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# **Does Indiscriminate Violence Incite Insurgent Attacks? Evidence from a Natural Experiment**

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**Abstract:** Does a state's use of indiscriminate violence incite insurgent attacks? Nearly all existing theories and empirical studies conclude that such actions only fuel insurgencies by provoking insurgent mobilization. This proposition is tested using a natural experiment that draws on random artillery strikes by Russian forces in Chechnya (2000-05) to estimate the impact of indiscriminate violence on subsequent insurgent violence. A difference-in-difference (DD) estimation method is adopted in which shelled villages are matched with similar non-repressed settlements over identical time periods to estimate treatment effects. The findings are counterintuitive. Shelled villages and their home districts (raiony) exhibit less post-treatment violence than control groups. In addition, commonly-cited "triggers" for insurgent retaliation, including the lethality and duration of indiscriminate violence, are either insignificant or negatively correlated with insurgent attack propensity.

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Well, we disturb the locals, but there is nothing to be done. This is a war, you know.

Russian Artillery Officer, Chechnya, November 2003

### 1 Introduction

Does a state's use of indiscriminate violence incite insurgent attacks? At first glance, the answer would appear obvious. Indeed, one recent review cites no fewer than 100 studies and 45 historical cases in which a state's reliance on collective targeting of the noncombatant population provoked greater insurgent violence (Kalyvas, 2006, 146-72). Indiscriminate violence, it is argued, creates new grievances while destroying economic opportunities among fence-sitters in a population, thus leaving aggrieved parties few options other than a resort to arms.<sup>1</sup> As a result, state brutality plays a central role in current theorizing as a catalyst that sparks retaliation, which, in turn, fuels new rounds of violence that increase the war's destructiveness. Once set in motion, these escalatory dynamics are difficult to arrest, often resulting in the state's defeat as its resources and willpower become exhausted.

Surprisingly, however, we possess almost no systematic investigation of indiscriminate violence's impact on subsequent insurgent behavior. Data limitations are partly to blame. Crossnational data, often pitched at the annual level, are too aggregate to capture the outcome of microlevel state-insurgent interactions (Sambanis, 2004). Collecting the necessary data is, of course, an often dangerous, if not impossible, task. In addition, conflict data is the product of strategic interaction rather than experimental design. Severe problems

<sup>&</sup>lt;sup>1</sup>Indiscriminate violence is defined as the collective targeting of a population with excessive means, unpredictable timing, and without credible efforts to separate combatants from civilians. Here, such efforts aim at population control, not extermination. "Pacification" campaigns, aerial bombardment, and extrajudicial mass killings are all examples of state-orchestrated indiscriminate violence.

stemming from simultaneity bias and selection effects are therefore likely to be present in existing observational studies that, if not explicitly addressed, will yield mistaken causal inferences.

This paper uses a natural experiment (Posner, 2004; Miguel, 2004) created by Russian counterinsurgency practices in Chechnya (2000-05) to test the presumed relationship between indiscriminate violence and insurgent action. Random artillery strikes on some, but not all, populated settlements act as a "treatment" that permits difference-in-difference (DD) estimation of post-treatment insurgent attack propensities between matched treated and control groups (Rubin, 2006).

Contrary to existing studies, this study finds that (1) indiscriminate violence actually reduces insurgent violence; (2) that this negative relationship holds across the repressed villages's larger home districts, suggesting a diffusion effect is at work; and (3) that commonlycited "triggers" for insurgent attacks, including casualties and damage inflicted, are often *negatively* correlated with insurgent violence. These findings directly challenge current views of state-directed indiscriminate violence as a causal mechanism with a one-sided (i.e. positive) impact on insurgent behavior.

The article proceeds as follows. The first section offers a critique of the dominant "spiral" model of violence in civil war. The next section examines why indiscriminate violence may be a logical, if morally appalling, means of reducing insurgent violence. A third section details the natural experiment, including data, internal validity checks, randomization mechanisms, and matching procedure. The fourth section uses DD estimation to assess treatment effects on insurgent violence at the village and district levels. The impact of variation in treatment lethality, damage, and duration is also examined. A fifth section addresses possible criticism of the study's findings. A final section concludes with thoughts for future research.

### 2 Strategic Interaction and Violence in Civil War

Nearly all studies of civil war rest, either explicitly or implicitly, on the assumption that violence is the product of repeated interaction between strategic actors. These actors — normally, an "incumbent" (usually the government), rebels, and the public — typically find themselves trapped within an escalatory "spiral" of violence (Posen, 1993) as each side's actions create incentives for retaliation. In turn, each reprisal simultaneously widens the war geographically and intensifies its brutality as more members of each side are drawn into the conflict. Violence, in this model, begets violence, with incumbent indiscriminate violence acting as the chief mechanism behind this escalatory process.

A graphic example of this escalatory logic is provided by Liakhovo, a Russian village occupied by German forces in 1941.

The elder of the village of Liakhovo, together with some villagers and German soldiers, robbed a partisan base. The next day the partisan detachment demanded that Liakhovo's peasants return all that had been taken. The elder promised, but the next day tried to hide and was caught on the road and killed. The German HQ sent soldiers to the village... The partisan detachment destroyed the German convoy with seven men. After this, German soldiers razed the settlement to the ground with tanks.(Hill, 2005, 52)

Scholars seeking to draw a link between German repression and the rise of the Soviet partisan movement would seemingly find ample support here. Indeed, archival evidence reveals that the number of partisan bands in Liakhovo's *oblast'* rose from 39 to 74 after its sacking. By 1944, some 24,202 locals had joined the partisans (Hill, 2005, 78,174).

Yet the fact that incumbent and insurgent strategies are interdependent, and that violence is a joint outcome, raises several methodological red flags.

Take, for example, the problem of simultaneity (endogeneity).<sup>2</sup> The example of Liakhovo illustrates how difficult it can be to determine whether incumbent repression was the *cause* of insurgent actions or a *response* to previous patterns of insurgent violence.

 $<sup>^{2}</sup>$ Endogeneity occurs when independent variables are a consequence or response to the dependent variable rather than a cause.

Where we cut into the causal chain of interdependent events can substantially alter our inferences (Manski, 1995, 110-26). This problem only intensifies as events accrue — there are at least six state-insurgent interactions in the simple Liakhovo example — unless we are fortunate enough to observe a random and external intervention in the cycle of violence.

Indeed, the failure to note that increases in insurgent attacks can also lead to greater repression will produce mistaken inferences. We must recognize that (1) the conditional probability that a population suffered indiscriminate repression given an insurgent attack is not the same as (2) the probability that these inhabitants will organize attacks given repression. Too often, however, these probabilities are treated as identical.

We also know from studies of deterrence in international relations that there are severe selection effects present when we only observe failures (Achen and Snidal, 1989). A similar problem is present in civil war studies, where victim-turned-insurgent testimonials figure prominently. This evidence is often used to assert the causal link between incumbent excess and insurgent attacks. Without the negative cases — that is, people who considered becoming insurgents but decided otherwise — we create sample selection bias.

There are two, perhaps unobservable, counterfactuals at work here. First, we would want to know how many more insurgents would have been created had violence *not* been used. Perhaps for every "new" insurgent created an unknown number of fence-sitters tipped the other way and chose not to take up arms. These individuals, however, are invisible to most data collection efforts since they are non-events. By itself, the fact that some individuals become insurgents after victimization does not necessarily mean that coercion "failed" since the unobserved majority of potential insurgents may have foregone participation in the war.

Second, we would (ideally) also have sufficient data to match Liakhovo with a comparable, but non-repressed, village, to examine changes in patterns of insurgent violence. More specifically, we need not only Liakhovo's baseline of insurgent violence prior to German repression but also data from a similar village over the same timeframe if we are isolate repression's independent causal effect. Although not yet adopted in civil war studies (Ward and Bakke, 2005), matched research designs help control for the heterogeneity present in sample populations not derived by experimental methods. Without these case controls, it is difficult to assess whether the observed "surge" in insurgent behavior after Liakhovo's destruction represented an increase or decrease in attack propensity.<sup>3</sup>

### **3** A Theory of Indiscriminate Violence

Given the intuitive logic of this spiral, why would an incumbent risk setting it in motion? To some, these strategies are proof of desperation (Downes, 2006) by an incumbent faced with an entrenched insurgency (Hultman, 2007; Valentino and Balch-Lindsay, 2004). Others suggest that indiscriminate repression is the product of non-rational causes such as unit breakdown (Humphreys and Weinstein, 2006; Azam, 2002), institutional culture (Shepherd, 2004), or the absence of sufficient information to sift insurgents from the population selectively (Kalyvas, 2006).

Whatever its origins, scholars largely agree that indiscriminate violence is counterproductive because it facilitates insurgent mobilization. It does so by creating new grievances that radicalize fence-sitters within the population (Lacquer, 1998). The destructiveness of indiscriminate violence also lowers costs of participating in an insurgency by ravaging the economy, foreclosing alternative opportunities besides insurgency (Collier, 2004). Such tactics also signal that the government is insensitive (at best) to the suffering of the repressed population (Bueno de Mesquita and Dickson, 2007). This is especially so since mobile insurgents are typically able to sidestep government violence, leaving the blow to fall squarely on noncombatants. Government overreaction also enables insurgents to manipulate state power by provoking disproportionate reactions in areas not yet controlled by the insurgency (Kalyvas and Kocher, 2007; Leites and Wolf, 1970, 112-118).

Yet if we entertain the idea that existing studies may have misread the impact of indis-

<sup>&</sup>lt;sup>3</sup>In fact, Hill (2005, pp.78, 169-70) concludes that Nazi repression succeeded in *suppressing* insurgent violence in this *oblast*' until autumn 1943, when the war's turning tide became apparent. The "surge" noted after Liakhovo's destruction was actually due to a halving of existing partian bands: German violence had made it too dangerous to concentrate in large groups.

criminate violence, it becomes clear that our theories also possess an impoverished view of state strategies during civil war. The assumption that such violence almost never "works," and thus must be the function of non-rational causes, has largely pushed the state into the background as an (hapless) accelerant of insurgent violence. But it is plausible that states deliberately choose to target noncombatants as a means of defeating an insurgency. Such actions, while morally appalling, have a clear strategic logic: they decrease the efficiency of insurgent efforts to control, recruit, and fight over a given population.

First, it should not be surprising to note that most people are cowed most of the time by indiscriminate violence. Typically, however, our existing theories make heroic assumptions about individual motives, suggesting that actors invariably act on grievances at a rate greater than replacement. It is equally plausible, however, that indiscriminate violence actually terrorizes the bulk of the population, both directly (in terms of losses) and indirectly (in terms of implied future pain). Anger is of course generated, but this is not incompatible with increased rates of denunciation of insurgent forces, even collaboration with incumbent forces, if such actions hold out the promise of escaping similar fate in the future. Behavioral, rather than preference, change, is the objective of such violence.

Repression can drive a wedge between insurgents and populace by persuasively demonstrating the insurgents' inability to credibly protect would-be supporters. This, in turn, makes it more difficult for insurgents to exercise control. Indeed, noncombatants may appeal to insurgents to abandon their settlements or to change their tactics to avoid involving civilians. The prospect of future repression can therefore hasten collaboration with the incumbent in the near term.

At the extreme, indiscriminate repression decreases insurgent violence by encouraging populations to flee, thereby shrinking the "sea" that shelters insurgent "fish" (Azam and Hoeffler, 2002; Valentino and Balch-Lindsay, 2004; Tse-tung, 2000, 93). In South Vietnam's Dinh Tuong province, for example, massive shelling provoked rural depopulation, dismantling Viet Cong support networks. "People hated the Americans a lot," an insurgent cadre noted, "but they are also frightened," so they moved to government-run camps.

As a result, "the pacification campaign shrank [safe] areas bit by bit, like a piece of meat drying in the sun" (Elliot, 2002, 911-20, 1156-1164, quote on p.1178).

Insurgents are thus left with a crippled tax base that undermines their military capability. Indeed, in some situations, indiscriminate repression offers an incumbent the opportunity to bolster its capabilities at insurgent expense. Looting, for example, enables weak incumbents to acquire capital and lower reservation wages for joining its military (Azam, 2006, 2002) while simultaneously weakening its insurgent foe.

We might imagine that state brutality merely redistributes insurgent violence spatially. Refugee camps, for example, are often cited as surrogate bases for displaced and aggrieved populations (Salehyan, 2006; Lischer, 2005). But this logic works in the opposite direction, too. Lessons of brutality diffuse, short-circuiting the spiral model's escalatory logic as non-victimized populations observe the consequences of incomplete compliance. Deterrent effects also extend beyond the original target, thus raising costs for non-compliance while cementing the incumbent's reputation for resolve among neighboring populations.

The combination of a terrorized population and declining control intensifies the insurgents' recruitment dilemma. Repression makes it difficult to persuade would-be insurgents that the incumbent is losing or that their families will be protected from future reprisals. These difficulties only multiply if the insurgents react to incumbent violence by targeting the civilian population to punish it or demonstrate resolve. For example, the FLN practice of targeting fellow Muslims proved a potent recruitment device for the French Army: considerably more Algerians sided with French forces than the FLN during the Algeria Civil War (Horne, 1977, 254-55, 321-22).

Finally, indiscriminate violence complicates insurgent strategy. "Free-fire" zones or sweep operations disperse insurgents, making it difficult to establish safe bases or to concentrate forces. Though insurgents are often viewed as operating in small, mobile units, the reverse of insurgent strategy — concentration on an incumbent's weak points — should not be overlooked. Note that this is true even if violence is random since no effective counterstrategy exists for anticipating, and thus avoiding, the costs imposed by repression. As a result, indiscriminate repression may actually undercut insurgent military effectiveness by multiplying logistical difficulties while inhibiting the coordination necessary to respond effectively.

It is theoretically plausible, then, that indiscriminate violence has exactly the opposite impact on insurgent behavior than assumed in current theories of civil war. The next section tests this proposition empirically.

### 4 Research Design

A natural experiment offers one means for disentangling the causal relationship between indiscriminate repression and insurgent attacks in the face of severe simultaneity and selection biases. This approach, now gaining greater currency in political science, consists of (1) a treatment or intervention that is (2) applied randomly and exogenously to (3) part, but not all, of a sample thought representative of a broader population. Ideally, two conditions hold: (1) the treatment is not correlated with population characteristics, including past behavior; and (2) the treated and control groups are (nearly) identical, allowing us to isolate the independent effect of receiving the treatment.

I use random artillery strikes by Russian forces on populated centers in Chechnya (2000-05) as a "treatment." Unconnected to village attributes, this shelling created control and treated villages that were then matched on key attributes to reduce bias in our estimates of treatment effects. A difference-in-difference (DD) design was then used to measure the changes in pre- and post-strike insurgent violence across these populations over identical time frames. DD estimation was then repeated at the larger district level to measure whether treatment externalities such as spillover effects change the behavior of the shelled village's neighbors.

The second Chechen War represents a "most likely" case (Eckstein, 1975) for observing the link between indiscriminate repression and increased insurgent attacks. The war has witnessed astonishing levels of brutality by both sides and has often been described in escalatory terms as each side's violence radicalized the other's tactics and aims (Hahn, 2007; Wilhelmsen, 2005).

The war began in August 1999 when two Salafist insurgent commanders, Shamil Basayev and Khattab, launched an invasion of neighboring Dagestan from their Chechen bases (Souleimanov, 2007; Evangelista, 2002). Seeking to construct an Islamic Khanate, and badly misjudging public support for their ambitions, Basayev and Khattab's forces were quickly driven back into Chechnya by Russian and local forces. In turn, the Russian Army's reentry into Chechnya in October 1999 sparked the mobilization of Chechen insurgent groups. A series of brutal urban battles ensued, ending in June 2000 with the insurgent's abandonment of direct battle. Since then, the war has degenerated into a grinding (counter-)insurgency that has tied down nearly 80,000 Russian soldiers in an area the size of New Jersey (Kramer, 2005/06).

The conflict has become synonymous with excesses by Russian forces and their pro-Russian Chechen allies. Human Rights Watch, the European Court of Human Rights, and local NGOs have issued a stream of reports decrying the use of indiscriminate violence by these forces, including artillery and air strikes on populated places. Village sweep operations (*zachistiki*) are routinely marked by forced disappearances (about 5,000 since 1999) and extrajudicial killings (Human Rights Watch, 2002*a*,*b*, 2006). Insurgents have retaliated with suicide bombings, mass hostage-takings, and a relentless campaign of hitand-run strikes against Russian patrols. An estimated 15,000-25,000 civilians and at least 5,000 Russian soldiers have died since 1999; roughly 100,000 citizens were also temporarily internally displaced. As a "small corner of Hell," (Politkovskaya, 2003) Chechnya would appear a clear example of indiscriminate repression fueling an insurgency.

#### 4.1 Identification Strategy: Doctrine and Drunks

The treatment consists of 158 random artillery strikes from two Russian bases — Shali and Khankala — in Chechnya. Following Russian standard operating procedures, each base houses three detachments of six 152mm 2A65 field guns, each with a range of 30 kilometers (*Jane's Armour and Artillery* 2006). Though technically secret, base locations were identified using Arcview 9.1 GIS software by drawing 30km radial plots from each strike's location to observe clusters where the plots intersected. Satellite imagery was then used to confirm each base's location and to rule out alternative sites.

As Figure 1 illustrates, the sample consists of all populated centers (N=129) and districts (N=10) falling within range of at least one base's artillery.<sup>4</sup> Data were drawn from Russian and Western human rights organizations, official Russian press releases, rebel websites, and local and national newspapers: some 20 sources in four languages (Russian, English, French, and Chechen) were used. All artillery strikes occurred during the war's counter-insurgency phase (June 2000-December 2005). In total, 71 populated centers were struck at least once; the control group consists of 58 centers. The total dataset records 774 annual observations at the village level and 60 at the district level.

#### [Figure 1 about here.]

These artillery strikes were responsible for at least 159 deaths and 212 wounded citizens. In addition, many buildings were either destroyed (88) or damaged (161), and numerous farms as well as herds of livestock were destroyed. This is clearly only a fraction of the total violence visited on Chechnya's population.

The treatment was distributed via one of two randomization mechanisms. Shali's fire, which accounts for 71% of all shelling, derived its randomness from Russian military doctrine. This base's central purpose is to suppress insurgent behavior using a standardized barrage pattern known as "harassment and interdiction (H&I)." H&I fire is an ideal treatment: it is explicitly designed to consist of barrages at random intervals and of varying duration on random days without evidence of enemy movement. H&I fire was, and remains, a staple of Soviet (Lebedev, 1984, 373-75) and Russian artillery practices ("Report by the Chief of Artillery," *Grani.ru*, 14 December 2000).

<sup>&</sup>lt;sup>4</sup> Collectively, about 34% of Chechnya (5272km<sup>2</sup>) is within range of at least one base. The bases are 19km apart and possess overlapping fields of fire of about  $380 \text{km}^2$ . When assessing distance, I allowed for a +2km margin of measurement error to account for wind and imprecision in village location.

In effect, H&I fire approximates the lottery mechanism commonly used in natural experiments. The purpose of this "disturbing fire" (*bespokoyashchii ogon*') is simple enough: it restricts insurgent mobility by raising the costs of passage across terrain. It creates the possibility of being caught in a sudden strike, for example, while complicating insurgent strategy since the shelling's location and duration remains unknown and unpredictable. These same properties, however, also make H&I fire lethally indiscriminate for noncombatants trapped within its barrage pattern. Such tactics not only kill and maim but also scatter unexploded ordinance (UXO) throughout agrarian lands and forests, rendering them unusable (e.g., "Villages Shelled, Elderly Person Dies," *Prima-News*, 6 March 2003). In one graphic example of H&I's consequences, humanitarian organizations have shipped firewood to four heavily forested districts inside Shali's operating radius since 2001 because the forests are littered with UXO (*Landmine Monitor* 2006).

At Khankala, Russia's main base in Chechnya, the remaining shelling (29%) was due to soldier inebriation. Russia's military forces in Chechnya are notorious for indiscipline, with drunk (or high) soldiers often participating in combat operations. Khankala itself is distinguished by its possession of Chechnya's worst traffic safety record due to soldiers driving their armored vehicles while inebriated (e.g., "Bronirovannye ubiitsy," *Chechenskoe Obshchestvo*, 22 February 2006).

We can deduce that Khankala's artillery fire is due to random indiscipline in part because of legal prosecution of drunk soldiers under Chapter 33, Section 349 (Part 1) of the Russian Criminal Code ("Violation of the Rules for Handling Arms and Hazardous Materials"). This chapter punishes soldiers for "weapons abuse followed by infliction of grave bodily harm." Though enforcement is weak, we have recorded prosecutions of soldiers for the "mistaken" discharge of artillery while inebriated (e.g., "Six Civilians Die," *Reliefweb.org*, 17 July 2000; "Chechen prosecutor's office opens criminal case," *RFE/RL*, 16 August 2002; "Aiming Error May Cost Officer," *ITAR-TASS Weekly*, 11 November 2005). Soldiers have even shelled themselves accidentally ("Zdes' zhivut liudi," *Memorial*, July 2000). We also have eyewitness testimony from both Russian officers and residents of the shelled villages. As Aslan, a company commander, put it, soldiers "get drunk as pigs, lob out a few shells, claim combat pay and get drunk again" (*Time*, 24 October 2000). One village leader noted after a strike that "I'm sure there was no necessity in this shelling. As a rule, they fire every time they get drunk" ("Settlement was shelled," *Memorial*, November 2005). Villagers often petition Russian authorities to cease fire, citing drunkenness as the motive behind the wanton violence (e.g., "Otkrytoe Pis'mo," *Groznenskii Rabochii*, 19 July 2001).<sup>5</sup>

#### 4.2 Internal Validity

Randomization eliminates many threats to internal validity, including bias from selection and maturation effects, by distributing the treatment without regard for group properties (both observed and unobserved) possibly correlated with insurgent violence (Cook and Campbell, 1979, 50-58). The treatment's clearly non-voluntary nature also eliminates selection bias arising from partial compliance (Horiuchi and Taniguchi, 2007).

But can we be sure that the treatment is random and exogenous? Any correlation with an unseen variable will bias estimates of treatment effects by inducing changes in the subject populations that may skew subsequent behavior. Perhaps, for example, Khankala's shelling is correlated with payday; that villagers know when payday falls; and that they adopt specific behaviors (e.g., hiding in basement shelters) that alter the post-treatment response in non-random ways.

Yet as Figure 2 illustrates, there are no specific probabilities of a strike attached to a particular day. Their timing is also random: in the 88 strikes with these data, attacks occurred at all hours of night and day, with Shali exhibiting a weak preference for 11pm-7am (with intermittent firing inside this interval). The shortest barrage was one minute; the longest was 56 hours parceled over seven days. Shelling was also distributed fairly

<sup>&</sup>lt;sup>5</sup>Additional evidence on treatment randomness was gathered through interviews with local human rights observers. Due to security and privacy concerns, I do not cite their reports directly. All reports of shelling required at least two independent sources to be included in the dataset.

evenly across conflict years: 35 were recorded in 2000, followed by 32 (2001), 16 (2002), 21 (2003), 26 (2004), and 28 (2005).

#### [Figure 2 about here.]

To guard against a village's unobserved "self-selection" into an artillery strike, I dropped any use of artillery in close support of Russian soldiers during an engagement or where an insurgent attack had been recorded in the preceding 48 hours. This inclusion rule helps minimize possible simultaneity bias. An additional 44 strikes (most likely an undercount) struck non-populated areas and were dropped from the dataset.

Finally, logistic regression using the matched populations reveals that the treatment is not correlated with any village or district characteristics (outlined below). This includes the pre-treatment frequency and mean levels of insurgent attacks once we control for the capital city, Groznyy, and its outskirts.<sup>6</sup> In short, the treatment is random and exogenous.

#### 4.3 Variables

The dependent variable, ATTACK, is defined as an insurgent-initiated attack against Russian or proxy military forces, their local representatives, and civilians. Attacks were plotted to the exact village (if known) and the village's home district. To facilitate pre— and posttreatment comparisons, ATTACK was operationalized by (1) the number of attacks within 90 day windows before and after an artillery strike and (2) the lag time in days between the treatment and the first attack at village and district levels.

I adopted 90-day treatment windows for two reasons. First, prevailing theories assume a tight temporal link between action and reaction, suggesting these windows are sufficient to capture treatment effects. Second, DD estimates of treatment effects are most reliable in the short-to-medium term (Duflo and Kremer, 2007, 17). As the length between treatment and observed response grows, confidence in our measures is diminished since opportunity

 $<sup>^{6}</sup>$ Groznyy is a clear outlier: its population (210,000) and size (186km<sup>2</sup>) dwarf all other settlements, while its status as the capital and its location within both bases' firing range makes it much more likely to experience insurgent violence.

increases for (unobserved) events to intervene. These windows represent a pragmatic compromise: long enough to establish treatment effects, but not so long that causal claims become tenuous.

The substantive meaning of these attacks also needs to be addressed. I interpret a decrease in attacks as evidence that indiscriminate violence is suppressing insurgent violence. In this view, attacks are both means to fight Russian forces directly and to demonstrate resolve to Russian and Chechen audiences. One could imagine, however, that a decrease in attacks means indiscriminate violence is actually ineffective. If insurgents target civilians to force them to fight Russians, for example, then a diminished attack rate may indicate that Russian repression is solving the insurgents' recruitment dilemma. Less attacks would therefore be necessary than in the past (Hultman, 2007).

Though plausible, this is not the case in Chechnya. First, insurgents have rarely directly targeted noncombatants inside Chechnya, though innocent bystanders have been killed during their attacks. Indeed, only 5% of attacks in Chechnya (2000-05) have targeted civilians directly, with annual averages ranging from 2.5% (2005) to 6.7% (2004). Second, insurgents are motivated by a complex system of cultural norms (*adat'*) to seek immediate revenge for injuries inflicted (Souleimanov, 2007, 270-76). It is unlikely that aggrieved parties would not try to average losses, especially if their failure to do so was interpreted by the broader community as a failure of resolve.

Collectively, the villages and districts within the sampling frame account for over 820 known insurgent attacks. These attacks resulted in at least 1699 killed and 2398 wounded individuals. Attack data were drawn from over 35 Russian and Western media sources, including local newspapers, human rights organizations, official releases and casualty reports, interviews, and rebel websites and videos. Though the danger of missing data is always present, especially in a war zone, triangulation across multiple sources helps minimize bias introduced by the particular agenda of any one organization or group.

Ten independent variables are incorporated in this study. TREATMENT is a dummy variable that records whether a populated center or district was shelled. Demographic information was also collected for each village and district in the sample. POPULATION records the log of a village or district population in 2002, the war's midpoint. Since wartime conditions frustrate accurate census counts, I draw on three sources of information: the 2002 All-Russia census, which includes all villages over 3000 individuals but tends to inflate estimates; pre-war estimates of ambulatory capabilities and expected caseloads of regional hospitals and clinics (World Health Organization, 2003); and the size of village wheat shipments delivered by humanitarian organizations in 2002 (Danish Refugee Council, 2002).

Since refugee camps are often cited as facilitators of insurgent violence, I code whether a village or district was home to an UNHCR-sponsored temporary accommodation center (TAC) in 2002. Thirty-one camps were located within the sample population, with 23 in Groznyy, followed by Argun (3), Achkhoi-Martan (3), and Gudermes (2).

We also need to take into account the conflict area's spatial geography. DISTANCE measures the log of the distance in kilometers that a populated center resides from the closest artillery base. District distances are measured from the district's administrative capital (*raitsentr*). TERRAIN is a composite measure that consists of two interacting variables: the log of a village's elevation (meters) and a scaled index of landcover. For the latter, I used LANDSAT imagery and the USGS Land Use Index to create a five-fold index, ranging from sparse vegetation (1), plains or small urban settings (2), large urban centers (3), light forest or transitional zones (4), and heavy forest (5). Village terrain scores are point estimates drawn from a village's location. District terrain scores are averages of a district's four corners. TERRAIN therefore measures the degree of difficulty faced by mechanized Russian forces when attempting to control different regions of Chechnya.

Control of the battlefield can also be an important determinant of insurgent attacks. I adopt Kalyvas' (2006, 421) measure of control zones to delineate the amount of control exerted by the incumbent's forces. DEADGROUND records whether a village is located in a zone of equal control by incumbent and insurgent forces. It is here where we might most plausibly expect fence-sitters to "tip" toward joining an insurgent organization after indiscriminate Russian violence (Gates, 2002). Russian military deployments can also shape the pattern of insurgent violence. Insurgents may opt to attack where Russian forces are weakest, for example. Alternatively, they may seek targets in areas with the highest known concentration of Russian forces to demonstrate resolve or strike a particularly valuable target. GARRISON therefore records the known location of all Russian permanent bases at the village level during 2000-05 (Human Rights Watch, 2006; Memorial and Demos Center, 2007).

Insurgent characteristics may determine attack propensities (Weinstein, 2007; Humphreys and Weinstein, 2006; Wood, 2004). Our sampling frame is dominated by two insurgent organizations with sharply different ideological profiles. Shamil Basayev's units adhere to radical Salafism and exhibit a preference for suicide bombing and mass hostage-takings. By contrast, Doku Umarov's forces are principally nationalist in orientation and have generally eschewed indiscriminate violence. REBEL captures whether a village was controlled by Basayev or Umarov.

Finally, BASE records whether a population was struck by fire from Khankala or Shali. On average, Khankala's fire lasted about an hour, while Shali's extended to nearly eight hours. This facilitates comparison of the impact of treatment duration ("dosage") on insurgent behavior.

#### 4.4 Sample Balancing via Matching

Confidence in our estimates of treatment effects is increased if the treated and control populations are similar along covariates thought to influence insurgent violence. Nearest neighbor pairwise matching with replacement was therefore used to match the 158 treated cases with an equal number of control cases along eight covariates: POPULATION, TAC, DISTANCE, TERRAIN, DEADGROUND, GARRISON, REBEL, BASE. Because there are far fewer district observations, matching occurred on only four variables: POPULATION, TAC, DISTANCE, TERRAIN. MatchIt was used for matching (Ho, 2006).

This matching strategy was chosen over either optimal matching without replacement or full matching (Hansen, 2004) because it provided the largest reduction in standardized bias across the matching covariates. Sampling with replacement yielded better matches due to Chechnya's spatial settlement patterns. The sample's geography contains only one large city, a modest number of medium-sized centers, and a high number of small villages. Due to treatment's random nature, we would lack sufficient observations for the mediumto-large cities if we matched without replacement.

Table 1 summarizes the marked improvement of matched data over the original dataset. A standardized bias below .25 is considered a "good" match (Ho, 2007, 23fn15). All matched covariates at both levels of observation are well under this threshold. The village matching is especially close, with four covariates — TAC, DEADGROUND, GARRISON, and BASE — identical across groups.

Moreover, the substantive meaning of residual standardized bias is small. Treated villages are about 20 individuals larger than control villages (mean village population is 4244, excluding Groznyy). Holding landcover constant, treated villages are approximately 79 meters higher than their counterparts. Control villages are also slightly closer to a base — about 1.25 kilometers — than treated villages. Finally, Basayev's Salafist units are slightly overrepresented in the treated group, with 10 more observations.

Matching at the district level also substantially improves the balance between treated and control groups. The administrative centers of treated districts are an average of six kilometers closer to an artillery base than the control group (mean distance: 28 kilometers). There are 7162 more citizens in a treated district than a control district on average (mean district population: 80,080). Terrain differences are negligible across the groups, while there are slightly more TAC observations in the control (70) than treated (63) group. The upshot of this matching procedure is that we are left with very similar control and treatment groups.

I also controlled for treatment history in two ways. First, once a village was shelled, all subsequent years in which the village was not struck again were removed from the prematched dataset. For example, Tsa-Vedeno was struck on 21 October 2003 and again on 6 January 2005. The year 2004 was thus dropped to prevent it from being used as a control for another village. This inclusion rule guards against inadvertently smuggling treatment effects in as a "control." This is a particular danger if there is a lag between treatment and effect. Since all districts were struck, leaving us with no control districts, this form of matching was not possible.

Second, control villages were matched on identical treatment windows with struck villages, removing potential bias associated with a common trend line unrelated to the treatment. For example, Elistanzhi was shelled on 1 June 2000. Insurgent attacks 90 days before and after the treatment were then recorded. Elistanzhi was then matched with Malye Shuani, a similar control village, with insurgent attacks 90 days before and after the 1 June 2000 treatment date recorded. The same procedure was followed at the district level, which partially compensates for the absence of "pure" controls (i.e. districts that never received a treatment). These treatment windows enable us to assess the sample average treatment effect (SATE) by measuring differences in insurgent violence between treated and control groups before and after each artillery strike over identical time periods.<sup>7</sup>

### 5 Findings

The empirical analysis consists of two stages. First, DD is used to compare treatment effect on the frequency of insurgent attacks at the village and district levels. Second, hazard models are employed to estimate the treatment effect on the timing and conditional probability of observing an insurgent attack in the post-treatment period. This section also tests for different "triggers" that might provoke an insurgent attack, including casualties, property damage, and treatment duration.

#### 5.1 Treatment Effect

Does indiscriminate violence suppress insurgent violence? In brief, yes. As Table 2 outlines, the village SATE is a nearly 28% reduction in the amount of post-strike insurgent violence

<sup>&</sup>lt;sup>7</sup>More formally, the DD estimator is obtained:  $DD = (Y_1^t - Y_0^t) - (Y_1^c - Y_0^c)$ , where  $Y_x \in (0, 1)$  are the pre- and post-treatment periods, T denotes the treatment group, and C denotes the control group.

in the 90 days following an artillery strike. The treatment effect for treated villages alone is a staggering -35.7% change in the amount of insurgent attacks. This result holds if we examine only Groznyy and its outskirts (-31%) or we exclude it (-43%) from the matched sample. Nearly identical control villages, by contrast, only record a modest -8.1% drop in insurgent violence over the same time period.

#### [Table 1 about here.]

The treatment effect is also substantively large. Collectively, the treated villages accounted for 300 attacks in the pre-treatment era, resulting in almost 83 "missing" attacks that would have occurred if the treatment had not been applied (between 17 and 97 attacks with a 95% confidence interval). Similarly, the difference in means between pre- and posttreatment attacks (1.94 and 1.25, respectively) from treated villages is highly significant.<sup>8</sup> By contrast, the difference is not significant among control villages. Interestingly, 71% of treated villages never record an attack in the post-treatment period. If a village was violence-free in the pre-treatment era, it typically remained so after incumbent repression, suggesting that grievances, if created, were not acted upon. Even in the midst of a war zone, then, violence is not uniform, and islands of violence can coexist with broader oceans of relative calm.

Table 3 reports the results from different statistical models that test the relationship between incumbent repression and insurgent violence. These findings reinforce the conclusion that there is a robust negative relationship between TREATMENT and the frequency of insurgent attacks. Moreover, this result is robust across different statistical models and specifications of the dependent variable. Model 1, for example, uses OLS regression with the percentage change in attack frequency as the dependent variable. Model 2 uses an ordered probit model with a categorical dependent variable (decrease, no change, increase in violence). In both models, the treatment is statistically significant and associated with a decrease in insurgent violence. The relative risk ratio estimate for treatment effect,

<sup>&</sup>lt;sup>8</sup> Significant at p < .01 using paired t-tests, with t(155) = -2.86 (-1.17, -.21).

derived from multinomial logistic regression with clustered robust errors, is 2.12 and is statistically significant (p=.04). Thus, the odds favoring a decline rather than an increase in post-treatment insurgent violence is twice as high for treated villages than control villages, holding all other variables constant.

#### [Table 2 about here.]

Two additional variables merit attention. First, while BASE is negatively correlated with post-strike activity, Khankala's fire appears particularly suppressive, being associated with an -86% drop in the relative risk ratio of observing an increase in insurgent attacks (p=.035). Put differently, Khankala's shelling is even more strongly associated with the non-likelihood of increasing insurgent violence than Shali. There are several reasons why this may be the case. It may be that Khankala's short-term fire is a better suppressant of attacks than Shali's long-term fire, a proposition I test below. More simply, Khankala's shelling was typically more lethal and destructive than Shali's barrages.<sup>9</sup> This evidence supports the hypothesis that the negative relationship exists between the level of indiscriminate violence and the frequency of insurgent attack.

Second, REBEL is also negative and statistically significant, suggesting that villages controlled by Salafist forces exhibit fewer post-strike attacks. The likelihood that Basayev's forces are associated with a decrease in post-treatment attacks is 4.5 times greater than an increase (p value=.003). Given the population's dislike of Salafism, it is possible that Basayev felt less beholden to public opinion than nationalist forces and thus did not initiate demonstrative attacks to maintain his reputation among aggrieved inhabitants. If Umarov's audience is local, Basayev's may be less so, and thus his commanders may have more flexibility in where they strike, including outside of Chechnya itself. Umarov's forces, by contrast, are perhaps tethered by their rhetoric to demonstrate their commitment to the nationalist cause and thus initiate post-treatment attacks with higher relative frequency.

<sup>&</sup>lt;sup>9</sup> Khankala's shelling killed 1.1 individuals, wounded 1.9, and destroyed 2.3 buildings on average. Shali's means were .95, 1.1, and 1.32, respectively.

Note, too, that of the remaining demographic and spatial variables, only TERRAIN meets conventional significance levels.

Perhaps we are looking in the wrong place for insurgent attacks, however. One drawback of a village focus is that it can be too narrow, thus neglecting the surrounding environment. Smart insurgents, for example, would probably be reluctant to stage attacks from home villages if it meant calling down even heavier Russian repression. Instead, we may observe a spike in violence in neighboring (control) villages and areas as insurgents maneuver to strike while concealing their true residence. Redistribution, rather than repression, may therefore be the actual consequence of indiscriminate violence. Ironically, this same redistribution may lead unwary officers to declare their policy a "success" even as their actions redirect violence into neighboring regions.

I therefore reset the 90-day pre- and post-strike windows around all villages within the treated and paired control villages' district (minus the repressed village itself). The results are surprising: there is an 12.5% reduction in total insurgent attacks among treated districts in the post-treatment period. The mean pre-treatment district possessed 7.91 attacks, falling to 6.8 in the post-treatment period, a statistically significant difference.<sup>10</sup> This decrease is even more remarkable given that the number of attacks in control districts is increasing (by 13.7%) over the same timeframe. With treated districts accounting for 476 pre-treatment attacks, we are "missing" 123 attacks in the post-treatment period. With a 95% confidence interval, between 239 and 33 attacks are missing.

Perhaps, however, we are observing a form of insurgent "John Henry" effect in which attacks are displaced onto control districts as insurgent commanders coordinate to avoid shelling.<sup>11</sup> There are two possible scenarios at work. First, insurgent forces may be moving across district lines in search of refuge. Second, insurgents may be coordinating so that forces in control districts assume a greater burden of attacking Russian forces. If either is true, then the treatment effect of indiscriminate violence may be displacement, not

<sup>&</sup>lt;sup>10</sup> Significant at p=.005 using paired t-tests, with t(53)=-.26 (-1.96, -.28).

<sup>&</sup>lt;sup>11</sup> John Henry effect: when the control group reacts by altering its behavior to be consistent with treated group even though no treatment is administered.

suppression. In either case, our "controls" might be imitating shelled villages without actually having received the treatment itself (Dulfo et al., 2007, 68-69).

This argument does not, however, withstand scrutiny. Since districts are shelled randomly, insurgent commanders cannot select a control district since they don't (and can't) know which district will remain unshelled. Given that there are only 10 districts, they risk selecting themselves into another strike if they shift locations. Moreover, Chechen insurgents are organized at the village level, with district commanders exercising (partial) control within, but not across, district boundaries. Commanders are often at odds with one another, especially if they have competing higher allegiances, and thus cross-district cooperation inside Chechnya is rare.

Returning to Table 3, we find that TREATMENT is once again significant in the OLS regression and narrowly misses conventional levels of significance in the ordered probit model (p=.12). Relative risk ratio estimates derived from multinomial regression with clustered robust errors underscore the treatment's effect: there is a 20-fold increase in the risk of experiencing a decrease in post-strike violence relative to observing no change (the baseline category). Here the importance of control cases is made apparent, for the relative risk ratio associated with an increase in violence is 10 times higher than the "no change" baseline category. Without the control, we might erroneously conclude that shelling increases the probability of violence when in reality the relative risk is twice as high for a decrease in violence. None of the other covariates are correlated with attack frequency, suggesting that demographic, terrain, and control variables play only a minor role in shaping patterns of insurgent violence. In sum, we find robust evidence for a negative relationship between indiscriminate violence and insurgent attacks across multiple statistical models and levels of analysis.

### 5.2 Treatment Severity and Duration

We are also interested in uncovering what affects the hazard rate of post-strike insurgent attacks. Here I consider the impact of group properties and variation in the treatment's severity and duration on the scale and scope of insurgent attack hazard rates.

Cox proportional hazard models are first used to estimate the hazard of post-treatment insurgent violence (Table 4). Cox regression was chosen because it is the most general of time-series models and does not assume a specific distribution of hazard/failure rates. The proportionality assumption at the village level is robust when tested using Schoenfeld and scaled Schoenfeld residuals.<sup>12</sup>

Model 5 estimates the impact of the treatment and village properties on hazard rates. Interestingly, the treatment itself, along with most village characteristics, does not have a significant effect on the timing and probability of insurgent attacks. Indeed, only two variables — POPULATION and TAC — increase hazard rates. The presence of a refugee camp has a particularly large impact, with a 204% increase in the likelihood of observing an attack relative to villages without TACs. DEADGROUND flirts with conventional levels of significance (p=.15) and, at .615, suggests that areas trapped between Russian and Chechen forces are less associated with insurgent violence (Kalyvas, 2006, 293-97).

#### [Table 3 about here.]

Model 6 repeats the analysis at the district level.<sup>13</sup> The results are disappointing, however, for neither the treatment nor district characteristics emerge as significant. Only TERRAIN even approaches conventional levels of significance (p=.15), suggesting that while insurgents may establish their bases in Chechnya's mountains, they concentrate their attacks in the more accessible central plains.

Perhaps, however, insurgent attacks are tied to variation in the shelling's destructiveness and duration rather than village-specific properties. It is commonly assumed, for example,

<sup>&</sup>lt;sup>12</sup>With a global test chi2=10.58, p=.31. Only TREATMENT and DISTANCE violate assumption of proportional effects. Following Box-Steffensmeier (2004, 131-33), I created interactive terms between these variables and a dummy variable for each year. The results obtained do not differ from those reported here, and so I omit the interactive terms for clarity.

<sup>&</sup>lt;sup>13</sup>The district level analysis violates the global test of proportionality (chi2=15.45, p=.01). This is due principally to three variables: POPULATION, TERRAIN and TAC. Interacting each variable with a dummy year indicator does not change the findings, so I dropped the interaction terms from the analysis. All analyses available from the author.

that insurgent attacks are especially likely in the aftermath of atrocities by incumbent forces. I therefore relax the assumption that the treatment is constant and allow it to vary across two dimensions: the severity of the strike, including extent of casualties and property damage, and duration (Table 5).

I estimate treatment effects using Weibull regressions with gamma random-effects frailty distributions. As before, standard errors remain clustered on individual villages or districts. This model was chosen for two reasons. First, hazard rates monotonically decrease over time, calling for a Weibull distribution (Box-Steffensmeier, 2004, 25-27). Second, gamma frailty terms were chosen to address unit-level heterogeneity present in the treated populations. Since matched control villages were dropped from this portion of the analysis, we need to ensure that we control for variance in frailty rates as high-frailty subjects are selected out over time. Failure to do so will result in underestimates of hazard rates and overestimates of survival times.

Models 7 and 8 reveal several counterintuitive findings that support the broader claim that indiscriminate repression can suppress insurgent violence. For example, the number of fatalities is most frequently cited as *the* "trigger" that drives revenge-seeking. Here, however, we find the opposite result: each individual killed in an artillery strike (KILLED) *reduces* the hazard of an insurgent attack at the village level by about 13%. Moving from zero to ten fatalities results in an almost 76% drop in the hazard rate.

#### [Table 4 about here.]

On the other hand, the number of WOUNDED does have an positive impact on the village-level hazard rate of insurgent attack. Each individual wounded increases hazard rates by about 8%; shelling that wounds 10 villagers would therefore raise the likelihood of attack by 123%. Property damage, which includes both residential buildings as well as agrarian facilities, does not have a significant effect at either level of analysis.

The largest substantive impact on insurgent hazard rates is associated with treatment duration (Figure 3). As captured by BASE, the move away from Shali's long-duration fire to Khankala's short-duration fire results in a substantial *increase* in hazard rates (Figure 3). A 284% increase in the likelihood of attack is recorded at the village level as we move toward Khankala. A similar 68% increase is observed at the district level (p=.12).

Put differently, the longer the shelling, the more suppressive its effects, independent of the strike's actual costs. This finding is consistent with the argument proposed here, namely, that unpredictable repression can suppress insurgent violence by creating and sustaining a climate of fear among targeted populations. Subjected to lengthy bombardments in and around their homes, villagers may elect to "tip" toward the Russian side (i.e. by joining pro-Russian militias) or withhold support from insurgents in the hopes of escaping future repression. H&I fire, with its protracted and unpredictable bombardment pattern, would seem ideal for driving home the message that insurgents cannot credibly protect a terrorized population.

#### [Figure 3 about here.]

These findings suggest that the link between incumbent and insurgent violence is much more complicated than assumed by existing theories. Indeed, these micro-level findings typically underscore the suppressive effect of incumbent violence. As a result, they are difficult to reconcile with allusions to an escalatory spiral at the macro-level.

### 6 Discussion

These findings are counterintuitive and pose a serious challenge to existing explanations of violence in civil war. Critics may, however, remain unconvinced. Indeed, at least two criticisms could be levied. First, the choice of a natural experiment methodology raises questions of external validity (Cook and Campbell, 1979). Second, critics may contend that the 90-day pre- and post-treatment windows are too brief to capture treatment effects entirely if they are cumulative in nature. I address each point in turn.

### 6.1 External Validity

The criticism that these findings lack external validity has two components. First, some might argue that specific characteristics of the sample population inhibit generalization beyond Chechnya. Second, the treatment itself may be unique, or at least rare, and thus occurs only in a tiny fraction of cases.

While there are always limits to what can be derived from a single case, it is not clear why we should consider the sample population restrictive. In particular, it exhibits substantial variance across key variables cited when explaining insurgent violence, including: terrain (including flatlands, urban centers, and deep forests), elevation ( $\pm 1200$ m), insurgent organizations, population settlement size (between 50 inhabitants and 210,000), and differential incumbent control over space and time.

Nor does Chechnya represent a "costless" situation for Russian forces to use violence indiscriminately without fear of sanction. An active, albeit small, antiwar movement does exist, for example, while Russian soldiers have been prosecuted by the European Court of Human Rights (Lyall, 2006). Insurgents also remain lethal, with at least 402 attacks recorded in 2005 alone.

That said, however, the insurgency has undoubtedly weakened over this time period, with perhaps only 500-750 active combatants remaining from a wartime height of 10,000 in 1999 (Interview, October 2005). By contrast, pro-Russian Chechen militias, staffed primarily by former insurgents, have grown steadily and now command about 10,000 soldiers. These divergent recruitment rates suggest that insurgent weakness is a symptom, not a cause, of Russian strategy, as the absence of willing recruits is a *product* of Russian military practices.

Perhaps, however, the quasi-experiment hinges on a unique treatment. Unfortunately, this is not the case, either. Indiscriminate shelling, including H&I fire, has been employed frequently, including by France in Algeria (Horne, 1977, 166); the United States in Vietnam (Hawkins, 2006) and Iraq today (Ricks, 2006, 232-34); the Soviet Union in Afghanistan

(Grau, 2002) and Russia during the first Chechen war (Smith, 2006); Britain in Afghanistan today (*The Times*, 9 September 2006); and Israel during the 2006 Lebanon war (*New York Times*, 6 October 2006). While local conditions certainly vary, the treatment itself is similar across these conflicts.

#### 6.2 Treatment Windows

The strength of the natural experiment research design is that it enables us to isolate the causal impact of indiscriminate violence over the short-to-medium term. It is possible, however, that the common assumption of a tight temporal link between intervention and response is mistaken. In this view, resentment builds over time, and thus an artillery strike in 2000 may still be the catalyst for an insurgent attack years later. If this is the case, then the research design here would overstate treatment effects by truncating the response window prematurely.

There are strong reasons to reject this view. First, as noted above, the longer the window between treatment and possible response, the more uncertainty we introduce since we increase the likelihood of confounding events (including new attacks, updated strategies, or changing perceptions of victory) intervening between treatment and response.

Second, smoothed hazard estimates of the timing of the insurgents' first post-treatment attack decrease monotonically over time (Figure 4). Indeed, the common assumption of a tight temporal link between (incumbent) action and (insurgent) reaction appears justified at the village and district level.

Kaplan-Meier failure functions reveal, for example, that attacks ("failures") are most likely in the initial 20 days following an artillery strike. After the 20 day mark, however, the function tapers off, with only a 25% chance of observing an attack 59 days after the shelling; the function never exceeds 27%. Control villages are somewhat slower to "fail," and their cumulative probability of experiencing an attack does not exceed 25% over 90 days.<sup>14</sup> We are therefore most likely to observe an insurgent attack in the immediate

<sup>&</sup>lt;sup>14</sup>A log-rank test of village failure functions is nearly significant (p=.13).

aftermath of incumbent repression.

#### [Figure 4 about here.]

This pattern of monotonically decreasing hazards of insurgent attack is repeated at the district level. Kaplan-Meier function estimates that treated districts have a 25% chance of observing an insurgent attack within five days after the treatment. This probability rises to 50% within only 11 days, and a 75% chance of an attack 32 days from the initial artillery strike. In total, there is an 87% chance of observing a failure within the 90-day post-treatment window. Control groups are similarly quick to fail, with a 25% chance of attack within five days. The two functions then separate, as control villages reach the 50% probability mark at 17 days and require 38 days to reach the 75% threshold. Both district groups fail quickly and at nearly identical rates; treated districts, however, tend to fail faster seven days after the treatment.<sup>15</sup>

In short, both village and district level failure functions underscore that the probability of observing an insurgent attack is highest in the immediate days and weeks following repression. Both control and treated groups have short-time lags, suggesting that the 90-day windows are sufficient to capture post-treatment responses.

### 7 Conclusion

This paper should not be read as endorsing indiscriminate violence as a policy instrument. The shelling recorded here is a war crime under both international and Russian law. Similarly, these findings do not necessarily contradict the broader claim that *under many conditions* the use of indiscriminate violence will prove counterproductive. Indeed, even a reduced rate of insurgent violence may still be sufficient to overwhelm a state's efforts, especially if it is a war-weary democracy (Merom, 2003).

This caveat should not be exaggerated, however. These findings pose a significant challenge to existing theories, especially their core assumption that indiscriminate repression

<sup>&</sup>lt;sup>15</sup>A log-rank test of district-level failure functions is significant (p=.06).

uniformly incites insurgent violence. Two findings stand out in particular: the relative decrease in insurgent violence after artillery strikes and the negative correlation between many oft-cited "triggers" and insurgent retaliation. Notably, these counterintuitive results were obtained using a research design that, in conjunction with micro-level data, helped control for methodological problems present in prior observational research.

The discrepancy here between findings generated in quasi- and non-experimental settings underscores the pressing need to identify the scope conditions under which indiscriminate repression increases, and decreases, insurgent activity. Testing causal mechanisms will be a close-range task by necessity: (cross)national indicators are simply too crude to capture the effects of local-level violence. This is especially likely if observed behavior hinges on contextual factors such as perceptions of relative deprivation (Gurr, 1970) or the chances of insurgent victory.

Future research also should theorize the state's role more directly. Per capita income, for example, is a widely used yet poor proxy for a state's coercive power. State power is multi-faceted, and our theories should reflect this by incorporating various dimensions of power, including military base locations and types of strategies adopted. One obvious extension of this research would be to consider how different state strategies interact to affect insurgent violence. Cross-cutting intervention research designs would be highly useful, for example, in explicitly comparing the impact of multiple "treatments" and the interactions between them across groups. These efforts will demand both research design and conflictspecific knowledge but hold out promise of substantially enriching our understanding of the dynamics of violence during civil war.

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Covariates	Village			District		
	Mean Difference	Std. Bias	% Balance Improvement	Mean Difference	Std. Bias	% Balance Improvement
Population(log) TAC Distance(log) Terrain(log) Deadground Garrison Rebel Base	.097 0.00 024 .135 0.00 0.00 063 0.00	.052 0.00 042 0.12 0.00 0.00 126 0.00	$\begin{array}{c} 92.2 \\ 100 \\ 90.87 \\ 7.14 \\ 100 \\ 100 \\ 51.14 \\ 100 \end{array}$	.056 006 115 .018	.066 02 185 .012	89.73 86.35 73.85 46.27

# Table 1: Sample Balancing Through Matching

*Note:* 158 village and district pairs.

Control Villages  $\odot$ Treated Villages Bases Chechnya Groznyy 6  $\odot$ • 00  $\odot$  $\odot$  $\odot$ kala  $\overline{\mathbf{O}}$  $\odot$ .  $\odot$  $\odot$  $\bigcirc$ • •  $\bigcirc$  $\odot$ 30 Kilometers 3.75 7.5 22.5 15

Figure 1: The Natural Experiment

NOTE. 129 total populated settlements (71 treated, 58 control).



Figure 2: Distribution of Artillery Strikes

NOTE. 158 observations, 2000-05 (112 at Shali, 46 at Khankala).



Figure 3: The Impact of Shelling Duration on Insurgent Attack Hazard Rates

NOTE. 158 observations at each level of analysis. Population hazard estimates were obtained from Weibull regression using gamma frailty distributions and standard errors clustered on individual villages/districts.



Figure 4: Smoothed Hazard Estimates of Insurgent Response Times

NOTE. 316 Observations at each level of analysis.

Groups	Village	District
Treated	-35.7% -8.1%	-12.5% +13.7%
SATE	-27.6%	-26.2%
Note: 158	3 village and	district pairs.

 Table 2: Treatment Effect on Frequency of Post-Treatment Attacks

Variables	Village		District	
	Model 1 (OLS)	Model 2 (OProbit)	Model 3 (OLS)	Model 4 (OProbit)
Treatment	-0.1/**	-0.26**	-1 /9**	-0.21
ireaument	(0.07)	(0.13)	(0.55)	(0.21)
Population(log)	-0.02	-0.02	-0.39	-0.17
1 opulation(108)	(0.20)	(0.02)	(0.65)	(0.23)
TAC	-0.03	-0.30	0.21	-0.03
	(0.07)	(0.22)	(0.32)	(0.10)
Distance(log)	-0.08	-0.15	-0.57	-0.25
( 0)	(0.05)	(0.09)	(0.59)	(0.19)
$\operatorname{Terrain}(\log)$	-0.06*	-0.09	-0.07	-0.10
	(0.03)	(0.07)	(0.45)	(0.12)
Deadground	0.01	0.03	· · · ·	
	(0.06)	(0.14)		
Garrison	0.10	0.16		
	(0.07)	(0.16)		
Rebel	-0.14**	-0.37***		
	(0.06)	(0.11)		
Base	-0.24***	-0.40*		
	(0.09)	(0.21)		
Constant	1.92***		6.71	
	(0.37)		(10.85)	
Cutpoints	(0101)	-2.63	()	-3.95
1		-0.42		-2.84
N (Clusters)	316 (105)	316 (105)	315 (10)	315 (10)
$R^2$	0.04	0.03	0.03	0.01
F Test/Wald	2.54**	31.59***	$3.28^{*}$	5.86

Table 3: Determinants of Insurgent Attacks

Note: Clustered robust errors in parentheses. \*Significant at 10% \*\*Significant at 5% \*\*\*Significant at 1%

Variables	Village	District
Group Properties	Model 5 (Cox)	Model 6 (Cox)
Treatment	1.35 (0.40)	1.11 (0.29)
Population(log)	1.97***	1.04
TAC	(0.28) $3.04^{***}$	(0.26) 1.00
Distance(log)	$(1.13) \\ 1.17$	$\begin{array}{c}(0.11)\\0.83\end{array}$
$\operatorname{Terrain}(\log)$	(0.34) 1.11	$\begin{array}{c}(0.20)\\0.84\end{array}$
Deadground	$(0.15) \\ 0.61$	(0.10)
Garrison	$(0.22) \\ 0.79$	
Rebel	(0.33) 1.29	
	(0.37)	
Subjects(Failures) Log Pseudolikelihood Wald chi2	316 (80) -395.69 257.61***	316 (279) -1392.36 16.92***

Table 4: Determinants of Insurgent Attack Hazard Rates

*Note:* Dependent variable is the hazard rate: the probability that a group will witness an insurgent attack in the 90 days following an artillery strike. Hazard rates obtained using Cox regression with standard errors clustered at the village/district level. Breslow method for ties. \*Significant at 10% \*\*Significant at 5% \*\*\*Significant at 1%

"Triggers"	Village	District
	Model 7 (Weibull)	Model 8 (Weibull)
Killed	.87*	1.00
	(0.06)	(0.04)
Wounded	1.08**	0.98
	(0.04)	(0.02)
Property	1.03	1.00
	(0.03)	(0.02)
Base	$3.84^{*}$	1.68
	(1.38)	(0.56)
Subjects (Failures)	158(45)	158 (144)
Log likelihood	-142.09	-275.05
LR $chi2(4)$	8.74*	3.05
LR Test $\theta$	27.75***	3.69**
Shape Parameter	0.80(0.11)	$0.83\ (0.05)$

Table 5: Varying Treatment Severity and Duration

*Note:* Dependent variable is the hazard rate: the probability that a treated population will witness an insurgent attack in the 90 days following an artillery strike. Hazard ratios were obtained using Weibull regression with gamma frailty terms. Clustered errors in parentheses. \*Significant at 10% \*\*Significant at 5% \*\*\*Significant at 1%