

THE IMPACT OF BRITISH COUNTERTERRORIST STRATEGIES ON POLITICAL VIOLENCE IN NORTHERN IRELAND: COMPARING DETERRENCE AND BACKLASH MODELS*

GARY LAFREE
LAURA DUGAN
RAVEN KORTE

Department of Criminology and Criminal Justice
University of Maryland

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Since philosophers Beccaria and Bentham, criminologists have been concerned with predicting how governmental attempts to maintain lawful behavior affect subsequent rates of criminal violence. In this article, we build on prior research to argue that governmental responses to a specific form of criminal violence—terrorism—may produce both a positive deterrence effect (i.e., reducing future incidence of prohibited behavior) and a negative backlash effect (i.e., increasing future incidence of prohibited behavior). Deterrence-based models have long dominated both criminal justice and counterterrorist policies on

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responding to violence. The models maintain that an individual's prohibited behavior can be altered by the threat and imposition of punishment. Backlash models are more theoretically scattered but receive mixed support from several sources, which include research on counterterrorism; the criminology literature on labeling, legitimacy, and defiance; and the psychological literature on social power and decision making. In this article, we identify six major British strategies aimed at reducing political violence in Northern Ireland from 1969 to 1992 and then use a Cox proportional hazard model to estimate the impact of these interventions on the risk of new attacks. In general, we find the strongest support for backlash models. The only support for deterrence models was a military surge called Operation Motorman, which was followed by significant declines in the risk of new attacks. The results underscore the importance of considering the possibility that antiterrorist interventions might both increase and decrease subsequent violence.

The belief that credible threats of apprehension and punishment deter crime is as old as criminal law itself and it has broad appeal to both policy makers and the public. Deterrence models generally assume that human beings are rational, self-interested actors who seek to minimize personal cost while maximizing personal gain (Gibbs, 1975; Paternoster, 1987; Ross and LaFree, 1986). An important implication of such perspectives is that individual behavior can be altered by the threat and imposition of punishment. Deterrence models have been applied to a wide variety of criminal behavior, such as drunk driving (Nagin and Paternoster, 1993), burglary (Wright and Decker, 1994), robbery (Wright and Decker, 1997), shoplifting (Piquero and Tibbetts, 1996), income tax evasion (Klepper and Nagin, 1989), drug selling (Jacobs, 1996), and white-collar crimes (Paternoster and Simpson, 1996; Simpson, Piquero, and Paternoster, 1998). Deterrence models seem to be especially appropriate for understanding terrorist violence, given that many terrorist attacks are carefully planned and seem to include at least some consideration for risks and rewards. Indeed, deterrence-based thinking has dominated counterterrorist policies in most countries since the origins of modern terrorism in the late 1960s (Braithwaite, 2005; Collins, 2004; Frey and Luechinger, 2002), and substantial research supports the argument that deterrence-based policies can reduce terrorist violence (Clarke and Newman, 2006; Dugan, LaFree, and Piquero, 2005; Enders and Sandler, 2006).

However, research on terrorism (McCauley, 2006; Nevin, 2003) and, more generally, research from criminology (Braithwaite 1989, 2005; Pride-more and Freilich, 2007; Sherman, 1993) and psychology (Brehm and Brehm, 1981; Tyler, 1990) suggests that the threat and/or imposition of

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punishment does not always deter future acts of violence and may increase violence in some cases. In this article, we conceptualize these two possibilities by distinguishing between *deterrence effects* (i.e., the extent to which government threats or imposition of punishment reduces the future incidence of prohibited behavior) and *backlash effects* (i.e., the extent to which government threats or imposition of punishment increases the future incidence of prohibited behavior). We analyze terrorist attacks that occurred over a 23-year period in Northern Ireland to test the relative strength of deterrence and backlash models for the risk of new terrorist strikes. We assume that a significant decrease in the hazard of new strikes after a major government counterterrorist intervention is consistent with a deterrence effect, and that a significant increase is consistent with a backlash effect. A null effect suggests that either no relationship exists or that deterrence and backlash dynamics have neutralized each other.

Based on an extensive literature review, we identified six high-profile counterterrorist interventions used by the British to reduce republican violence in Northern Ireland from 1969 to 1992—the years of the greatest terrorist activity in the modern history of this region. Using data on 2,603 republican strikes during these years from the Global Terrorism Database (GTD; LaFree and Dugan, 2007), we use a multivariate hazard model to determine whether the future risk of attacks during this period increased, decreased, or remained the same after each of the major interventions implemented by the British. Republican groups in Northern Ireland are predominantly Catholic and have fought for a united Ireland independent of Britain. In contrast, loyalist groups are predominantly Protestant and have fought for continued British rule.

In general, the results were more consistent with backlash than deterrence explanations; three of the six British interventions produced significantly higher hazards of future terrorist strikes by republicans, and two interventions had no significant effects. The only support for deterrence among these six interventions was for a major military surge, which significantly reduced the hazard of new attacks. In the next two sections, we consider in greater detail the theoretical justification and relevant research that supports deterrence and backlash interpretations of responses to terrorist and other criminal violence. We conclude with the argument that reducing the threat of future terrorist strikes requires a sophisticated understanding of the policy options that affect the deterrence–backlash nexus.

DETERRENCE AND BACKLASH EFFECTS FOR
TERRORIST VIOLENCE

We define terrorist attacks as “the threatened or actual use of illegal force and violence by a non state actor to attain a political, economic,

religious or social goal through fear, coercion or intimidation” (LaFree and Dugan, 2007: 184). For our purposes, deterrence is measured by net decreases in the prevalence, incidence, or seriousness of future terrorist attacks in a given political location. Backlash is measured by net increases in these attacks. The roots of deterrence perspectives extend back to the classical school of criminology and the work of philosophers Cesare Beccaria and Jeremy Bentham. The principle of utility, which was advanced by Bentham, proposes that individuals act in their own self-interest. Therefore, effective punishment will deter them from engaging in specific actions that serve their self-interest. As advanced by Cornish and Clarke (1986), contemporary deterrence theory assumes that offenders are rational actors who seek to maximize their pleasure and minimize their pain.

The deterrence perspective suggests that individuals commit a given act when the expected benefits exceed the expected costs (Nagin and Pater-noster, 1993). Specific deterrence occurs when offenders already punished for breaking the law decide not to repeat their behavior because they fear punishment; general deterrence occurs when members of the public decide not to break the law because they fear punishment. Closely related to deterrence models are incapacitation models, which assume that the de-escalation of violence is best achieved by taking those who use violence out of circulation (Gibbs, 1975: 22; Gross and von Hirsch, 1981: 187). In the case of criminal justice policies, incapacitation generally refers to keeping high-risk offenders in prison for longer periods of time; in the case of counterterrorism policies, it may mean either arrest or imprisonment of high-profile offenders or targeted assassination (Braithwaite, 2005: 96–7). Apart from its own effects on future violent behavior, incapacitation may also reduce future criminal violence by increasing the effectiveness of either general or specific deterrence. Of course, possible preventive mechanisms of legal punishments are not limited to deterrence and incapacitation. For example, Gibbs (1975: 92–3) lists nine separate ways that legal punishments might prevent future crime apart from pure deterrence, which include not only incapacitation, but also surveillance, enculturation, and reformation. However, because our principle focus in this article is on determining whether government policies result in increasing or decreasing subsequent terrorist violence, we are summarizing all of these potential preventive mechanisms here under the general heading of deterrence, which is the most inclusive of these concepts.

Compared with research on deterrence models, research on conditions that promote escalating violence in response to either counterterrorist or criminal justice interventions is less common and more theoretically scattered. Nevertheless, strong support for the argument that the imposition of punishment on a particular individual or group may *increase* future levels

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of violence is found in research on terrorism and, more generally, in research from criminology and psychology. Researchers (Atran, 2003; Crenshaw, 2002; Higson-Smith, 2002) have long argued that terrorists frequently rely on the response of governments to mobilize the sympathies of would-be supporters. The extent to which government-based counterterrorist strategies outrage participants or energize a base of potential supporters may increase the likelihood of more terrorist strikes. Sharp (1973) refers to this phenomenon as “jijitsu politics,” and McCauley (2006) points out that because of this principle, the responses to terrorism can be more dangerous than terrorism itself. Thus, according to Benjamin and Simon (2005: 21), Osama bin Laden’s decision to authorize the September 11 attacks was animated in large part by the belief that American retaliation would inevitably kill innocents and thereby demonstrate the extent of American hatred toward Muslims.

More generally, extensive psychological literature supports the conclusion that under the right circumstances, punishment may elicit increases in proscribed behavior. Thus, in their formulation of reactance theory, Brehm and Brehm (1981: 4) argue that when an individual or group is threatened with some new form of social control, they are immediately motivated to act to eliminate this control and to restore their original freedom. Relatedly, much psychological literature (Duckitt and Fisher, 2003; LeVine and Campbell, 1972) suggests that threats from out-groups generally increase the cohesion of in-groups as well as the pressure on in-group deviants to conform and support in-group leaders.

Criminology literature also provides some support for backlash models. Thus, early labeling theorists (Becker, 1963; Tannenbaum, 1938) pointed out that punishment leads to identity changes in individuals as well as to social changes in society that result in criminal offenders increasing their deviant behavior after their official labeling. Lemert (1951: 77) famously referred to this concept as “secondary deviance.” Sherman (1993) has argued that whether punishment results in deterrence will depend on whether offenders experience sanctioning as legitimate, the social bonds between the sanctioning agent and the individuals or groups being sanctioned, and the extent to which offenders can deny their shame by seeking support from others in the community. Support for the conclusion that punishment is more likely to be perceived as legitimate by the punished when the punishment is perceived to be procedurally fair is supplied in several survey-based studies by Tyler (2000; for a review, see Tyler, 2006).

It is important to note that the perceived legitimacy of government actions is likely to be far more salient to the many potential supporters of terrorism than to the handful of individuals already actively involved in terrorism; this group may change little regardless of government response. In other words, a backlash model predicts that government responses to

terrorist violence may not only embolden those who already participate in terrorist attacks but also may encourage others to join terrorist organizations, support those organizations, or look the other way when they witness the activities of supporters.

Empirical research that directly tests which specific counterterrorist policies escalate or de-escalate violence has been limited (for reviews, see Dugan, LaFree, and Piquero, 2005; Lum, Kennedy, and Sherley, 2006). Moreover, far more studies test for deterrence than backlash effects of counterterrorist policies. However, growing empirical literature tests the effects of specific counterterrorist measures in Northern Ireland and elsewhere, as well as more general tests of counterterrorist interventions across several countries. We review this empirical literature in the sections that follow.

EMPIRICAL TESTS OF DETERRENCE AND BACKLASH MODELS

NORTHERN IRELAND CASE STUDIES

Peroff and Hewitt (1980) assess the effects of reformist, repressive, and constitutional policies by the British on rioting in Northern Ireland between 1968 and 1973. The authors conclude that none of these strategies were successful in decreasing monthly levels of rioting. Although it provided little support for deterrence models, the study was limited to a 5-year period and concentrates on riots rather than on terrorist attacks, which is the focus of the current research.

Soule (1989) studied the goals and methods of the Provisional Irish Republican Army and the counterterrorist responses of the authorities over a 20-year period. He tracks annual terrorism-related deaths experienced by security forces and civilians, as well as persons charged with terrorist and other criminal offenses. He concludes (Soule, 1989: 31) that the two sides have become locked in a "ritualistic dance of death" as each side adapts its tactics to new adaptations on the other side. Although Soule does not directly examine the impact of specific counterterrorist strategies and offers no formal statistical analysis, his results fail to support a deterrence interpretation.

Campbell and Connolly (2003) focus specifically on the impact of the 1970 Falls Curfew on subsequent terrorist strikes in Northern Ireland. This 36-hour military curfew and search operation, which was imposed by the British government, was designed to locate Irish Republican Army (IRA) members and weapons stockpiles. Drawing on prior empirical studies and archival materials, Campbell and Connolly conclude (2003: 372) that although the military force deployed was largely successful in achieving control over the area included in the military curfew, this success came

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with long-term costs: The inherently indiscriminate nature of the curfew punished the innocent along with the perpetrators and seriously undermined the legitimacy of counterterrorist efforts. The result was the creation of an environment “in which political violence and terrorism were either tolerated or supported” (Campbell and Connolly, 2003: 373). The conclusions of Campbell and Connolly are relevant for the research being undertaken here; however, the authors concentrate on only one counterterrorist intervention and offer no statistical tests.

OTHER CASE STUDIES

Other case studies have examined the deterrent effect of specific military counterterrorist strategies elsewhere, especially the United States and Israel. Enders and Sandler (1993) use ITERATE data on terrorist events (Mickolus, 1982) to examine the deterrent impact of the following six counterterrorist strategies: 1) the installation of metal detectors in U.S. airports in January 1973; 2–4) the fortification of U.S. embassies in 1976, 1985, and 1986; 5) the enactment in October 1984 of tougher U.S. laws for terrorist-related acts; and 6) the U.S. decision to bomb Libya in 1986 for its alleged involvement in the terrorist bombing of a West Berlin discotheque. The authors find a significant deterrent effect for both metal detectors and embassy fortifications, but in both cases, these effects are counterbalanced by “substitution effects”: A decline in aerial hijackings attributable to metal detectors was met by a significant increase in other kinds of hostage-based attacks; a decline in embassy-related barricade attacks was met by a significant increase in assassinations. The tougher U.S. laws and the bombing of Libya had no significant long-term effects on curbing future terrorist attacks against the United States. Although these results have obvious relevance for the test proposed here, Enders and Sandler do not test whether responses to counterterrorist strikes may elicit significant backlash as well as deterrence effects.

Collins (2004) evaluates the impact of military force, unilateral economic sanctions, and multilateral economic sanctions on the frequency of Libyan-supported terrorist attacks against the United States during the 1980s [see also Malvesti (2002) and Roberts (2002)]. Using data from the U.S. State Department and several secondary sources on terrorist strikes, Collins concludes that whereas the U.S. air strike on Libya in 1986 did reduce the number of subsequent incidents, it failed to reduce the number of American casualties associated with Libyan-supported terrorist attacks. By contrast, Collins argues that multilateral economic sanctions were eventually successful in curtailing Libyan sponsorship of terrorist attacks. However, Collins provides no statistical analysis.

Brophy-Baermann and Conybeare (1994) identified six major Israeli strategies aimed at reducing terrorism and then conducted a time-series

analysis of terrorist attacks from 1968 to 1989 to gauge the impact of these strategies. The authors conclude that Israeli counterterrorist strategies did significantly reduce future terrorist strikes, but these strategies were only effective to the extent that they exceeded the level of counterterrorist violence anticipated by terrorist groups (the “natural rate”). Moreover, the effects were only short term and lasted no more than 9 months. Thus, whereas the authors find some support for deterrence models, the findings lead them to conclude that Israel might experience fewer terrorist strikes if it limited its own military interventions. Although the Brophy-Baermann and Conybeare findings on deterrence are important, their research focuses on selecting an optimal rate for government response to terrorism rather than on whether deterrence or backlash models best explain the postintervention hazards of future terrorist attacks—the subject of our analysis. Moreover, as with several other studies that focus on Israel (Eppright, 1997; Greener-Barcham, 2002), it is unknown to what extent we can generalize results from terrorist attacks in Israel to other countries and regions.

MULTINATIONAL STUDIES

In addition to studies that focus directly on a particular country, several studies examine the impact of counterterrorist strategies across multiple countries. Nevin (2003) examines governmental strategies to combat terrorism in the following seven global situations: British response to the Jews in Palestine, 1945–1948; French response to the Moroccan independence movement, 1953–1956; French response to the Algerian independence movement, 1954–1956; British response to the IRA, 1971–1973; Spanish response to Basque separatists, 1973–1983; Sri Lankan response to Tamil separatists, 1983–1987; and Peruvian response to Shining Path guerrillas, 1991–1993. Nevin quantified accounts from the New York Times Index and the Encyclopedia of World Terrorism (Crenshaw, 1997) and found no evidence that government military campaigns significantly decrease terrorist activity. Instead, he concludes that the intensity of terrorist attacks either increase or decrease less after a more violent government military intervention; this finding contradicts deterrence models. Although Nevin provides one of the few quantitative assessments of the effect of counterterrorist policies on the likelihood of subsequent terrorist strikes, his reliance on only two public sources for data likely undercounts the actual events and thereby increases the risk of systematic bias.

Chalk (1998) evaluates the impact of counterterrorist interventions directed at the Red Brigades in Italy in the early 1970s, at ETA in Spain during the 1980s, and at the Tupac Amaru Revolutionary Movement and Shining Path in Peru between 1992 and 1996. Government counterterrorist responses in these cases included providing support for extreme right-wing

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political organizations (in Italy), providing secret funding for kidnapping and assassinating terrorist suspects (in Spain), and suspending the constitution and greatly increasing the power of the police (in Peru). Chalk argues that in all three cases, the counterterrorist interventions imposed by government provided exactly the overreaction that had been sought by the terrorists that were the objects of the new policies. However, Chalk's argument is not that counterterrorist strategies increased terrorist violence—in fact, some of these counter measures seem to have been at least partially successful in reducing violence—but rather, that governments in these cases were replacing one form of terrorism with counterterrorist polices that were equally dangerous to liberal democratic governments. Also, like most of the research reviewed, Chalk's study includes no empirical data or statistical tests.

SUMMARY AND CONCLUSIONS

Existing research on the effectiveness of counterterrorist measures is mixed, with some studies showing support for deterrence effects (Brophy-Baermann and Conybeare, 1994; Enders and Sandler, 1993; Eppright, 1997; Greener-Barcham, 2002) and other studies (Malvesti, 2002; Nevin, 2003; Peroff and Hewitt, 1980; Roberts, 2002) showing either no deterrence effects or, in some cases, backlash effects. This raises the obvious possibility that some interventions are more likely than others to produce deterrence or backlash effects. However, no prior study of which we are aware has collected data and has applied formal statistical tests that simultaneously compare deterrence and backlash models. In this article, we specifically compare these two models after the implementation of six major British counterterrorist interventions in Northern Ireland. In the next section, we briefly describe the recent political violence in Northern Ireland and the counterterrorist interventions that are the subject of our analysis.

THE CASE OF NORTHERN IRELAND

Approximately 60 percent of Northern Ireland's population of 1.5 million is Protestant, and most of the remaining population is Catholic.¹ The escalation of terrorist violence in the late 1960s in Northern Ireland can be traced back at least to 1920, when Britain divided Ireland into two administrative units in an attempt to ensure a loyalist (predominantly Protestant) majority in Northern Ireland that supported the union between Britain and Ireland (McGloin, 2003; O'Leary, 2005; O'Leary and

1. For convenience, we contrast Catholics and Protestants here, but we should make it clear that the struggle in Northern Ireland is more complex than this. For example, many individuals with little or no religious identity are actively involved in both sides of this conflict.

McGarry, 1993). In the late 1960s, Irish republicans (predominantly Catholic) began a movement to protest the perceived political and economic discrimination against Northern Ireland (O'Leary and McGarry, 1993). The IRA, which is a paramilitary group that supports the republican agenda, was committed to the goal of a united Ireland independent of Britain. According to Irish republicans, Britain was occupying their country by force; therefore, an armed struggle was justifiable and necessary to rid Northern Ireland of its occupiers (Alonso, 2001; McGloin, 2003).

Although the IRA is the most active and well known of the Irish republican groups that have employed terrorist methods, several less visible groups are also involved, notably the Irish National Liberation Army and the Irish People's Liberation Organization. We combine attacks by all of these republican groups in the analysis. Although republican paramilitary organizations desperately fought for a Northern Ireland free of British rule, paramilitary organizations loyal to Britain were equally committed to preventing this outcome. Rather than attacking British interests, the loyalists most often targeted Irish republicans and other civilians (Sutton, 1994; White, 1997). The most active of the loyalist groups that used terrorist tactics were the Ulster Volunteer Force and the Ulster Defense Association. As competing groups staged increasingly violent attacks in Northern Ireland in the late 1960s, the British government called on its police and military forces to contain the violence.

Northern Ireland provides a strategic test of the impact of counterterrorist measures on the risk of future terrorist attacks because as noted by O'Connor and Rumann (2002–2003: 1750), the British tried “almost every conceivable form of emergency power” to quell the violence in Northern Ireland over a 30-year period. After an extensive literature review, we settled on six British interventions for our test. We specifically sought out interventions that received a good deal of press coverage. We also selected a variety of response types. Thus, two interventions were primarily based on criminal justice and four were primarily based on military. The two criminal justice interventions were “the internment” and “criminalization and Ulsterization.” During the internment period (August 9, 1971 to December 5, 1975), a total of 1,981 suspected terrorists were detained by the authorities. Of those detained, nearly 95 percent were Catholics or republicans.

Criminalization/Ulsterization was implemented on March 26, 1976. After the implementation of criminalization, the jailed terrorist suspects were treated as criminals rather than as political prisoners. Ulsterization shifted the primary responsibility for providing security in Northern Ireland from the British military to the local police force (first the Ulster Defense Regiment and, later, the Royal Irish Regiment). Both interventions were aimed at treating terrorist suspects as criminals rather than as

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political prisoners, which thereby reduced the perceived legitimacy of their actions (McEvoy, 2001: 21).

The four military interventions were the Falls Curfew, Operation Motorman, and the Loughall and Gibraltar incidents. Although all interventions were military based, these four interventions represent very different strategies. The Falls Curfew, which was implemented between July 3 and 5, 1970, was a 36-hour military curfew and search operation designed to locate IRA members and weapons stockpiles. The military killed four people and severely damaged several homes during the intervention. Operation Motorman was a massive British military deployment launched on July 31, 1972, aimed at eliminating “no go” areas in Londonderry and Belfast. Over 30,000 armed-service personnel took part, which made it the largest British deployment since World War II and the largest troop concentration in Ireland in the twentieth century. Both Loughall and Gibraltar were targeted assassinations carried out by the British Special Air Service (SAS). The Loughall incident took place on May 8, 1987, when eight IRA paramilitary group members were killed—the greatest loss of life suffered by the IRA in any single incident up to that point. Similarly, on March 6, 1988, during the Gibraltar incident, the British SAS shot and killed three IRA members as part of a planned military operation.

To summarize, we have identified six major actions taken by the British to reduce republican violence in Northern Ireland during the 1970s and 1980s. We interpret significant decreases in attacks after each of these actions as support for deterrence models and significant increases as support for backlash models. In the analysis that follows, we focus on republican-motivated strikes because the British interventions were directed mostly at the republicans. To control for rival explanations, we include several control variables, which are described below.

DATA AND METHODS

DATA

Our analysis is based on 2,603 attacks claimed by republican groups from 1969 to 1992 drawn from the GTD compiled by LaFree and Dugan (2004, 2007) and supplemented by additional cases originally collected by Sutton (1994); these data were made available from the Conflict Archive on the Internet (CAIN, 2005).² Because the GTD are described in detail

2. Because most open-source databases on terrorism are limited to international data, they largely miss conflicts such as the one in Northern Ireland that mostly involved domestic violence. Thus, although our analysis includes a total of 2,603 republican terrorist strikes from 1969 to 1992, the comparable figure from ITER-ATE (the most commonly analyzed of the open-source terrorism databases) is 24 incidents. By contrast, police statistics from Northern Ireland (e.g., Taylor, 2002:

elsewhere (LaFree and Dugan, 2007), we offer only a brief explanation here. The GTD were collected by trained researchers who recorded terrorism incidents from wire services (including Reuters and the Foreign Broadcast Information Service), U.S. and foreign government reports, and U.S. and foreign newspapers (e.g., the *New York Times* and the *British Financial Times*). The same coding scheme was used during the entire 28 years of data collection.³

METHODS

We begin the analysis in 1969—the beginning of the British military presence in Northern Ireland and 18 months before the Falls Curfew was enacted—and end in 1992—just before a major pause (1994–1996) in the republican insurrection (O’Leary, 2005: 227) and 45 months after the Gibraltar incident. We use Cox (1972) proportional hazard models to estimate the impact of the six British interventions on the risk of future republican attacks controlling for all other interventions and a set of relevant variables. Following the strategy described in Dugan, LaFree, and Piquero (2005), we use continuous-time survival analysis with the dependent variable measured as the number of days until the next terrorist attack, and the independent variables measured at the time of the current attack. We used the following specification for the hazard model in the analysis:

$$h(Y) = \lambda_0(Y) \exp(\beta_1 \text{GOVERNMENT INTERVENTIONS} + \beta_2 \text{CONTROLS})$$

We estimate the coefficients associated with the hazard of a new attack (estimated by the number of days until the next attack, Y) as a function of an unspecified baseline hazard function and other risk or protective factors measured at the time of the current attack represented by the vectors **GOVERNMENT INTERVENTIONS** and **CONTROLS**.⁴

2) include many incidents (e.g., armed robberies and shootings) that cannot be unambiguously classified as terrorism. Our approach has been to include only those cases that are reported in open-source media accounts and that fit the definition of terrorism provided above.

3. The GTD is continuously updated with new information about previously recorded incidents as well as the addition of incidents not initially captured by the original data collectors. The analysis in the current data set is based on the February 2007 version of the GTD.

4. Some attacks are coordinated. In these cases, the incidents can be weighted to represent the actual number of attacks. For example, on August 6, 1977, the IRA bombed 19 British firms in Belfast. Because these multiple-part events were clearly planned as a single event and the purpose of this research is to estimate how interventions affect the hazard of new attacks, we counted such multiple-part events as one attack in the analysis.

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The variables used in the analysis as well as their means, standard deviations, and ranges are shown in table 1. The dependent variable Y is the number of days until the next terrorist attack. We examine separately all attacks attributed to the republicans between January 1, 1969 and December 31, 1992.⁵ The mean number of days between attacks was slightly more than 3.

GOVERNMENT INTERVENTIONS

To measure the impact of the interventions, we used a series of dummy variables with values of “1” for attacks that were perpetrated during the period relevant to the intervention and “0” otherwise. Thus, internment was coded as “1” from August 9, 1971 to December 5, 1975. According to table 1, almost one quarter of the republican attacks occurred during the internment. The other criminal justice intervention, criminalization/Ulsterization, also had a clear start date (March 26, 1976), but its end date is complicated. The criminalization portion of this policy provoked immediate protests from republican inmates, the most successful of which was a set of highly effective hunger strikes (Beresford, 1987; McEvoy, 2001). In response, the British government rescinded its criminalization policy on October 6, 1981. However, its Ulsterization policy remained in effect throughout the period included in our analysis. Therefore, we measured the criminalization/Ulsterization effect in two ways—one that ends the effect on October 6, 1981 (with the end of criminalization) and the other that continues to the end of the analysis. Because the results were substantively identical with either strategy, we report findings for the first strategy only. According to table 1, criminalization/Ulsterization (when coded jointly) was in effect for about one quarter of the republican attacks.

The start dates for the four military interventions are clear, but none have precise end dates. We initially set the end time for all four events at 1 year. In all cases, this time was shorter than the beginning of the next intervention, which allowed us to avoid picking up effects of the following interventions as we measure the current intervention. Thus, because the Falls Curfew lasted for only 2 days, its end date is 12 months and 2 days after it began. To guard against the possibility that a 1-year cutoff overlooks important long-term effects of these interventions, we also conduct

5. The first three attacks are omitted from the analysis because two independent variables are lagged over prior attacks. Our analysis includes all republican attacks even if they occurred outside of Northern Ireland. In fact, 86.6 percent of the republican attacks occurred in Northern Ireland, and another 9 percent occurred in the rest of the United Kingdom. Ireland accounted for 2.6 percent of the cases. Other cases were scattered throughout Europe (33 cases), the United States (3 cases), and Zaire (1 case). These last four cases all involved letter bombs directed at overseas British embassies.

Table 1. Descriptive Statistics, Terrorist Attacks by Republicans, Northern Ireland, 1969 to 1992 (N = 2,600)

Variable	Mean	Standard Deviation	Minimum Value	Maximum Value
Days until next attack	3.163	4.774	0	73
Interventions				
Internment (Aug. 9, 1971–Dec. 5, 1975)	.236	.425	0	1
Criminalization/Ulsterization (Mar. 26, 1976–Oct. 6, 1981)	.251	.434	0	1
Falls Curfew (July 3, 1970–July 5, 1971)	.006	.076	0	1
Operation Motorman (July 31, 1972–July 31, 1973)	.054	.226	0	1
Loughall (May 8, 1987–May 8, 1988)	.029	.168	0	1
Gibraltar (Mar. 6, 1988–Mar. 6, 1989)	.057	.232	0	1
Controls				
Troops last year (thousands)	18.782	3.962	2.700	25.343
“Bloody Sunday” (Jan. 30, 1972–June 30, 1972)	.094	.188	0	1
Loyalist attacks	3.212	3.877	0	20
Success	.862	.345	0	1
Number killed	.802	1.229	0	21
Attack density	6.558	11.170	0	315
Crimes reported	523.234	125.847	248.100	682.550
CAIN	.392	.488	0	1
Months	156.329	79.841	18	288

sensitivity tests that vary the end dates for all the military interventions, 1 month at a time up to 36 months (appendix A). Finally, to test for the possibility that intervention effects change over time, we also included a measure of each intervention by month. According to table 1, less than 1 percent of the attacks occurred during the first year after the Falls Curfew, compared with over 5 percent during the year after Operation Motorman and the Gibraltar incident and nearly 3 percent the year after the Loughall incident.

CONTROL VARIABLES

To maximize our confidence in the results, we sought to control for a variety of competing explanations. To test the possibility that it was not specific government interventions but military presence in general that predicted republican attacks, we included a measure of the total strength

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of armed forces in Northern Ireland over time from Neumann (2003: 190).⁶ To reduce bias caused by endogeneity, we measured the total number of troops in Northern Ireland during the calendar year *prior* to the attack (in thousands). Consequently, the three 1969 cases were dropped from the analysis. According to table 1, an average of more than 18,700 troops were in action during the analysis period, weighted by the number of attacks in the following year.

We include a measure called “Bloody Sunday” to control for the possible escalation effects of this incident on subsequent terrorist attacks. This event occurred on January 30, 1972, when the British Army killed 14 Catholic protesters. Although the government asserted that the slain protesters had been IRA members, witnesses insisted that they were unarmed civilians. Researchers (O’Leary and McGarry, 1993; Sutton, 1994) argue that this incident greatly contributed to an atmosphere of distrust in the government among Irish Catholics and prompted one IRA member to acknowledge only half jokingly that “the British security forces are the best recruitin’ officer we have” (Geraghty, 2000: 36). Because Bloody Sunday occurred only 6 months before Operation Motorman began, we set its effect to end at the beginning of Motorman to guard against the possibility that Bloody Sunday after this period is picking up effects that are really caused by Motorman. Note that this coding strategy is conservative in the sense that if Bloody Sunday continued to have effects after the start of Motorman, then they may reduce any deterrent effects of Motorman. According to table 1, over 9 percent of all republican attacks occurred during the 5-month period after Bloody Sunday.

Because prior research (Soule, 1989; White, 1997) has emphasized the retaliatory nature of loyalist and republican attacks in Northern Ireland, we include a measure of the total number of loyalist attacks in the previous calendar month (from 725 total loyalist attacks). Again, we measure loyalist attacks *prior* to the current attack to reduce biases from endogeneity. According to table 1, an average of more than three loyalist attacks occurred per month for the period spanned by the data when weighted by the number of attacks the following month.

We also include a measure of the success of each terrorist strike based on the specific methods employed. For example, for a successful bombing, the bomb detonates and destroys property and/or kills individuals, whereas an unsuccessful bombing is one in which the bomb is discovered and defused or detonates early and kills the perpetrators. We did not have

6. We also examined separate measures of troop strength depending on the source, which could be Great Britain, Royal Ulster Constabulary, and Ulster Defence Regiment. Because none of the variations produced substantively different results, we report here only results for total army strength.

sufficient information to judge success in terms of the long-term goals of the perpetrators. For example, a bomb that exploded in a building was counted as a success even if it did not succeed in bringing the building down. Based on this broad definition of success, nearly 90 percent of the republican incidents were coded successful. The number killed measures the total number of deaths directly attributable to each incident. Here we observe that less than one death occurred for each attack.

Because terrorist attacks might rely on the momentum of previous attacks, we include a measure of attack density. This measure represents the number of days covered by the three most recent attacks, which includes the current attack. This variable is important because it is likely that earlier terrorist attacks led to changes in the length and timing of the six interventions as well as to changes in the troop level and the number of loyalist attacks. By adding attack density as a control, we reduce bias caused by possible endogeneity between earlier attacks and the timing of the interventions. We observe from table 1 that the average number of days between three attacks was more than six. Note also that the largest 3-day spread was 315 days, which was measured in June 1970. Because this variable requires a lag, the first two attacks are dropped.

To control for changing rates of non-terrorist-related crime and criminal violence over time, we include the annual number of crimes reported by police in Northern Ireland (British Home Office, 2005). Also, because the proportion of incidents in the GTD that were obtained from the Conflict Archive on the Internet (CAIN, 2005) database varied over time, we include a dummy variable to indicate the source of the information in the database.

Finally, to measure the possibility that the impact of the interventions varied over time, we include as a main effect the monthly count since the beginning of the analysis in January 1969. The mean number of attacks occurred 156 months into the series in December 1981, and the median occurred 6 months earlier just prior to June 1981.

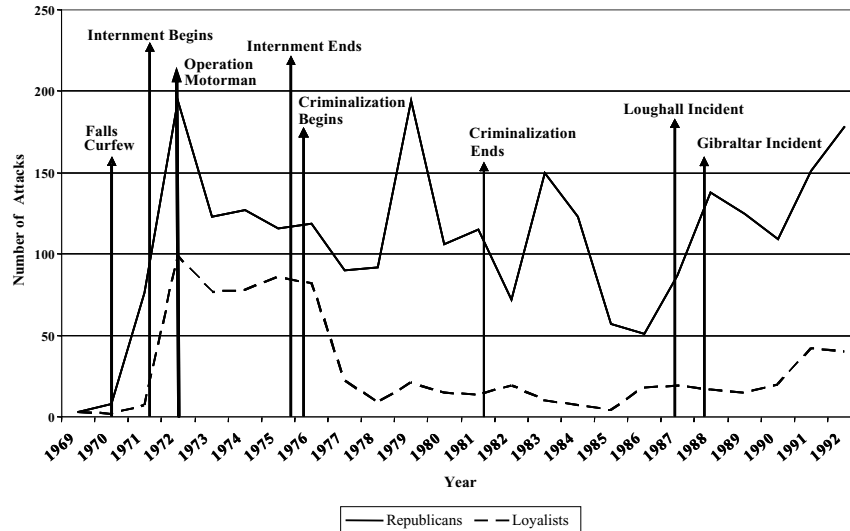
RESULTS

Figure 1 shows trends in terrorist activity over time attributed to either republican or loyalist groups. As expected, republican strikes are far more common than loyalist strikes. And confirming our decision to control for loyalist strikes in the analysis, note that republican and loyalist strikes are significantly correlated ($r = .46$, $p = .02$). We observe a large increase in republican attacks from the beginning of the series in 1969 where republicans only perpetrated 3 acts to a peak of nearly 200 acts in 1972. Then rates decrease substantially in the mid-1970s but increase rapidly again with a peak in 1979. Republican rates again declined somewhat during the

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first half of the 1980s. However, in the mid-1980s, another major escalation is observed with republican attacks reaching a peak during the last year of the series.

Figure 1. Terrorist Attacks by Republicans and Loyalists, Northern Ireland, 1969 to 1992 (N = 24)



To allow comparisons, figure 1 includes the timing of each of the six British interventions examined here. During the first 2 years of the British occupation, authorities responded with two major counterterrorist interventions (Falls Curfew and the internment). Directly after these interventions, terrorist activity increased but then decreased somewhat until increasing again in the late 1970s. Operation Motorman was initiated during the fourth year of the British occupation. Figure 1 shows a substantial decline in republican attacks after Motorman. Although this decline is interrupted by a spike in attacks between 1978 and 1980, there seems to be a downward trajectory in republican attacks for many years after the initiation of Operation Motorman.

Just after the internment ended, the criminalization/Ulsterization intervention began. And shortly after criminalization/Ulsterization was implemented, a major increase was observed in republican strikes. The last two interventions (Loughall and Gibraltar incidents) occurred in the middle of the major increases in terrorist violence that began in the mid-1980s. In both cases, republican attacks seem to increase. Based on inspection of figure 1, Operation Motorman seems to be the best candidate for a deterrence effect, and the other interventions all seem to be good candidates

for backlash effects. However, without controlling for the other interventions and variables, these findings are speculative, and therefore, we turn to the multivariate analysis.

HAZARD MODEL RESULTS

Table 2 shows that four of the six interventions are statistically significant. Recall that significant negative coefficients support deterrence interpretations and significant positive coefficients support backlash interpretations. Overall, the results show three significant backlash effects and one significant deterrence effect. The hazard of subsequent attacks increased significantly after internment, criminalization/ulsterization, and the Gibraltar incident. The sensitivity analysis (appendix A) shows that the Gibraltar finding is robust even when the end date is extended to 36 months. The hazard of subsequent attacks decreased significantly after Operation Motorman. In the tests for interaction between interventions and time in months only, the Loughall incident was marginally significant ($p < .10$) and is therefore the only incident by time interaction reported. The results for the Loughall incident suggest that it had an increasing deterrent effect that was nearly significant ($p < .10$), as shown by the negative interaction term. However, the sensitivity analysis that examines the effects with end dates out to 36 months (appendix A) shows that the impact of Loughall falls to zero and remains there when we extend the end date beyond 14 months. The Falls Curfew had neither a deterrent nor a backlash effect. This interpretation is confirmed by the sensitivity analysis (appendix A) that shows the Falls Curfew slightly under and then slightly over the zero effect base line as we extend the end date up to 36 months.

Interestingly, the total troop strength failed to significantly affect the hazard of terrorist attacks. This result is in contrast to the highly publicized Operation Motorman—which was itself a measure of troop strength. The sensitivity analysis of Motorman (appendix A) confirms a negative (i.e., deterrent) effect regardless of the end date through the 36 months tracked. In a separate analysis (available on request), we found that troop strength was negative and nearly significant ($p < .10$) when Operation Motorman and the other interventions were excluded from the model.

The remaining significant findings follow our expectations. The Bloody Sunday incident had a significant positive effect on terrorist attacks. The lagged measure of loyalist attacks had a positive effect on republican attacks, although it fell short of statistical significance ($p < .10$). This finding is in line with the correlation between republican and loyalist attacks shown above, but it also suggests that the extent to which republican attacks were purely retaliatory should not be exaggerated. Also, our measure of attack density was significant and negative. Thus, when the three most recent attacks occurred over a short period of time, the hazard of

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Table 2. Coefficients for Cox Proportional Hazard Models, Violent Terrorist Attacks by Republicans, Northern Ireland, 1969 to 1992 (N = 2,600)

	Coefficient Estimates	Standard Error
Interventions		
Internment	.627**	.198
Criminalization/Ulsterization	.349**	.130
Falls curfew	-.513	.430
Operation Motorman	-.612*	.244
The Loughall incident	17.894†	9.749
Loughall × monthly count	-.079†	.043
The Gibraltar incident	.469**	.161
Controls		
Troops last year (thousands)	-.001	.012
“Bloody Sunday”	.832**	.232
Loyalist attacks	.021†	.012
Success	-.048	.100
Number killed	-.015	.025
Attack density	-.017**	.004
Crimes reported	.001	.001
CAIN	-.102	.082
Months	.002†	.001

† $p \leq .10$. * $p \leq .05$. ** $p \leq .01$ (two-tailed).

another attack was high. Finally, our measure of total months was positive and nearly significant ($p < .10$), which indicates a generally positive trend in attacks from the start of the series to its end.

DISCUSSION AND CONCLUSION

In this article, we identified six highly visible British interventions aimed at reducing terrorist violence by republicans in Northern Ireland and tested their impact on subsequent attacks from 1969 to 1992. Based on a region about the size of Connecticut with a population of about 650,000, the republicans launched nearly continuous strikes mostly in Northern Ireland during this entire period. These republican strikes made Northern Ireland the most politically violent region in the European Community (later the Union). In duration, the conflict that began in the late 1960s outranks all others in twentieth-century Ireland, and only the Irish Civil War exceeds it in lethality (O’Leary, 2005: 236).

Overall, we conclude that three of the six interventions produced backlash effects; they were followed by an increased risk of future attacks. During internment, a total of 2,158 orders of imprisonment were signed

(Kelley, 1988: 155). Erroneous arrests of nonterrorists during the implementation of the internment policy seriously undermined the legitimacy of the operation. Additionally, the policy of internment without trial represented an obvious departure from widespread norms of rule by law, which is deeply ingrained in western liberal democracies.

The criminalization/Ulsterization policy was meant to treat paramilitary prisoners convicted of political-violence-related offenses as ordinary criminals, rather than giving them special status as political prisoners. In response, imprisoned republicans first mounted “blanket” protests (prisoners refused to wear prison uniforms, wrapping themselves instead in blankets) and “dirty” protests (prisoners smeared excreta on their cell walls; Coogan, 1980) before beginning a hunger strike on October 27, 1980. McEvoy (2001: 105–6) succinctly describes the impact of the hunger strike on British efforts to criminalize the protesters: “Prisoners who were willing to starve themselves to death were clearly able to expropriate power, denigrate legitimacy, and politically contest the position of an intransigent British government.” Because these two interventions began at exactly the same time, we cannot reach any conclusions about whether the strong positive effects were produced by criminalization, Ulsterization, or some combination of the two. However, we do know that allowing the effects of Ulsterization to continue beyond the end of criminalization did not change the substantive conclusions reported here.

The loss of life suffered by the IRA in the Gibraltar incident at the hands of the British military was easily construed by activists as brutal overreaction. This event made it relatively simple for the republicans to portray those murdered as martyrs (Geraghty, 2000; O’Leary and McGarry, 1993). The Gibraltar incident was still associated with positive increases in terrorist attacks 36 months after it occurred. By contrast, the results failed to show any long-term change in the risk of new terrorist attacks after the Loughall incident.

Given the controversial nature of the searches that took place under the Falls Curfew, its failure to reduce terrorist attacks is hardly surprising. The main stated purpose of the Curfew (Campbell and Connolly, 2003) was to disarm and incapacitate potentially dangerous activists. However, it seems likely that any strategic benefits were overshadowed by the unprovoked ransacking of private homes and the killing of civilians.

Operation Motorman provides the strongest evidence for a deterrent effect among the six interventions examined. In fact, Smith and Neumann (2005: 413) concluded that Motorman “shattered the IRA’s military bargaining strategy” and propelled “the republican movement down a path that would eventually lead it to question the value of its armed struggle.” The fact that Peroff and Hewitt (1980) did not find significant effects of Motorman may be because they tracked the length or severity of Catholic

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riots in their analysis rather than total terrorist attacks. The results of the sensitivity analysis for Operation Motorman suggest that it still had a strongly negative association with terrorist attacks 36 months after its implementation began.

Although we have struggled to assemble a comprehensive longitudinal database on terrorist attacks in Northern Ireland, our study has several limitations. First, like most macrolevel tests of deterrence models, we have no perceptual data that would allow us to examine the individual motivations of those who commit terrorist violence. In fact, data on the individual motivations of perpetrators and supporters are especially difficult to collect, but such information would allow researchers to improve their understanding of how those who resort to terrorist tactics actually interpret policies and sanctions. Second, although the database examined here includes many variables shown by prior research to be associated with terrorist attacks, it is certainly plausible that other variables not available to us would be useful. For example, it would be helpful to have annual measures of changing levels of public support for various types of British counterterrorist campaigns during the period spanned by the data. Finally, although we have argued that Northern Ireland during this period offers a strategic opportunity for contrasting deterrence and backlash models, it is important to emphasize that this is a case study. We do not know whether government counterterrorist strategies have had similar effects in other regions of the world, on different groups or individuals, or even in this region of the world during different time periods.

This last point could lead to useful avenues for additional research. Because our analysis was limited to Northern Ireland, we could not examine measures that are likely to generate deterrence or backlash responses in different types of political conflicts. For example, criminology research for decades has shown that the certainty of punishment is likely to be more effective than the severity of punishment in terms of deterring potential misbehavior. It is tempting to speculate that the reason why Operation Motorman was more effective in reducing future terrorist strikes than the other interventions is that it succeeded in raising the perception that acts of rebellion and violence would be met with an immediate response. Despite the fact that Motorman was a massive operation—involving 38 army battalions, 27 infantry battalions, and 2 armored battalions—it was not a particularly violent operation. It met with little resistance, and only two people lost their lives during the operation. But it greatly increased the British military presence in Northern Ireland and, thus, the certainty that republican strikes would have important consequences. By contrast, the far more limited but in some ways more severe methods used in the Loughall and Gibraltar operations produced more evidence of backlash than deterrence. Equally interesting is the fact that

the highly visible announcement of the deployment of troops in Motorman had a more important impact on subsequent terrorist attacks than a measure of actual troop strength.

Although we offer the first statistical comparison of deterrence and backlash models of terrorism in Northern Ireland of which we are aware, the basic finding that the imposition of harsh criminal justice and military interventions to reduce terrorism may be counterproductive is by no means new (Collins, 2004; Geraghty, 2000; Kenney, 2003; Lichbach, 1987; Malvesti, 2002; Nevin, 2003; Soule, 1989; Turk, 2002). Given the evidence that deterrence-based thinking with regard to terrorism is often demonstrably unsuccessful, we must ask ourselves why it remains the most common reaction of governments to terrorist threats. One obvious answer is that governments that experience terrorist strikes face tremendous pressure to take some action and often few obvious alternatives to harsh military or criminal justice interventions are available. The British began to experiment with a variety of counterterrorist strategies after their forces moved into Northern Ireland in 1969. This research suggests that only one of the interventions examined here showed signs of significantly reducing risks of more republican violence. Of course, the British did eventually find other strategies—bigovernmentalism, consociation, and federalizing institutions—that have brought hope for a permanent peace (O’Leary, 2005).

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Gary LaFree is a professor of criminology and criminal justice and the director of the National Consortium for the Study of Terrorism and Responses to Terrorism (START) at the University of Maryland. Much of his recent research has dealt with national and international macrolevel trends in political and criminal violence. Related work underway includes studies of U.S. and global homicide trends, global trends in terrorism, the

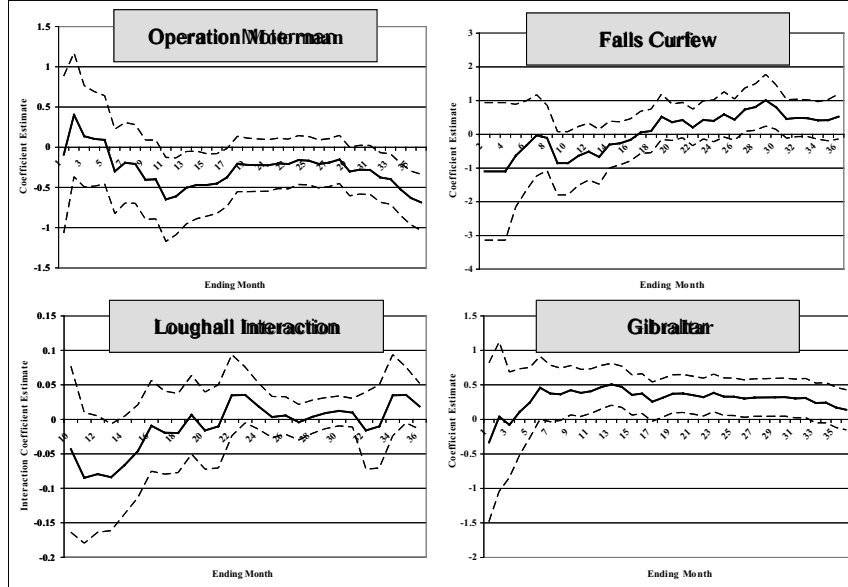
sudden desistance of terrorism, connections between terrorism, democratization and failed states and the effectiveness of counter terrorist strategies.

Laura Dugan is an associate professor in the department of criminology and criminal justice at the University of Maryland. She is an active member of the National Center for the Study of Terrorism and the Response to Terrorism, the National Consortium on Violence Research, and the Maryland Population Research Center. Her research examines the consequences of violence and the efficacy of violence prevention/intervention policy and practice. She also designs methodological strategies to overcome data limitations inherent in the social sciences.

Raven Korte received her Master of Arts degree in criminology and criminal justice from the University of Maryland. She is now a special agent with the Naval Criminal Investigative Service (NCIS) assigned to the counterterrorism unit in Naples, Italy.

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Appendix A. Sensitivity of Military Interventions to End Dates



NOTE: Before specifying final models, we first examined whether the slope of each intervention differs from the overall trend by including an interaction for each. A series of likelihood ratio tests confirmed that the only interaction that contributed to the model was for the Loughall incident. Thus, for Operation Motorman, Falls Curfew, and Gibraltar, we track main effects; for Loughall we track the nearly significant interaction effect. The horizontal axis represents the number of months for which the intervention was measured after it ended. The solid lines show coefficient estimates, and the dashed lines show the upper and lower 95% confidence bounds. Because the large standard errors in the Loughall interaction model made it difficult to observe longer term effects, in the figure above, we highlight months 10 through 36.