# Bombing to Lose?
# Airpower and the Dynamics of Coercion
# in Counterinsurgency Wars*

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Are airstrikes an effective tool of coercion against insurgent organizations? Despite the question's historical and contemporary relevance, we have few dedicated studies, and even less consensus, about airpower's effectiveness in counterinsurgency wars. I draw on newly declassified United States Air Force records of nearly 23,000 airstrikes and non-lethal shows of force in Afghanistan (2006-11) to examine how insurgents respond to actual and threatened coercion. A new form of dynamic matching is adopted to facilitate village level causal inference over variable temporal and spatial windows. Several findings emerge: both airstrikes and shows of force are associated with increased insurgent attacks; these effects are highly localized; and civilian casualties appear to play little role in driving these attacks. Instead, these air operations create opportunities for insurgents to build and maintain reputations for resolve by quickly responding to counterinsurgent actions with their own violence.

*Don't hit someone else's door with a
finger because your door might be hit
with a fist.*

Dari Proverb

Are airstrikes an effective tool of coercion against insurgent organizations? Since 1911, when the first halting steps toward aerial bombardment were made by Italian pilots over Tripolitania's deserts, states have sought to harness airpower's presumed coercive potential to the task of influencing insurgent behavior. The past decade alone has witnessed extensive air campaigns against insurgents in Afghanistan, Iraq, Pakistan, Yemen, Palestine, Russia, Somalia, Myanmar, Syria, Sudan, Mali, Nigeria, Colombia, and Libya. Yet we possess only a handful of (contradictory) studies of airpower's effects in counterinsurgency wars.[1] Indeed, nearly all existing work on airpower remains interstate and crossnational in focus, where "effectiveness" is usually defined in terms of strategic outcomes such as victory/defeat.[2] Here, too, there is substantial debate over airpower's effectiveness both across time and in high-profile cases such as Kosovo.[3]

I take up the challenge of theorizing and testing the coercive effects of airpower in counterinsurgency wars. I argue that airpower, for all its ability to threaten and harm an adversary, generate incentives for insurgents to build and then maintain their reputations for war-fighting by escalating their attacks after experiencing airstrikes. Insurgents face two audiences—the counterinsurgent and local populations—that they seek to influence through their own coercive strategies. By quickly striking back at counterinsurgent forces, insurgents demonstrate their resiliency and continued ability to impose costs, helping to bolster their bargaining leverage. These attacks also reveal that insurgents retain the capacity to punish would-be defectors from the civilian populace that they seek to control. To be sure, airstrikes can attrit insurgent organizations. Yet these costs are unlikely to outweigh the bargaining leverage to be gained if insurgents develop a reputation for resolve.

To test this reputational argument, I draw on declassified United States Air Force

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[1]Corum and Johnson 2003; Kocher, Pepinsky and Kalyvas 2011; Johnston and Sarhabi 2013.

[2]Pape 1996; Byman and Waxman 1999; Horowitz and Reiter 2001; Byman and Waxman 2002; Gray 2012; Allen 2007; Van Creveld 2011.

[3]Byman and Waxman 2000; Stigler 2002/03; Pape 2004.

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(USAF) data and open source satellite imagery to detail nearly 23,000 air operations in Afghanistan (2006-11). These air operations are divided between airstrikes and shows of force—simulated bombing runs where no weapons are released—and facilitate testing the effects of actual and threatened coercive action. I draw on the advantages of an SQL relational database to implement a new form of dynamic matching that estimates causal effects of air operations over variable temporal and spatial windows as fine-grained as a single day and kilometer around bombed villages and their controls.

Four main findings emerge. First, airstrikes and, to a lesser extent, shows of force, are strongly associated with net increases in the mean number of post-event insurgent attacks in targeted villages relative to control villages. Second, these increases are fairly long-lived, lasting at least 90 days after an air operation, though the magnitude of the effect dissipates over time. Third, consistent with a reputation-based argument, these effects are largest in the immediate vicinity of the targeted location. Finally, and perhaps most counterintuitively, these effects are not associated with civilian casualties. Instead, battlefield dynamics provide nearly all the explanatory leverage when accounting for post-event patterns of insurgent attacks. These results are robust to multiple placebo tests, cross-validation with two different datasets of insurgent violence, and alternative models.

The shift from crossnational to subnational data helps test and extend theories of interstate coercion in two ways. First, interstate coercion is a relatively rare event. The most comprehensive dataset of compellent threats, for example, records 210 cases from 1918-2001.[4] Similarly, leading studies of strategic bombing draw on a relatively small number of wars when drawing inferences about its effectiveness as a coercive tool.[5] By contrast, our dataset encompasses 23,000 discrete air operations (nearly 13,000 shows of force). These disaggregated data permit a far closer investigation of the temporal and spatial dynamics of coercion, along with the mechanisms underpinning these effects, than possible with existing crossnational data.

Second, counterinsurgency wars represent a "most likely" domain for observing reputation dynamics at work. A vigorous debate still rages over whether (and when) reputations matter for states in international crises and wars.[6] Given that counterinsurgency wars are

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[4]Sechser 2011, 379.

[5]Pape 1996 draws on 33 wars while Horowitz and Reiter 2001 use 53 cases in their crossnational statistical analysis.

[6]For a recent review of this vast literature, see Dafoe, Renshon and Huth 2014. Key works include Slantchev 2011; Sechser 2010; Press 2005; Sartori 2002; Schultz 2001; Mercer 1996; Fearon 1994; Huth

marked by repeated interaction between combatants over time, actors should invest heavily in their reputations for resolve since they (correctly) anticipate that they will continue to meet on the battlefield.[7] Credible commitment issues also abound in these wars: sharp power asymmetries between the counterinsurgent and an insurgent organization mean that commitments to not exploit a weaker opponent are non-credible.[8] Aside from their pressing policy relevance, counterinsurgency wars offer an important venue for advancing our theoretical and empirical understanding of the dynamics of coercion.

# 1    Defining Coercion

The study of interstate coercion has been heavily dominated by the writings of Thomas Schelling, who famously characterized coercion as the "power to hurt" an enemy that rests on the "threat of damage, or more damage to come," if compliance with one's demands is not forthcoming.[9] He further distinguished between two types of coercive threats: compellent threats (that "make an adversary do something") and deterrent threats (that "keep him from starting something"), though the distinction between these two categories of threat often disappears once the engagement starts.[10]

Schelling contrasted coercive threats with "brute force," or the use of violence to exterminate a foe.[11] While at first glance airstrikes might appear to be "brute force" and thus outside the theoretical framework of coercion, this is not the case. Absent genocidal intent by a counterinsurgent, airpower represents a coercive instrument in a wider bargaining framework where combatants use latent and actual violence to obtain an advantageous political settlement. Coercion in civil wars is necessarily a dynamic process between three actors—counterinsurgents, rebels, and the civilian population—who anticipate facing one another repeatedly since no single coercive act (or its threat) can destroy an insurgency.[12] For that reason, nearly all scholars of airpower firmly situate their studies in a framework

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1988; Huth and Russett 1984.

 [7]For evidence of reputation-building in lab experiments during repeated play situations, see Walter and Tingley 2011.

 [8]Fearon 1995.

 [9]Schelling 2008, 3.

 [10]Schelling 2008, 69-72, cite on p.80.

 [11]Schelling 2008, 5.

 [12]Byman and Waxman 2002, 3-6.

of coercion, not brute force.[13]

I therefore define *coercion* as the use of latent and actual violence to compel an adversary to change his behavior through the threat and imposition of (additional) harm if current demands are not met.

In our context, airpower as a coercive instrument comes in two forms. First, *shows of force* are non-lethal threats that signal to insurgents that punishment will be forthcoming if they do not cease their actions. Air forces routinely use such tactics, as evidenced by recent usage in Afghanistan, Iraq, Syria, and Ukraine, to simulate airstrikes: one or more aircraft "buzz" insurgents at low level and high speed (often while dispensing flares in a pyrotechnic display) but no weapons are released. Schelling himself included such practices as an example of latent coercion: "battlefield tactics that frighten soldiers so that they run, duck their heads, or lay down their arms and surrender represent coercion based on the power to hurt."[14]

Second, *airstrikes* represent the actual imposition of harm on insurgent organizations to compel them to abandon their political ambitions by imposing costs on rebels, their leadership, and supporters among the populace. Airstrikes are clearly designed to have effects on remaining insurgents even if some are initially targeted and killed. Drawing on the example of American Indian Wars, Schelling himself noted that "If *some* Indians were killed to make *other* Indians behave, that was coercive violence—or intended to be, whether or not it was effective."[15] Strategies of attrition or leadership decapitation via airpower should therefore be considered coercion unless they are aimed at the total extermination of an insurgent organization (and, presumably, its civilian supporters).[16]

Despite a century of experience with airpower as a coercive instrument, however, there is little agreement on its effectiveness. I first review the debate and then advance my own theoretical account of airpower's effects in a counterinsurgency environment.

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[13]Van Creveld 2011; Horowitz and Reiter 2001; Pape 1996; Byman and Waxman 1999; Hoffman 1989.

[14]Schelling 2008, 9.

[15]Schelling 2008, 2.

[16]Modern aerial campaigns fitting this description are fortunately rare, however. Even the tremendous aerial devastation unleashed by the US in Vietnam or the Soviets in Afghanistan was designed to coerce their foes, not destroy them entirely, which was quickly regarded as an impossible task.

## 1.1 Airpower as an Effective Tool of Coercion

Strategists heralded the advent of airpower as a cheap, effective, and "civilized" means of fighting insurgents as early as the 1920s. Prominent early advocates, including Winston Churchill, Hugh Trenchard, and Giulio Douhet, were influenced by their experiences in "aerial policing" campaigns—including Somaliland, Mesopotamia, Tripolitania, Northwest Frontier Province, and Transjordan—that bombing restive populations was both desirable and feasible.[17] Schelling's own writings, though typically associated with nuclear strategy, actually draw heavily on airpower examples (especially Vietnam) to illustrate the properties of "ideal" coercive acts.[18]

In this view, airpower creates bargaining leverage by acting as a signal of a counterinsurgent's latent power to hurt an adversary as well as its ability to impose escalating levels of harm if compliance is not forthcoming.[19]Airpower generates coercive leverage through at least three mechanisms — decapitation, attrition, and punishment — that singularly and collectively suggest airstrikes should decrease insurgent attacks.

Airstrikes may cripple insurgent organizations by decapitating their leaders, degrading command and control structures and in turn reducing their capacity to conduct attacks. Airstrikes may also influence insurgent actions through attrition of an organization's rank-and-file. Killing insurgents at a faster clip than the replacement rate may reduce future attacks by shrinking the available pool of rebels while dissuading would-be insurgents from taking up arms. Airstrike effects might also be governed by a punishment logic among insurgent supporters. Bombing may persuade supporters to curb their material aid to the insurgency, withhold information about counterinsurgent behavior, place operational restrictions on attacks, and, most drastically, switch sides.[20]

A careful study of nearly 400 drone strikes in Pakistan (2007-11) illustrates how airstrikes can negatively affect militant violence.[21] Using an agency- and week-level fixed effects estimation strategy, these authors conclude that militant attacks decreased an average of almost five percentage points during weeks with at least one drone strike. Moreover, the lethality of these militant attacks decreased by nearly 25 percentage points during the

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[17]Van Creveld 2011, 51-78.

[18]Schelling 2008, 6,8,13,16,17-18,25,30. All of these references are from the first chapter alone.

[19]Schelling 2008, 89.

[20]Pape 1996; Byman and Waxman 2002; Lyall 2009.

[21]Johnston and Sarhabi 2013, 36.

week of a drone strike. While the authors caution against making strong causal claims given their empirical strategy,[22] these findings suggest that airpower can reduce insurgent attacks in a modern civil war setting.[23]

Theorists have also emphasized airpower's deterrence effects. Once unleashed, airpower, it is argued, creates a credible deterrent that dissuades insurgents from launching attacks for fear of being subjected to further aerial coercion. "Prompt action by the air force at the first sign of trouble calmed tribal insubordination... before it could grow dangerous," Sir Basil Hart wrote about uprisings in Mesopotamia, "and there has been an immense saving of blood and treasure to the British and Iraqi governments."[24]

Non-lethal shows of force should therefore also be successful coercive instruments. These threats are signals to insurgents and their supporters about future costs if present actions are not reversed.[25] Shows of force have clear incentive structures and are openly communicated to adversaries, two requirements for successful coercion.[26] To be sure, such actions may lack credibility if conducted in isolation from a bombing campaign; the counterinsurgent must invest in a reputation for using coercion if these signals are to be perceived as credible threats.

Instead, these shows of force, against a general background of a bombing campaign, represent the first stage of a risk strategy designed to demonstrate that future bombing is conditional on compliance by the targeted insurgent organization. For insurgent leaders and rank-and-file alike, the appearance of airpower over the battlefield can disrupt their current attack by forcing them to scatter. Shows of force complicate future planning by increasing the difficulty of organizing collective action given the need to disperse or remain hidden to avoid detection. "Life in a cave," noted one early airpower enthusiast, "is no high life casino."[27] As a result, there is reason to believe that both airstrikes and shows of force are associated with net decreases in insurgent attacks.

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[22]Johnston and Sarhabi 2013, 27,40.

[23]More generally, Byman (2006) and Johnston (2012) argue that decapitation strikes can degrade insurgent capabilities and help bring about counterinsurgent victories.

[24]Hart 1932, 155.

[25]Fearon 1994.

[26]Schelling 2008, 80.

[27]Peck 1928, 542.

## 1.2   The Case Against Airpower

Two different schools of thought have converged on an unflattering view of airpower's use in counterinsurgency contexts as (at best) a fool's errand and, at worst, counterproductive. As one survey put it, "the use of airpower in [civil] wars has been the record of almost uninterrupted failure."[28]

Existing studies of strategic bombing have concluded that these campaigns are unlikely to bring about desired outcomes.[29] If coercion against states is typically ineffective, then counterinsurgency wars are the edge case, since insurgents typically lack the key assets—capitals, infrastructure, and fielded forces—that must be threatened if coercion is to have a chance at being successful. Even simply identifying insurgents can be difficult if they blend within the population. Civilians may also not exercise any influence over insurgent decision-making, making punishment futile. Some insurgent organizations may be sufficiently decentralized to foil leadership decapitation efforts.[30] As Robert Pape concludes, "Guerrillas should be largely immune to coercion."[31]

Microlevel scholarship has also emphasized the counterproductive nature of airpower in counterinsurgency wars. No matter how precise, airstrikes will kill civilians, shifting support away from the counterinsurgent while creating new grievances that fuel insurgent recruitment.[32] This logic is on display in a careful study of US bombing of South Vietnam, where airstrikes were associated with a shift of hamlets from pro-government to pro-Vietcong control from July to December 1969.[33] While the dependent variable in this study is territorial control, not Vietcong attacks, the account is consistent with the claim that civilian casualties lead individuals to shift their allegiance away from the perpetrator, fueling further violence.

If airstrikes backfire, then it is likely that shows of force are also unreliable coercive threats. These public signals impose little cost on the sender and so may be disregarded

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[28]Van Creveld 2011, 338.

[29]Pape 1996; Corum and Johnson 2003; Byman and Waxman 2002; Horowitz and Reiter 2001; Clodfelter 1989; Thies 1980.

[30]Jordan 2009.

[31]Pape 1996, 74. He does note, however, that theater air power—like that deployed in Afghanistan—is a "much stronger coercive tool" than strategic bombing against irregular forces when combined with ground forces (p.318).

[32]Petersen 2001; Kalyvas 2006; *U.S. Army Field Manual No.3-24* 2007; Condra and Shapiro 2012; Ladbury 2009.

[33]Kocher, Pepinsky and Kalyvas 2011.

by targeted audiences as "cheap talk."[34] Repeated exposure to these signals may simply inure rebels to their use, a problem noted as early as the 1920s.[35] The mere presence of aircraft overhead, however impressive visually, may do little to sway individuals who have already committed to the risky path of insurgency. Since these operations impose no material costs, an insurgent organization's capacity for conducting attacks is undiminished, suggesting that at best shows of force will have only a nuisance value by complicating the logistics of insurgency.

From this viewpoint, we should expect that airstrikes are associated with increased insurgents attacks, especially after civilians are killed or wounded. A positive relationship should also be present between non-lethal shows of force and insurgent violence as emboldened insurgents take advantage of the counterinsurgent's unwillingness to impose costs. At a minimum, no reduction in attacks should be observed since shows of force do not attrit insurgents' capabilities.

# 2 Argument: Reputation-Building Through War-Fighting

These theories and related mechanisms are plausible but overlooked an additional source of insurgent motivation: reputation. The dynamic nature of civil war violence helps create incentives for insurgents to invest in costly actions that build and then maintain their reputations for resolve in the eyes of two audiences: the counterinsurgent and the local population.[36] The use of airpower can perversely create incentives for insurgents to demonstrate their continued resolve by stepping up attacks in the aftermath of airstrikes and shows of force. Indeed, these air operations represent signaling opportunities for insurgents to reveal their "type" to different audiences—and to maintain their control over local populations— by using violence to drive home the message that they retain the organizational capacity to harm opponents.[37]

Insurgents, for example, are clearly engaged in a struggle to impose costs on the counterinsurgent. Maintaining a reputation for resolve and resiliency in the face of coercive challenges is therefore valuable since it shapes the likelihood and nature of the war's even-

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[34]Fearon 1994; Schultz 1998.

[35]Peck 1928.

[36]Though my focus here is on insurgent reputation-building, governments also clearly value their reputations in civil wars, especially when facing multiple challengers. See Walter 2009.

[37]On the importance of territorial control in civil war, see Kalyvas 2006.

tual political settlement. Demonstrating the ability to absorb punishment and still inflict harm on the counterinsurgent thus becomes an important goal for the insurgent organization. Continued attacks are a kind of currency that pays for eventual gains at the negotiating table even if the material cost to the counterinsurgent is modest. Battlefield losses may not undermine an insurgent organization's leverage; instead, losses may actively bolster it by revealing new information to the counterinsurgent about insurgents' cost tolerance and persistence.[38] In this view, strategies of attrition may not have a tipping point; instead, they create incentives to continue fighting even if losses mount.

These incentives suggest that the tit-for-tat rhythm of initiating, absorbing and then responding to harm inflicted may be the preferred state of affairs for at least some insurgent organizations. Insurgents are not (mis)guided by false optimism about their prospects of overturning the prevailing balance of power.[39] Given the protracted nature of most insurgencies, it is clear that these organizations are only too aware of the relative power imbalance. In fact, as power asymmetries increase, the incentives for investing in one's reputations for resilience via costly war-fighting actually increase as the returns for inflicting harm accrue disproportionately to the weaker side. War-fighting is thus about absorbing and then inflicting costs to demonstrate to the counterinsurgent that a political solution is preferable to a continuation of a grinding, increasingly futile, war.

Insurgents must also appeal to a second audience: their local supporters. The degradation of militant capabilities caused by airstrikes can challenge the ability of insurgent organizations to retain control of a given population. Insurgent losses may embolden locals to defect to the counterinsurgent's side, for example. This may take the form of withholding material assistance such as food and shelter or the imposition of restrictions on operations as local leaders organize to limit the damage from airstrikes. Local informants may also provide tips to the counterinsurgent about insurgent identities and behavior. At the extreme, civilians may even counter-mobilize against insurgents by forming their own militia or siding openly with counterinsurgent forces.

Violence therefore becomes a means by which insurgent organizations can blunt the counterinsurgent's efforts to drive a wedge between rebels and locals. Failure to respond may in fact invite whispers that control is slipping away. The Pakistani Taliban in Waziris-

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[38]This mirrors the logic of coercion in interstate crises where weaker states invest in reputations for resolve by fighting against stronger opponents to forestall future exploitation. See Sechser 2010, 653.

[39]Mack 1975; Blainey 1988, 56.

tan, for example, "came to realize that the increasingly effective drone strikes made them look weak," and they began taking precautions (including cordoning off attack sites) to discourage rumors of weakness from spreading.[40] Revealing the capacity to "hit back" at the counterinsurgent after an airstrike thus carries the implicit message that these coercive abilities could also be turned against would-be civilian defectors and wavering insurgents.[41]

Defection can also take the form of locals throwing their weight behind another insurgent organization that appears to be more effective against the counterinsurgent. The potential emergence of a rival organization and the corresponding loss of "market share" will further reinforce the value of a reputation for resolve against counterinsurgent forces. By imposing costs on the counterinsurgent, an insurgent organization could satisfy popular demands while forestalling the entry (or creation) of rival organizations in an area. Indeed, local civilians may even shrug off casualties inflicted by insurgents while striking back, particularly if those individuals have been victimized by the counterinsurgent.[42]

Given these reputational dynamics, even shows of force, which impose no material costs on insurgents or populations, should influence insurgent behavior. They are highly visible reminders of both the counterinsurgent's ability to impose costs and the lack of a symmetrical insurgent reply. These signals may also serve as visible reminders of the counterinsurgent's hated occupation.[43] Rather than rest on a purely attritional logic, the reputational argument advanced here suggests that shows of force should trigger the same response from insurgents as airstrikes. Contrary to existing theories, non-lethal shows of force do represent credible threats but are *spurs*, not deterrents, to future action.

The nature of the rebel-population relationship is therefore an important mediating variable. We should not expect all insurgent organizations to respond in identical fashion to attempted coercion. Instead, the effects of airpower are likely conditional on two factors: the extent of rebel governance in an area and the number of potential rival insurgent organizations.[44]

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[40]Shah 2013, 242.

[41]These threats may actually materialize as concerted efforts to assassinate suspected collaborators. An additional empirical test of this argument would be tracking changes in violence against civilians before and after these air operations.

[42]Lyall, Blair and Imai 2013.

[43]Alternatively, insurgents may fear that such displays drive a wedge between insurgents and civilians by illustrating the counterinsurgent's comparative restraint. Emphasizing such restraint, along with the provision of aid and services, is a central plank of ISAF's "hearts and minds" campaign in Afghanistan, for example.

[44]On the importance of theorizing the conditional effects of violence, see Lyall 2010.

Scholars have now turned their attention to studying rebel governance in civil wars.[45] It is possible to array rebel-civilian "social orders"[46] along a simple spectrum from coercive to consensual relationships. At one extreme, "roving bandits" have no affinity for the local population and simply extract (violently or otherwise) taxes and other matériel needed for war-fighting.[47] Other insurgent organizations may espouse broader ethnic or political goals that dovetail with efforts to provide limited governance; the SPLM-A in South Sudan offer one such example. On the other extreme, insurgent organizations may enter into a "social contract" with locals and provide services and formal governance structures in which civilians hold influence over decision-making, as with Hezbollah, LTTE in Sri Lanka, or the FARC in some regions of Colombia.[48] This relationship may change over time; it may also vary spatially across the same organization.

As insurgent organizations become increasingly embedded consensually within a local population, they are more likely to value their reputations. The deeper these ties, the more likely insurgents will believe they must demonstrate their resolve through war-fighting. Roving bandits, on the other hand, are less likely to value their reputations. Unencumbered by a social contract, these organizations can respond to potential defection by locals through either moving to a new location or unleashing violence against the civilian population, not the counterinsurgent.

These claims are, of course, falsifiable. Deeply enmeshed organizations may actually have a "cushion" of popular support that curbs the need to demonstrate resolve after every (or any) coercive action by the counterinsurgent. Similarly, more predatory organizations could be more prone to jumping at shadows, retaliating after every counterinsurgent action to check the erosion of support among already disgruntled civilians.

The number of insurgent organizations competing for popular support is a second mediating variable. This argument anticipates that insurgent organizations are likely to feel pressure to defend their reputations by fighting "fire with fire" if several armed groups are competing for the allegiance of the same population. Organizations that are deeply embedded in their local populations and that fear loss of control to rival organizations (whether insurgent or state-created) are most likely to be governed by reputational logic when choosing their response to the counterinsurgent's coercive efforts. By contrast, preda-

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[45] Arjona 2010; Metelits 2009; Mampilly 2011; Wood 2003; Parkinson 2013; Staniland 2012.
[46] Arjona 2010.
[47] Olson 1993.
[48] Arjona 2010.

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Table 1: Coercive Logics and their Mechanisms by Type of Air Operation

|  | Mechanisms | |
| --- | --- | --- |
| Expected Relationship | Airstrikes | Shows of Force |
| *More Air Operations, Fewer Insurgent Attacks* | Decapitation Attrition Punishment | Credible Threat |
| *More Air Operations, More Insurgent Attacks* | Grievances Reputation | Cheap Talk Reputation |

tory marauders facing little or no competition are least likely to value their reputations since they can move to new areas if they are engaged by the counterinsurgent or rival insurgent organizations.

## 2.1 Hypotheses

Not all of these claims can be tested within the confines of a single article or, indeed, with observational data. Capturing the relationship between insurgents and civilians in particular requires close-range qualitative and survey data.[49] The goal here is more modest: to test three initial hypotheses about how reputational motives affect the direction and magnitude of insurgent responses to airpower against leading explanations drawn from the interstate coercion literature. Table 1 summarizes these different theoretical camps and the mechanisms associated with their arguments.

First, if this reputational argument is correct, then we should observe a positive association between airstrikes and a net increase in insurgent attacks relative to similar non-bombed locations (Hypothesis 1). Second, reputational demands should lead to rapid insurgent "push-back" after the airstrike; responses are governed by a "quick fuse" rather than "slow burn" logic and so should be observed days and weeks, not (many) months or years, after the event (Hypothesis 2). Third, insurgent responses should be centered around

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[49]These issues are addressed in a book-length monograph. See [redacted].

the bombed location and should decay over distance given the importance of demonstrating resolve to a local audience (Hypothesis 3).

These predictions are observationally equivalent to grievance-based explanations, and so two additional tests can be used to separate these theoretical accounts. Grievance-based accounts suggest that individuals who have experienced victimization at the hands of the counterinsurgent will be easier to recruit since joining the insurgency provides opportunities for revenge-seeking.[50] We should not, however, expect increased insurgent violence after non-lethal shows of force since no material costs were imposed on civilians. A reputation-based account would expect the opposite: namely, that insurgents respond to shows of force in the same fashion as airstrikes since reputational concerns, not civilian casualties, drive insurgent behavior.

Examining insurgent responses to airstrikes that do and do not inflict civilian casualties offers a second test of reputation- and grievance-based accounts. If the reputational account offered here is correct, then insurgent reactions should be similar after both types of events since insurgent behavior is not conditional on civilian fatalities. If, however, insurgent violence spikes after civilian casualties relative to airstrikes that do not harm civilians, then it is likely that grievance-based mechanisms are at work as revenge, not reputation, is guiding post-strike insurgent behavior.

## 3    Empirical Strategy

The dynamic nature of coercion in civil wars plays out in dozens, if not hundreds, of daily counterinsurgent-insurgent actions that can frustrate efforts at causal inference. All empirical strategies in this setting must deal, for example, with the possibility that violence (and the threat of its use) is not random but is instead the product of strategic deliberation, raising the specter of selection bias in both treatment assignment and observed reactions. In addition, a viable empirical strategy must also be flexible enough to capture the diffusion of effects both temporally and spatially. Finally, our empirical strategy should identify relevant counterfactuals that answer the question: what would have happened to patterns of insurgent violence had the airstrike or show of force *not* occurred?

Matching offers one possible approach. Based on the Neyman-Rubin Causal Model,

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[50]An expanded view might include property damage since it lowers the opportunity costs of becoming an insurgent. I test this alternative grievance-based account below.

matching involves the identification of counterfactual "control" observations that possess similar, if not identical, characteristics as "treated" cases (here, villages that are bombed or experience a show of force) but that did not receive the treatment.[51] These counterfactuals provide baseline observations that (ideally) adjust for selection processes, key covariates that might otherwise explain outcomes, and temporal trends not connected to the treatment.

Our empirical investigation here is aided by quasi-randomness in the assignment of airstrikes and shows of force to specific insurgent events. For example, strike aircraft are sortied daily to fly over Afghanistan in pre-determined "race track" patterns and are a relatively scarce commodity; only a fraction of daily insurgent attacks are countered with an air operation. The likelihood of a specific attack being met depends on a mix of aircraft availability, distance to the event, suitability for the desired mission, and the nature (and length) of the insurgent attack itself. Aircraft operating cycles are unknown to insurgents, who lack the ability to monitor aircraft flight patterns. Attacks are therefore not conditioned on a known probability of experiencing an airstrike at that specific location. Air Force planners also face difficulty in tasking aircraft to events since the daily set of insurgent attacks is (obviously) unknown to them as well. As one interviewee noted, the Air Force acts as a "bucket brigade," trying to extinguish as many fires as possible each daily but without knowing where and when the next fire will occur.[52]

Despite this contingency in treatment assignment, however, the empirical strategy adopted here relies instead on matching to help adjust for possible selection effects and other confounding variables. Matching is no panacea, of course. One well-known issue centers around its inability to control for unobserved covariates, leaving the research design open to challenges of omitted variable bias. In civil war settings, where decisions to use violence likely involve some measure of private information, this can be a serious drawback, though the contingent nature of air operations lessens this concern here.

A second—and to date, largely ignored—issue centers around the disconnect between theories that assume spatio-temporal processes are continuous and matching approaches (and software) that bin data into aggregated spatial and temporal units. For example, scholars typically "scale up" and present their findings in terms of a discrete subnational unit over a single time period. Yet averaging effects over one month or greater intervals

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[51]Rubin 2006; Ho et al. 2007; Rosenbaum 2010.
[52]Air Force Officer, CAOC, Qatar, November 2011.

for a subnational administrative unit (e.g., a district, municipality, or province) that is far larger than the affected location risks mistaken inferences. The effects of an airstrike in a tiny village may not ripple (evenly) across a district with dozens, if not hundreds, of other populated centers, an assumption made when assigning that district treatment status. Conversely, there is no reason to assume that effects are contained within these subnational units; spillover via social networks may occur, especially with the ready availability of cheap telecommunications technology and social media.[53] More generally, binning data at aggregated territorial units over a single time period throws away many of the advantages of microlevel data, including the ability to distinguish cause and effect at a fine-grained (e.g, daily, village) level.

I therefore adopt an alternative approach: dynamic matching. An SQL relational database (PostGIS, an extension to Postgres) is utilized to calculate dynamically the pretreatment covariates (detailed below) for treated and control observations for user-specified temporal and spatial windows at the village level. As an illustration, take the small village of Khowja Lahl in Helmand province, which was bombed on 1 April 2010. The matching program first calculates values on pretreatment covariates such as prior insurgent attacks over a specified temporal (say, 7-days) and spatial (say, 2km$^2$) windows around the village. It then repeats these calculations for all possible control cases using the same spatio-temporal windows. The same anchoring point (1 April 2010) is used to compile covariate values for control observations. The process continues until treated cases have been matched with similar controls or are dropped due to the absence of a suitable match.

The result is a better fit between theoretical expectations and empirical strategy. It becomes possible to conduct longitudinal analysis of effects over different spatio-temporal boundaries as required by the reputational theory proposed here. Each covariate also has its own "caliper" that governs the strictness of the required matching procedure. This in turn facilitates robustness checks, as we are able to test the stability of estimates across different types of matching (e.g., exact, nearest neighbor) while using substantive knowledge to decide how strict the matching procedure should be across covariates.

In the analysis below, I estimate the causal effects of air operations across multiple temporal windows (from 7 to 120 days post-event) and spatial boundaries (from 2km$^2$ to 100km$^2$) around a village. I provide estimates using exact matching for all dynamic

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[53]Pierskalla and Hollenbach 2013.

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covariates and then repeat the procedure using a less restrictive criterion for goodness of matching.[54] All matching is done with replacement. Villages are eligible to be controls until they experience either an airstrike or a show of force, after which they are removed from the pool of possible controls. In cases where multiple control cases are identified, one is chosen randomly to prevent "fitting" or overusing a particular control observation.

## 3.1 Air Operations Data

I draw on multiple sources to construct a dataset of nearly 23,000 airstrikes and shows of force in Afghanistan during 2006-11. The bulk of the dataset stems from declassified data from the USAF Central Command's (AFCENT) Combined Air Operations Center (CAOC) in Southwest Asia, which record the location, date, platform, and type/number of bombs dropped between January 2008 and December 2011.

Substantial recoding was required before these data could be used since the Air Force did not code its airstrikes consistently over time. For example, it is possible for an airstrike in which five bombs were released on a target to be coded as a single airstrike (since one target was hit) or five (given how many weapons were released). I therefore recoded events to remove duplicates and to unify multiple observations that occur in roughly the same location and time into a single airstrike regardless of the number of aircraft involved or weapons released.[55] The same coding procedure was followed for shows of force to avoid inflating our number of observations by falsely treating related observations as independent. Events where both an airstrike and a show of force were used were dropped from this analysis to allow for "clean" estimates of the effects of each type of air operation singularly.

CAOC data was supplemented by two other sources. Declassified data from the International Security Assistance Force's (ISAF) Combined Information Data Exchange Network (CIDNE) was incorporated for the January 2006 to December 2011 era. Press releases by the Air Force's Public Affairs Office (the "Daily Airpower Summary," or DAPS) were also used.

Once merged, these data sources illustrate the importance of seeking multiple sources of data in conflict settings. There is almost no overlap between CAOC, CIDNE, and DAPS data; only 448 events were found in all three sources. Table 2 summarizes these data while

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[54]Following Ho et al. 2007, I use a $\leq.25$ standardized bias score as the measure of closeness of fit for each covariate in this "best matching" approach.

[55]Events occurring within .5km and three hours of one another were collapsed into a single event.
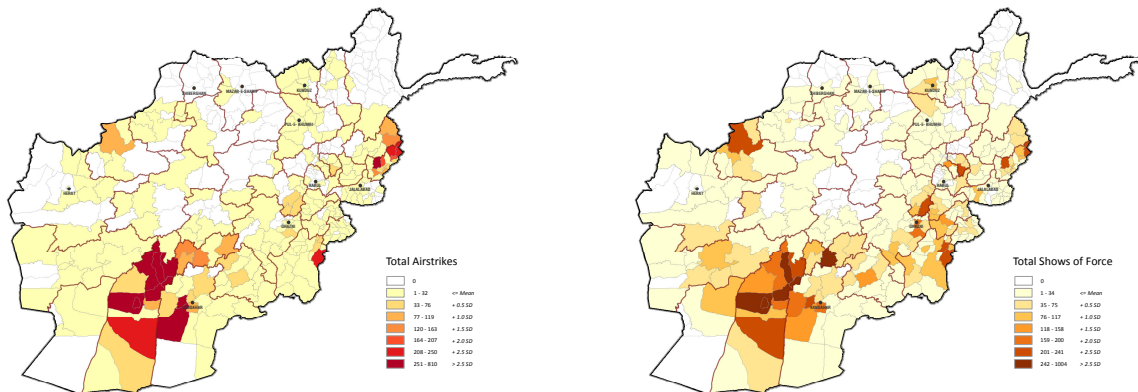
Table 2: Air Operations in Afghanistan, 2006-11

| Year | Airstrikes | Shows of Force | Mixed | Total |
|------|-----------|----------------|-------|-------|
| 2006 | 594 | 50 | 3 | 647 |
| 2007 | 907 | 713 | 161 | 1781 |
| 2008 | 2017 | 3478 | 310 | 5805 |
| 2009 | 2000 | 3301 | 450 | 5751 |
| 2010 | 1521 | 2500 | 228 | 4249 |
| 2011[a] | 1815 | 2695 | 183 | 4693 |
| *Total* | 8,854 | 12,737 | 1,335 | 22,926 |

*Note:* [a] Data for 2011 are partly incomplete, with airstrikes and shows of force recorded until 8 December and 2 December, respectively. The "mixed" category captures events where both airstrikes and shows of force are recorded and are dropped from this analysis.

Figure 1 details the distribution of airstrikes (Panel a) and shows of force (Panel b). The lion's share of observations are from CAOC (N=16,642), followed by DAPS (N=5,912), and CIDNE (N=2,977). Unsurprisingly, the correlation between these sources is mostly negative: -.85 between DAPS and CAOC, for example, and -.40 between CAOC and CIDNE. Only DAPS and CIDNE are positively correlated at .33. These records also exclude (most) operations by Special Forces and Central Intelligence Agency assets—an estimated two percent of overall airstrikes—and all attacks by helicopters.

A small number of observations were dropped because they did not occur within 10 $km^2$ of a populated location. Matching requires a specific point (e.g., a village) in order to identify controls and calculate spatial windows and so air operations were clipped to the closest populated location. CAOC and CIDNE data use 10-digit Military Grid Reference System (MGRS) coordinates to assign locations; these are accurate to one meter resolution. DAPS records were merged using village and district names that were cross-referenced with village location data from Afghanistan's Central Statistical Office (see below).

(a) ISAF Airstrikes, 2006-11. N=8,854.     (b) ISAF Shows of Force, 2006-11. N=12,737.

Figure 1: ISAF Air Operations Over Afghanistan, 2006-11. N=21,591.

## 3.2 Dependent Variable

The dependent variable, *attacks*, is defined as the difference-in-difference in mean insurgent attacks against ISAF forces between treated and control villages before and after each airstrike over identical time periods.[56] Data on insurgent violence are drawn from CIDNE, which records the date and location (using MGRS) of 104,575 insurgent-initiated operations against ISAF forces between 1 January 2005 and 1 July 2012. Thirteen types of insurgent attacks are combined in the *Attacks* variable.[57]

## 3.3 Dynamic Covariates

Given the real-time nature of air operations in Afghanistan, it is likely that the greatest threat to inference lies in selection bias: villages experiencing an airstrike or show of force may systematically differ from non-bombed villages. This bias is most likely to hinge on dynamic covariates, namely, the nature of prior or on-going clashes between insurgents and ISAF in a given location. Four battle-level covariates are therefore dynamically generated

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[56]More formally, the difference-in-difference 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.

[57]The specific event categories are: Assassination, Attack, Direct Fire, IED Explosion, IED False, IED Founded/Cleared, IED Hoax, Indirect Fire, Mine Found, Mine Strike, Surface-to-Air Fire (SAFIRE), Security Breach, and Unexploded Ordinance. Attacks involving improvised explosive devices represent 43% of all incidents.

for specified temporal and spatial windows around bombed villages. Control villages were then identified with similar characteristics to reduce potential selection bias. We are aided in this endeavor by the richness of CIDNE data, which tracks insurgent and ISAF actions at the daily level with precise grid coordinates.

First, the number of insurgent attacks prior to the air operation is calculated to account for insurgent violence and the presence of ISAF forces (*Prior Attacks*). Second, the number of pre-air operation ISAF military operations around a treated or control village is calculated (*ISAF Ops*).[58] These two variables account for the patterns of violence in and near a specified village as well as the battlefield distribution of forces.

We might imagine that targeting is also driven partly by private information held about a particular village. A third covariate, *Info*, records whether ISAF has received information about threats to ISAF forces and bases in a given location. There are 21,683 recorded threats against ISAF forces and installations across five threat categories.[59]

Fourth, a "Troops in Contact" (*TIC*) covariate is constructed to indicate whether the air operation was intended to provide close air support for ISAF soldiers. If the air operation was a response to an insurgent or ISAF operation, then *TIC* is assigned a value of 1. In these situations, potential control observations must also record an insurgent or ISAF operation on the same day to be eligible for matching. The distinction between TIC and non-TIC settings is important both for tracking the presence of ISAF soldiers and because these air operations may have systematically different effects. Human rights organizations, for example, have argued that restrictions on the use of airpower are less severe when soldiers are under fire, as the need for a timely response outweighs the avoidance of collateral damage. TIC situations may therefore account for a disproportionate share of airstrike-induced civilian casualties.[60]

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[58]There are 23,080 ISAF-initiated events (excluding airstrikes) in these data. Fourteen CIDNE categories are included: Cache Found/Cleared, Arrest, Counter-insurgency, Direct Fire, ERW/Turn in, Escalation of Force, Friendly Action, Indirect Fire, Kidnapping Release, Operations, Search and Attack, Small Arms Fire, Surrender, and Weapons Found/Cleared.

[59]These include Threat Report, Suspicious Incident (Surveillance), Attack Threat, IED Threat, and SAFIRE Threat.

[60] United Nations Assistance Mission in Afghanistan 2011; Human Rights Watch 2008.

## 3.4 Static Village Level Covariates

Matching is also used to adjust for village level imbalances between treated and control observations that might explain insurgent violence. The village's (logged) population size, often thought positively associated with insurgent attacks, is measured using the Central Statistical Office's 2005 census. The dataset contains information on 35,755 villages. To control for the possibility that more rugged terrain favors insurgency, village elevation (logged, in meters) was calculated from Shuttle Radar Topographic Mission (SRTM) satellite imagery.[61] A village's neighborhood was also taken into account by counting the number of settlements within a 5 km² radius. This measure captures the likelihood of spillover of violence to nearby settlements; the greater the number of neighbors, the greater the possibility that an air operation has effects that extend beyond the targeted location. Finally, matching also occurred on the village's dominant language as recorded during the 2005 CSO census. These data provide a crude proxy for a village's ethnic composition in the absence of more reliable, fine-grained data.

## 4 Findings

Four empirical tests are conducted below. I first examine the relationship between airstrikes and subsequent insurgent attacks. I then explore how these effects might differ when villages are subjected to repeated bombing. Next, I turn to the issue of whether drone strikes yield different behavioral outcomes than conventional airpower. Finally, I investigate whether airstrike effects diffuse to other neighboring villages or remain localized, as expected by the reputation theory proposed here.

## 4.1 Effects of Airstrikes

Do airstrikes reduce subsequent insurgent attacks? Put simply, no. As Table 3 details, there is a persistent *positive* relationship between airstrikes and insurgent violence across multiple time periods and matching procedures. Beginning with exact matching, the difference-in-difference between bombed and control villages is .289 more attacks in only the first seven days after an airstrike (with 95% confidence interval at [.234, .335]). We observe .683 more attacks per treated village (95% CI at [.479, .885]) compared with control villages by the

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[61]Fearon and Laitin 2003.

45 day mark. By 90 days, the difference-in-difference has increased to 1.03 more attacks (with 95% CI at $[.671, 1.395]$). Given 8,854 airstrikes, the 90 day difference-in-difference amounts to some 9,150 more attacks due to airstrikes than would otherwise have occurred given trend rates in non-bombed villages (with 95% CI at $[5, 940]$ to $[12, 351]$ attacks).

While exact matching provides the most stringent (and intuitive) set of paired comparisons, this rigor comes at a cost. Table 3 reveals that the proportion of treated observations used for exact matching diminishes to only 25% of all airstrikes when we reach the 90 day time window. This attrition stems from two sources. First, the requirement imposed by the *TIC* covariate dramatically reduces the pool of available controls since there were on average "only" 40 insurgent attacks each day over this time period. Second, large urban centers such as Kabul or Kandahar City, and even medium-sized district centers (e.g., Sangin) typically lack a suitable control given their population size. Since these locations also tend to be more violent, the exact matching results should be viewed as applying to smaller villages, which represent the vast majority of Afghanistan's settlements.

To reduce bias arising from incomplete matching due to attrition of treatment observations,[62] I relax the strict requirements of exact matching. All covariates in these "best matching" models are permitted to "float" within specified ranges. The result is a significant improvement in the number of treated observations included in these models.

The results remain largely unchanged, however. Once again, airstrikes are positively correlated with increases in post-strike insurgent attacks. In substantive terms, there is an average of .371 more attacks in the initial 7 days after each airstrike (with a 95% CI of $[.31, .44]$) relative to non-bombed villages. At the 45 day mark, there are 1.29 more attacks on average in each of the bombed locations (95% CI at $[.99, 1.58]$). By the time we reach the 90 day post-strike threshold, there are 2.34 more attacks on average in each of the bombed locations (95% CI at $[1.82, 2.86]$). Taking the 90 day difference-in-difference, there are 20,718 additional insurgent attacks above the control baseline that can be attributed to airstrikes cumulatively over these time windows (95% CI at 16,114 to 25,332 attacks).

These findings suggest that decapitation, attrition, and punishment mechanisms, if operative, are not sufficient to degrade the capacity of insurgent organizations to generate violence. Instead, the robust nature of the positive relationship between airstrikes and subsequent violence points toward both reputational and grievance-based mechanisms.

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[62]Rosenbaum 2010, 85-86.

Table 3: Airstrike Effects Over Time

| Treatment Effect (ATE) | Exact Matching | | | Best Matching | | |
|---|---|---|---|---|---|---|
| | 7 day Model 1 | 45 days Model 2 | 90 days Model 3 | 7 day Model 4 | 45 days Model 5 | 90 days Model 6 |
| *Treatment* | 0.289*** | 0.683*** | 1.033*** | 0.371*** | 1.288*** | 2.339*** |
| | (0.023) | (0.103) | (0.185) | (0.033) | (0.150) | (0.265) |
| Constant | 0.984*** | 2.672*** | 3.262*** | 1.360*** | 6.258*** | 11.499*** |
| | (0.199) | (0.715) | (1.055) | (0.255) | (1.125) | (1.809) |
| F stat | 38.95*** | 13.15*** | 6.96*** | 45.20*** | 36.13*** | 40.78*** |
| $r^2$ | 0.11 | 0.05 | 0.06 | 0.15 | 0.10 | 0.15 |
| Treatment Coverage (%) | 43% | 29% | 25% | 60% | 56% | 53% |
| Villages (N) | 4,600 | 3,544 | 3,122 | 5,395 | 5,017 | 4,879 |
| Total N | 7,670 | 5,156 | 4,390 | 10,574 | 9,888 | 9,404 |

*Note:* Models include all covariates. "Treatment coverage" refers to the percentage of total treatment cases used in the estimation. "Village (N)" refers to the combined number of treated and control villages. Exact matching was used for prior insurgent and ISAF violence, ISAF private information, troops in contact, and the primary language of the village's inhabitants. Best matching allows these covariates to "float" within $\leq .2$ standardized bias of one another. A 2km$^2$ radius was used in all models to delineate the calculation of pre- and post-insurgent violence. Robust standard errors clustered on individual villages. ***$p=<.001$, **$p=<.01$, *$p=<.05$, †$p=<.10$

As an initial attempt to disentangle these two accounts, I split the "best matching" samples into TIC and non-TIC airstrikes. From the reputation argument's perspective, we should expect TIC airstrikes to be associated with a large post-strike increase since these airstrikes stem directly from combat between the warring parties. This is indeed what we find: TIC airstrikes do generate a larger estimated difference-in-difference when compared with non-TIC airstrikes, though in each case the estimated difference is highly statistically significant. At the 7 day mark, there are .79 more attacks (95% CI at $[.52, 1.05]$) after TIC airstrikes and .275 after non-TIC airstrikes (95% CI at $[.233, .318]$), relative to their respective controls. At the 45 day mark, that difference has grown to 2.37 more attacks in TIC airstrikes (95% CI at $[1.32, 3.42]$) versus 1.08 in non-TIC airstrikes (95% CI at $[.84, 1.32]$). Finally, at the 90 day mark, that difference has grown to 4.55 more attacks in TIC airstrikes (95% CI at $[2.52, 6.59]$) versus 1.99 in non-TIC airstrikes (95% CI at $[1.58, 2.39]$).[63]

## 4.2   The Effects of Repeated Airstrikes

Given the dynamic nature of counterinsurgencies, it is unsurprising that many of the villages within our matched samples experienced multiple airstrikes over time. Repeated exposure to bombing enables us to explore whether airstrike effects are cumulative in nature. In particular, does repeated bombing lead to increased attrition of insurgents and punishment of civilians, thereby reducing attacks? Or do these coercive attempts only backfire, either by multiplying grievances or by creating additional incentive for insurgents to bolster the reputations by attacking counterinsurgent forces?

To tackle this question, I created *History*, which is the (logged) number of airstrikes a populated location has experienced before the current airstrike that is being matched on. I then reestimate Models 1-6 with the new *History* covariate.

Two main findings emerge (see Table 4). First, it is clear that while the inclusion of *History* leads to some attenuation of airstrike effects, the difference-in-difference estimate remains highly statistically significant and positively associated with insurgent attacks in all six models.[64] Second, *History* also emerges as positively associated with post-strike insurgent attacks in four of six models while just missing conventional levels of significance

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[63]TICs represent 20%, 19% and 17% of the matched 7 day, 45 day, and 90 day samples, respectively.

[64]This attenuation is unsurprising given the high degree of correlation between the two variables in both the exact (.62) and best (.70) matching datasets.

Table 4: More Bombing, Less Insurgent Violence? The Effects of Repeated Airstrikes

| Treatment Effect (ATE) | Exact Matching | | | Best Matching | | |
|---|---|---|---|---|---|---|
| | 7 day | 45 days | 90 days | 7 day | 45 days | 90 days |
| Treatment | 0.204*** | 0.421** | 0.827*** | 0.255*** | 0.827*** | 1.586*** |
| | (0.030) | (0.142) | (0.258) | (0.039) | (0.172) | (0.302) |
| History | 0.036** | 0.130† | 0.110 | 0.048*** | 0.192** | 0.318*** |
| | (0.012) | (0.071) | (0.148) | (0.014) | (0.067) | (0.116) |
| Constant | 1.133*** | 3.272*** | 3.776*** | 1.547*** | 7.145*** | 13.029*** |
| | (0.212) | (0.793) | (1.148) | (0.261) | (1.199) | (1.929) |
| F stat | 34.68*** | 11.61*** | 6.50*** | 40.47*** | 32.67*** | 37.31*** |
| $r^2$ | 0.11 | 0.06 | 0.06 | 0.16 | 0.10 | 0.15 |
| Treatment Coverage (%) | 43% | 29% | 25% | 60% | 56% | 53% |
| Villages (N) | 4,600 | 3,544 | 3,122 | 5,395 | 5,017 | 4,879 |
| Total N | 7,670 | 5,156 | 4,390 | 10,574 | 9,888 | 9,404 |

*Note:* Models include all covariates. "Treatment coverage" refers to the percentage of total treatment cases used in the estimation. "Village (N)" refers to the combined number of treated and control villages. Exact matching was used for prior insurgent and ISAF violence, ISAF private information, troops in contact, and the primary language of the village's inhabitants. Best matching allows these covariates to "float" within $\leq.2$ standardized bias of one another. A 2km$^2$ radius was used in all models to delineate the calculation of pre- and post-insurgent violence. Robust standard errors clustered on individual villages. ***$p=<.001$, **$p=<.01$, *$p=<.05$, †$p=<.10$

in a fifth. This finding runs counter to the claim that repeated bombing can successfully attrit insurgent organizations or drive their supporters away. To be sure, the level of coercive violence wielded in Afghanistan pales in comparison to other cases (e.g., Vietnam). Yet these data do contain locations that were struck dozens of times, including Urgun in Paktika province (N=127), Lashkar Gah (N=117) and Gereshk (N=115) in Helmand province, and Tirin Kot (N=96) in Uruzgan province. If airstrike effects are subject to curvilinear trends, it is apparent that these bombing levels are insufficient to reach a "tipping point" after which attrition leads to the degradation of insurgent capabilities.

## 4.3 Drones

A prominent public debate has arisen around the effectiveness (and ethics) of drone strikes. Proponents emphasize the precise and selective nature of these airstrikes, characteristics that represents a "most likely" case for observing a negative relationship between airstrikes and subsequent insurgent attacks.[65] Critics contend, however, that these airstrikes predominantly kill civilians, creating grievances that facilitate insurgent recruitment and increase militant attacks both locally and abroad.[66]

Much of this debate has centered around the use of drones in Pakistan and Yemen. Afghanistan, by contrast, has been relatively ignored, despite the fact that more drone strikes have occurred here (N=943) than in all other locations combined. I examine the question of drone effectiveness by reestimating Models 1-6 with a binary variable, *Drone*, designating whether an airstrike was conducted by a remotely piloted vehicle. As Table A1 (see Appendix) outlines, *Drones* only (barely) reaches conventional levels of statistical significance in one of six models. Moreover, *Drones* are positively associated with an increased amount of insurgent attacks, exactly the opposite relationship expected by proponents of their use. The estimated increase in insurgent attacks is, however, substantially smaller than non-drone airstrikes, suggesting that drones are at least less counterproductive than their manned counterparts.[67] While the current focus on drones is perhaps understandable, a fixation on their use to the exclusion of comparison with other aircraft misses the fact that neither type of airstrike appears to be having the intended suppressive effect on

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[65]Johnston and Sarhabi 2013; Byman 2013; Bergen and Rowland 2013.

[66]Cronin 2013; The Bureau of Investigative Journalism 2013; NYU/Stanford 2013.

[67]*Drones* is also not significant when used as a treatment for matching across the entire airstrike dataset (see Table A1).

insurgent attacks.

## 4.4 Do Effects Diffuse?

The reputation argument proposed here also suggests that the effects of airstrikes on insurgent violence should be quite localized. To test this claim, I reestimate Models 4-6 from Table 3, with two modifications. First, I lengthen the temporal windows to 120 days after the airstrike to further capture diffusion of effects over time. Second, the spatial catchment windows around each village, set at 2 km$^2$ in Models 4-6, are increased to 4 km$^2$, 6 km$^2$, 8 km$^2$, 10 km$^2$, 50 km$^2$ and 100 km$^2$. These variable spatial windows now permit the testing of whether airstrike effects decay or continue to ripple (and perhaps increase) over distance.[68]

Several trends are notable (see Table 5). Mirroring results above, the estimated difference-in-difference between bombed locations and their control villages suggests that airstrikes are associated with increased insurgent attacks. All differences are statistically significant, though only barely at the 50 km$^2$ and 100 km$^2$ spatial windows for the 120 post-strike time period, suggesting some decay in effect as distance increases.

The absolute value of the estimated difference-in-difference is also increasing as the spatial window is widened for each temporal window. At the 7 day mark, for example, the estimated difference is .371 more insurgent attacks in the 2km$^2$ around the targeted village (with a 95% CI of [.31, .44]). That difference increases to 2.14 attacks with a 100 km$^2$ radius around the bombed village (95% CI of [1.13, 3.15]) for the same 7 day period. Similarly, we observe an increase of 3.08 attacks in the 120 days following an airstrike (95% CI of [2.43, 3.73]) with a 2km$^2$ radius but 16.81 more attacks with a 100 km$^2$ radius at the same 120 day mark (95% CI of [−.3.07, 35.94]).

We should not conclude, however, that airstrike effects are mechanically increasing over distance, for two reasons. First, the rate of increase in the size of the estimated difference-in-difference is consistently largest when moving from 2 km$^2$ to 10 km$^2$; that is, within the local vicinity of the bombed location. By contrast, when shifting from 50 km$^2$ to 100 km$^2$, the rate of increase is on average less than one-half that of the 2 km$^2$ to 10km$^2$ shift despite sharply increase the spatial catchment area. To take one example, the estimated difference-in-difference increases 3.4x when shifting from 2 km$^2$ to $10^2$ but only 1.3x times

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[68]Also helps adjust for possible inaccuracies in bomb strike location.

Table 5: Do Airstrike Effects Diffuse Across Space and Time?

| Distance | Temporal Windows | | | |
| | 7-day | 45-day | 90-day | 120-day |
|---|---|---|---|---|
| 2km$^2$ | 0.371*** | 1.288*** | 2.339*** | 3.081*** |
| | (0.033) | (0.150) | (0.265) | (0.331) |
| 4km$^2$ | 0.619*** | 2.117*** | 3.951*** | 5.548*** |
| | (0.062) | (0.287) | (0.578) | (0.754) |
| 6km$^2$ | 0.750*** | 2.444*** | 5.598*** | 7.224*** |
| | (0.089) | (0.438) | (0.885) | (1.181) |
| 8km$^2$ | 0.925*** | 3.451*** | 7.713*** | 9.861*** |
| | (0.112) | (0.560) | (1.112) | (1.485) |
| 10km$^2$ | 0.985*** | 3.706*** | 7.973*** | 11.221*** |
| | (0.134) | (0.658) | (1.376) | (1.841) |
| 50km$^2$ | 1.680*** | 6.22** | 10.86** | 9.49$^\dagger$ |
| | (0.374) | (1.99) | (4.12) | (5.78) |
| 100km$^2$ | 2.14*** | 7.99** | 20.55** | 16.81$^\dagger$ |
| | (0.509) | (2.74) | (7.85) | (10.14) |

*Note:* Models 4-6 from Table 3. The minimum distance between treated and control observations is reset with each change to ensure that controls are not drawn from within the spatial boundaries around treated observations. ***$p=<.001$, **$p=<.01$, *$p=<.05$, $^\dagger p=<.10$

when moving from a 50 km$^2$ to 100 km$^2$ radius for the 45 day time period.[69] Put differently, the rate at which insurgent attacks increase slows markedly once we move beyond the fairly narrow 10 km$^2$ area around the bombed location.

Second, the difference-in-difference estimate comes to represent a declining share of the bombed village's post-strike insurgent violence as distance from the village increases. For example, the .371 more attacks observed in the 2km$^2$, 7 day temporal window represents 44% of total attacks from (and near) that village (95% CI of [36%, 51%]). By contrast, the 2.14 more attacks observed at the 100km$^2$, 7 day temporal window only represents 5.5% of total insurgent attack in and near that location (95% CI of [3%, 8%]). Similarly, the estimated 3.081 more attacks we observe at the 2km$^2$, 120 day temporal window represents about 35% of total post-strike violence in and near that bombed village (95% CI of [27%, 42%]). Resetting the spatial parameter at 100km$^2$ for the same 120 day temporal window reveals that the 16.81 increased attacks represents only 3.6% of the total post-strike violence around that bombed village (95% CI of [−.65%, 7.8%]).

In short, airstrikes have remarkably persistent effects on insurgent attacks over different spatial and temporal windows. The bulk of these effects, however, are concentrated spatially in the immediate vicinity of the bombing, with the rate of increase falling sharply once we move beyond 10 km$^2$ of the targeted village. These findings are consistent with the expectations that insurgents will privilege responding locally, and quickly, to airstrikes.

# 5    Robustness Checks

I reexamine these findings from Models 1-6 using multiple robustness checks (see Appendix for details). Four in particular deserve special mention.

First, I conduct a placebo test by randomly reassigning (with replacement) all airstrikes to three different sets of populated centers, which preserves the within-village auto-correlation of outcomes.[70] If the airstrikes are indeed having a positive effect on subsequent insurgent attacks, this difference should disappear once we compare placebo treated locations and their control counterparts since no airstrike actually occurred. As Tables A2, A3, and A4 demonstrate, this is indeed the case: once the airstrikes are reassigned randomly, a

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[69]The corresponding rate of increase for the 7 day period is 2.65x (2 km$^2$ to 10 km$^2$) and 1.27x (50 km$^2$ to 100 km$^2$); for the 90 day period, 3.4x to 1.89x; and 3.6x to 1.8x at the 120 day temporal window.

[70]Bertrand, Duflo and Mullainathan 2004.

statistically significant difference between placebo treated and control villages is observed only once in 18 trials (Models 1-6 repeated on each pseudo-sample). This placebo test ensures that the treatment effects of airstrikes are genuine rather than an artifact of the data collection or estimation process.

Second, I cross-validate these findings using a second, independently-collected, dataset of insurgent and ISAF-initiated violent events. These data were collected by iMMAP, a non-governmental organization that pools together field reports from various NGOs and government agencies (but not ISAF) operating throughout Afghanistan. About 98,000 observations were recorded for the 1 January 2008 to 1 June 2012 timeframe. The dataset's coverage of insurgent attacks against ISAF is less comprehensive than ISAF's own CIDNE. It does, however, have the advantage of recording attacks against Afghan National Security Forces, including the Afghan National Army and Afghan National Police, that are omitted from CIDNE. Reestimating Models 1-6 with iMMAP data returns similar results; airstrikes are positively associated with increased insurgent attacks in all models and the results are statistically and substantively similar (Table A5). These findings are not products of CIDNE's coding rules or data generating process.[71]

Third, I reestimate these models using each of the airstrike dataset's three constituent sources (CAOC, DAPS, and CIDNE) separately. This is a particularly strict test given the lack of overlap between these sources and their own coding idiosyncrasies. Yet despite these differences, the models return remarkably consistent estimates of airstrike effects across the three sources. In all models, the difference between bombed locations and their controls is highly significant and positively associated with increased post-strike insurgent attacks (Table A6). CAOC data generally provides the largest estimates of airstrike effects, though the coefficients are similar across all three sources.

Fourth, I split the best matching sample according to a binary disturbance term (*Disturbance*) that indicates whether additional airstrikes occurred within the 7, 45, or 90 day post-airstrike windows.[72] These additional airstrikes could confound our estimates since they represent a violation of difference-in-difference's assumption of parallel trends in treated and control observations. As Table A7 outlines, our estimates of treatment effects remain largely unchanged statistically or substantively in the observations without post-

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[71]Since iMMAP is not privy to ISAF's internal deliberations, these models were run without matching on the ISAF private information covariate.

[72]This disturbance term is generated dynamically for each treatment-control pair.

strike disturbances, the vast majority of observations in each sample. Villages recording at least one additional airstrike in the post-treatment window, though a small percentage of the overall sample, do exhibit different treatment estimates. Airstrikes no longer have a statistically significant relationship with insurgent attacks. These villages are typically the target of rare, sustained military operations designed to capture strategic locations. As such, they pose a special challenge for causal inference since isolating the effect of any one airstrike is difficult when so many are occurring within tight temporal and spatial windows.

Finally, I conducted additional robustness checks as outlined in the Appendix. These include: (1) subsetting the results annually to test for period effects associated with exogenous changes such as the 2010 troop surge (Table A8); (2) reestimating these models with five district-level covariates (Table A9);[73] (3) subsetting the data to examine whether locations with no insurgent attacks in the pre-treatment window differ markedly from villages with pre-strike insurgent violence (Table A10); and (4) recoding the dependent variable as an ordinal variable (increase/no change/decrease) and reestimating models with ordered logistic regression (Table A11). In nearly every case, airstrikes are statistically significant and positively associated with increased post-strike insurgent attacks. Estimates of treatment effects remain remarkably resistant to the inclusion of additional district variables and subsetting efforts, increasing our confidence in the direction and magnitude of the relationship between airstrikes and insurgent violence.

## 6    Mechanisms and Alternative Explanations

These initial tests provide clear evidence to adjudicate between competing theories and their associated mechanisms. It is apparent, for example, that oft-cited mechanisms of decapitation, attrition, and punishment are not producing the expected (negative) relationship between airstrikes and subsequent insurgent violence. These tests, however, cannot distinguish between existing grievance/revenge-based explanations and the reputational argument advanced here.[74]

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[73]These district-level variables are: a binary variable indicating whether the district borders Pakistan (*Pakistan*) or Iran (*Iran*); the length of paved roads in the district, as a proxy measure of relative development (*Roads*); aid expenditures in a given district by two programs, the National Solidarity Program and ISAF's own Commander's Emergency Response Program (*Aid*); and the presence of Taliban courts in 2006 (*Courts*).

[74]On identifying causal mechanisms in coercive contexts, see Mueller 1998, 186.

Two additional comparisons are therefore helpful in isolating the mechanisms under-pinning the positive association between airstrikes and insurgent attacks. First, I examine whether similar effects are observed when non-lethal shows of force are conducted. If in-surgents respond similarly to shows of force as airstrikes, then reputational dynamics are likely explaining insurgent attacks since no material harm was imposed on either insurgents or potential ones (e.g., civilians). This comparison also identifies whether insurgents treat shows of force as credible threats of (future) punishment or merely as cheap talk. Second, I compare airstrikes that inflicted civilian casualties with those that did not. If airstrikes that harm civilians are met with relatively greater insurgent violence than airstrikes with-out so-called "collateral damage," we should conclude that revenge/grievance motives are at work. Alternatively, if insurgent responses appear unconnected to civilian casualties, then we can conclude that reputational concerns are driving insurgent behavior.

## 6.1 Shows of Force

Shows of force represent an especially valuable comparison because they are employed in similar situations and locations as airstrikes. In fact, it appears that the choice between airstrikes and shows of force has an element of quasi-randomness about it.[75] As one USAF Targeting Officer declared:

> A commander one day may call in a show of force and the same commander the next day call for dropping a bomb. Conversely, in the absolutely identical situation with two different commanders, one might for a SOF while the other calls for a bomb...Only machines make the same decisions over and over again given the same inputs. I would say there is a large amount of discretion in how the ground commanders are allowed to respond to the situations they face.[76]

Do shows of force generate the same type of effects as airstrikes? Surprisingly, yes. As Table 6 demonstrates, estimates from exact matching reveal that shows of force are strongly associated with increased insurgent violence at all three time intervals. At the 7 day mark, villages that experienced a show of force record .203 more insurgent attacks

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[75]Guidelines governing the use of airstrikes and shows of force are officially classified.

[76]USAF Targeting Officer, Air Operations Center, Bagram Airfield, Afghanistan, 5 April 2011. Email correspondence.

(95% CI of [.172, .232]), a difference that increases to .5 more attacks (95% CI of [.42, .59]) and .74 more attacks (95% CI of [.58, .90]) per village at 45 and 90 day temporal windows, respectively.

Once again I relax the assumptions of exact matching to incorporate a larger proportion of treatment observations. Nearly two-thirds of all shows for force are now being examined in these models. The results, however, remain largely unchanged. Shows of force are associated with increased insurgent violence after shows of force for all three models. At 7 days, the difference-in-difference is .241 attacks (95% CI of [.21, .28]), increasing to .93 attacks (95% CI of [.77, 1.10]) at 45 days and 1.69 attacks (95% CI of [1.34, 2.03]) at the 90 day mark. If we extend the 90 day difference-in-difference estimate to all shows of force, there are collectively 21,526 attacks that can be attributed to these non-lethal operations (95% CI of [17, 068] to [25, 856]).

In light of these findings, it is difficult to suggest that shows of force act as credible deterrents to future insurgent behavior. Yet dismissing them as mere "cheap talk" also misses the mark. Insurgents are clearly responding to these "cost-less" operations in ways that suggest they find such actions threatening even if no material cost is being imposed. To be sure, a comparison of the magnitude of difference-in-difference estimates after airstrikes and shows of force indicate that airstrikes are generating greater insurgent "push-back," at least as measured here by the number of attacks. Nonetheless, the fact that shows of force are being met with increases in violence without imposing material costs or incurring civilian casualties suggest that insurgents are maneuvering to protect their reputations for effectiveness in the eyes of the counterinsurgent and local audiences.

## 6.2   What Role for Civilian Casualties?

This section draws on all 8,854 airstrikes to test whether the nature of post-strike insurgent violence is conditional on civilian victimization. Satellite imagery is used to expand our notion of civilian victimization beyond estimated numbers of individuals killed or wounded to include damage to property (compounds and buildings), infrastructure (roads), and economic livelihoods (farms). I also incorporate contextual data, including the number of weapons dropped and whether the airstrike was conducted by remotely-piloted vehicles and intended for high-value targets (HVT) such as insurgent leaders. I then use Coarsened Exact Matching as a robustness check to more narrowly match airstrikes that harmed

Table 6: SOF: Effects by Event Time, Days, by Different Matching Procedures

| Treatment Effect (ATE) | Exact Matching | | | Best Matching | | |
|---|---|---|---|---|---|---|
| | 7 day | 45 days | 90 days | 7 day | 45 days | 90 days |
| Coefficient | 0.203*** | 0.505*** | 0.738*** | 0.241*** | 0.930*** | 1.690*** |
| | (0.015) | (0.043) | (0.082) | (0.017) | (0.084) | (0.178) |
| Constant | 0.482*** | 1.216*** | 3.014*** | 0.594*** | 3.061*** | 7.509*** |
| | (0.119) | (0.325) | (0.611) | (0.131) | (0.487) | (1.132) |
| F stat | 57.56*** | 27.99*** | 17.50*** | 83.56*** | 32.53*** | 27.64*** |
| $r^2$ | 0.11 | 0.05 | 0.05 | 0.24 | 0.08 | 0.07 |
| Treatment Coverage (%) | 53% | 38% | 32% | 67% | 63% | 62% |
| Villages (N) | 7377 | 6034 | 5306 | 8253 | 7801 | 7622 |
| Total N | 13,606 | 9742 | 8218 | 17,174 | 16,104 | 15,684 |

*Note:* Models include all covariates. "Treatment coverage" refers to the percentage of total treatment cases used in the estimation. "Village (N)" refers to the combined number of treated and control villages. Exact matching was used for prior insurgent and ISAF violence, ISAF private information, troops in contact, and the primary language of the village's inhabitants. Best matching allows these covariates to "float" within $\leq.2$ standardized bias of one another. A 2km$^2$ radius was used in all models to delineate the calculation of pre- and post-insurgent violence. Robust standard errors clustered on individual villages. ***$p=<.001$, **$p=<.01$, *$p=<.05$, †$p=<.10$

civilians with "control" airstrikes that did not result in civilian casualties.[77]

Existing scholarship almost exclusively relies on estimates of fatalities (and, less often, the number of individuals wounded) to measure civilian victimization. In these terms, about 2.5% of airstrikes killed or wounded at least one civilian between 2006 and 2011 (N=216). I draw on five reporting sources to generate minimum and maximum estimates of civilian deaths and wounded. These include: iMMAP; the United Nations Assistance Mission to Afghanistan (UNAMA); USAID's Afghan Civilian Assistance Programs I and II, which works directly with individuals harmed by ISAF actions; Lexis-Nexus key word searches in international and local media (such as Pajhwok); and ISAF's Civilian Casualty Tracking Cell (CCTC, 2009-10 only).[78]

Airstrikes that inflicted civilian casualties occurred at a pace of once every ten days for 2006-11 and killed an estimated 1,654 to 3,048 individuals while wounding another 698 to 797. These casualties represent an average of nearly 60% of all ISAF-inflicted casualties over this time period. These estimates should, of course, be considered the floor, not the ceiling, of airstrike-inducted casualties. It is also noteworthy that only 82 of these airstrikes are recorded in the CAOC dataset while 68 and 40 are tracked in CIDNE and DAPS, respectively. Only eight airstrikes that harmed civilians are found in all three datasets. Figure A1 plots the location of all 216 incidents.

Using civilian deaths as our central measure of victimization omits other forms of suffering that may also be drivers of insurgent violence, however. To overcome this limitation, every airstrike and show of force was cross-referenced with open source satellite imagery of the targeted location. All 23,000 events were examined independently by two coders using a six-fold classification scheme: compounds (e.g., homes); other buildings; farms; roads; other settlement types; and unpopulated areas. A blast radii for the given bomb size was dynamically generated and then superimposed over the location's grid coordinates to identify which objects to code.[79] Initial intercoder reliability was high (85%); all remaining

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[77] Iacus, King and Porro 2012.

[78]ISAF data are unfortunately far from complete and seriously underreport civilian casualties inflicted by airstrikes. The CCTC uses two categories—confirmed and unconfirmed—to generate estimates. By these standards, 71 or 132 individuals were killed by airstrikes between January 2009 and March 2010, respectively. By contrast, our data suggest between 312 and 634 individuals were killed over the same time span. Moreover, many CIVCAS airstrikes are relegated to the "unconfirmed" category for reasons that remain unclear. For example, the September 2009 airstrike in Kunduz that killed between 56 and 150 civilians only appears in the "unconfirmed" category.

[79]The bomb's blast radius was determined by: $R = 35 \times \frac{W^{(1/3)}}{P^{(.58)}} \times .3048$, where $R$ is the blast radius (in meters), $W$ is the weight of the bomb (assumed here to be 50% explosive by weight) and $P$ is the blast

discrepancies were reconciled by a third coder.

These data indicate that at least 1,478 compounds were struck, along with 2,911 farms, 418 buildings, and 882 road segments. A further 3,975 strikes hit unpopulated areas; these are though to reflect efforts to hit insurgents as they move through forests or other terrain features.[80] This more granular view of civilian harm allows us to link types of property damage to different theories of radicalization of individuals.[81] Revenge motives, for example, are tied most closely with residential property damage, which directly affects the affected individual(s). Damage to farms or infrastructure such as roads may lead to economic immiseration in the form of lost livelihoods (including the hazard of unexploded ordinance in fields) and freedom of movement. In turn, these factors may lower the opportunity costs for participating in the insurgency by destroying outside options while heightening the lure of a steady (rebel) paycheck. Bombing unpopulated spaces suggests a third mechanism—namely, attrition—where airpower is directly applied to (suspected) insurgents without damaging civilian property.

I begin by estimating a model that includes all covariates from Models 1-6 above (*Prior Attacks, ISAF Ops, Info, TIC, Population, Elevation, Neighbors*, and *Pashtun*). I also include a dummy variable to account for Afghanistan's so-called "fighting season" (April-September, *Season*); indicator variables for *Compounds, Buildings, Farms, Roads*, and *Settlements*; an indicator variable for 90 (successful) decapitation strikes as reflected in ISAF press releases (*HVT*);[82] an indicator variable to capture whether the airstrike was conducted by a remotely-piloted vehicle (*Drone*); a logged count of the number of bombs dropped (*Bombs*); a count variable (logged) for number of prior airstrikes (*History*), generated dynamically by the matching program for each airstrike; and, finally, a binary variable for whether civilians were harmed during the airstrike (*CIVCAS*).

In total, 19 covariates are included. Given the model's complexity, I use these estimations as a "first-pass" to identify potentially significant covariates (as reported in Table

overpressure generated as measured by pounds per square inch (PSI). I use a PSI value of 5 here, which is deemed sufficient to destroy typical buildings in Afghanistan within this radius. Note that fragmentation radius is often much larger but these more detailed calculations require additional (classified) information, including fuse settings, angle of attack, and altitude of weapons release. I thank Ted Postol for a detailed discussion of this issue. See also Driels 2004.

[80] A further 1064 airstrikes were conducted within a settlement but did not hit buildings, roads, or farms.
[81] See, for example, Ladbury 2009.
[82] Without access to classified material, this is surely an undercount, both in the numbers of decapitated leaders and failed attempts.

A10). I then estimate a reduced form regression using only covariates that obtained a $p=0.05$ level of statistical significance. The resulting models have a more manageable 10 covariates; results are presented in Table 7.

Several findings emerge. First, *CIVCAS* is typically associated with a *decrease* in insurgent attacks, though this relationship only reaches statistical significance in one model. Second, there is some evidence that airstrikes that hit compounds and (especially) farms are associated with an increase in post-strike insurgent attacks. These results do not extend beyond the 45-day mark, however, and in the case of compounds—perhaps the form of property damage most closely tied to civilian harm—the effect does not even reach the 45-day mark. Evidence for grievance-based accounts is therefore quite modest.

By contrast, nearly all of the covariates that capture war-fighting dynamics are statistically significant and substantively important. Troops-in-contact situations, where insurgent and ISAF forces are directly engaged, are especially prone to observe an armed insurgent response even 90 days after the initial event. Similarly, a history that includes past ISAF operations and being repeatedly bombed is associated with a sharp increase in post-strike insurgent attacks. Notable, too, is the fact that *Season* is also associated with an marked increase in post-strike insurgent attacks. These findings are consistent with the expectations of the reputation-based argument advanced here: airstrikes create opportunities to build or maintain reputations through fighting, which we observe through stepped up insurgent attacks in the post-strike period.

The claim that insurgent attacks appear unaffected by, or even negatively correlated with, civilian casualties is undoubtedly controversial. I therefore reestimate these models using minimum and maximum estimates (logged) of killed and wounded civilians (see Table A13). Once again, *CIVCAS* is typically negatively associated with insurgent attacks, a relationship that just misses conventional significance levels at the 90-day mark.

This estimation strategy may be problematic if "control" airstrikes are not representative of airstrikes that harmed civilians, however. I therefore re-estimate the reduced form regression using 1:1 Coarsened Exact Matching and *CIVCAS* as the treatment. As Table 7 reveals, the results remain unchanged: civilian casualties are unconnected to observed changes in insurgent attacks.[83]

Finally, I test two interaction terms: *CIVCAS*Compound*, which denotes the "most

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[83]Reestimating with a weighted approach to Coarsened Exact Matching does not alter these findings.

Table 7: The (Non-)Effects of Civilian Casualties: All Airstrikes and Coarsened Exact Matching (CEM)

| Covariate | 7 days | | 45 days | | 90 days | |
|---|---|---|---|---|---|---|
| | CIVCAS only | All Covariates | CIVCAS only | All Covariates | CIVCAS only | All Covariates |
| CIVCAS | 0.172 (0.156) | 0.176 (0.144) | -0.349 (0.612) | -0.550 (0.429) | -0.763 (1.189) | -1.276$^{\dagger}$ (0.785) |
| Prior Attacks | | -0.520*** (0.024) | | -0.516*** (0.028) | | -0.564*** (0.031) |
| ISAF Ops | | 0.143* (0.069) | | 0.218* (0.094) | | 0.361*** (0.095) |
| TIC | | 0.820*** (0.063) | | 3.527*** (0.364) | | 6.460*** (0.574) |
| Season | | 0.254*** (0.051) | | 2.117*** (0.299) | | 3.334*** (0.532) |
| Compound | | 0.251** (0.051) | | 0.703 (0.501) | | 0.819 (0.830) |
| Farm | | 0.183** (0.059) | | 0.622* (0.317) | | 0.389 (0.577) |
| History | | 0.076*** (0.015) | | 0.411*** (0.100) | | 0.595*** (0.174) |
| Neighbors | | 0.044* (0.02) | | 0.391*** (0.095) | | 0.908*** (0.163) |
| Elevation | | -0.380*** (0.097) | | -2.205*** (0.527) | | -4.792*** (0.783) |
| Constant | 0.006 (0.027) | 2.639*** (0.685) | -0.171 (0.131) | 15.030*** (3.741) | -0.771** (0.257) | 33.027*** (5.611) |
| F stat | 1.21 | 75.82*** | 0.32 | 58.48*** | 0.41 | 51.31*** |
| $r^2$ | 0.00 | 0.27 | 0.00 | 0.29 | 0.00 | 0.34 |
| Total N | 8,854 | 8,854 | 8,854 | 8,854 | 8,854 | 8,854 |
| *CEM* | | | | | | |
| CIVCAS | 0.203 (0.188) | 0.199 (0.180) | -0.868 (0.724) | -1.052 (0.669) | -0.126 (0.926) | -0.266 (0.827) |
| Constant | 0.053 (0.099) | 1.209 (1.386) | 1.353 (0.707) | 23.967*** (9.125) | 1.378 (0.908) | 40.229*** (12.809) |
| F stat | 1.17 | 1.42 | 1.44 | 1.43 | 0.02 | 6.76*** |
| $r^2$ | 0.00 | 0.12 | 0.00 | 0.18 | 0.00 | 0.28 |
| Total N | 266 | 266 | 272 | 272 | 270 | 270 |

*Note:* Reduced form models are run on the entire 8,854 airstrike sample. CEM 1:1 is then used to match civilian casualty-inducing airstrikes with airstrikes that did not harm civilians. Matches on the reduced model. Robust standard errors clustered on individual villages. A 2km$^2$ radius was used to calculate pre- and post-insurgent violence. Significance levels: ***$p$=<.001, **$p$=<.01, *$p$=<.05, $^{\dagger}p$=<.10

likely" instance where we might observe a link between airstrikes, grievances/revenge, and subsequent insurgent attacks; and *CIVCAS\*History*, where civilian casualties and repeated exposure to bombing might also generate grievances that translate into insurgent violence. These tests muster little evidence for a grievance-based interpretation of post-strike insurgent attacks (Table A14). *CIVCAS\*Compound*, for example, only (barely) reaches conventional levels of statistical significance in one model, while the constituent parts of the interaction term point consistently point in opposite directions, reaching statistical significance in different time windows (if at all). Similarly, *CIVCAS\*History* only reaches statistical significance in the initial 7-day time window. *History*, by contrast, is consistently significant across all three models. For both interaction terms, the inclusion of *CIVCAS* appears to provide little leverage in explaining insurgent attacks.

## Conclusion

This paper has marshaled evidence to support the claim that a robust positive relationship exists between airstrikes and insurgent attacks. Driven by reputational demands, the Taliban and other insurgent organizations are seizing the signaling opportunities created by ISAF's aerial coercion to solidify their bargaining positions relative to the counterinsurgent and local populations. The swift nature of insurgent responses, coupled with their highly localized nature, suggest that these organizations value their reputations and are willing to invest in costly actions to maintain them. That shows of force elicit nearly the same reaction as airstrikes underscores the relative importance of reputational pressures rather than revenge motives when explaining insurgent behavior.

While the costs of airstrike-induced civilian casualties certainly should not be minimized, these findings also indicate that civilian fatalities do not explain the uptick in insurgent attacks after both airstrikes and shows of force. This surprising (non-)finding may stem partly from the literature's too-narrow conception of civilian harm as fatalities: compound and farm damage, for example, was positively associated with net increases in insurgent attacks in several models, suggesting an alternative pathway by which grievances could explain insurgent violence. Ultimately, however, battlefield dynamics, as anticipated by the reputational theory advanced here, provided most of the explanatory leverage.

These findings speak to the broader debate about whether, and when, reputation matters for explaining the wartime conduct of states and other actors. In settings marked by

repeated interaction and credible commitment problems, we are likely to observe actors investing heavily in creating and then maintaining reputations for resolve despite—indeed, because of—the costs involved. Paradoxically, as power asymmetries increase, we are likely to observe increased returns for insurgents when investing in their reputations and a corresponding decrease in the effectiveness of the counterinsurgent's coercive efforts even if costs are being imposed. While attrition and decapitation of leaders may well be occurring, it is likely that in many counterinsurgency wars the increase of reputation-induced violence is sufficient to swamp these other mechanisms.

Several theoretical extensions are also suggested by these findings. Much more work needs to be done in exploring how the rebel-population relationship conditions wartime dynamics, including the value insurgents place on their local reputations. Subsetting our microlevel datasets according to insurgent organization (and numbers operating in the same space) to test for conditional average treatment effects is one obvious next step. The reputational theory articulated here could also be extended to examine different dependent variables, including violence against civilians, the sophistication of rebel tactics, or the nature of tactical substitution across the group's portfolio of violence. The adoption of other empirical approaches, including survey experiments to measure wartime attitudes toward insurgent organizations indirectly, would provide the non-observational data necessary to examine the incentives driving insurgent organizations when responding to the counterinsurgent's coercion.[84]

On the joint methodological-empirical front, the paper's approach to capturing wartime dynamics can be extended in several directions. The interaction *between* different forms of aerial coercion could be set in a dynamic treatment framework that would explicitly analyze how switching between strategies, as well as the cumulative effects of these switches over time, affect insurgent behavior.[85] Similarly, the interaction of these strategies with nonviolent approaches — notably, the use of aid programs to win "hearts and minds" — could be modeled directly to enrich our understanding of the conditionality of violence. How coercive attempts are perceived by local audiences may hinge at least partly on economic assistance programs that condition who is blamed for inflicting harm and damage within a given village, for example.

Finally, an important question for future research lies in exploring how local effects

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[84]Lyall, Blair and Imai 2013.

[85]On dynamic treatment regimes, see Blackwell 2013.

"scale up" to affect macrolevel war outcomes. Clearly air operations have more than just local effects: much of the strategic-level animosity between President Karzai and American officials stems from his longstanding objection to airstrikes given their toll on civilians, for example.[86] Nor can we conclude from this study that airstrikes are an ineffective coercive tool in all contexts or that their use necessarily translates into eventual military defeat. Instead, we can infer that the magnitude and severity of airstrikes (and shows of force) as practiced in Afghanistan have been insufficient to achieve a decisive breakthrough against the Taliban. Similar efforts would be unlikely to defeat insurgent organizations with comparable or greater resolve and skill. Moreover, the historical record is not optimistic about the prospects of escalation as a means of reaching a decisive breakthrough against insurgent foes. The two most severe bombing campaigns in history—the United States in Vietnam and the Soviet Union in Afghanistan—illustrate how insurgents can absorb tremendous losses and still continue to fight. Much more comparative research is required to test theories and mechanisms that link micro- and macrolevel outcomes to understand the scope conditions under which airpower is an (in)effective tool of coercion.

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[86] "How Is Hamid Karzai Still Standing?" *New York Times Magazine*, 20 November 2013.

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