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What is This?

Does Indiscriminate Violence Incite Insurgent Attacks?

Evidence from Chechnya

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Does a state's use of indiscriminate violence incite insurgent attacks? To date, most existing theories and empirical studies have concluded that such violence is highly counterproductive because it creates new grievances while forcing victims to seek security, if not safety, in rebel arms. This proposition is tested using Russian artillery fire in Chechnya (2000 to 2005) to estimate indiscriminate violence's effect on subsequent patterns of insurgent attacks across matched pairs of similar shelled and nonshelled villages. The findings are counterintuitive. Shelled villages experience a 24 percent reduction in posttreatment mean insurgent attacks relative to control villages. In addition, commonly cited "triggers" for insurgent retaliation, including the lethality and destructiveness of indiscriminate violence, are either negatively correlated with insurgent attacks or statistically insignificant.

Keywords: civil war; indiscriminate violence; insurgent attacks; matching; Chechnya

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

> *Russian Artillery Officer, Chechnya, November 2003*

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 one hundred studies and forty-five historical cases in which a state's reliance on collective targeting of the noncombatant population provoked greater insurgent violence (Kalyvas

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2006, 146-72). Indiscriminate violence, it is argued, solves the collective-action problem facing insurgents by forcing would-be free riders to seek sanctuary in rebels' arms. As a result, state-orchestrated brutality plays a key role in our theories of violence during civil wars by sparking a spiral of action and reaction that facilitates insurgent mobilization while widening the war's geographic scope and destructiveness. Once set in motion, these escalatory dynamics are difficult to arrest, often leading to the state's own defeat as its resources and willpower become exhausted.[1]

Yet we also possess studies, albeit fewer in number, by scholars and by practitioners arguing that a state's use of indiscriminate violence can actually suppress an insurgency, at least under certain conditions (Stoll 1993; Downes 2008; Merom 2003; Kalyvas 2006, 167-71). Indeed, if indiscriminate violence were so consistently counterproductive, it is puzzling why militaries have wielded such a blunt instrument against noncombatants with alarming regularity. Yet strategies as diverse as scorched-earth campaigns, aerial bombardment, and forced population resettlement remain hallmarks of state efforts to suppress insurgencies (Slim 2008; Valentino 2004; Valentino, Huth, and Balch-Lindsay 2004).

Indeed, the particular practice examined in this article—indiscriminate artillery shelling—was adopted 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*, September 9, 2006); and Israel during the 2006 Lebanon war (*New York Times*, October 6, 2006), to cite a few examples.

Ultimately, it is difficult to isolate the causal effects of indiscriminate violence, given the complexity of civil war battlefields. Data limitations are partly to blame. Cross-national data, for example, are simply too aggregate to capture micro-level outcomes of state–insurgent interaction (Sambanis 2004). Collecting the necessary data is, of course, an often dangerous, if not impossible, task. In addition, conflict data are the product of strategic interaction rather than experimental design. Severe problems stemming from simultaneity bias and selection effects are therefore likely to exist in observational studies that, if not explicitly addressed, will yield mistaken causal inferences.

This article uses Russian artillery strikes on populated settlements in Chechnya (2000 to 2005) to test the presumed relationship between a state's indiscriminate violence and insurgent attacks. This shelling offers an identification strategy that allows the researcher to compare levels of insurgent violence before and after an artillery strike in a shelled village with those of a similar but not victimized village during identical time frames. Contrary to most existing studies, this difference-in-difference estimation finds that indiscriminate violence actually reduced the mean number of insurgent attacks relative to nonshelled villages. Moreover, commonly cited "triggers" for insurgent attacks, including the number of casualties inflicted and the amount of property damage suffered, are either negatively correlated with insurgent violence or statistically insignificant.

The article proceeds as follows. The first section offers a survey of the methodological challenges associated with identifying the casual effects of violence in a civil war setting. The next section examines why a state's use of indiscriminate violence is deemed counterproductive. The third section presents the case for why indiscriminate violence may suppress, rather than incite, insurgent violence. A fourth section details the research design, including data, variables, internal validity checks, and matching procedure. The fifth section assesses treatment effects on village-level insurgent violence. The effect of variation in shelling lethality, damage, and frequency is also examined. A sixth section addresses possible criticism of the study's findings, including issues surrounding treatment externalities, the timing of insurgent retaliation, and the study's external validity. A final section concludes.

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 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.[2]

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 area (*oblast'*) rose from thirty-nine to seventy-four 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 should raise several methodological red flags.

Take, for example, the problem of simultaneity (endogeneity).[3] 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. 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.

Furthermore, 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. To date, however, the literature remains under-specified on this score, and as a result, has not tackled the question of which mechanisms are at work (and when) in shaping insurgent responses to state violence.

We also know from studies of deterrence in international relations that severe selection effects are present when we only observe failures (Achen and Snidal 1989). A similar problem is present in civil war studies, in which 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 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 nonevents (but see Wood 2003, 18). 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 nonrepressed village to examine changes in patterns of insurgent violence. More specifically, we need not only Liakhovo's baseline of insurgent violence before German repression but also data from a similar village during the same time frame if we are to isolate repression's independent causal effect. Without these comparisons, it is difficult to assess whether the observed "surge" in insurgent behavior after Liakhovo's destruction represented an increase or decrease in attack propensity.[4]

Why Indiscriminate Violence Is Thought to Be Counterproductive

Given the intuitive logic of these escalatory dynamics, why would a state ever risk setting them in motion? For some, indiscriminate strategies are proof of an incumbent's desperation when faced with an entrenched insurgency (Hultman 2007; Valentino,

Huth, and Balch-Lindsay 2004). Others suggest that such strategies reflect unit indiscipline (Azam 2002), perverse institutional cultures (Shepherd 2004), or the absence of sufficient information to sift insurgents from the population selectively (Kalyvas 2006).

Despite disagreement over its causes, scholars largely agree that indiscriminate violence is counterproductive because it reduces, if not eliminates entirely, the collective action problem facing insurgent organizations (Olson 1965; Tullock 1971; Popkin 1979; Lichbach 1995; Wood 2003). From this view, insurgents must convince individuals to assume the private risks of combating the state, despite the obvious threat of costly sanction (i.e., death), when the benefits of insurgent victory are mostly nonexcludable. Given this mix of private risk and public reward, rational individuals are likely to "free ride" rather than side with the insurgency, creating potentially debilitating recruitment problems that may thwart successful collective action.

A state's use of indiscriminate violence, however, is thought to reduce free-rider incentives via two mechanisms. First, such actions create new grievances among individuals who then join the insurgency for revenge (Tishkov 2004, 142; Hashim 2006, 99-104; Anderson 2005, 46-47). Rational insurgent organizations can similarly capitalize on these grievances by shaping their appeals to reflect widespread desires for vengeance and by advertising the possibility for "pleasure in agency" by striking back at the hated incumbent (Wood 2003, 18-19). Such violence, in other words, creates demand for an insurgency and its continuation.

Second, indiscriminate violence drives individuals into the insurgency out of a need to seek some measure of protection from a capricious state (Leites and Wolf 1970, 112-18; Mason and Krane 1989; Goodwin 2001; Kalyvas 2006, 151-59). Faced with state violence that does not distinguish between insurgents and noncombatants, rational individuals will decide that the risks of nonparticipation may actually be higher than fighting, since joining an insurgent organization offers at least some minimal prospect of security, if not safety. As Stathis Kalyvas and Matthew Kocher (2007, 183) argue, "individuals may participate in rebellion not in spite of the risk but in order to better manage it."

Indiscriminate state violence also creates opportunities for insurgents to withhold protection if local populations are deemed insufficiently supportive of the rebel organization. For example, insurgents may deliberately provoke state overreaction by staging attacks in areas that they do not yet fully control. In such cases, the state itself becomes the insurgents' own private enforcer, its violence turned against itself in areas where the state might enjoy the best prospects for exercising control (Elliott 2003, 873; Kalyvas 2006, 149-60).

As an insurgent organization's ranks swell, so too does its coercive capacity. More specifically, it is reasonable to assume that insurgent organizations with larger memberships are more capable of planning and conducting a greater number of attacks than their smaller counterparts. This is likely to be true independent of the skill level of the particular insurgents in question. For example, while organizations with fewer combatants may be able to match the output of larger organizations temporarily, their long-term ability to do so is questionable since sustaining such a rate

of violence means demanding more attacks per insurgent, thus exposing these insurgents to relatively higher risk of death or capture. Indiscriminate violence thus appears doubly counterproductive: it not only helps alleviate the insurgent's collective action dilemma but also increases the amount of "action" that an organization can generate and sustain over time.

A Theory of Indiscriminate Violence

It is possible, however, that indiscriminate violence actually has the opposite effect—namely, that it *reduces* insurgent violence. Indeed, an insurgency's ability to capitalize on the opportunities created by indiscriminate state violence hinges on whether the organization can survive in the face of state coercion. More pointedly, states not only resort to indiscriminate tactics and strategies when insurgents are weak and cannot protect populations (Kalyvas 2006, 167) but can actually use their violence to cripple insurgent organizations. Put differently, insurgent weakness may be the result, not the cause, of the state's use of indiscriminate violence.

We can imagine two mechanisms at work, conditional on the level of the state's indiscriminate violence.

First, widespread indiscriminate violence creates enormous logistical problems for insurgencies. At the extreme, indiscriminate violence can erode rebel resources through forcible population resettlement. Concentration camps and "free fire" zones in conflicts as diverse as the Boer War (Downes 2008, 156-77), the Sanusi uprising in Libya (Evans-Pritchard 1949), and Darfur today (Daly 2007, 282-89) suggest that incumbents have long sought to shrink the "sea" that shelters insurgent "fish" through violence (Azam and Hoeffler 2002; Valentino, Huth, and Balch-Lindsay 2004; Tse-tung 2000). 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" (Elliott 2003, 911-20, 1156-64, quote on 1178).

Such sweeping efforts have several negative effects on insurgent organizations. First, these policies reduce an insurgency's tax base and thus degrade its ability to acquire the necessary material to sustain its war effort. Insurgencies that rely on the provision of selective incentives, such as spoils, to maintain recruitment may find themselves particularly hard-pressed if these lootable goods are destroyed or removed by the fleeing population (Lichbach 1995; Weinstein 2007). Second, coercive population resettlement can promote counter-mobilization by immiserating locals, thus lowering the reservation value for joining an incumbent's military (Azam 2002, 2006).[5] Finally, such efforts complicate insurgent strategy by making it difficult for insurgents to maintain supply lines, protect safe refuges, and concentrate their forces.

Second, even lesser amounts of indiscriminate violence can undermine an insurgent organization's military effectiveness by driving a wedge between locals and insurgents. While we typically assume that insurgents are unconstrained in their choice of strategies, indiscriminate violence by the state may facilitate collective action on the part of locals against insurgents, thus imposing constraints on insurgent war-fighting that can compromise its effectiveness. Indeed, if local populations come to blame insurgents, not the incumbent, for the state's repressive acts, then an insurgency may be forced to curb, if not abandon, its current tactics and strategy to avoid provoking further counter-mobilization.

Far from passive actors, noncombatants have historically engaged in several types of collective action against insurgents. For example, noncombatants have petitioned rebel authorities to cease their activities, as was the case in Vietnam (Elliott 2003, 1135), Sudan (Daly 2007), and World War II–era insurgencies in France (Todorov 1996, 43-44) and the Eastern Front (Hill 2005, 85-89). Moreover, whole villages have defected to the incumbent side, often in concert with their neighbors, for protection, as in Algeria (Horne 1977, 222-24) and nineteenth-century Chechnya (Gammer 1994, 286). Finally, they have formed their own civil-defense organizations, as in Guatemala (Stoll 1993), Kenya (Anderson 2005), and Mozambique (Weinstein 2007), to protect their villages from insurgents and to signal their loyalty to incumbent forces. Note that these responses are not necessarily mutually exclusive and should be conceptualized as a continuum that begins with petitions and ends with armed countermobilization, contingent on insurgent responses.

Insurgent leaders can therefore find themselves in a severe bind: if they continue to mount attacks from within (or near) an aggrieved population, they risk (further) alienation of disillusioned locals, raising the specter of defection to the incumbent's side; if they curb their violence in recognition of popular pressures, they risk introducing inefficiencies into their strategy by reducing their war-fighting capabilities.

Indiscriminate violence thus reshapes the relationship between insurgents and populace by underscoring that the insurgency cannot credibly protect the population, and moreover, that its continued presence endangers noncombatants. Without an adequate response to state violence, insurgents are likely to be perceived as the weaker side, thus removing an important incentive for joining the insurgency.

In addition, the question of whether (and how) to respond to public pressures can introduce fissures into the insurgent organization and its strategy. Indeed, the first victim of this factionalism may be the rebel's own strategy. Rather than fluidly updating to take advantage of state weaknesses, rebels may instead be forced into a situation in which their strategy appears as a "patchwork quilt" of constrained and unconstrained operating areas that may be suboptimal from the narrow vantage point of military effectiveness. Local imperatives and the need to devolve control of strategy to local commanders may therefore create obstacles that inhibit coordination across the insurgency. This decentralization also hinders leaders' attempts to monitor and credibly deter would-be defectors from their ranks, further eroding the insurgency's coercive capabilities.

Indiscriminate violence can therefore *restore* the insurgent's collective action dilemma by either undermining its logistics (if the violence is large-scale in nature) or by driving a wedge between the population and insurgents. In either case, state violence reveals that the insurgency cannot credibly protect the population nor respond in kind, feeding the perception that the insurgency is both likely to lose and is endangering locals without bringing tangible benefits. In these conditions, it is not clear that siding with the insurgency is the only way to manage risk. Collective action on the part of the locals to force changes in insurgent strategy offers another means by which the population can safeguard itself amid the confusion of a guerrilla war.

Of course, not all insurgent organizations are equally responsive to locals' demands. We might expect, for example, that weaker groups—that is, those organizations lacking external support and heavily reliant on the population for assistance—are most likely to accede to popular desires to restrict, if not cease outright, their attacks. More specifically, organizations that are actively attempting to foster a particular ideological, ethnic, or nationalist bond with the people are more likely to suspend or alter their strategy when confronted by locals. These organizations must maintain some ability to protect local populations if they are to find a receptive audience for their appeals. By contrast, those groups that recruit and maintain cohesion through battlefield spoils (Weinstein 2007) are apt to ignore popular wishes, especially if the assets they are looting are fixed, thereby eliminating the exit option for aggrieved populations.

In short, it is plausible that indiscriminate violence's effects are suppressive, rather than escalatory, in nature. Moreover, it is likely that its effects are not uniform but are conditional on the nature of the insurgent organization itself. This discussion suggests at least one key observable implication: once victimized, populations will record fewer insurgent attacks relative both to prior levels of violence and to non-victimized populations more generally. Similarly, we can anticipate that "triggers" for insurgent attacks—that is, the lethality and destructiveness of the incumbent's violence—may not lead to more attacks but are instead negatively correlated with attacks as aggrieved populations turn away from the insurgency.[6]

Research Design

I use artillery strikes by Russian forces on populated settlements in Chechnya (2000 to 2005) as an identification strategy to isolate the causal effect of indiscriminate violence. These strikes have an important property: they are uncorrelated with key spatial and demographic variables thought to drive insurgent-attack propensity, including population size, presence of incumbent bases, and terrain. Yet, the most important determinants of insurgent violence may be war-induced dynamics that arise out of the interaction of Russian and rebel strategy. To account for this possibility, I adopt matching to create populations of shelled ("treated") and nonshelled villages that are similar across background covariates as well as prior Russian and insurgent military activities. Difference-in-difference estimation is then used to

gauge shelling's effect by comparing mean differences in insurgent attacks before and after a strike across shelled and control villages during identical time periods.

Why Chechnya?

The second Chechen War represents a "most likely" case (Eckstein 1975) for observing the link between indiscriminate repression and increased insurgent attacks. Indeed, the war has witnessed astonishing levels of brutality by all 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). "Chechnya," one observer has concluded, "is above all a lesson in the devastating spiraling dynamic of violence" (Zürcher 2007, 113).

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 spring 2000 with the insurgent's abandonment of direct battle.

Since then, the war has degenerated into a grinding counterinsurgency war that has tied down nearly eighty thousand Russian soldiers in an area the size of New Jersey (Kramer 2005/2006). Indeed, by April–May 2000, some seventy to eighty groups of insurgents, each with ten to twenty-five fighters, were waging an extensive and bloody campaign of hit-and-run strikes and mine warfare against Russian forces and their proxies. Insurgent commanders, led by emirs such as Shamil Basayev, Khattab, and Doku Umarov, created a Madjlis-Shura (led by the nominal president of Chechnya, Aslan Maskhadov) that was designed to coordinate insurgent actions by dividing Chechnya into several "fronts." In practice, however, the Madjlis-Shura was bitterly divided by internal disagreements and largely devolved control to the village-level deputies (naibs) who commanded the rebel groups (Souleimanov 2007, 261-67). By 2005, only 500 to 750 active fighters remained, a far cry from the estimated 10,000 to 12,000 who initially resisted Russia's operation in Chechnya (interview with local human-rights observer, October 2005).

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 nongovernmental organizations (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 (*zachistki*) are routinely marked by forced disappearances (about five thousand since 1999) and extrajudicial killings (Human Rights Watch 2002a, 2002b, 2006). Insurgents have retaliated with suicide bombings, mass hostage-takings, and a relentless campaign of attacks against Russian patrols. An estimated fifteen thousand to twenty-five thousand civilians and

at least five thousand Russian soldiers have died since 1999; roughly one hundred thousand 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.

Crucially, the war's evolution from positional battles to guerrilla warfare during spring 2000 created a "pause" in which human-rights organizations, especially Memorial and Human Rights Watch, could return to establish a network of observers throughout the area being investigated in this article. NGOs, notably the Danish Refugee Council, were also able to conduct baseline household economic surveys (March–April 2000) that enable researchers to estimate population levels and poverty levels that reflect war-induced changes but that are not confounded with the artillery strikes that began in June 2000. Similarly, the establishment of Russian garrisons, along with the creation of village-organized rebel groups, preceded the artillery strikes, allowing for the separation of the effect of shelling from potentially confounding variables.

Dependent Variable

The dependent variable, *attack*, is defined as an insurgent-initiated attack against Russian or pro-Russian Chechen proxy forces, their local representatives, and civilians at the village level. Data were drawn from more than thirty-five Russian and Western media sources, including local newspapers, newswires (such as Itar-Tass), human-rights organizations such as Memorial and Human Rights Watch, official releases and casualty reports from the Ministry of Defense and Interior Ministry, interviews, and rebel Web sites and videos. Although 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.[7]

Attack is operationalized as the difference in the mean number of insurgent-initiated attacks during identical postshelling and preshelling time periods (ninety days after and before a strike). Given that insurgent attacks are coercive means to fight Russian forces and to demonstrate resolve, I interpret a relative decrease in attacks as evidence that state actions are suppressing insurgent violence.

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).

Although plausible, this is not the case in Chechnya. First, insurgents have rarely directly targeted noncombatants inside Chechnya, although innocent bystanders have been killed during their attacks. Indeed, only 5 percent of attacks in Chechnya (2000 to 2005) have targeted civilians directly, with annual averages ranging from 2.5 percent (2005) to 6.7 percent (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

avenge losses, especially if their failure to do so was interpreted by the broader community as a failure of resolve.

Confounding Variables

When attempting to isolate the causal impact of indiscriminate violence, we need to take stock of as many potentially confounding variables as possible. This section details eight demographic, spatial, and conflict-related variables commonly used to explain patterns of insurgent violence. All measures were taken before the artillery shelling (June 2000 to December 2005) that acts as our treatment.

Population records the number of inhabitants in a populated settlement (logged) in March–April 2000, when the first comprehensive household survey was conducted (Danish Refugee Council 2000). Missing values were imputed from World Health Organization (WHO) estimates of prewar ambulatory facilities and expected caseloads of regional clinics, and in rare cases, from the size of wheat shipments by humanitarian organizations (World Heath Organization 2003; Danish Refugee Council 2002). While estimating population size in war zones is notoriously difficult, these figures do have the advantage of incorporating both permanent residents and internally displaced persons (IDPs), thus reflecting war-induced population movement. It is plausible that larger urban centers are associated with higher rates of violence simply because the pool of possible recruits is larger.

Poverty uses a threefold classification scheme to rank the severity of a population's need for humanitarian assistance. Drawing on the 2000 Danish Refugee Council household survey, humanitarian organizations used daily caloric intake as a means of assessing whether assistance was urgently required (a "3"), modestly required (a "2"), or not required at all (a "1"). All districts in Chechnya were assigned one of these values. According to existing research (Collier and Hoeffler 2004; Justino 2008), higher levels of poverty are likely to translate into higher levels of insurgent violence since inhabitants have more incentives to take up arms to acquire loot or supplies to ward off destitution and starvation.

While Chechnya's population is overwhelmingly Sufi Muslim, it is nonetheless divided internally into two brotherhoods (or Tariqa), the Naqshbandiyya and the Qadiriyya, that have their own historical experiences with Russia. Naqshbandiyya teachings were first introduced into Dagestan and Chechnya in the early 1800s and quickly became the basis for anti-Russian resistance. The 1864 defeat of their most famous adherent, Shamil of Gimry, created an opening for the spread of pacifist Qadiriyya teachings that had been circulating since the 1840s. This pro-coexistence stance was quickly abandoned in the 1870s, however, in the face of Russian repression of leading Qadiriyya leaders. Now, Qadiriyya adherents assumed the anti-Russian mantle, with Naqshbandi populations favoring collaboration. By the 1890s, the Qadiri had replaced the Naqshbandi in most, but not all, regions of Chechnya. These roles have remained mostly, although not entirely, stable across the ensuing decades and wars (Gammer 2006, 45-52, 68-81; Zelkina 2000, 121-35, 169-85; Gammer 1994, 39-46).

At present, it is estimated that the Naqshbandi represent 10 percent of Chechnya's population. They are geographically concentrated in Chechnya's northern districts and have sizable populations in two large towns, Urus-Martan and Tolstoy-Yurt. Using these historical settlement patterns as well as known Naqshbandi shrine locations, I created *Tariqa*, a binary variable that records whether a populated place is dominated by the Naqshbandi.

Elevation records a village's altitude in meters (logged). Rough terrain is associated with a higher probability of insurgent violence since these locations provide refuges that are difficult for state forces to penetrate (Fearon and Laitin 2003; Collier and Hoeffler 2004; Galula 2006, 23-25).

Isolation measures the number of settlements that are found within 5 km^2 of the swept village. This captures the belief held among practitioners that isolated villages are easier to suppress because insurgents have few or no options when seeking to escape or hide within the local populations (U.S. Army Field Manual 2007, 185).[8] I also used hierarchical ordering in ArcGis 9.2 to measure the distance in kilometers (logged) between the swept village and its nearest neighbor (*neighbor*). This allows us to measure the amount of spillover, if any, between the swept location and its neighbor in terms of observed changes in posttreatment insurgent violence.

Garrison demarcates whether a Russian garrison was stationed in a particular village by June 1, 2000. Locations were identified through reports by Human Rights Watch and Memorial (Human Rights Watch 2000, 2006; Memorial and Demos Center 2007), newspaper accounts of Russian force deployments, and in some cases, satellite imagery. The relative level of control exercised by incumbents and insurgents is a leading factor in explaining types and patterns of violence in civil war settings (Kalyvas 2006; Stoll 1993). On one hand, we might hypothesize that the presence of Russian bases deters rebels from launching attacks. On the other hand, the very presence of these bases might actually increase insurgent violence since they offer the highest concentration of potential targets.

Finally, *rebel* records whether a village was located in a district controlled by or aligned with Shamil Basayev or Doku Umarov. These leading rebel commanders were guided by different ideologies, and as a result, pitched their recruitment appeals around either radical Islamic tenets (Basayev) or nationalism (Umarov). As both Wood (2003) and Weinstein (2007) have argued, rebel ideologies may shape the nature of insurgent organization, their choice of targets, and their resilience in the face of state counterinsurgency efforts. Data on insurgent organization was taken from published Russian reports, interviews, and rebel statements (most notably, on the leading insurgent Web site, kavkaz.org).

Identification Strategy: Doctrine and Drunks

The treatment consists of 159 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). Although technically secret, base locations were identified using Arcview GIS software by drawing 30 km 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 falling within range of at least one artillery base ($N = 147$).[9] Data were drawn from Russian and Western human-rights organizations, official Russian press releases, rebel Web sites, and local and national newspapers: some twenty sources in four languages (Russian, English, French, and Chechen) were used. All artillery strikes occurred during the war's counterinsurgency phase (June 2000 to December 2005). In total, seventy-three populated centers were struck at least once; the control group consists of seventy-four centers. The total data set records 882 annual village-level observations.

These artillery strikes were responsible for at least 265 deaths and 368 wounded citizens. In addition, at least 583 buildings and farms were damaged or destroyed during these strikes. This is clearly only a fraction of the total violence visited on Chechnya's population, especially since unexploded shells in farmland and forests can nonetheless cripple inhabitants' livelihood.[10] To be included, an artillery strike must have landed in or near a populated settlement. Shelling that occurred in Chechnya's forests, in close support of Russian soldiers during battle, or whose location was vague ("southern Chechnya") was omitted. This is not to minimize the impact of these actions but instead to recognize that we must have accurate geographic coordinates to isolate treatment effects.

Perhaps most importantly, these artillery strikes are uncorrelated with many of the variables commonly cited as explaining insurgent violence. Shali's fire, which accounts for 71 percent of all shelling, derived its apparent 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*, December 14, 2000).

In effect, H&I fire approximates a lottery assignment mechanism. 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 remain 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 throughout agrarian lands and forests, rendering them unusable (e.g., "Villages Shelled, Elderly Person Dies," *Prima-News*, March 6, 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* 2002).

Figure 1
Chechnya
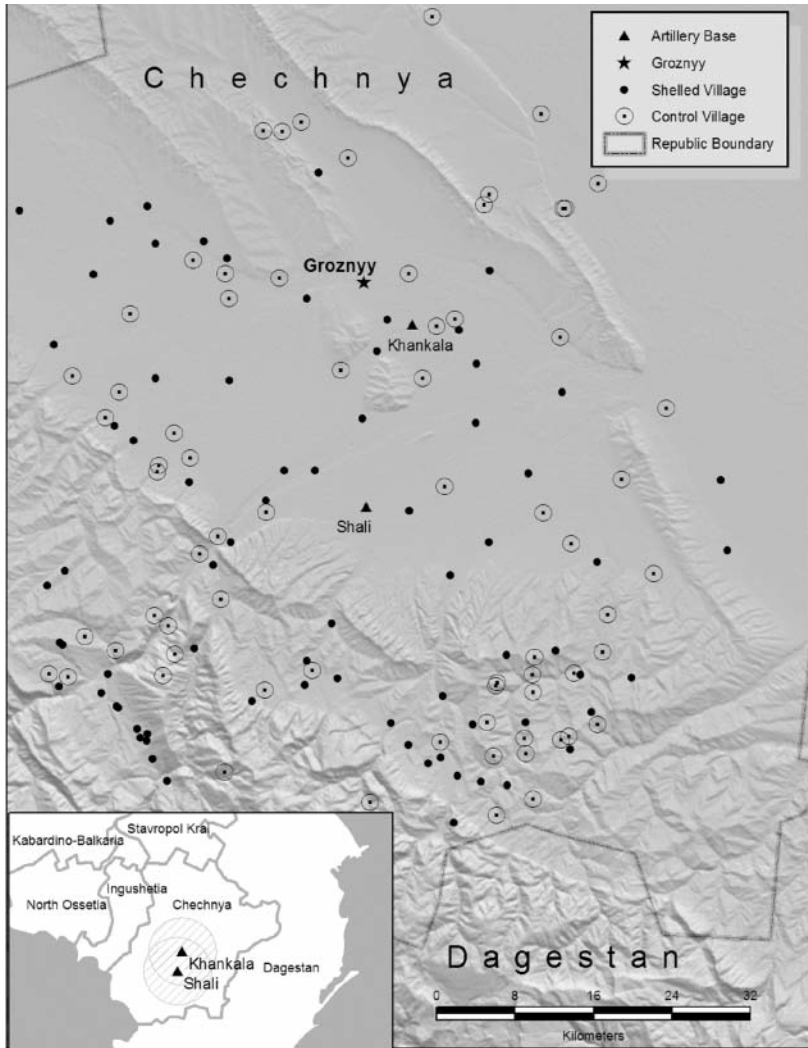
Note: One hundred forty-seven populated settlements (seventy-three treated, seventy-four control). The sample population is defined by the range of Russian artillery (see inset map).

At Khankala, Russia's main base in Chechnya, the remaining shelling (29 percent) was principally because of soldier inebriation and/or accidents. 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*, February 22, 2006).

We can deduce that Khankala's artillery fire is the result of indiscipline thanks to 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." Although enforcement is weak, we have recorded prosecutions of soldiers for the "mistaken" discharge of artillery while inebriated (e.g., "Six Civilians Die," Reliefweb.org, July 17, 2000; "Chechen prosecutor's office opens criminal case," *RFE/RL*, August 16, 2002; "Aiming Error May Cost Officer," *ITAR-TASS Weekly*, November 11, 2005). Soldiers have even shelled themselves accidentally ("Zdes' zhivut liudi," *Memorial*, July 2000).

We also have eyewitness testimony from Russian officers and residents of the shelled villages. As one Russian commander put it, soldiers "get drunk as pigs, lob out a few shells, claim combat pay and get drunk again" (*Time*, October 24, 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*, July 19, 2001).[11]

Internal Validity: Checking As-if Randomization

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

Yet, as Susan Hyde notes (2007, 46), the burden rests on the researcher to demonstrate that the treatment—whose assignment was clearly not supervised by this author—can be treated as if its assignment were indeed random. Despite the qualitative evidence detailed above, any correlation between the treatment and an unseen variable will bias estimates of treatment effects by inducing changes in subject populations that may skew subsequent behavior in nonrandom ways.

Fortunately, there are several means of assessing randomness of assignment. One piece of evidence is provided simply by noting that the distribution of artillery strikes is remarkably even across days of the week.[12] This alleviates the concern that

Table 1
Village Level "As-if" Randomization Tests and Postmatching Statistics

Covariates	Mean Treated	Mean Control	Mean Difference	Std. Bias	Rank Sum	K-S Test
"As if" randomization						
Population	7.364	7.020	0.344	0.209	0.248	0.133
Poverty	2.425	2.284	0.141	0.245	0.163	0.802
Tariqa	0.027	0.068	−0.041	−0.244	0.255	–
Elevation	5.933	5.756	0.177	0.225	0.202	0.169
Isolation	4.424	3.959	0.465	0.171	0.641	0.990
Neighbor	0.742	0.899	−0.157	−0.213	0.321	0.542
Garrison	0.178	0.122	0.056	0.145	0.339	–
Rebel	0.548	0.446	0.102	0.204	0.218	–
Postmatching						
Population	7.830	7.759	0.071	0.046	0.951	0.516
Poverty	2.321	2.239	−0.082	−0.137	0.300	0.983
Tariqa	0.050	0.057	−0.007	−0.030	0.803	–
Elevation	5.834	5.766	0.068	0.095	0.650	0.219
Isolation	3.767	3.836	−0.069	−0.028	0.655	0.516
Neighbor	0.896	0.882	0.014	0.021	0.839	0.516
Garrison	0.258	0.283	−0.025	−0.057	0.614	–
Rebel	0.585	0.522	0.063	0.128	0.260	–
Attacks	2.113	2.151	−0.038	−0.001	0.871	0.713
Sweeps	0.478	0.447	0.031	0.031	0.987	1.000

particular days are associated with a higher probability of shelling, a situation that might arise especially at Khankala if artillery strikes are associated with payday, leading villagers to adopt nonrandom behaviors (i.e., hiding in shelters) in anticipation of being bombarded. Similarly, the strikes themselves are distributed nearly evenly across conflict years.[13]

We can also assess the covariate balance between treated and control villages. Table 1 presents the mean differences between treated and control villages for the eight variables detailed above as well as three different tests of balance. Standardized bias is the difference in means of the treated and control groups, divided by the standard deviation of the treated group. A value less than .25—meaning that the remaining differences between groups is less than one-quarter of a standard deviation apart—is considered a "good match" (Ho et al. 2007, 23, note 15). Following Keele, Mcconnaughy, and White (2008), I also provide Wilcoxon rank-sum test values to determine if we can reject the null hypothesis of equal population medians. Finally, Kolmogorov-Smirnov equality-of-distribution tests are also generated; values less than .1 suggest that the distribution of means is highly dissimilar, while values approaching 1 signify increasing similar distributions (Sekhon 2006).

As Table 1 reveals, the balance across treated and control groups is remarkably similar, though not identical. While some values, particularly for population size and elevation, do demonstrate some evidence of imbalance, minimum thresholds for all tests are met. These results help underscore the indiscriminate nature of the treatment and suggest that strong evidentiary grounds exist for considering these artillery strikes as if randomly assigned.

Finally, we can examine how well these eight variables do in predicting the location of an artillery strike. To do so, I compared the explanatory weight of each variable to a "gold standard" receiver operating characteristic (ROC) curve. In essence, the ROC curve plots each variable's ability to predict correctly whether a village was shelled against a nondiscrimination line that represents random chance (or $p = .5$). The highest ROC value obtained by any covariate was village elevation, at .56, followed by poverty (.559) and population size (.55), suggesting that models with these variables have only a slightly better than even chance of correctly identifying which village was shelled. Once again, the as-if assumption is upheld.[14]

Matching and Difference-in-Difference Estimation

As noted with the example of Liakhovo, however, we must also acknowledge the possibility that these artillery strikes are correlated with battlefield dynamics that arise endogenously from the interaction of Russian and Chechen strategies. Selection effects may be present, for example, if Russians are deliberately shelling villages with the highest levels of insurgent violence independent of other demographic and spatial variables. More subtly, a form of substitution effect might be at work: perhaps Russians rely on artillery barrages instead of policing, meaning that shelled villages may have lower levels of pretreatment violence simply because they have fewer targets to attack. In turn, if mean levels of attacks are sharply different across groups, then the magnitude of the treatment effect, and even its direction, will be highly sensitive to functional form assumptions (i.e., how violence is measured; see Duflo, Glennerster, and Kremer 2007, 16-17).

Put differently, despite the as-if random assignment of Russian shelling given pre-strike spatial and demographic factors, we cannot eliminate the possibility that target selection reflects private Russian information about specific villages' conflict propensities. Rather than assume that these war-induced dynamics are randomly distributed, I turn to matching as a means of explicitly controlling for these selection and substitution issues.

To capture Russian battlefield practices, I recorded the number of sweep operations conducted by Russian and pro-Russian Chechen forces in each village during the ninety days preceding an artillery strike. These sweeps (*zachistki*), a staple of Russian counterinsurgency practices, are operations in which villages are first blockaded and then "swept" by forces seeking to sift insurgents from the noncombatant population through systematic identification checks. In many cases, these sweeps are

occasions for soldiers to inflict abuse on the noncombatant population in the form of disappearances, extrajudicial executions, and widespread theft. Data were drawn from Lyall (2008). In addition, I also recorded the number of insurgent attacks that occurred in the village ninety days before the artillery strike (*attacks*) to control for rebel strategy.

I then matched to create pairs of similar treated and control villages (Rubin 2006; Ho et al. 2007). Matching is a form of data preprocessing that has several advantages in this context. First, by pairing treated villages directly with similar control villages, we can eliminate any residual imbalance that remains between treated and control populations. Second, we can directly control for pretreatment levels of insurgent violence, thereby removing any functional form dependencies. And, third, by matching on identical time intervals, we can account for secular trends in the attack data that are unrelated to the treatment itself.[15]

MatchIt was used to create 159 pairs of similar treated and control villages (Ho et al. 2006). Specifically, these pairs were constructed through nearest-neighbor matching with replacement and were matched along all eight spatial and demographic variables as well as two additional measures of wartime practices to control for endogeneity.

Treatment history was controlled for 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 data set. For example, Tsa-Vedeno was struck on October 21, 2003, and again on January 6, 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.

Second, control villages were matched with shelled villages along identical treatment windows. For example, the small village of Sharo-Argun was shelled on June 5, 2005, killing one inhabitant. Sharo-Argun was then matched with a similar control village, Guni. In each case, the number of sweep operations, along with the mean number of insurgent attacks, was calculated for the period ninety days before the shelling itself (see Table 2). Once 159 pairs had been generated, the mean number of attacks in each village was calculated for the ninety days following the shelling itself. 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 during identical time periods.[16]

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

Table 2
Paired Villages: An Example of Matching

Covariates	Treated Village (Sharo-Argun)	Control Village (Guni)
Population	540	710
Poverty	High	High
Tariqa	0	0
Elevation	893 m	678 m
Isolation	6 neighbors	6 neighbors
Neighbor	1.8 km	3.1 km
Garrison	No	No
Rebel	Basayev	Basayev
Attacks	0	0
Sweeps	0	0

Note: Sharo-Argun was shelled on June 5, 2005. Attacks and sweeps are measured ninety days before this date. There are 159 pairs in total.

compromise: long enough to establish treatment effects but not so long that causal claims become tenuous.[17]

Table 1 summarizes the balance across treated and control groups as well as the degree of improvement across the (already very similar) nonmatched population. In brief, our sample now consists of two similar populations across both preshelling fixed covariates as well as important war-induced factors such as sweeps and preshelling levels of insurgent violence.

Findings

The empirical analysis unfolds over two stages. First, difference-in-difference estimation is used to compare the effect of artillery shelling on insurgent attacks across matched pairs of similar villages. Second, the causal weight of different aspects of indiscriminate violence, including its lethality, destructiveness, and frequency, are tested.

Treatment Effect

Does indiscriminate violence incite insurgent attacks? In brief, no. Indeed, exactly the opposite result is identified. Shelled villages, for example, record a steep drop from a mean of 2.11 pretreatment attacks to only 1.50 in the postshelling window, a reduction of 28.9 percent.[18] By contrast, control villages only witness a 5 percent reduction in the mean rate of attack, falling from 2.15 to 2.05 across identical posttreatment windows.[19] The difference-in-difference is therefore a .51 decrease in the mean rate of attack—a 24.2 percent reduction—that can be ascribed to Russian shelling.

Table 3
Treatment and Insurgent Violence

	Treatment Only *All Villages*	Treatment with Covariates *All Villages*	Treatment Only *Groznyy Dropped*	Treatment with Covariates *Groznyy Dropped*
	1	2	3	4
Treatment	–0.516**	–0.506***	–0.444**	–0.579***
	(0.214)	(0.168)	(0.188)	(0.185)
Constant	–0.101	–0.645	–0.062	–1.112
	(0.093)	(0.785)	(0.835)	(0.893)
F	(1, 121) = 5.80	(11, 121) = 3.45	(1, 118) = 5.58	(11, 118) = 7.90
Prob > F	0.02	0.00	0.02	0.00
N (clusters)	318 (123)	318 (123)	298 (119)	298 (119)

Note: Robust cluster-adjusted (on village) standard errors in parentheses.
*significant at 10%; **significant at 5%; ***significant at 1%.

Table 3 provides estimates for both the unbiased treatment effect (Model 1) and for the complete model that includes all ten of the variables that these villages were matched on (Model 2). The inclusion of these variables is important for screening out potentially confounding factors that might bias our estimates. As can be seen, our estimates of the suppressive effect of artillery shelling remain virtually unchanged when we incorporate these additional variables, suggesting that we have correctly specified the treatment effect. In addition, these results are robust to different specifications of the dependent variable, including its recoding as an ordinal variable (decrease/no change/increase) and as a binary variable (increase/no increase and decrease/no decrease).[20]

We might worry that these results are driven at least in part by violence in Groznyy, Chechnya's highly contested capital, whose symbolism has made it a focal point of insurgent and incumbent efforts alike. This concern is somewhat alleviated by the fact that Groznyy was disaggregated into its four constituent districts to facilitate matching.[21] Models 3 and 4 therefore estimate treatment effects with Groznyy omitted. In neither case does the basic negative relationship between indiscriminate violence and level of insurgent violence change.

An average decrease of .51 attacks may appear to be a substantively small effect. The cumulative effect is quite large, however. With at least 336 attacks identified in the cumulative preshelling windows of treated villages, about 81 attacks are missing from the posttreatment interval because of artillery strikes (or between 28 and 136 missing attacks with a 95 percent confidence interval). More bluntly, the average insurgent attack in the cumulative pretreatment window of shelled villages killed 0.88 Russian soldiers and pro-Russian Chechen militia members and wounded another

1.21 soldiers. This reduction in insurgent attacks therefore translates into about seventy-one soldiers who avoided being killed by insurgent violence (or between 25 and 120 with a 95 percent confidence interval), along with a further 107 soldiers (or 34 to 165, with a 95 percent confidence interval) who escaped being wounded. These figures lay bare the brutal logic behind indiscriminate violence: it can suppress insurgent violence, and in so doing, degrade insurgents' military capabilities.

Testing "Triggers" of Insurgent Violence

Existing theories also implicitly suggest that state violence works through several mechanisms (or "triggers") to change patterns of insurgent violence. It is often assumed, for example, that a positive relationship exists between the lethality and destructiveness of state actions and the amount of violence subsequently generated by insurgents. This section explicitly tests this relationship by moving beyond the binary treated–control distinction to examine various facets of the treatment itself. More specifically, three aspects of the treatment are examined here: its lethality (the number of individuals killed and wounded in a strike); its destructiveness (the number of buildings and farms damaged or destroyed in a strike); and the frequency of shelling (the number of times a village was struck).

I therefore distinguished between artillery strikes that killed individuals, wounded individuals, and destroyed property from those that did not using dummy variables. I also created a dummy variable to reflect whether a village had been shelled more than once. I also created variables with the actual count for each type of violence inflicted on settlement inhabitants; for property damage and multiple strikes, I logged these values to reduce skew and kurtosis.

Table 4 details causal estimates for each treatment subcategory (Models 5 to 12) using models that incorporated all matched variables. Several findings emerge.

First, all but one of these eight measures of the type and severity of violence inflicted are negatively associated with increases in postshelling insurgent violence. Setting aside the dummy variable for property damage, every measure contradicts the contention that state actions provide a catalyst for increased insurgent violence.

Second, only two measures—the dummy variables for deaths inflicted and for wounded individuals—reach conventional levels of significance. These findings suggest that insurgent retaliation may vary in part on the type of violence inflicted by the state, with lethality clearly triggering a (negative) response not mirrored by either property damage or frequency of shelling. Interestingly, little evidence appears to exist for the contention that higher levels of violence inflicted (as captured in the count variables) are strongly associated with either increased or decreased insurgent violence. We must be cautious in interpreting this finding, however, since the small number of observations available here can provide only an initial test of the relationship between severity of indiscriminate violence and observed insurgent responses.

Table 4
"Triggers" and Insurgent Attacks

	Deaths (Yes/No)	Deaths (No. Fatalities)	Wounded (Yes/No)	Wounded (No. Wounded)	Property (Yes/No)	Property (No. Destroyed)	Multiple Shelling (Yes/No)	Multiple Shelling (Times Struck)
	5	6	7	8	9	10	11	12
Coefficient	−0.449**	−0.023	−0.452*	−0.034	0.054	−0.030	−0.103	−0.145
	(0.235)	(0.094)	(0.253)	(0.035)	(0.189)	(0.093)	(0.282)	(0.176)
Constant	−0.212	−0.489	−0.662	−0.295	−0.570	−0.451	−0.494	−0.556
	(1.426)	(1.419)	(1.425)	(1.503)	(1.480)	(1.489)	(1.445)	(1.415)
$F(11, 73)$	2.25	3.30	4.97	7.39	2.02	1.99	2.11	2.24
Prob > F	0.02	0.00	0.00	0.00	0.04	0.04	0.03	0.02
N	159	159	159	159	159	159	159	159
Clusters	(74)	(74)	(74)	(74)	(74)	(74)	(74)	(74)

Note: Robust cluster-adjusted (on village) standard errors in parentheses. All covariates are included in these models.
*significant at 10%; **significant at 5%; ***significant at 1%.

Discussion

These findings are counterintuitive. We might wonder, however, whether threats to causal inference lurk amid the tangle of complex events and processes that compose the second Chechen War. This section addresses three inferential treats. First, the possibility exists that Russian shelling creates local externalities; that is, it may suppress insurgent violence in shelled villages but at the cost of redistributing it—and perhaps increasing it—among neighboring villages. Second, the decision to adopt ninety-day treatment windows may overestimate the effect of shelling by truncating the response window if grievances are cumulative in nature. Finally, scholars using micro-level research strategies must confront the question of whether their findings are generalizable.

Local Treatment Externalities

One possible objection to these findings centers on the claim that indiscriminate violence merely redistributes, rather than represses, insurgent violence. Indeed, rational insurgents may simply move to new locations to avoid calling down further repression on their own (shelled) villages. Given this view, we might expect that artillery shelling generates local externalities or "spillover" in the form of increased levels of attacks in neighboring villages relative to settlements that neighbor control villages. In short, the net effect may be an uptick in insurgent attacks even as the original target of the state's indiscriminate actions records diminished insurgent violence. It is precisely the neglect of these externalities that may lead unwary militaries to conclude that their violence "worked" even as ever-greater amounts of insurgent violence diffuse among neighboring settlements.

The problem of assessing treatment externalities is one of the most difficult and least acknowledged in the study of civil wars (on externalities, see Miguel and Kremer 2004). Compounding this situation is the lack of fine-grained biographic details of insurgents necessary to determine who is conducting attacks, the rate of village recruitment, and the degree of cross-village movement within (and outside) the conflict area.

In Chechnya, however, the problem of externalities is partly mitigated by the rebels themselves, who are principally organized at the village level. Indeed, cross-village and (especially) cross-district joint raids within Chechnya are the exception, not the norm. The combination of a localized command structure and the perishable nature of the insurgent's information advantage as distance from familiar settings increases suggests that we are most likely to observe insurgent reactions in or near the shelled village.

Moreover, externalities are also reduced, but not eliminated, via identification strategy. Given that Russian shelling is not correlated with key variables such as terrain or population size, it becomes difficult for insurgents to identify safe havens ex ante,

especially since Russians are shelling with replacement (that is, the same villages may or may not be shelled repeatedly). Armed with knowledge of both Russian barrage patterns and their own location, insurgents will either remain in place—thus eliminating externalities—or will leave for control and shelled villages in similar proportions since they cannot anticipate strike locations.

To ensure that insurgent violence was not spiking "off-stage" in neighboring villages, I shifted the focus from treated and control villages to their neighbors. Drawing on isolation, I created a data set consisting of all villages neighboring the treated village and its paired control village within a 5 km^2 radius. For example, the village of Khatuni, which was shelled on October 3, 2005, was surrounded by three neighbors ("treated neighbors"). Khatuni's match, Goichu, also possessed three neighbors within a 5 km^2 radius, providing three "control neighbor" observations. As before, deserted villages were dropped from the analysis, leaving 939 observations.

I then matched these "treated" and "control" neighbors (without replacement) using all of the variables detailed above. While matching, all cases in which the same village was simultaneously a control and a treated neighbor—a function of close spatial matching in the original pairing—were dropped. The same ninety-day pretreatment and posttreatment windows were retained. In addition, a new variable, *treatment history*, was added to record the number of times a particular village had been shelled in the past. This ensures that any treatment effects are balanced across the treated-neighbor and control-neighbor populations. The matched data set consists of 840 observations (420 treated neighbors, 420 control neighbors) and passes all standardized bias, rank sum, and Kolmogorov-Smirnov tests for equality of distribution.[22]

Do villages neighboring a shelled village record significantly different (and higher) levels of insurgent attacks than control neighbors? No. In fact, settlements neighboring shelled villages actually record a sharper decline in the mean rate of postshelling attacks than neighbors of controls. Neighbors of treated villages shift from a mean of 1.55 insurgent attacks in the preshelling window to 1.43 mean attacks (about –7 percent). Control neighbors, by contrast, drop from 1.72 attacks to 1.62 (or about –6 percent) during the same period. In each case, the change in pretreatment to posttreatment violence is statistically significant, a trend that reflects the overall decline in insurgent violence over the 2000 to 2005 era.[23] The difference-in-difference estimate of the treatment effect, however, is not significant, a fact confirmed by re-estimating Models 1 and 2 from Table 2 with these new data.[24] Put simply, the evidence does not support the claim that violence is redistributed to neighboring villages.

Treatment Windows

The strength of difference-in-difference estimation resides in its ability to isolate the causal impact of indiscriminate violence over the short-to-medium term. Yet, this framework implicitly assumes that a tight temporal link exists between state actions

and insurgent responses and that attacks, if observed, will occur in the initial hours or days after state violence.[25] This view may be mistaken, however, because expectations about the timing of insurgent attacks typically remains underspecified in existing theories. It is possible, for example, that grievances accumulate slowly during many months, even years, before reaching a tipping point that results in insurgent violence.[26] If this "slow fuse" view is correct, then using ninety-day intervals will exaggerate shelling's suppressive effects by prematurely truncating the response window.

We can address this concern by examining the timing of the insurgents' first attack (if any) following an artillery strike. Specifically, we must test whether the probability (the "hazard") of observing an insurgent attack increases, decreases, or remains constant over time.

Figure 2 illustrates the hazard rate for shelled villages. Estimates were obtained using Weibull regression, which allows hazard-rate distributions to increase or decrease monotonically across time (Box-Steffensmeier and Jones 2004, 31-37).

Several results emerge. First, we are most likely to observe an insurgent attack in the initial two weeks following a Russian strike, after which the hazard rate steadily decreases. Using Nelson-Aalen estimates, no change in the treated villages' cumulative hazard rate is observed after the seventy-five–day mark, suggesting that stretching the treatment window is unlikely to capture more initial attacks associated with being shelled.

In addition, treatment is associated with a 30 percent decrease in the hazard of observing a first insurgent attack, a result that is both statistically significant ($p = .04$) and robust to multiple specifications of parametric regression (including exponential and Gompertz distributions). Indeed, shelled villages recorded a *lower* probability of observing a posttreatment attack than control villages.[27] Both of these findings run counter to "slow fuse" expectations. Given that hazard rates do not monotonically increase over time and that treated villages are actually less likely to observe an insurgent response than controls, the decision to retain fairly short treatment windows in this setting appears justified.

External Validity

Perhaps, however, these results hinge on specific contextual characteristics of the sample population that inhibit generalization beyond Chechnya. There are, after all, limits to the insights derived from a single case, and only replication in different subnational and cross-national contexts will reveal whether these findings are broadly generalizable.

Yet, there are strong reasons to discount the argument that these findings are products of case-specific idiosyncrasies. There is little distinctive about either the form of indiscriminate violence examined here or its assignment mechanism, for example. Indeed, part of the article's puzzle arises from the fact that these apparently

Figure 2
Are Treatment Windows Too Short? The Hazard Rate of
Insurgents' First Attack in Shelled Villages
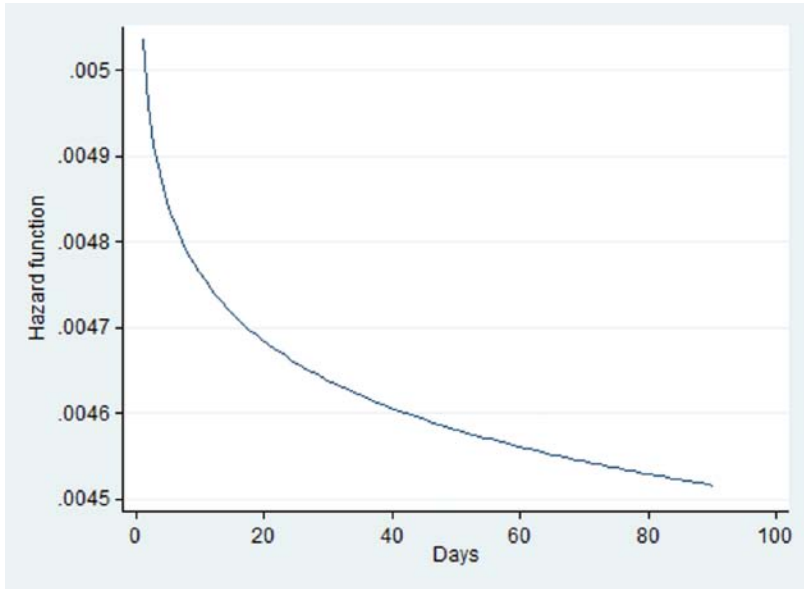
Note: Hazard estimates derived from Weibull regression with robust standard errors clustered on individual villages. The parameter value is .91, indicating a monotonically decreasing hazard rate over time. $N = 159$.

counterproductive strategies have in fact been used by many states—democracies included—in their counterinsurgency efforts for two centuries, a trend that shows little evidence of abating.

Nor is Chechnya an outlier in the broader distribution of civil wars. In fact, by many accounts, Chechnya is an archetypal example of an ethnic civil war, one in which a distinct population in a geographically compact area seeks separatism through violence. According to Fearon and Laitin (2003), two-thirds of all post-1945 civil wars (75 of 106) are coded as principally or at least partially ethnic in nature, suggesting that Chechnya is, from this view, the norm rather than the exception. Similarly, the presence of oil in Chechnya provides contraband funds that are a hallmark of many civil wars (Collier and Hoeffler 2004; Fearon 2004). Even Chechnya's estimated per capita income—US$478 in 2006—is unremarkable, situated between that of Afghanistan (US$335 in 2006) and Iraq (US$949). In historical terms, this income level is comparable to that experienced during civil wars in Liberia, Mali, and Laos.[28]

Finally, Chechnya also exhibits variation among explanatory variables commonly cited as shaping insurgent violence. These include variation in terrain (ranging from flat plains to forested mountains), insurgent mobilizing appeals (nationalism/Salafism), settlement size and spatial density, and levels of control wielded by the incumbent. This is not to deny that Chechnya has unique attributes, of course. Few conflicts can boast—if that is the proper term—of a legacy of resistance that stretches over more than two centuries, as is the case in Chechnya (Gammer 2006). Moreover, the weak nature of public opposition to Russian excesses in Chechnya (Lyall 2006) may also grant an increasingly autocratic Kremlin a freer hand to brutalize civilians than its democratic counterparts (but see Downes 2008). These differences aside, it appears that Chechnya is sufficiently representative that these findings are likely to be replicated in other conflicts as well.

Conclusion

This article set out to investigate the relationship between a state's use of indiscriminate violence against noncombatants and the subsequent patterns of insurgent attacks. Drawing on new micro-level data, two surprising results emerged. First, artillery strikes led to a decrease in poststrike insurgent attacks when compared with nearly identical control villages. Second, many of the theorized "triggers" for retaliatory insurgent attacks, including the lethality and frequency of the state's violence, were negatively correlated with being shelled.

These findings are both counterintuitive and controversial. The article clearly should not be read as endorsing the use of random violence against civilians as a policy instrument. Such actions are morally abhorrent and are rightly regarded as war crimes under both international law and Russia's own legal system. Nor can we evaluate the *relative* effect of such tactics, since the Russian military eschewed a "hearts and minds" approach that may have proven even more effective at reducing insurgent violence. Finally, difference-in-difference estimates are most reliable in the near-to-medium term because the identifying assumption of parallel trends is more likely to be obtained during short time intervals. We should therefore resist the temptation to extrapolate from discrete findings that hold during the ninety-day treatment windows used here to broader (long-term) questions of war outcomes.

That said, however, the fact that indiscriminate violence can have suppressive effects helps explain the otherwise puzzling persistence of these practices among the world's militaries. Policy makers and human-rights activists with a stake in abolishing such practices will have a difficult task before them given that their best rhetorical strategy—pointing out that indiscriminate violence is ineffective—will not hold true in at least some conflict settings.

To be sure, one study is insufficient to overturn the near consensus surrounding the effects of state-directed indiscriminate violence. This article does, however, raise new questions about the scope conditions under which indiscriminate violence

suppresses insurgent violence as well as the mechanisms by which state violence shapes insurgent behavior. Similarly, these findings raise questions about how state violence may have a variable effect across different types of insurgent organizations, contingent on their goals and their relationship with the local populace.

Testing these relationships in multiple contexts, as well as sorting out which mechanisms are at work (and why), will be a close-range task by necessity: national-level data are simply too crude to capture these dynamics. Even case-based methods may prove inadequate if the often-severe inferential threats that lurk in civil war data are not addressed with appropriate research designs. These efforts will demand both conflict-specific knowledge and methodological skills but hold out the promise of substantially enriching our theories of the dynamics of violence in civil wars.

Notes

1. *Indiscriminate violence* is defined as the collective targeting of a population without credible efforts to distinguish between combatants and civilians. This definition assumes that the nature of violence (selective or indiscriminate) is independent of the scale of the state's violence.

2. Note that this micro-level logic is consistent with different macro-level outcomes, including "spiral" models of civil-war violence (i.e., Posen 1993) as well as grinding, protracted wars in which the net effect of state violence maintains an insurgency at or slightly above replacement levels (i.e., Fearon 2004).

3. Endogeneity occurs when independent variables are a consequence or response to the dependent variable rather than a cause.

4. In fact, Hill (2005, 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 caused by a halving of existing partisan bands: German violence had made it too dangerous to concentrate in large groups.

5. Large-scale violence can also disillusion the population about the prospects of insurgent victory, stoking a desire for stability and the war's end, regardless of incumbent behavior. See, for example, Stoll (1993, 122-28).

6. A full test of this theory would require extremely detailed ethnographic time-series data on individual participation, insurgent recruitment rates, and the mobility of individuals within the conflict zone. These data typically do not exist for most conflicts, including Chechnya. I therefore focus on coercive capabilities, including the frequency and location of insurgent attacks, as a (relatively) more tractable observable implication.

7. A complete list of sources is provided in the data set's codebook, available on the author's Web site.

8. While a 5 km^2 radius may appear quite small, the actual area encompassed by such a coding rule (78.5 km^2) is quite large, especially in a relatively small Chechnya.

9. Collectively, about 34 percent of Chechnya (5,272 km^2) is within range of at least one base. The bases are 19 km apart and possess overlapping fields of fire of about 380 km^2. When assessing distance, I allowed for a +2-km margin of measurement error to account for wind and imprecision in village location.

10. Moreover, unexploded ordinance (UXO) is often recycled by insurgents as the basis for improvised explosive devices (IEDs).

11. Additional evidence on treatment randomness was gathered through interviews with local human-rights observers. Because of 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 data set.

12. Strikes by day: Sunday (twenty-three), Monday (nineteen), Tuesday (twenty-four), Wednesday (twenty-three), Thursday (twenty-three), Friday (twenty-four), and Saturday (twenty-one).

13. The number of artillery strikes by year: 2000 (thirty-six), 2001 (twenty-nine), 2002 (sixteen), 2003 (twenty), 2004 (twenty-five), and 2005 (thirty).

14. Bonferroni-adjusted *p* values were not significant for any variable.

15. Note that the use of matching pushes the research design out of a natural-experiment framework, which assumes that covariate balance (at least in expectation) is achieved solely through random assignment rather than data preprocessing. On natural experiments, see Posner (2004), Miguel (2004), Hyde (2007), and Dunning (2008).

16. More formally, the difference-in-difference (DD) estimator is obtained as follows: $DD = (Y_1^t - Y_0^t) - (Y_1^c - Y_0^c)$, where $Y_T \in (0,1)$ are the pretreatment and posttreatment periods, T denotes the treatment group, and C denotes the control group.

17. By contrast, annual-level measures are simply too aggregate to disentangle cause and effect, especially if the assumption that the treatment and the response must be contained within the same annual observation imposes artificial breaks in the data (as will be the case if artillery strikes occur early or late in a given year). On the problem of circular data, see Gill and Hangartner (2008).

18. Statistically significant at $p = .0001$, $t(-4.69, 158$ df$)$.

19. This difference is not statistically significant, with $p = .19$, $t(-0.89, 158$ df$)$.

20. For space reasons, I report these additional models in the online appendix. These results also hold if we restrict our analysis to either Khankala or Shali.

21. With an estimated population of 67,000 in March 2000—down from a prewar estimate of 210,000—Groznyy dwarfed all other populated settlements, necessitating its disaggregation to find suitable matches.

22. These balance statistics are provided in the online appendix.

23. The posttreatment–pretreatment difference for treated neighbors is significant at $p = .03$. The posttreatment–pretreatment difference for control neighbors is significant at $p = .06$.

24. The difference-in-difference is not significant using a two-tailed t-test ($p = .79$). Statistical results are provided in the online appendix.

25. Evidence for this "quick trigger" view can be found in Berrebi and Lakdawalla (2007, 126-27), Stoll (1993, 117), and Iyengar and Monten (2008).

26. See, for example, Wood (2003, 237-40), Horowitz (2003, 136-48), and Gurr (1970).

27. A log-rank test of equality of survivor functions narrowly misses conventional levels of statistical significance ($p = .17$).

28. According to Fearon and Laitin's (2003) data, Chechnya's logged per capita income is less than 1 standard deviation away from the mean per capita income level for all 106 post-1945 civil wars. Care should be taken when interpreting these figures, however, since national-level data typically obscure lower income levels in the war-torn region itself.

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