Bombing to Lose?
Airpower, Civilian Casualties, and the Dynamics of Violence in Counterinsurgency Wars*

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March 27, 2015

Are airstrikes an effective tool 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. To answer this question, I draw on declassified United States Air Force records of nearly 23,000 airstrikes and non-lethal shows of force in Afghanistan (2006-11), satellite imagery, and a new SQL-enabled form of dynamic matching to estimate the causal effects of airstrikes on insurgent attacks over variable temporal and spatial windows. Evidence consistently indicates that airstrikes markedly increase insurgent attacks relative to non-bombed locations for at least 90 days after a strike. Civilian casualties play little role in explaining post-strike insurgent responses, however. Instead, these attacks appear driven by reputational concerns, as insurgent organizations step up their violence after air operations to maintain their reputations for resolve in the eyes of local populations.

*This paper was made possible by a small army of research assistants at Yale (Hans Teju, Meredith O’Hare, Sesen Yehdego, Sage Price, Kishen Patel, Jared Jones, Jaime de Leon, Geoffrey Smith, Elizabeth Rodrick, Joyce Shi, Mary Polk, Luka Kalandarishvili, Taylor Stib, Catherine Padhi, and Wazhma Sadat) and Stanford (Christy Abizaid, Molly Elgin, Sarah Mantels, Valerie Szybala, Tyler Pratt, and Marie Claire Vasquez). In Afghanistan and Southwest Asia, I thank Kyle Pizzey, Lt Gen David Goldfein, Maj Gen James Jones, Lt Col Michael Winters, Gen (Ret.) Robert Elder, and Lt. Erik Lin-Greenberg for assistance in securing these data. I thank David Laitin for his early encouragement of this project as well as Allan Dafoe, Kelly Greenhill, and Patrick Johnston, along with numerous individuals who wish to remain anonymous. Earlier versions were presented at Emory University, University of Ottawa, EITM 2011, UCLA, London School of Economics, and College of William and Mary. Support from the Air Force Office of Scientific Research (Grant #FA9550-09-1-0314) is gratefully acknowledged. These views do not represent the official opinion or policy of the Department of Defense, US Air Force, or AFOSR; I am responsible for all errors. The appendix contains additional analyses and robustness checks. Replication data and data visualizations posted at:

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Are airstrikes an effective tool 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 destructiveness to the task of defeating insurgents. The past decade alone has witnessed extensive air campaigns against insurgents in Afghanistan, Gaza, 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. 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. Here, too, there is substantial debate over airpower’s effectiveness across time and in high-profile cases such as Kosovo.

I take up the challenge of testing airpower’s effects in counterinsurgency wars. To do so, I use 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 both the effects of airstrikes and the mechanisms that shape insurgent responses. 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. Satellite imagery is employed to identify the targets of these airstrikes, permitting a much more fine-grained testing of mechanisms behind these effects, including the oft-cited claim that civilian casualties increase insurgent attacks.

Four main findings emerge. First, airstrikes are strongly associated with net increases in the mean number of post-strike insurgent attacks in targeted villages relative to control villages. Second, these increases are fairly long-lived, lasting at least 120 days after an air operation, though the magnitude of the effect dissipates over time.

Third and perhaps most counterintuitively, these effects are not associated with civilian casualties. Instead, the Taliban respond in equal measure to airstrikes that do, and do not, kill civilians. Instead, a different mechanism, one that privileges the role of reputational dynamics, may be at work. Given their destructive and highly visible nature, airstrikes create incentives for insurgent organizations to invest in reputations for resolve in the eyes of local populations by striking back at counterinsurgent forces. Failure to do so may embolden civilians to withhold support or even defect from the insurgency, challenging its ability to maintain control and possibly given rise to armed counter-mobilization. Fourth, and consistent with a reputational account, insurgents also respond to non-lethal shows of force with additional violence, an odd finding if civilian casualties are driving insurgent violence. These results are robust to multiple placebo tests, cross-validation with two different datasets of insurgent violence, and alternative statistical models.

1 Airpower and Insurgent Violence: The Debate

Though studies remain few and far between, we can nonetheless identify two broad positions in the debate over airpower’s effectiveness in counterinsurgency wars.

As early as the 1920s, for example, strategists heralded the advent of airpower as a cheap, effective, and “civilized” means of fighting rebel forces.\(^4\) 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.\(^5\) More recently, 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.\(^6\)

Scholars have cited at least three airpower strategies — decapitation, attrition, and pun-
ishment — that individually and collectively 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.

A careful study of nearly 400 drone strikes in Pakistan (2007-11) illustrates how airstrikes can negatively affect militant violence. 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 week of a drone strike. While the authors caution against making strong causal claims given their empirical strategy, these findings suggest that airpower can reduce insurgent attacks in a modern counterinsurgency setting.

A second camp has challenged these claims and instead has 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.” Existing studies of strategic bombing have concluded that these campaigns are unlikely to bring about desired outcomes. If airpower against states is typically ineffective, then counterinsurgency wars represent the edge case, since insurgents typically lack the key assets—capitals, infrastructure, and fielded forces—that must be threatened if airpower is to have a chance at being

Pape 1996; Byman and Waxman 2002.
Byman and Waxman 2002; Lyal 2009.
Johnston and Sarhavi 2013, 36.
Johnston and Sarhavi 2013, 27,40.
More generally, Byman 2006 and Johnston 2012 argue that decapitation strikes can degrade insurgent capabilities and help bring about counterinsurgent victories.
Van Creveld 2011, 338.
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. As Robert Pape concludes, “Guerrillas should be largely immune to coercion.”

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. This logic is on full display in a detailed 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. 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. Put simply, airpower is thought far too blunt an instrument to wield effectively in counterinsurgency wars, suggesting that we should observe a positive association between airstrikes and subsequent insurgent violence.

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. 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—

1 Jordan 2009
2 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).
4 Kocher, Pepinsky and Kalyvas 2011
5 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


by using violence to drive home the message that they retain the organizational capacity to harm opponents.\textsuperscript{16}

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 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 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\textsuperscript{20} 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.\textsuperscript{21} 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 ex-

\textsuperscript{16}On the importance of territorial control in civil war, see Kalyvas 2006.
\textsuperscript{20}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.
\textsuperscript{21}Mack 1975, Blainey 1988 56.
treme, 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 Waziri-stan, 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. 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.

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.

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. 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.

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23 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.
24 Lyall, Blair and Imai 2013.
25 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.
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.

Scholars have now turned their attention to studying rebel governance in civil wars. It is possible to array rebel-civilian “social orders” 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. 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. 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 me-
Table 1: Coercive Logics and their Mechanisms by Type of Air Operation

<table>
<thead>
<tr>
<th>Expected Relationship</th>
<th>Mechanisms</th>
<th>Airstrikes</th>
<th>Shows of Force</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>More Air Operations, Fewer Insurgent Attacks</strong></td>
<td>Decapitation</td>
<td>Credible Threat</td>
<td>Punishment</td>
</tr>
<tr>
<td></td>
<td>Attrition</td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>More Air Operations, More Insurgent Attacks</strong></td>
<td>Grievances</td>
<td>Cheap Talk</td>
<td>Reputation</td>
</tr>
<tr>
<td></td>
<td>Reputation</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

diating 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, predatory 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. 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.

31 These issues are addressed in a book-length monograph.
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 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. 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 Context

To test these competing accounts, 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,

\[32\] 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.
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. 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 sources. Declassified data from the International Security Assistance Force’s (ISAF) Combined Information Data Exchange Network (CIDNE) was incorporated for January 2006-December 2011. 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 km² 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.

33Events occurring within .5km and three hours of one another were collapsed into a single event.
Table 2: Air Operations in Afghanistan, 2006-11

<table>
<thead>
<tr>
<th>Year</th>
<th>Airstrikes</th>
<th>Shows of Force</th>
<th>Mixed</th>
<th>Total</th>
</tr>
</thead>
<tbody>
<tr>
<td>2006</td>
<td>594</td>
<td>50</td>
<td>3</td>
<td>647</td>
</tr>
<tr>
<td>2007</td>
<td>907</td>
<td>713</td>
<td>161</td>
<td>1781</td>
</tr>
<tr>
<td>2008</td>
<td>2017</td>
<td>3478</td>
<td>310</td>
<td>5805</td>
</tr>
<tr>
<td>2009</td>
<td>2000</td>
<td>3301</td>
<td>450</td>
<td>5751</td>
</tr>
<tr>
<td>2010</td>
<td>1521</td>
<td>2500</td>
<td>228</td>
<td>4249</td>
</tr>
<tr>
<td>2011(^a)</td>
<td>1815</td>
<td>2695</td>
<td>183</td>
<td>4693</td>
</tr>
<tr>
<td>Total</td>
<td>8,854</td>
<td>12,737</td>
<td>1,335</td>
<td>22,926</td>
</tr>
</tbody>
</table>

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.

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).

4 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 \textit{not} occurred?

Matching offers one possible approach. Based on the Neyman-Rubin Causal Model, matching involves the identification of counterfactual “control” observations that possess
similar, if not identical, characteristics as “treated” cases (here, villages that are bombed) but that did not receive the treatment. 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.

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 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. 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, 2km²) 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

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36 Pierskalla and Hollenbach 2013
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 while testing possible mechanisms at close range. 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 2km² to 100km²) 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. 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 using a random seeding strategy to prevent “fitting” or overusing a particular control observation.

4.1 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. Data on insurgent violence are drawn from CIDNE, which records the date and precise location 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.

37 Following Ho et al. 2007, I use a ≤.25 standardized bias score as the measure of closeness of fit for each covariate in this “best matching” approach.

38 More formally, the difference-in-difference estimator is obtained: $DD = (Y_t^T - Y_0^T) - (Y_t^C - Y_0^C)$, where $Y_x \in (0, 1)$ are the pre- and post-treatment periods, T denotes the treatment group, and C denotes the control group.

39 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.
4.2 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 hinges in turn 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 for each specified temporal and spatial window around first bombed villages and then for remaining non-bombed control villages.

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). These two variables account for the patterns of violence in and near a specified village as well as the battlefield distribution of forces.

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.

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 on that day, 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

---

40There 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.

41These include Threat Report, Suspicious Incident (Surveillance), Attack Threat, IED Threat, and SAFIRE Threat.
disproportionate share of airstrike-induced civilian casualties.  

4.3 Static Village Level Covariates  

Matching is also used to adjust for imbalances between treated and control villages 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.  

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.

5 Findings  

Three initial 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. Finally, I turn to the issue of whether drone strikes—thought to be extremely selective and precise in their targeting—yield different behavioral outcomes than conventional airpower.

5.1 Effects of Airstrikes  

Do airstrikes reduce subsequent insurgent attacks? Put simply, no. As Table 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
Table 3: Airstrike Effects Over Time

<table>
<thead>
<tr>
<th>Treatment Effect (ATE)</th>
<th>Exact Matching</th>
<th>Best Matching</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>7 day Model 1</td>
<td>45 days Model 2</td>
</tr>
<tr>
<td></td>
<td>7 day Model 4</td>
<td>45 days Model 5</td>
</tr>
<tr>
<td>Treatment</td>
<td>0.289***</td>
<td>0.683***</td>
</tr>
<tr>
<td></td>
<td>(0.023)</td>
<td>(0.103)</td>
</tr>
<tr>
<td>Constant</td>
<td>0.984***</td>
<td>2.672***</td>
</tr>
<tr>
<td></td>
<td>(0.199)</td>
<td>(0.715)</td>
</tr>
<tr>
<td>F stat</td>
<td>38.95***</td>
<td>13.15***</td>
</tr>
<tr>
<td></td>
<td>(0.11)</td>
<td>(0.05)</td>
</tr>
<tr>
<td>Treatment Coverage (%)</td>
<td>43%</td>
<td>29%</td>
</tr>
<tr>
<td>Villages (N)</td>
<td>4,600</td>
<td>3,544</td>
</tr>
<tr>
<td>Total N</td>
<td>7,670</td>
<td>5,156</td>
</tr>
</tbody>
</table>

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 ≤.2 standardized bias of one another. A 2km² 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

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 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, 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.

Some heterogeneity in the size of the effect does exist, however, across TIC and non-TIC airstrikes. Indeed, 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])

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44 Rosenbaum 2010, 85-86.
45 TICs represent 20%, 19% and 17% of the matched 7 day, 45 day, and 90 day samples, respectively.
Table 4: More Bombing, Less Insurgent Violence? The Effects of Repeated Airstrikes

<table>
<thead>
<tr>
<th>Treatment Effect (ATE)</th>
<th>Exact Matching</th>
<th>Best Matching</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>7 day</td>
<td>45 days</td>
</tr>
<tr>
<td>Treatment</td>
<td>0.204*** (0.030)</td>
<td>0.421** (0.142)</td>
</tr>
<tr>
<td>History</td>
<td>0.036** (0.012)</td>
<td>0.130† (0.071)</td>
</tr>
<tr>
<td>Constant</td>
<td>1.133*** (0.212)</td>
<td>3.272*** (0.793)</td>
</tr>
<tr>
<td>F stat</td>
<td>34.68***</td>
<td>11.61***</td>
</tr>
<tr>
<td>r²</td>
<td>0.11</td>
<td>0.06</td>
</tr>
</tbody>
</table>

| 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 ≤2 standardized bias of one another. A 2km² 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

5.2 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 attempts only backfire, multiplying grievances among civilian populations and leading to increased violence?

To tackle this question, I created History, which is the (logged) number of airstrikes a settlement 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. 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 bombing in Afghanistan pales in comparison to outlier cases such as US bombing campaigns in Vietnam or Soviet efforts in Afghanistan. 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.

5.3 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. Critics contend, however, that these airstrikes predominantly kill civilians, creating grievances that facilitate insurgent recruitment and increase militant attacks both locally and abroad.

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

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46 This attenuation is unsurprising given the high degree of correlation between the two variables in both the exact (.62) and best (.70) matching datasets.
47 Johnston and Sarhabi 2013; Byman 2013; Bergen and Rowland 2013.
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\footnote{Drones is also not significant when used as a treatment for matching across the entire airstrike dataset (see Table A1).}. 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 insurgent attacks.

6 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\footnote{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.\footnote{Since iMMAP is not privy to ISAF’s internal deliberations, these models were run without matching on the ISAF private information covariate.}

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 term (\textit{Disturbance}) that indicates whether additional airstrikes occurred within the 7, 45, or 90 day post-airstrike windows.\footnote{This disturbance term is generated dynamically for each treatment-control pair.} 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 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, additional robustness checks are 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);\footnote{These district-level variables are: a binary variable indicating whether the district borders Pakistan (\textit{Pakistan}) or Iran (\textit{Iran}); the length of paved roads in the district, as a proxy measure of relative devel-} (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.

7 Mechanisms

These empirical tests drive home the conclusion that airstrikes had counterproductive effects on insurgent violence in Afghanistan. The mechanisms underpinning this relationship remain unclear, however. It is tempting, for example, to simply ascribe these findings to civilian casualties: conventional wisdom, after all, suggests that airstrikes, no matter how precise, kill civilians, creating grievances that lead to increased insurgent recruitment and renewed cycles of escalating violence. Yet as we observe below, there is little evidence to suggest that the observed uptick in insurgent attacks stems from civilian fatalities. Instead, evidence suggests that an alternative mechanism—insurgent concerns about their local reputations for resolve—may dictate how insurgents meet the challenge posed by airpower.

7.1 The Surprising (Non-)Role of Civilian Casualties

Are grievances arising from civilian casualties the link between airstrikes and observed increases in post-strike insurgent attacks? To test this mechanism systematically, I draw 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.

As Mueller 1998, 186 notes, scholars have fixated on measuring the effects of airpower without undertaking systematic investigation of the mechanisms that produce these effects.
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 civilians with “control” airstrike 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).

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 fatalities represent an average of nearly 60% of all ISAF-inflicted fatalities over this time period. These estimates should, of course, be considered the floor, not the ceiling, of airstrike-induced fatalities. 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;

\[55\] Iacus, King and Porro 2012.

\[56\] ISAF data are unfortunately far from complete and seriously underreport civilian casualties inflicted by airstrike. The CCTC uses two categories—confirmed and unconfirmed—to generate estimates. By these standards, 71 or 132 individuals were killed by airstrike 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 airstrike 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.
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 iden-
tify which objects to code.\footnote{The bomb’s blast radius was determined by: \( R = 35 \times \frac{W^{(1/3)}}{P_{\text{over}}^{(1/3)}} \times .3048 \), 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.} 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
reflect efforts to hit insurgents as they move through forests or other terrain features.\footnote{A further 1064 airstrikes were conducted within a settlement but did not hit buildings, roads, or farms.} This more granular view of civilian harm allows us to link types of property damage to
different theories of radicalization of individuals.\footnote{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 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 the analysis 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);\footnote{Without access to classified material, this is surely an undercount, both in the numbers of decapitated leaders and failed attempts.} an indicator variable to capture whether the
airstrike was conducted by a remotely-piloted vehicle (Drone); a logged count of the num-
ber of bombs dropped (Bombs); a count variable (logged) for number of prior airstrikes

\begin{itemize}
\item \textit{Prior Attacks}
\item \textit{ISAF Ops}
\item \textit{Info}
\item \textit{TIC}
\item \textit{Population}
\item \textit{Elevation}
\item \textit{Neighbors}
\item \textit{Pashtun}
\end{itemize}
(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 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 5.

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.

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 5 reveals, the results remain unchanged: civilian casualties are unconnected to observed changes in insurgent attacks.\footnote{Reestimating with a weighted approach to Coarsened Exact Matching does not alter these findings.}
Table 5: The (Non-)Effects of Civilian Casualties: All Airstrikes and Coarsened Exact Matching (CEM)

<table>
<thead>
<tr>
<th>Covariate</th>
<th>CIVCAS only</th>
<th>All Covariates</th>
<th>CIVCAS only</th>
<th>All Covariates</th>
<th>CIVCAS only</th>
<th>All Covariates</th>
</tr>
</thead>
<tbody>
<tr>
<td>CIVCAS</td>
<td>0.172 (0.156)</td>
<td>0.176 (0.144)</td>
<td>-0.349 (0.612)</td>
<td>-0.550 (0.429)</td>
<td>-0.763 (1.189)</td>
<td>-1.276† (0.785)</td>
</tr>
<tr>
<td>Prior Attacks</td>
<td>-0.520*** (0.024)</td>
<td>-0.516*** (0.028)</td>
<td>-0.361*** (0.083)</td>
<td>-0.564*** (0.031)</td>
<td>-0.763 (1.189)</td>
<td>-1.276† (0.785)</td>
</tr>
<tr>
<td>ISAF Ops</td>
<td>0.143* (0.069)</td>
<td>0.218* (0.094)</td>
<td>-0.361*** (0.083)</td>
<td>-0.564*** (0.031)</td>
<td>-0.763 (1.189)</td>
<td>-1.276† (0.785)</td>
</tr>
<tr>
<td>TIC</td>
<td>0.826*** (0.063)</td>
<td>3.527*** (0.364)</td>
<td>6.460*** (0.574)</td>
<td>3.334*** (0.532)</td>
<td>3.334*** (0.532)</td>
<td>3.334*** (0.532)</td>
</tr>
<tr>
<td>Season</td>
<td>0.254*** (0.051)</td>
<td>2.117*** (0.299)</td>
<td>3.527*** (0.364)</td>
<td>3.334*** (0.532)</td>
<td>3.334*** (0.532)</td>
<td>3.334*** (0.532)</td>
</tr>
<tr>
<td>Compound</td>
<td>0.251*** (0.051)</td>
<td>2.117*** (0.299)</td>
<td>3.527*** (0.364)</td>
<td>3.334*** (0.532)</td>
<td>3.334*** (0.532)</td>
<td>3.334*** (0.532)</td>
</tr>
<tr>
<td>Farm</td>
<td>0.183** (0.059)</td>
<td>0.703 (0.501)</td>
<td>0.819 (0.830)</td>
<td>0.819 (0.830)</td>
<td>0.819 (0.830)</td>
<td>0.819 (0.830)</td>
</tr>
<tr>
<td>History</td>
<td>0.076*** (0.015)</td>
<td>0.411*** (0.100)</td>
<td>0.595*** (0.174)</td>
<td>0.595*** (0.174)</td>
<td>0.595*** (0.174)</td>
<td>0.595*** (0.174)</td>
</tr>
<tr>
<td>Neighbors</td>
<td>0.044* (0.02)</td>
<td>0.301*** (0.093)</td>
<td>0.908*** (0.163)</td>
<td>0.908*** (0.163)</td>
<td>0.908*** (0.163)</td>
<td>0.908*** (0.163)</td>
</tr>
<tr>
<td>Elevation</td>
<td>-0.380*** (0.097)</td>
<td>-2.205*** (0.527)</td>
<td>-4.792*** (0.783)</td>
<td>-4.792*** (0.783)</td>
<td>-4.792*** (0.783)</td>
<td>-4.792*** (0.783)</td>
</tr>
<tr>
<td>Constant</td>
<td>0.006 (0.027)</td>
<td>2.639*** (0.685)</td>
<td>-0.171 (0.131)</td>
<td>15.030*** (3.741)</td>
<td>-0.771** (0.257)</td>
<td>33.027*** (5.611)</td>
</tr>
<tr>
<td>$F$ stat</td>
<td>1.21</td>
<td>75.82***</td>
<td>0.32</td>
<td>58.48***</td>
<td>0.41</td>
<td>51.31***</td>
</tr>
<tr>
<td>$r^2$</td>
<td>0.00</td>
<td>0.27</td>
<td>0.00</td>
<td>0.29</td>
<td>0.00</td>
<td>0.34</td>
</tr>
<tr>
<td>Total N</td>
<td>8,854</td>
<td>8,854</td>
<td>8,854</td>
<td>8,854</td>
<td>8,854</td>
<td>8,854</td>
</tr>
</tbody>
</table>

CEM

<table>
<thead>
<tr>
<th>Covariate</th>
<th>CIVCAS only</th>
<th>All Covariates</th>
<th>CIVCAS only</th>
<th>All Covariates</th>
</tr>
</thead>
<tbody>
<tr>
<td>CIVCAS</td>
<td>0.203 (0.188)</td>
<td>0.199 (0.180)</td>
<td>-0.868 (0.724)</td>
<td>-1.052 (0.669)</td>
</tr>
<tr>
<td>Constant</td>
<td>0.053 (0.099)</td>
<td>1.209 (1.386)</td>
<td>1.353 (0.707)</td>
<td>23.967*** (9.125)</td>
</tr>
<tr>
<td>$F$ stat</td>
<td>1.17</td>
<td>1.42</td>
<td>1.44</td>
<td>1.44</td>
</tr>
<tr>
<td>$r^2$</td>
<td>0.00</td>
<td>0.12</td>
<td>0.00</td>
<td>0.18</td>
</tr>
<tr>
<td>Total N</td>
<td>266</td>
<td>266</td>
<td>272</td>
<td>272</td>
</tr>
</tbody>
</table>

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 radius was used to calculate pre- and post-insurgent violence. Significance levels: ***$p=<.001$, **$p=<.01$, *$p=<.05$, †$p=<.10$.
Finally, I test two interaction terms: \textit{CIVCAS*Compound}, which denotes the “most likely” instance where we might observe a link between airstrikes, grievances/revenge, and subsequent insurgent attacks; and \textit{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). \textit{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, \textit{CIVCAS*History} only reaches statistical significance in the initial 7-day time window. \textit{History}, by contrast, is consistently significant across all three models. For both interaction terms, the inclusion of \textit{CIVCAS} appears to provide little leverage in explaining insurgent attacks.

This discussion is not meant to minimize the suffering caused by airstrikes, of course. Yet it appears unlikely that the “quick fuse” logic of civilian victimization rapidly producing surges in insurgent violence is at work in this case. While the Taliban’s use of airstrikes as a recruitment device is well documented\textsuperscript{62} it appears that the link between heightened recruitment and subsequent increases in violence is not as automatic as typically assumed. At least in the short term, insurgent violence appears unconnected to civilian casualties and instead governed by tactical or other considerations\textsuperscript{63}.

7.2 Alternative Mechanism: Reputational Dynamics

While we are not privy to internal Taliban deliberations, the notion that they value their reputations for resolve at the local level finds initial support from interviews and survey data. Taliban commanders and foot soldiers alike have routinely singled out airstrikes as especially problematic for Taliban strategy, for example. These accounts stress both the fact that airstrikes are far more destructive than comparable ISAF operations and they are asymmetric in nature, with the Taliban lacking an equivalent response in the absence of their own airpower\textsuperscript{64}. Worse from the Taliban standpoint is the highly visible nature of

\textsuperscript{62}See, for example, Smith 2013; Ladbury 2009.

\textsuperscript{63}The difference-in-difference empirical strategy used here cannot rule out “slow burn” variants of this argument, where grievances from airstrikes (or other violence) accumulate and fester for many months, even years, before an individual takes action against counterinsurgent forces. Assigning causal weight to any one event becomes harder, however, as the time lag between the incident and the response lengthens.

\textsuperscript{64}Smith 2013, 205-07.
air power, which removes the ability to discount or ignore their effects, placing additional pressure on insurgents to match these actions with their own.

Taliban strategy has also evolved over time to reflect the importance of cultivating a positive image among local populations, though there are limits to such efforts. The Taliban have resorted to an extensive assassination campaign against government officials, along with those suspected of assisting the Karzai government, since 2007. Above all, there is an awareness that populations could curtail their support—or even push the Taliban from a village—if cracks developed in the facade of local Taliban control. As one villager berated a local Taliban commander after an airstrike in the village of Galoch: “Don’t be a coward and hide among civilians...if you want to fight Americans, go down south and leave us alone.”

Territorial control may therefore hinge on establishing and maintaining a reputation for resolve, particularly if this reputation forestalls defection while helping local units garner a greater share of overall insurgent resources.

This reputational mechanism suggests two behavioral indicators. First, if reputational dynamics are driving insurgent violence, then we should witness insurgent “push-back” even after non-lethal shows of force — simulated bomb runs where no ordinance is actually dropped — since these are also highly visible signals that could drive a wedge between rebels and locals. Second, this mechanism suggests that insurgent responses should be localized. That is, the causal effect of an airstrike should dissipate sharply as distance from the bombed location increases since insurgents emphasize their local standing.

7.2.1 Shows of force

Seeking to reduce civilian casualties, the US Air Force began experimenting with the use of non-lethal shows of force in 2007 to dissuade insurgents from pressing home their attacks. From a causal inference standpoint, these shows of force provide an ideal opportunity to test the competing logics of the civilian victimization and reputational mechanisms. Shows

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65For these reasons, the Taliban has routinely cast the use of airpower as a “cowardly” way of fighting since it limits the Taliban’s ability to engage directly with ISAF soldiers in “fair fights.” See Gopal 2014, 3522.
66Giustozzi 2013, 248-49.
67See, for example, United Nations Assistance Mission in Afghanistan 2011b, Malkasian 2013, 154-55, 211-12.
68Gopal 2014, 3617.
69The Taliban’s leadership pays bonuses for outstanding attacks that match or blunt ISAF’s own visible successes (i.e. an airstrike that kills Taliban commanders). See Gopal 2014, 3887.
of force were conducted in similar locations and situations as airstrikes but, as simulated bomb runs, did not impose any harm on civilians or their property. Moreover, there is also an element of quasi-randomness to the choice between airstrikes and shows of force. Though the specific guidelines governing the selection of these operations are classified, a USAF Targeting Officer emphasized the contingency inherent in this choice:

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.\footnote{USAF Targeting Officer, Air Operations Center, Bagram Airfield, Afghanistan, 5 April 2011. Email correspondence.}

I therefore repeat the dynamic matching process used above, this time using shows of force as the treatment condition. 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]) than control villages, 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
Table 6: Shows of Force: Effects by Event Time, Days, by Different Matching Procedures

<table>
<thead>
<tr>
<th>Treatment Effect (ATE)</th>
<th>7 day</th>
<th>45 days</th>
<th>90 days</th>
<th>7 day</th>
<th>45 days</th>
<th>90 days</th>
</tr>
</thead>
<tbody>
<tr>
<td>Coefficient</td>
<td>0.203***</td>
<td>0.505***</td>
<td>0.738***</td>
<td>0.241***</td>
<td>0.930***</td>
<td>1.690***</td>
</tr>
<tr>
<td></td>
<td>(0.015)</td>
<td>(0.043)</td>
<td>(0.082)</td>
<td>(0.017)</td>
<td>(0.084)</td>
<td>(0.178)</td>
</tr>
<tr>
<td>Constant</td>
<td>0.482***</td>
<td>1.216***</td>
<td>3.014***</td>
<td>0.594***</td>
<td>3.061***</td>
<td>7.509***</td>
</tr>
<tr>
<td></td>
<td>(0.119)</td>
<td>(0.325)</td>
<td>(0.611)</td>
<td>(0.131)</td>
<td>(0.487)</td>
<td>(1.132)</td>
</tr>
<tr>
<td>F stat</td>
<td>57.56***</td>
<td>27.99***</td>
<td>17.50***</td>
<td>83.56***</td>
<td>32.53***</td>
<td>27.64***</td>
</tr>
<tr>
<td>r²</td>
<td>0.11</td>
<td>0.05</td>
<td>0.05</td>
<td>0.24</td>
<td>0.08</td>
<td>0.07</td>
</tr>
<tr>
<td>Treatment Coverage (%)</td>
<td>53%</td>
<td>38%</td>
<td>32%</td>
<td>67%</td>
<td>63%</td>
<td>62%</td>
</tr>
<tr>
<td>Villages (N)</td>
<td>7377</td>
<td>6034</td>
<td>5306</td>
<td>8253</td>
<td>7801</td>
<td>7622</td>
</tr>
<tr>
<td>Total N</td>
<td>13,606</td>
<td>9742</td>
<td>8218</td>
<td>17,174</td>
<td>16,104</td>
<td>15,684</td>
</tr>
</tbody>
</table>

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 ≤ .2 standardized bias of one another. A 2km² 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
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 local audiences.

7.2.2 Do Effects Diffuse?

The proposed reputation mechanism also suggests that airstrike effects 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.\footnote{These variable windows also help mitigate any possible mis-measurement arising from inaccuracies in bomb strike location data.}

Several trends are notable (see Table 7). For example, airstrikes are associated with a net increase in insurgent attacks across all seven spatial windows. These difference-in-difference estimates are all statistically significant, though only barely so at 50 and 100 km$^2$ radii for the 120 post-strike window, 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]).
Table 7: Do Airstrike Effects Diffuse Across Space and Time?

<table>
<thead>
<tr>
<th>Distance</th>
<th>7-day</th>
<th>Temporal Windows</th>
<th>45-day</th>
<th>90-day</th>
<th>120-day</th>
</tr>
</thead>
<tbody>
<tr>
<td>2km²</td>
<td>0.371***</td>
<td>1.288***</td>
<td>2.339***</td>
<td>3.081***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.033)</td>
<td>(0.150)</td>
<td>(0.265)</td>
<td>(0.331)</td>
<td></td>
</tr>
<tr>
<td>4km²</td>
<td>0.619***</td>
<td>2.117***</td>
<td>3.951***</td>
<td>5.548***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.062)</td>
<td>(0.287)</td>
<td>(0.578)</td>
<td>(0.754)</td>
<td></td>
</tr>
<tr>
<td>6km²</td>
<td>0.750***</td>
<td>2.444***</td>
<td>5.598***</td>
<td>7.224***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.089)</td>
<td>(0.438)</td>
<td>(0.885)</td>
<td>(1.181)</td>
<td></td>
</tr>
<tr>
<td>8km²</td>
<td>0.925***</td>
<td>3.451***</td>
<td>7.713***</td>
<td>9.861***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.112)</td>
<td>(0.560)</td>
<td>(1.112)</td>
<td>(1.485)</td>
<td></td>
</tr>
<tr>
<td>10km²</td>
<td>0.985***</td>
<td>3.706***</td>
<td>7.973***</td>
<td>11.221***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.134)</td>
<td>(0.658)</td>
<td>(1.376)</td>
<td>(1.841)</td>
<td></td>
</tr>
<tr>
<td>50km²</td>
<td>1.680***</td>
<td>6.22**</td>
<td>10.86**</td>
<td>9.49‡</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.374)</td>
<td>(1.99)</td>
<td>(4.12)</td>
<td>(5.78)</td>
<td></td>
</tr>
<tr>
<td>100km²</td>
<td>2.14***</td>
<td>7.99**</td>
<td>20.55**</td>
<td>16.81†</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.509)</td>
<td>(2.74)</td>
<td>(7.85)</td>
<td>(10.14)</td>
<td></td>
</tr>
</tbody>
</table>

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, †p<.10
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 10 km$^2$ 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 when moving from a 50 km$^2$ to 100 km$^2$ radius for the 45 day time period. 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 0.371 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 represents only 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.

## Conclusion

This paper has marshaled evidence to support the claim that a robust positive relationship exists between airstrikes and insurgent attacks. While the costs of airstrike-induced civil-

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72The 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.
ian 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 sometimes positively associated with net increases in insurgent attacks, suggesting an alternative pathway by which grievances could explain insurgent violence.

These empirical patterns are better explained by appealing to the reputational demands facing local Taliban groups, however. An emphasis on reputational dynamics not only accounts for the positive association between airstrikes and insurgent violence but also explains the localized nature of responses, the insensitivity of insurgent violence to ISAF-induced civilian casualties, and the fact that non-lethal shows of force can still provoke increased insurgent attacks. In short, “face,” as Schelling famously noted, “is one of the few things worth fighting over.”

Several theoretical extensions flow from 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. Variation in reputational concerns could also be used to explain 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 violence.

On the joint methodological-empirical front, the paper’s SQL-enabled 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. Similarly, the interaction of these strategies with non-violent approaches — notably, the use of aid programs to win

\[\text{Schelling 2008, 124.} \]

\[\text{Lyall, Blair and Imai 2013.} \]

\[\text{On dynamic treatment regimes, see Blackwell 2013.} \]
“hearts and minds” could be modeled directly to enrich our understanding of the conditionality of violence. How violence and casualties 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.

Despite the substantive importance of studying the United States’ longest and most expensive war, we might wonder about the generalizability of these findings beyond Afghanistan. There are, of course, limits to any single study. Yet there are two reasons to believe that these findings about both airstrike effects and the mechanisms underpinning them have external validity. First, most modern air campaigns resemble Afghanistan in scale and scope; in this regard, the Vietnam War, often held as a key example of the futility of airpower, is an extreme outlier in terms of sorties, ordinance dropped, and civilian casualties. Air operations in Iraq (2003-11), Pakistan, and Yemen, to name only a handful of cases, are far more similar to the Afghan campaign. Second, counterinsurgency environments may represent a “most likely” case for observing reputational dynamics. Given repeated interaction between combatants, credible commitment issues, and sharp asymmetries—all factors thought to contribute to reputation-building in international relations—counterinsurgency wars are marked by opportunities and incentives to use war-fighting to bolster local reputations. The proposed reputational mechanism may therefore travel across a wide range of cases, a claim that nonetheless requires further comparative study.

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

76 Dafoe, Renshon and Huth 2014; Walter and Tingley 2011; Sechser 2010; Fearon 1995.
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 against insurgent foes.
References


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