Airpower and Coercion in Counterinsurgency Wars: Evidence from Afghanistan*

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Are airstrikes an effective tool of coercion against insurgent organizations? Despite the historical and contemporary relevance of the research question, 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 cost tolerance and resiliency by quickly responding to counterinsurgent actions with their own violence.

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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 (Corum and Johnson, 2003; Kocher, Pepinsky and Kalyvas, 2011; Johnston and Sarhabi, 2013). 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 (Pape, 1996; Byman and Waxman, 1999; Horowitz and Reiter, 2001; Byman and Waxman, 2002; Gray, 2012; Allen, 2007; Van Creveld, 2011). Here, too, there is substantial debate over airpower's effectiveness both across time and in high-profile cases such as Kosovo (Byman and Waxman, 2000; Stigler, 2002/03; Pape, 2004).

I take up the challenge of theorizing and testing the coercive effects of airpower at the village level in counterinsurgency wars. I argue that airpower, for all its ability to threaten and inflict harm on 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 cost tolerance and resiliency.

To test this reputational argument, I draw on declassified United States Air Force (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 matching, dynamic matching, that estimates causal effects of air operations over variable temporal and spatial windows as fine-grained as one day and a single kilometer around the bombed village and its control.

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 correlated with civilian casualties. Instead, battlefield dynamics provide nearly all the explanatory leverage when accounting for post-event patterns of insurgent attacks.

1 Context: Wartime Coercion and its Effects

Though at heart a simple question, the study of airpower's wartime effects quickly runs into a host of conceptual and theoretical issues not encountered in the existing (voluminous) literature on interstate coercion. For example, interstate coercion is almost exclusively treated as a two-player, single-shot game with binary, strategic-level outcomes such as war/no war (Slantchev, 2011; Sechser, 2010; Schultz, 2001; Fearon, 1994; Huth, 1988; Huth and Russett, 1984). By contrast, wartime coercion is necessarily a dynamic, repeated game between at least three players (counterinsurgent, insurgent organization, and population) where outcomes are likely to be multifaceted and local in nature. I define *wartime coercion* here as the threat and use of violence to inflict harm on an adversary and its supporters now and in the future if current demands are not met.

Airpower is therefore a tool of coercion rather than simply "brute force" wielded against insurgent foes. As one form of wartime coercion, airpower is enmeshed in a broader political bargaining framework where violence (and the threat of its use) against *some* insurgents is intended to influence the behavior of *other* insurgents and their supporters over time (Schelling, 2008, 3-6). This political context also collapses the distinction between compellent and deterrent threats that is the hallmark of the study of interstate coercion.[1] As Thomas Schelling noted, "once an engagement starts… the difference between deterrence and compellence, like the difference between defense and offense, may disappear" (Schelling, 2008, 69-72, cite on p.80).

Yet despite a century of experience with airpower as a coercive instrument, there is no agreement on its effectiveness. I first review the debate and then advance my own theoretical account of airpower's effects in a counterinsurgency environment.

1.1 Airpower as Coercive Tool

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 (Van Creveld, 2011, 51-78). More recently, Thomas Schelling's influential writing on coercion, though typically associated with nuclear strategy, draws heavily on air power examples (especially Vietnam) to illustrate the properties of "ideal" coercive acts (Schelling, 2008, 6,8,13,16,17-18,25,30).

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 (Schelling, 2008, 89). Recent efforts to extend airpower's role in the "diplomacy of violence" to include remotely piloted vehicles ("drones") and precision munitions may further increase the counterinsurgent's ability to influence and punish insurgent organizations.

Airpower generates these effects through multiple mechanisms at different levels of analysis. Airstrikes can cripple insurgent organizations by decapitating their leaders, degrading command and control structures and in turn reducing their capacity to conduct

[1] Following Schelling (2008,69), compellent threats "make an adversary do something" while deterrent threats are "intended to keep him from starting something."

attacks. Evidence remains mixed, however, about the effectiveness of decapitation strikes.[2] 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.

Taken together, these mechanisms may create sufficient pressure to force an insurgent organization to the negotiating table or, failing that, lead to its outright destruction. 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 (Pape, 1996; Byman and Waxman, 2002; Lyall, 2009).[3]

Evidence of airpower's coercive abilities is provided by a careful study of nearly 400 US drone strikes in seven agencies in Pakistan during 2007-11 (Johnston and Sarhabi, 2013). Using an agency- and week-level fixed effects estimation strategy, these authors conclude that drone strikes are associated with a 24% decrease in insurgent attacks; a 16% decrease in the lethality of these attacks; a 22% reduction in the use of improvised explosive devices (IEDs), and possibly a 32% decrease in suicide bombing. There is also some evidence that these suppressive effects diffuse up to 125 kilometers beyond the initial target's location. While the authors caution against making strong causal claims given their empirical strategy (Johnston and Sarhabi, 2013, 27,40), these findings at least suggest that airpower can reduce insurgent attacks in a modern civil war setting.

Airpower enthusiasts have also emphasized how airstrikes can generate second-order deterrent 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 during uprisings in Mesopotamia, "and there has been an immense saving of blood and treasure to the British and Iraqi governments (Hart, 1932, 155)."

[2]Byman (2006) and Johnston (2012) argue that decapitation strikes can degrade insurgent capabilities and help bring about counterinsurgent victories. Price (2012) finds that these efforts have diminishing effects as organizations age, while Jordan (2009) and Pape (1996) contend that decapitation has no effect on insurgent resiliency or war outcomes.

[3]Supporters and, more generally, civilians in war zones may counter-mobilize not because of preference changes but because they seek to escape further punishment at the hands of the counterinsurgent. See Kalyvas (2006); Kalyvas and Kocher (2007).

Viewed from this perspective, non-lethal shows of force should also be considered as coercive instruments. These displays can be considered a form of signaling (Fearon, 1997) to insurgents and their supporters about future costs if present actions are not reversed. In particular, shows of force have clear incentive structures and are openly communicated to adversaries, two requirements for successful coercion (Schelling, 2008, 80). 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 also 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 airpower enthusiast, "is no high life casino" (Peck, 1928, 542). These non-lethal operations may also drive a wedge between insurgents and the populace by vividly underscoring the counterinsurgent's power to hurt and the lack of a symmetrical insurgent response.[4]

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. Crossnational studies of strategic bombing, for example, have concluded that these campaigns are unlikely to bring about desired outcomes since the scope conditions for success are absent (Pape, 1996; Corum and Johnson, 2003; Byman and Waxman, 2002; Horowitz and Reiter, 2001; Clodfelter, 1989; Thies, 1980). The few microlevel studies that exist have reinforced this conclusion by suggesting that airpower is unavoidably an indiscriminate weapon that kills civilians, feeding insurgent recruitment and sparking new rounds of violence (Kocher, Pepinsky and Kalyvas, 2011; Ladbury, 2009). In short, "the use of airpower in such [civil] wars has been the record of almost uninterrupted failure" (Van Creveld, 2011, 338).

[4]Shows of force may also aim to impress local populations with their restraint, drawing a contrast with insurgent actions that may be indiscriminately targeting civilians.

Insurgents, for example, typically lack the key assets—capitals, infrastructure, and fielded forces—that must be threatened with destruction if coercion is to be successful. Even identifying insurgents, let alone coercing them, can be difficult if they blend within the population. Civilians populations may also not possess the ability to exercise any influence over insurgent decision-making, making punishment futile. Some insurgent organizations may be sufficiently decentralized to foil leadership decapitation efforts. Tactical substitution can also allow insurgents to innovate around coercive pressure far more readily than conventional armies. As Robert Pape concludes, "Guerrillas should be largely immune to coercion" (Pape, 1996, 74).

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 (Petersen, 2001; Kalyvas, 2006; *U.S. Army Field Manual No.3-24*, 2007; Condra and Shapiro, 2012). This logic is on display in the case of South Vietnam, where US bombing was associated with a shift of hamlets from pro-government to pro-Vietcong control from July to December 1969 (Kocher, Pepinsky and Kalyvas, 2011). While the dependent variable is territorial control, not Vietcong attacks, this account is consistent with the claim that civilian casualties lead individuals to shift their allegiance away from the perpetrator, fueling further violence.[5]

Given these arguments, shows of force are also unlikely to represent credible threats. These public signals impose little cost on the sender and so may be disregarded by targeted audiences as "cheap talk" (Fearon, 1994; Schultz, 1998). Repeated exposure to these signals may simply inure rebels to their use, a problem noted as early as the 1920s (Peck, 1928). 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.[6]

We should therefore expect that airstrikes are associated with increased insurgents attacks, especially after civilians are killed or wounded. A positive relationship should also be

[5]These authors do not possess civilian casualty data; these are inferred from the amount of ordinance dropped on and near targeted locations.

[6]It is worth noting that there likely to be considerable heterogeneity among organizations in understanding the signal's intent; misinterpretation of their meaning may be common, at least initially.

Table 1: Summary of Existing Explanations and their Mechanisms

	Mechanisms	
Expected Relationship	Airstrikes	Shows of Force
More Air Operations, Less Attacks	Decapitation (Leaders) Attrition (Rank-and-file) Punishment (Supporters)	Credible Threat
More Air Operations, More Attacks	Grievances/Revenge	Cheap Talk

Note: These mechanisms are suggested by existing theories to account for net differences in the frequency of insurgent attacks after an air operation (relative to a pre-air operation baseline). Intended audiences for these coercive efforts are listed in parentheses; where absent, the proposed mechanism works similarly across different levels of analysis. "Attrition" as used here is consistent with Pape's definition of a denial strategy and, at the extreme, Schelling's notion of "brute force." Coercive strategies can draw on multiple mechanisms to create their effects.

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 expected since shows of force do not attrit insurgents' capabilities. Table 1 summarizes these two contending camps and the mechanisms associated with their arguments.

2 Reputation-Building Through War-Fighting

Overlooked in these debates is the role that reputational concerns play in driving insurgent responses to counterinsurgent actions. If we view wartime coercion as a dynamic bargaining process (Reiter, 2003), then it is probable that air operations create incentives for insurgents to escalate their violence in a bid to bolster their reputation for cost tolerance and resiliency. Insurgent organizations have two audiences: the counterinsurgent (including its external patron, if present) and local populations. Air operations represent both a challenge and an opportunity for insurgents to maintain their reputation for effectiveness by stepping up attacks that drive home to these audiences that they retain the organizational capacity to harm opponents. In this view, both airstrikes and non-lethal shows of force will be met with a net increase in insurgent attacks against counterinsurgent forces.[7]

Insurgents, for example, are engaged in a struggle to impose costs on the counterinsurgent as a means of maintaining their reputation for resiliency. A reputation for resolve and persistence is valuable in this context since it shapes the likelihood and nature of the war's eventual 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, in other words, 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 (in the interstate context, see Sechser, 2010, 653).[8]

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 (Mack, 1975; Blainey, 1988; Fey and Ramsay, 2007, 56). With 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 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, whether material or political (or both), to demonstrate that a political solution is preferable to a continuation of a grinding, increasingly futile, war effort.

Insurgents must also appeal to a second audience: their local supporters. From the insurgent organization's perspective, air operations can degrade capabilities, allowing dangerous cracks to emerge in the local population's support. Demonstrating resolve and the continued ability to violently coerce the counterinsurgent becomes important for deterring potential defection among supporters. Defection can take several forms, ranging from withholding material assistance and information to providing tips to the counterinsurgent.

[7]Experimental evidence suggests that individuals are more likely to invest in reputation-building if they believe they will encounter an adversary repeatedly in the future. Counterinsurgency wars represent a repeated play setting since no single coercive act can destroy either side. See Walter and Tingley (2011).

[8]In this view, strategies of attrition may not have a tipping point; instead, they create incentives to continue fighting even if casualties are incurred.

At the extreme, civilians may counter-mobilize against insurgents by forming their own militia or siding openly with counterinsurgent forces.

Revealing the capacity to "hit back" at the counterinsurgent after an airstrike carries the implicit message that these coercive abilities could also be turned against would-be civilian defectors and wavering insurgents. By contrast, failure to respond in kind may invite whispers that rebel control is slipping. The Pakistani Taliban in Waziristan, 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 (Shah, 2013, 242).

Yet why would shows of force, which impose no material costs on insurgents or populations, influence insurgent behavior? As symbolic, highly visible, reminders of the counterinsurgent's ability to inflict harm, shows of force can influence insurgent actions by threatening to open a wedge between an organization and its supporters, in two ways. First, shows of force may reinforce local hostility toward the counterinsurgent since they represent reminders of its occupying status. The population (or segments of it) may expect an insurgent response to assuage popular sentiment. Indeed, local civilians may even shrug off casualties inflicted by insurgents while striking back, particularly if those individuals have been victimized by the counterinsurgent (Lyall, Blair and Imai, 2013). 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. Contrary to existing theories, non-lethal shows of force do represent credible threats but are *spurs*, not deterrents, to future action.

Violence has conditional effects in wartime (Lyall, 2010), however, and so we should not expect all insurgent organizations to respond in identical fashion. Instead, the frequency and rate of insurgent response to the counterinsurgent's coercion hinges on the nature of the relationship between rebels and the local population. Scholars have now turned their attention to studying rebel governance in civil wars (Arjona, 2010; Metelits, 2009; Mampilly, 2011; Wood, 2003; Parkinson, 2013; Staniland, 2012). Here I treat this relationship as a mediating variable between the counterinsurgent's wartime coercion and the production of violence by an insurgent organization. While this relationship is partly a function of rebel-civilian interaction, it is heavily influenced by the counterinsurgent's own coercive actions, a fact typically overlooked by existing studies. Thus, while airstrikes may directly affect insurgent capabilities (e.g., via attrition), the degree to which insurgents are embedded within the local population will mediate subsequent insurgent violence.

Imagine, for example, possible rebel-civilian "social orders" (Arjona, 2010) arrayed along a 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 (Olson, 1993). 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 (Arjona, 2010). This relationship may change over time; it may also vary spatially across the same organization.

The expectation is that, 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.[9] 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 forestall the erosion of support among already disgruntled civilians.

Not all of these empirical claims can be tested with observational data. In particular, capturing the relationship between civilians and insurgents requires close-range qualitative and survey data. But this discussion does raise several hypotheses about insurgent behavior that differ from existing accounts. First, we should anticipate that airstrikes are associated with a net increase in attacks relative to similar non-bombed locations when an insurgent organization is enmeshed within a local population (Hypothesis 1). Second, reputational

[9] Pressures to "fight fire with fire" will only increase if there are multiple insurgent organizations claiming control over a population.

demands should lead to rapid insurgent "push-back" after the airstrike; response times are governed by a "quick fuse" rather than "slow burn" logic (Hypothesis 2). Third, insurgent responses should be centered around the bombed location and should decay over distance given the importance of demonstrating resolve to a local audience (Hypothesis 3).

Two additional tests can be used to separate these reputational concerns with grievance-based accounts that expect behaviorally equivalent outcomes. First, we should expect that insurgent attacks also increase after non-lethal shows of force as insurgent respond to the reputational threat inherent in these signals rather than material harm (Hypothesis 4). Second, changes in post-strike insurgent violence are unlikely to be connected to civilian casualties. That is, insurgents will respond equally to airstrikes that do (not) kill civilians since the motives for action are rooted in reputation, not revenge (Hypothesis 5).

3 Empirical Strategy

Drawing on microlevel battlefield data marks a substantial departure from the crossnational data and research designs that provide the empirical basis for studying interstate coercion. Indeed, disaggregated conflict data represents a scale-shift in the number of relevant observations of coercion. The Militarized Compellent Threats dataset—the most comprehensive of its kind—records 210 interstate compellent threats between 1918 and 2001 (Sechser, 2011, 379). By contrast, the universe of coercive acts during wartime, including village patrols, blockades, and air operations, can run in the tens, if not hundreds, of thousands of observations for a single conflict.

These daily occurrences of coercion across a war zone require theories and empirical testing that explicitly acknowledge the spatio-temporal dynamics of civil war violence. Yet current theories provide ambiguous guidance on when (and where) we should observe the effects of air operations on targeted (and neighboring) populations. We therefore need an empirical strategy that is flexible enough to identify counterfactual observations over variable spatial and temporal windows.

Matching offers one possible approach (Ho et al., 2007). Based on the Neyman-Rubin Causal Model (Rubin, 2006), 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. These counterfactuals provide baseline observations that (ideally) control for selection processes, key covariates that might otherwise explain outcomes, and temporal trends not connected to the treatment.

Yet matching has limitations. 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.

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 for a subnational administrative unit (e.g., a district, municipality, or province) that is far greater in size 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 (Pierskalla and Hollenbach, 2013). 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 pre-treatment 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 covariates and then repeat the procedure using a less restrictive criterion for goodness of matching.[10] 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.[11] The same coding procedure was followed for shows of force to avoid inflating our number of observations by falsely treating related observations as independent.

CAOC data was supplemented by two other sources. Declassified data from the Interna-

[10] I use a $\leq .25$ standardized bias score as the measure of goodness of matching (Ho et al., 2007).

[11] Events occurring within .5km and three hours of one another were collapsed into a single event.

tional 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 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).

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.[12] 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

[12]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.

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.

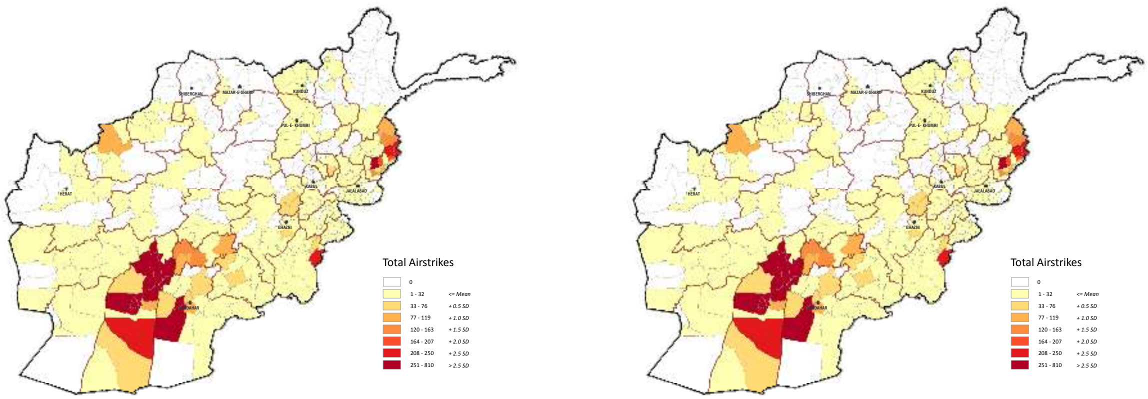
attacks are combined in the *Attacks* variable.[13]

3.3 Dynamic Covariates

Given the non-random nature of air operations in Afghanistan, we face a selection problem: villages experiencing an airstrike or show of force may systematically differ from non-treated villages, especially in terms of expected outcomes. We therefore rely on matching to identify similar control observations by adjusting for pre-treatment differences across targeted and non-targeted villages. We are aided in this endeavor by the richness of CIDNE data. Six covariates are dynamically generated for specified temporal and spatial windows around treated villages and then matched on to reduce the threat of potential selection bias.

First, the number of insurgent attacks prior to the air operation is calculated to account for insurgent violence and (indirectly) the presence of ISAF forces (*Prior Attacks*). Second, the number of pre-air operation ISAF military operations around a treated or control village

[13]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.



(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.

is calculated (*ISAF Ops*).[14] 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.[15]

Fourth, a "Troops in Contact" (*TIC*) covariate is constructed dynamically 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

[14] 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.

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

of airstrike-induced civilian casualties (United Nations Assistance Mission in Afghanistan, 2011; Human Rights Watch, 2008).

Finally, we must account for the fact that some villages will experience multiple airstrikes or shows of force. I account for this possibility in two ways. *History* records the number of prior airstrikes (or shows of force) that a particular location has witnessed to that date. Accounting for prior operations facilitates the use of dose-response tests to explore whether air operations have cumulative or threshold effects on *Attacks* (Rosenbaum, 2010, 124-25).

More subtly, it is possible that additional airstrikes or shows of force may fall within the post-treatment windows of a prior air operation, complicating efforts to estimate causal effects. In particular, as the temporal and spatial treatment windows are pushed outward, the odds of observing additional (post-treatment) air events increases, thus potentially disrupting the parallel trends assumption that difference-in-difference estimation relies on to isolate causal effects. The matching procedure therefore dynamically counts the number of air events that occur within a village's post-treatment window (*Disturbance*). This covariate allows us to identify and set aside, if necessary, observations where we may have concerns that violations of the parallel trends assumption are skewing our causal estimates.

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 (Fearon and Laitin, 2003), village elevation (logged, in meters) was calculated from Shuttle Radar Topographic Mission (SRTM) satellite imagery. A village's neighborhood was also taken into account by counting the number of settlements within a 5 km^2 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].) This .289 increase represents a staggering +5780% increase over the difference observed in control villages (with a 95% CI at [+4680%, +6700%]).[16] We observe .683 more attacks per treated village (95% CI at [.479, .885]) compared with control villages by the 45 day mark. This represents a +1364% increase over the control's observed difference (with 95% CI at [+958%, +1700%]). By 90 days, the difference-in-difference has increased to 1.03 more attacks (with 95% CI at [.671, 1.395]), or a +667% increase (with a 95% CI at [+433%, +900%]) over the control's observed difference. 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 more insurgent 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.,

[16] Defined as: $\frac{(Y_1^t - Y_0^t) - (Y_1^c - Y_0^c)}{(Y_1^c - Y_0^c)}$

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, $^\dagger p$=<.10

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 small villages, which represent the vast majority of Afghanistan's settlements.

To reduce bias associated with attrition of treatment observations (Rosenbaum, 2010), I relax the strict requirements of exact matching. All covariates in these "best matchings" models are permitted to "float" within specified ranges; I use a standardized bias score of $\leq .2$ as the measure of closeness of matching between treated and control cases (Ho et al., 2007). 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 are .371 more attacks in the initial 7 days after an airstrike (with a 95% CI of $[.31, .44]$). This represents a +331% increase relative to the difference observed in control villages (95% CI at $[273\%, 389\%]$). At the 45 day mark, there are 1.29 more attacks on average in the bombed locations (95% CI at $[.99, 1.58]$), a +678% increase over the difference observed in non-bombed villages (95% CI at $[522\%, 832\%]$). By the time we reach the 90 day post-strike threshold, there are 2.34 more attacks on average in the bombed locations (95% CI at $[1.82, 2.86]$), a +1170% increase (95% CI at $[910\%, 430\%]$). 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.

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 45 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 $[1.58, 2.39]$).[17]

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.[18] Second, *History* also emerges as positively associated with post-strike insurgent attacks in four of six models while just missing conventional levels of significance 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.

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

[18] 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, $^{\dagger}p$=<.10

4.3 Drones

A prominent public debate has arisen around the effectiveness (and ethics) of drone strikes (Johnston and Sarhabi, 2013; New America Foundation, 2013; The Bureau of Investigative Journalism, 2013). Proponents of their use point to the precise and selective nature of their airstrikes, characteristics that represent a "most likely" case to observe a negative relationship between airstrikes and subsequent insurgent attacks. To date, however, much of this debate has centered around drone use in Pakistan, Yemen and Somalia. Afghanistan, by contrast, has been relatively ignored, despite the fact that more drone strikes have occurred here (N=943) than in the other three cases combined.

I examine this question by reestimating Models 1-6 with a binary variable, *Drone*, designating whether an airstrike was conducted by a remotely piloted vehicle. Are drones more effective at reducing post-strike insurgent attacks than other aircraft? 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 opposite the relationship expected by proponents.[19] While the focus on drones is perhaps understandable, a fixation on their use to the exclusion of comparison with other aircraft misses the fact that effects on the ground do not appear to be platform-specific.

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.

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,

[19] *Drones* is also not significant when used as a treatment for matching across the entire airstrike dataset.

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 when moving from a 50 km^2 to 100 km^2 radius for the 45 day time period.[20] 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 $[-.65\%, 7.8\%]$).

[20]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.

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†
	(0.374)	(1.99)	(4.12)	(5.78)
$100km^2$	2.14***	7.99**	20.55**	16.81†
	(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

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 (Bertrand, Duflo and Mullainathan, 2004). 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 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.[21]

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 that indicates whether additional airstrikes occurred within the 7, 45, or 90 day post-airstrike windows. 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-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 targeted 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);[22] (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

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

[22]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*).

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.[23]

Two additional comparisons are therefore helpful in isolating the mechanisms underpinning 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 insurgents 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 that can be easily dismissed. 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 without 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 should conclude that reputational concerns, rather than revenge, are driving insurgent behavior.

[23]On identifying causal mechanisms in coercive contexts, see Mueller (1998, 186).

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.[24] 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.[25]

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 (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 the 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

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

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

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, $^{\dagger}p$=<.10

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 (Iacus, King and Porro, 2012) as a robustness check to more narrowly match airstrikes that harmed civilians with "control" airstrikes that did not result in civilian casualties.

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).[26] Airstrikes that inflicted civilian casualties occurred

[26]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

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.[27] Initial intercoder reliability was high (85%); all remaining 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.[28] This more granular view of civilian harm allows us to link types of property damage to different theories of radicalization of individuals (see, for example, Ladbury 2009). 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

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.

[27] 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 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.

[28] A further 1064 airstrikes were conducted within a settlement but did not hit buildings, roads, or farms.

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*);[29] 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*); 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 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

[29]Without access to classified material, this is surely an undercount, both in the numbers of decapitated leaders and failed attempts.

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.[30]

Finally, I test two interaction terms: *CIVCAS*Compound*, which denotes the "most 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 violence. Driven by reputational demands, the Tal-

[30]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†	(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

iban and other insurgent organizations are exploiting the opportunities created by ISAF's coercion to bolster their reputation for resiliency and cost tolerance. 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 maintaining them through war-fighting. That insurgents respond (nearly) equally to non-lethal shows of force further underscores the importance of reputation to insurgent decision-making rather than material costs or grievances.

And while the costs of airstrike-induced civilian casualties should not be minimized, these findings indicate that civilian fatalities do not explain the uptick in insurgent attacks after airstrikes or 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. Ultimately, however, battlefield dynamics, as anticipated by the reputational theory advanced here, provide most of the explanatory leverage.

These findings suggest several theoretical and methodological extensions. On the theoretical front, the paper has argued for adopting a conditional view the effects of violence Lyall (2010). Much more work needs to be done, however, in exploring how the nature of rebel-insurgent interaction can condition the nature of wartime dynamics, including the value an insurgent organization places on its reputation in the eyes of local audiences. Subsetting our datasets according to insurgent organization to test for conditional average treatment effects is one necessary next step. In addition, the adoption of other empirical approaches, including survey experiments to measure wartime attitudes toward insurgent organizations indirectly, would provide the context necessary to examine the incentives driving insurgent organizations when facing coercion by the counterinsurgent.

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 (on dynamic treatment regimes, see Blackwell 2013). Similarly, the interaction of these strategies with non-violent 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 "scale up" to affect strategic level outcomes. While airstrikes are fueling insurgent violence at the village level, how these effects cumulatively affect operational and strategic level processes, including the war's outcome itself, remains unclear. Understanding the mechanisms that link the micro- and macro-level promises to shed additional insight into the effectiveness of airstrikes as a coercive tool in counterinsurgency contexts in Afghanistan and beyond.

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