsurgency success or "victory." This variable is a dummy coded "1" if the incumbent defeated the insurgency in the year a decapitation strike occurred. As in Table 3, the right-hand side variable in the regressions presented in Table 4 is the success or failure of leadership targeting events.

The results shown in Table 4 suggest that campaign success is also more likely following successful leadership removals. The estimate in the first column suggests that incumbents are 32 percentage points more likely to defeat insurgencies in years where counterinsurgents remove top insurgent leaders than in years where similar attempts fail–a sizable advantage. This estimate is significant at the one percent level. Like the results reported in the campaign termination analysis in Table 3, the average causal effect of leadership decapitation is robust. Decapitation is significant at the one percent level in each of the four specifications; this holds in each of the Fisher exact tests. In each specification, the lower bound estimate of the 95 percent confidence interval is above zero. Including fixed effects does not significantly influence the results.

In all, the evidence allows us to reject the hypothesis that leadership decapitation is ineffectual or has a counterproductive effect on counterinsurgency campaign outcomes. To the contrary, the data strongly suggest that leadership decapitation has important causal effects; namely, removing militant leaders enhances counterinsurgents' prospects for both quick campaign terminations and for operational-level success .

[TABLE 4 ABOUT HERE]

3.2 Conflict Dynamics

3.2.1 Intensity

Table 5 displays the estimated causal effect of leadership decapitation on the intensity of violence in counterinsurgency. Consistent with the evidence that leadership decapitation increases the likelihood of war termination and counterinsurgent victory, the evidence also sug- gests that leadership decapitation reduces insurgent violence. This finding holds when examined in multiple ways. Columns 1 and 2 show the results of negative binomial regressions. The dependent variable is the number of people killed by an insurgency in a given campaign-year as measured by the Global Terrorism Database. As expected, the sign of the point estimate presented in Column 1 is in the expected negative direction; however, it is not statistically significant. The specification shown in Column 2 probes the result shown in Column 1. The same negative binomial regression is estimated, but the Column 2 specification includes region and attempt type fixed effects. This regression is significant at the one percent leve,l and the point estimate is roughly twice the size of the coefficient displayed in Column 1, suggesting that once time-invariant attempt type and regional effects are accounted for, the null hypothesis–-that leadership decapitation does not have a violence-reducing effect–-can be rejected.[20]

The results shown in Columns 3 and 4 provide additional evidence that that leadership decapitation reduces insurgent violence. These specifications present the same negative binomial regressions as Columns 1 and 2, but include the lagged dependent variable–-the number of people killed by an insurgency in the campaign-year before each attempt–-on the right-hand side. By accounting for the number of people killed by insurgencies at time $t-1$, the lagged dependent variable controls for cross-sectional differences in insurgent violence that would confound identification if my identifying assumption were untrue.

The results shown in Columns 3 and 4 suggest that lagging the left-hand side variable does not change the initial results. On the contrary, they provide additional confidence that the relationship between successful leadership targeting and insurgent violence is negative. Depending on the combination of fixed effects included in the specifications, the point estimates range from -0.915 in Column 3 to -0.975 in Column 4. These results are significant at the five and ten percent levels, respectively. This evidence suggests that removing leaders has a violence-reducing effect. In the next section, I discuss the effect of leadership removal on rates of insurgent attacks.

[20]As before, all regressions include fixed effects for the decade in which each decapitation attempt occurred and for the number of attempts that were carried out during each cam- paign-year with at least one attempt. This ensures that any violence-reducing effect observed in the regression results cannot be attributed to unusually aggressive targeting operations or temporal trends during the Cold War, for example, when there was less stigma attached to targeted killing programs that aimed to eliminate enemy leaders in covert operations, a trend which was at least temporarily reversed during the 1990s.

3.2.2 Insurgent Attacks

In this section I examine the impact of leadership decapitation on insurgent attacks. Levels of violence and numbers of attacks are useful to look at separately because they are believed to capture different conflict dynamics. Whereas levels of violence usually captures the quality of militant operations–-i.e., was the insurgency able to inflict losses on government actors and civilians?–-numbers of attacks measure the quantity of insurgencies' overall activities and reveal information about insurgencies' operational tempo.[21]

Attacks are defined as the number of insurgent-initiated violent incidents in a given campaign-year. The data come from the GTD. My strategy for estimating the effect of decapitation on insurgent attacks is similar to that used to in the previous section. My baseline specifications are negative binomial regressions, specified both with and without fixed effects. To test the robustness of the initial estimates, a lag of the dependent variable is included on the right-hand side.

Table 6 displays the results. It shows that while there is some evidence to suggest that leadership decapitation reduces insurgent attacks, these results are not conclusive. To be sure, the results shown in Table 6 are much less conclusive than those presented in the preceding sections. The results of the fixed effects specifications in Columns 2 and 4, for example, are negative and statistically significant at the five and one percent levels, respectively. The estimate shown in Column 3 is also negative, but it is small and statistically indistinguishable from zero. As with the results presented in the previous section, the results shown here suggest that once time-invariant attempt and

[21] Just as counterinsurgents' leadership decapitation attempts can fail, insurgent attacks can also fail. They can also be unproductive and inefficient. Yet even an insurgency that perpetrates a high volume of unproductive attacks is likely to (1) be feared by people living under constant threat of violence and (2) have a sufficient level of capabilities to coordinate and execute the attacks. Consequently, when rates of insurgent attack are low or decreasing, it generally means that they pose a lesser threat to the populations with which they intermingle and have a lower level of capability to inflict harm on counterinsurgents, government officials, and civilian loyalists.

regional effects are accounted for, the null hypothesis can be rejected. Yet at first glance, we cannot be sure of the finding's robustness; indeed, the point estimate in Column 1, which does not include fixed effects or the lagged dependent variable, is unexpectedly positive.

Given the estimate's small size and large p-value, it is likely that this positive result occurred by chance due to random measurement error in the insurgent attacks data. If this is true, detecting decapitation's effect on attacks would be more difficult than detecting an effect on violence. This would be the case because the GTD's attacks variable (1) is inclusive of a range of insurgent tactics and (2) counts incidents in which insurgents inflict no casualties, while its violence variable is restricted to incidents in which insurgents inflict casualties. Of course, this is exactly what we see-–the results from the violence regressions are more consistently negative and statistically significant than results of the attacks regressions. Despite this evidence, better data will be needed before a persuasive argument can be made about the effect of decapitation on insurgent attacks.

4 Success and Failure

Targeting insurgent leaders is a game of chance. More bids to capture or kill insurgent leaders fail than succeed. But what are the consequences of failure? The element of chance in leadership targeting enables causal identification. The evidence presented above suggests that these outcomes–-successful versus failed decapitation strikes–-have a significant impact on the dynamics and outcomes of counterinsurgency campaigns. Like previous studies of leadership decapitation in war, the purpose of this study is to explain the impact of removing enemy leaders on military effectiveness in war. While it is tempting to attribute the causal effects identified above to successful leadership targeting, doing so would be misleading. Because my identification strategy does not include an untreated control group-–it instead uses exogenous variation in successful and failed attempts-–the possibility that failed attempts are driving the observed relationships cannot be ruled out. Specifically, the putative negative externalities of failed decapitation strikes could underlie the identified effects. That is, when the tactics commonly used in decapitation strikes, such as bombings and raids, fail to eliminate insurgent leaders but incite mass resentment, failed attempts could decrease the chances of war termination and counterinsurgent victory and increase the chances of escalated levels of insurgent violence.

To address this potential issue, I assess the impacts of both successful and failed decapitation strikes on counterinsurgency outcomes. Identifying the independent effects of success and failure alone is more difficult than identifying the difference between them because while the evidence suggests that the success of decapitation strikes is exogenous conditional on an attempt taking place, decapitation strikes themselves do not occur at random. Treating them as if they do risks conflating the effects of successful and failed decapitation strikes with change that would have occurred regardless. For example, if decapitation strikes are more likely to occur when counterinsurgents believe that insurgents are growing, or are going to grow, in strength and lethality, they have an incentive to strike at the insurgency's leadership from a position of relative weakness. This dynamic appears to be motivating the U.S.' escalation of high-value targeting in Pakistan's Federally Administered Tribal Areas (FATA). This could lead analysts to erroneously attribute any observed growth in militancy to high-value targeting attempts, even though high-value targeting might have had no impact on the insurgency's capabilities.[22]

Propensity-score matching is the best tool available for addressing this challenge. Propensity-score matching uses observable characteristics to predict decapitation strikes and uses this information to stratify the sample into similar

[22] Alexander Downes (2008) convincingly makes an analogous argument about states' decisions to use force against civilians during desperate moments of interstate wars. For a general theory of leaders' decisions to "gamble for resurrection" in war, see (Goemans 2000).

control and treatment groups.[23] Stratifying the sample in this way enables me to compare similar years with and without decapitation strikes as if they were similar treatment and control groups. Compared with experimental and quasi-experimental approaches, the major weakness of matching is that assignment to treated and control groups is based on observables. Without full knowledge of the data-generating process, it remains possible that my estimates will be biased due to selection on unobservables. While selection bias is a potential concern when using any matching estimator, matching remains a useful technique for scholars seeking to identify causal effects because it ensures sample balance on observables and excludes extreme counterfactuals (King and Zeng 2006).

To implement this approach, for all countries c engaged in counterinsurgency campaigns in all years t, I use the following equation:

$$P(ATTEMPTct) = \Phi(pXct)$$

This estimator is used to predict attempts conditional on observables. Based on the predicted probabilities estimated from this equation, I form four blocks, denoted by b, for varying levels of the propensity score and then check the balance on the treatment and control covariates in each block. Once the sample is stratified and balanced, I estimate regressions using the equation:

$$y_{ib} = \alpha\beta SUCCESS_{ib} + \beta FAILURE_{ib} + y_{ib} + \varepsilon_{ib}$$

where αb indicates fixed effects for each propensity score block.

[23]For more on propensity-score matching, see, e.g., Ho et al. (2007); Caliendo and Kopeinig 2005; and Rubin 2006.

4.1 Predicting Attempts

Before estimating the effects of successful and failed decapitation strikes, I examine whether pre-treatment covariates in my data set can predict the observed decapitation strikes. Table 7 shows marginal effects of probit regressions that include the same variables used in Table 2. The results displayed in Columns 2 and 6 of Table 7 show that incumbent GDPPC is a significant predictor of decapitation attempts. This is intuitive; wealthier governments are more likely than poorer governments of have the material capabilities necessary to aggressively target insurgent leaders. It is likely that GDPPC proxies not only conventional military capabilities, but that governments of rich countries are more likely to have the necessary resources to develop sophisticated surveillance technology and to acquire other intelligence advantages that can be deployed selectively against insurgent leaders. When a variable measuring counterinsurgents' total number of military personnel in the year before decapitation attempts–a proxy of countries' conventional military power–is included on the left-hand side of the regression specified in Column 6, the estimated marginal effect of GDPPC decreases only slightly, from 0.049 to 0.039. The standard error of the estimate in Column 6 remains consistent, at 0.019, and the result is significant at the five percent level (p-value = 0.027).[24]

[TABLE 7 ABOUT HERE]

[24]This specification is simply to illustrate that the observed relationship between GDPPC and decapitation attempts does not change when a direct measure of conventional military power is included. In the interest of space, these results are not presented in Table 6.

4.2 The Impacts of Successful and Failed Decapitation Strikes

Table 8 shows separate estimates of the effects of successful and failed decapitation attempts on campaign termination and success. For each of these two dependent variables, three specifications are presented. The results of an OLS regression without controls, fixed effects, or propensity score matching are displayed in Column 1. The specification shown in Column 2 includes control variables and attempt type and region fixed effects; and the estimates in Column 3 include all of these regressors and use propensity-score matching.

Although the caveat about identification based on matching is relevant, the results in Table 7 suggest reason to believe that successful decapitation strikes, not failed attempts, drive the effects observed in the previous section. The estimates themselves are consistent with those displayed in Tables 3 and 4. Specifically, the effect of successful decapitation strikes remains similar in size; a successful decapitation strike is associated with a 27-percentage point increase (standard error = 0.073) in the probability of termination during the year in which the decapitation attempt occurred. Likewise, a successful decapitation strike is associated with a 29-percentage point increase (standard error = 0.070) in the probability of counterinsurgent victory during the year in which the decapitation strike occurred. Both results are significant at the one percent level.

In contrast, the effect of failure is indistinguishable from zero. The point estimate of failed attempts is negative in four of the six regressions, suggesting that failed decapitation strikes could have a counterproductive effect. However, the point estimates of failed attempts are small, and the p-values, which range from 0.356 to 0.788, do not approach the standards of social scientific inference. Because the point estimates are small and statistically insignificant, the impact of failed attempts–if one exists–cannot be distinguished from zero. The results are similar when examining both the unmatched and the matched sample and are robust to the inclusion of fixed effects and controls, suggesting that it is successful targeting of insurgent leaders rather than blowback from failed attempts that underlies the effects identified above.

[TABLE 8 ABOUT HERE]

5 Conclusion

This study used a new empirical approach to investigate the empirical effects of leadership decapitation. Specifically, failed decapitation attempts were used as controls for successful leadership removals. This approach minimizes unobserved heterogeneity; importantly, the exogeneity of leadership removals, conditional on attempts to remove leaders taking place, helps to avert any systematic reverse causality in the analysis. This is important, as previous studies have been unable to demonstrate whether observed effects reflect decapitation's independent impact on wartime effectiveness or if they reflect counterinsurgents' previous military effectiveness.

Previous scholarly research on leadership decapitation suggests that removing enemy leaders is at best ineffective and at worst counterproductive, Regardless of whether the adversary is a state, a terrorist organization, or a guerrilla insurgency, scholars suggest that policies of leadership decapitation and high-value targeting have little military value. Implicit in these claims is the premise that leadership plays a secondary role in determining wartime outcomes. As a result, diverting scarce resources from more productive efforts is both distracting and inefficient (Pape 1996; David 2002; Pape 2003; Hoffman 2006). Recent scholarly treatment pushes this perspective a step further. The author of a prominent 2009 study claims that leadership decapitation is not just ineffectual, but that it is counterproductive to effective counterterrorism

(Jordan 2009, 753-754). She argues that decapitation strategies are particularly counterproductive when used against larger and older organizations or organizations with religious and/or separatist aims.

All of these studies provide insightful theoretical explanations of leadership decapitation's putative ineffectiveness, but each suffers from the inferential issues described above. The key concern is selection bias. Unlike this paper, which uses probes the relationship between attempts to remove leaders and observed conditions prior to the attempts, previous studies fail to systematically investigate whether other common factors are correlated with both leadership removal and military ineffectiveness. While the observable effects of leadership decapitation certainly can and do vary from case-to-case, scholars' neglect of these issues makes it difficult to translate the findings of existing studies into a general understanding of the effects of removing enemy leaders.

The findings of this study cast doubt on earlier claims that leadership decapitation does not work.

On the contrary, the evidence presented in this paper suggests that (1) killing or capturing insurgent leaders is neither ineffective nor counterproductive and (2) the effect of removing insurgent leaders is not endogenous to pre-existing battlefield advantages. These results are consistent with the results of other studies in which scholars have attempted to use appropriate counterfactuals to identify the causal effects of leadership decapitation (Jones and Olken 2009; Price 2009). All three studies find evidence, for example, that leadership decapitation makes the termination of low-intensity conflicts more likely.

Another important contribution of this study is that it credibly demonstrates that battlefield advantages do not drive observed positive effects of decapitation (see also Jones and Olken 2009). Put differently, leadership decapitation in counterinsurgency is on average a cause, not an consequence, of military effectiveness. This suggests that insurgent leaders are critically important in determining the fates of their organizations. Although failed decapitation strikes are not without costs, failed attempts appear to have few negative consequences in terms of the outcomes that are most important to policymakers-–reducing insurgent violence and achieving quick, favorable campaign resolutions.

Whether because of their mobilization capacity, charisma, or operational skills and expertise, leaders matter. We can profit by paying more attention to how, when, and where they matter most. For students of security studies, additional study is needed on how variations in the ways insurgent leaders are removed–that is, whether leaders are captured or killed–impact counterinsurgency effectiveness. Preliminary analysis of this question, in both large-N and case study research, comes to varying conclusions. While some scholars do not find evidence that the way in which leaders are removed matters (Johnston 2009a; Staeheli 2010), others suggest that capturing enemy leaders has more strategic value due to the potential intelligence dividends that can be gained in post-capture interrogations (Cronin 2006). More research is also needed to test another common, but largely untested, assumption about leadership decapitation: that terrorist organization's organizational structures, particularly the extent to which their command is decentralized or networked, can affect the probability that leadership decapitation will work. Jordan (2009) probes this question by examining groups with differing aims, but better measurement of organization itself is needed to advance this research program.

These findings have implications for policy. The prevailing view in the United States is that "decapitating" militant organizations is at best ineffectual and at worst counterproductive. Proponents of this view argue that irregular threats are better addressed through comprehensive strategies of "counterinsurgency," in which operations are conducted with the multifaceted aims of building capable central governments, loyal and sustainable security apparatuses, improving governance and decreasing corruption, and fostering economic development. Proponents of this view argue, either explicitly or implicitly, that "counterterrorism" strategies, in which military operations are focused on direct action against militant networks, are unlikely to work against insurgencies because they do not target the population, a key source of insurgent resources. They suggest that direct action against insurgent leaders can inadvertently aid insurgencies by alienating civilians and siphoning resources from "population-centric" initiatives.

While popular support can certainly play an important role in insurgency and counterinsurgency, the present study finds no evidence that targeting militant leaders undermines effectiveness in irregular war. Strictly from a strategic perspective, the evidence presented here suggests that dismissing leadership decapitation from countermilitancy policy on the basis of efficacy alone would be a mistake. Yet the only major finding of this study is that the available evidence suggests that leadership decapitation can help to achieve military and political goals; it does not investigate the relative effectiveness of high-value targeting versus alternatives that are available to policymakers, nor does it address the normative considerations involved in targeted killing programs. Consequently, using this study as the basis for selecting one set of counterinsurgency or counterterrorism tactics over another would be inappropriate.

That said, the evidence does demonstrate that targeting militants directly can play an independent role in disrupting and defeating insurgencies. While policy should be made on a case-by-case basis, the evidence showing that removing insurgent leaders has on average advanced to policy goals implies that direct action approaches can work and that large nation building efforts might not be necessary for countering militancy successfully. There is little general evidence that "light footprint" approaches, such as that proposed for the Afghanistan War by Austin Long (Long 2010), cannot lead to successful outcomes while also reducing the costs of countering unconventional threats.

The implications of this perspective are significant. The United States' wars in Afghanistan and Iraq wars have stretched U.S. troops to near-capacity and, as of Summer 2009, had cost the U.S. more than $750 billion in direct military expenditures. American policymakers are now looking for alternative approaches to counterinsurgency and counterterrorism that can yield effective and sustainable results. Approaches that target militants directly, using both persuasion and coercion, are currently being discussed in Washington as viable alternatives to population-centric approaches. Additional analysis of how these tactics affect militancy is a pre-requisite to informed policy.

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TABLE 1: ARE SUCCESSFUL AND FAILED ATTEMPTS SIMILAR? PAIRWISE T-TESTS OF SAMPLE BALANCE

DV: Success	Success	Failure	Difference	p-Value
Democracy	0.59	0.53	0.05	0.42
	(0.05)	(0.04)	(0.07)	
GDPPC	7.69	7.70	-0.01	0.95
	(0.13)	(0.15)	(0.2)	
Population	11.13	10.36	0.77	0.07
	(0.32)	(0.27)	(0.42)	
Military Personnel	5.24	4.95	0.29	0.45
	(0.29)	(0.25)	(0.38)	
Elevation	5.87	6.09	-0.23	0.41
	(0.24)	(0.14)	(0.27)	
Distance	5.15	5.35	-0.19	0.71
	(0.38)	(0.34)	(0.51)	
Observations	45	58		

Table 2: Are Successful and Failed Attempts Similar? Evidence from Multivariate Regressions

DV: Success	(1)	(2)	(3)	(4)
Democracy	-0.101	0.002	-0.351	-0.186
	(0.260)	(0.254)	(0.294)	(0.328)
GDPPC	-0.026	0.079	0.082	0.114
	(0.086)	(0.089)	(0.107)	(0.114)
Population	0.132**	0.068	0.008	0.043
	(0.052)	(0.054)	(0.072)	(0.081)
Elevation	-0.014	-0.020	0.051	-0.034
	(0.041)	(0.043)	(0.061)	(0.065)
Distance	-0.055	-0.022	-0.104**	-0.029
	(0.037)	(0.035)	(0.046)	(0.051)
Type FE	No	Yes	No	Yes
Region FE	No	No	Yes	Yes
Observations	72	72	71	71

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 3: Leadership Decapitation and Campaign Termination

DV: Termination	(1)	(2)	(3)	(4)
Success	0.273***	0.290***	0.249***	0.260***
	(0.079)	(0.081)	(0.088)	(0.091)
Constant	-0.140**	-0.319**	-0.259**	-0.427**
	(0.068)	(0.126)	(0.112)	(0.179)
Type FE	No	Yes	No	Yes
Region FE	No	No	Yes	Yes
R-squared	0.154	0.202	0.211	0.265
Observations	103	103	103	103

Robust standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 4: Leadership Decapitation and Campaign Success

DV: Victory	(1)	(2)	(3)	(4)
Success	0.321***	0.338***	0.287***	0.310***
	(0.073)	(0.075)	(0.080)	(0.084)
Constant	-0.173**	-0.416***	-0.255**	-0.505***
	(0.075)	(0.129)	(0.110)	(0.171)
Type FE	No	Yes	No	Yes
Region FE	No	No	Yes	Yes
R-squared	0.210	0.318	0.261	0.384
Observations	103	103	103	103

Robust standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 5: Leadership Decapitation and Conflict Intensity

DV: Intensity	(1)	(3)	(5)	(7)
Success	-0.774	-1.994***	-0.898**	-1.637***
	(0.494)	(0.426)	(0.432)	(0.420)
Constant	-1.677*	-1.640*	-1.328	-1.308
	(0.890)	(0.868)	(0.899)	(0.950)
Type FE	No	Yes	No	Yes
Region FE	No	Yes	No	Yes
Observations	102	102	90	90

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 6: Leadership Decapitation and Insurgent Attacks

DV: Attacks	(1)	(2)	(3)	(4)
Success	0.212	-0.728**	-0.092	-1.685***
	(0.480)	(0.328)	(0.325)	(0.444)
Constant	-0.925*	-1.420*	-0.514	-1.392*
	(0.552)	(0.860)	(0.399)	(0.811)
Type FE	No	Yes	No	Yes
Region FE	No	Yes	No	Yes
Observations	102	102	90	90

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 7: Predicting Decapitation Attempts

DV: Attempt	(1)	(2)	(3)	(4)	(5)	(6)
Democracy	0.031					0.022
	(0.041)					(0.046)
GDPPC		0.049***				0.039**
		(0.017)				(0.019)
Population			-0.002			-0.008
			(0.007)			(0.010)
Elevation				-0.001		0.000
				(0.007)		(0.006)
Distance					-0.001	0.000
					(0.004)	(0.005)
Observations	833	790	747	926	926	741

Robust standard errors in parentheses

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 8: The Impact of Successful vs. Failed Attempts

DV	Termination			Victory		
	(1)	(2)	(3)	(4)	(5)	(6)
Success	0.282***	0.284***	0.284***	0.296***	0.289***	0.288***
	(0.072)	(0.073)	(0.074)	(0.069)	(0.070)	(0.070)
Failure	-0.010	-0.022	-0.030	-0.016	-0.022	-0.021
	(0.037)	(0.037)	(0.033)	(0.027)	(0.028)	(0.028)
Constant	0.039	0.155	0.028	0.020	0.012	-0.220
	(0.032)	(0.112)	(0.250)	(0.027)	(0.070)	(0.193)
Controls	No	Y	Y	No	Y	Y
Matching	No	No	Y	No	No	Y
Parm p-Success	0.000175	0.000186	0.000210	4.80e-05	7.97e-05	8.71e-05
Parm p-Failure	0.788	0.560	0.356	0.555	0.424	0.450
R-squared	0.064	0.090	0.093	0.094	0.101	0.102
Observations	932	932	932	932	932	932

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 9: Successful Removals of Insurgent Leaders

Government	Insurgency	Leader	Year
Morocco	Polisario	El-Ouali Mustapha Sayed	1976
Indonesia	Fretilin	Nicolau Lobato	1978
Mozambique	Renamo	Andre Matsangaissa	1979
Nigeria	Maitatsines	Mohammadu Marwa	1980
India	PLA	N. Bisheswar Singh	1981
India	PLA	Thoundam Kunjabehari	1982
Colombia	M-19	Carlos Toledo Plata	1984
Somalia	SSDF	Abdullahi Yusuf Ahmed	1984
India	PREPAK	R.K. Tulachandra	1985
Yemen	YSP	Abdul Fattah Ismail	1986
Pakistan	MQM	Altaf Hussain	1986
India	KCF	Manbir Singh Chaheru	1986
India	KLF	Aroor Singh	1988
India	KLM	Avtar Singh Brahma	1988
India	KCF	Labh Singh	1988
Sri Lanka	JVP	Rohana Wijeweera	1989
Pakistan	MQM	Altaf Hussain	1991
Peru	Shining Path	Abimael Guzman	1992
Chad	MDD	Goukouni Guet	1992
Algeria	GIA	Mansouri Meliani	1992
India	BKI	Sukhdev Singh	1992
Indonesia	Fretilin	Xanana Gusmao	1992
India	KLF	Gurjant Singh Budhsinghwala	1992
Chad	CNR	Abbas Koty	1993
Algeria	GIA	Abdelhak Layada	1993
Indonesia	Fretilin	Antonio Gomes da Costa	1993
Algeria	GIA	Cherif Gousmi	1994
Algeria	GIA	Mourad Sid Ahmed	1994
India	BTFK	Gurbachan Singh Manochahal	1994
Russia	Chechens	Dzhokhar Dudayev	1996
Sierra Leone	RUF	Foday Sankoh	1997
Chad	FDR	Laokein Barde	1998
Philippines	ASG	Abdurajik Abubakar Janjalani	1998
Peru	Shining Path	Oscar Ramirez	1999