Civilian Casualties, Humanitarian Aid, and Insurgent Violence in Civil Wars*

Jason Lyall†

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Indiscriminate violence against civilians has long been viewed as a catalyst for new rounds of violence in civil wars. Can humanitarian assistance reduce violence after civilians have been harmed? Crossnational studies are pessimistic, drawing a connection between humanitarian aid and increased civil war violence, lethality, and duration. To date, however, we have few subnational studies of wartime aid and subsequent violence. To examine this relationship, I draw on the Afghan Civilian Assistance Program (ACAP II), a USAID-funded initiative that investigated 1,061 civilian casualty incidents (2011-13). Aid was assigned as-if randomly to about half (55.8%) of these incidents, facilitating counterfactual estimation of how assistance affected Taliban attacks against the International Security Assistance Force, Afghan forces, and civilians. Challenging prior studies, ACAP was associated with an average 23% reduction in attacks against ISAF, but not Afghan forces or civilians, at the village level for up to two years after the initial incident.

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† Associate Professor of Political Science, Department of Political Science, Yale University, New Haven, CT 06520. Phone: 203–432–5264, Email: jason.lyall@yale.edu, URL: `www.jasonlyall.com`

Civilian casualties have long been considered a central driver of civil war violence (Department of the Army, 2014; Lyall, Blair and Imai, 2013; Condra and Shapiro, 2012; Kalyvas, 2006; Galula, 2006; Leites and Wolf, 1970). Morality and pragmatism have coalesced around the belief that belligerents incur costs, oftentimes steep ones, for inflicting harm on civilians indiscriminately. Counterinsurgents might find that that their careless violence has sparked new grievances among the populace, bolstering insurgent ranks while creating new rounds of revenge-seeking violence (Baicells, 2017; Petersen, 2001). Tearing families apart and destroying property can also lower the opportunity costs for participating in armed rebellion, pushing fence-sitting civilians into the arms of the insurgency (Blattman, Jamison and Sheridan, 2017; Becker, 1968). Exposure to harm might also shift civilian support behind the insurgent cause, solidifying its territorial control, while encouraging the clandestine sharing of information about counterinsurgent movements, augmenting insurgent military power (Shaver and Shapiro, 2018; Lyall, Shiraito and Imai, 2015; Berman, Shapiro and Felter, 2011). Insurgents, too, may not escape these dynamics. Civilians can punish wayward insurgents for their brutality by providing tips to counterinsurgent forces, in turn forcing them to fight harder to maintain their grip in the face of debilitating raids and airstrikes.[1]

What happens, however, when humanitarian assistance is delivered in the aftermath of a civilian casualty incident? Can the presumed link between civilian victimization and (increased) insurgent violence be dampened, if not severed completely? To date, humanitarian aid programs that focus on civilian casualties have been administered in diverse conflict settings, including Iraq, Mali, Yemen, Somalia, Syria, and Afghanistan. Yet we have few studies of their effectiveness, especially at the subnational level.[2] And while it is plausible that these programs might reduce motives and opportunities for armed rebellion, the evidence from existing crossnational studies is sobering. A near consensus now maintains that humanitarian aid increases the odds of civil war onset as well as the duration and lethality of these wars (Strandlow, Findley and Young, 2016; Wood and Sullivan, 2015; Narang, 2015; Nunn and Qian, 2014; Crost, Felter and Johnston, 2014; Fast, 2014; Nielsen et al., 2011; Polman, 2010). The combination of aid and civilian casualties may therefore

[1]Civilians with pro-insurgent sympathies might also choose to shrug off insurgent-inflicted harm or to shift blame to the counterinsurgent. See Lyall, Blair and Imai (2013).

[2]One meta-review of humanitarian aid effectiveness found only three rigorous evaluations in post-conflict settings (Puri et al., 2014). On the absence of such studies amid the broader move to evaluating development programs in conflict zones, see Zürcher (2017).

be an especially combustible one with wide-ranging implications: humanitarian assistance increased from $16.1 billion in 2012 to $27.3 billion in 2016, with seven of the ten top recipients embroiled in civil wars (Development Initiatives, 2017).

There is, then, pressing need to examine the subnational effectiveness of wartime humanitarian aid. To do so, I draw on the Afghan Civilian Assistance Program II (ACAP II), a $64 million USAID-funded initiative tasked with providing immediate assistance to civilians harmed by the International Security Assistance Force (ISAF) or Taliban in Afghanistan. The program investigated 1,061 civilian casualty incidents from 2011 to 2013. While humanitarian aid programs can be difficult to evaluate in wartime settings, ACAP II had two features that facilitate causal inference. First, all victims were civilians harmed accidentally by ISAF or the Taliban. The program took strict precautions to exclude actions that deliberately targeted civilians, helping lessen concerns about selection effects. Second, ACAP II aid, which consisted of food and household items, was administered through a bureaucratic process that approximated as-if randomization. By virtue of an unwieldy authorization process, ACAP II was only allowed to respond to just over half (55.8%) of the original 1,061 incidents it investigated. The remaining incidents, which resemble the authorized ones closely across 70 different covariates, were abandoned, receiving no aid despite confirmation of civilian harm and property destruction. These abandoned incidents provide counterfactuals for estimating how aid affects Taliban attacks against ISAF, Afghan National Defense and Security Forces (ANDSF), and civilians.

Unlike existing crossnational studies, I find that ACAP II aid is associated with a marked 23% reduction in Taliban attacks against ISAF for up to two years after the initial civilian casualty incident. Substantively, this reduction translates into nearly 12,000 "missing" Taliban attacks in the two years following aid delivery compared with villages that received no assistance. This is all the more remarkable given the modest value of aid provided, typically about US$195 per beneficiary. Taliban violence against ANDSF units was unchanged by aid delivery, however. More encouragingly, ACAP II aid did not provoke increased Taliban targeting of civilians, a notable concern for organizations programming in conflict zones (Crost, Felter and Johnston, 2014). ACAP II aid also proved most effective in locations close to ISAF military bases (within $4km^2$) and when responding to events with only moderate numbers of civilian casualties or property damage. Notably, spending more per beneficiary did not improve ACAP II's performance. ISAF-inflicted casualties represented the most difficult programming environment; much of ACAP II's inroads against Taliban violence lose momentum in the aftermath of these events. Together, these findings suggest several modifications to existing theories of insurgent violence and the delivery of wartime humanitarian assistance.

1 Humanitarian Aid and Violence

Three theoretical positions can be gleaned from the small literature addressing humanitarian aid and insurgent violence in civil wars.

First, humanitarian assistance can increase post-incident insurgent attacks relative to locations that experienced civilian casualties but received no aid. Such assistance has, in other words, *violence-increasing* properties. Why this result obtains remains contested, however. Several non-exclusive mechanisms are likely at work. Insurgents might attack aid sites or workers in an attempt to forestall aid delivery, contributing to a net increase in violence. Aid such as food and medical supplies might become a prize for insurgents to capture, especially if programming is occurring in contested or insurgent-controlled areas.[3]

Modest aid programs also risk further enflaming grievances if material assistance is deemed insufficient for perceived needs or, more generally, if the aid is poorly-designed or -delivered. Anger, too, might accompany foreign-funded aid programs if they underscore the inability of the host government to render assistance to its own citizens, further delegitimizing it. In this situation, humanitarian aid might encourage individuals to take up arms and carry out attacks on their own or to join already existing insurgent organizations. Bitter individuals might also share information with insurgents about government forces, facilitating increased insurgent attacks. Finally, there are second-order consequences to consider. Aggrieved citizens hurt by insurgent violence might share tips with the government that lead to diminished insurgent attacks in the near term. Yet reputational demands might lead insurgents to step up their attacks to demonstrate their continued control of a given area, swamping the initial decrease in attacks.[4]

Second, it is plausible that humanitarian assistance might have *violence-reducing* properties. Timely aid might increase the opportunity costs associated with armed rebellion

[3]For example, Sexton (2016) found that cash disbursements increased insurgent violence in contested areas but decreased it in government-controlled areas. See also Special Inspector General for Afghanistan Reconstruction (2018, 179-183).

[4]In some cases, indiscriminate repression may work to reduce insurgent violence in the absence of aid. See Lyall (2009).

if it manages to restore individuals (and families) to pre-incident levels of income and employment. This is especially the case if aid is targeted at repairing or replacing income-generating property (especially farms). Public acknowledgement of the harm inflicted by the counterinsurgent might also reduce grievances, preventing insurgents from capitalizing on casualties as a recruitment tool. Gratitude for material and symbolic restitution might further increase victims' willingness to provide actionable intelligence to counterinsurgents after being harmed by insurgent actions. Aid might, in other words, grease the skids of information sharing, helping counterinsurgents reduce insurgent capacity for violence via raids and other direct action. Insurgents themselves may be sensitive to public opinion, and so might reduce their attacks after killing civilians if assistance is being used to "spotlight" insurgent brutality.

Finally, aid may have *no net effect* on subsequent insurgent violence. The violence-increasing and -reducing properties of aid may both be present, canceling each other out. Harmed individuals may simply pocket the proffered aid while leaving their underlying political preferences and associated behavior unaltered.[5] Aid's effects may also be conditioned by individual-level traits: some harmed civilians might be assuaged by the humanitarian aid while others angered, generating countervailing pressures. One-time aid flows may also have only short-term effects; over time, violence may return to its pre-incident equilibrium in a given area.[6] Aid programs might also simply be irrelevant in these context, taking a backseat to intra-village and familial support networks.

2 Context

The protracted counterinsurgency campaign in Afghanistan has been deadly for civilians. By one estimate, some 28,291 civilians were killed, and another 52,366 were injured, during 2009-17 (United Nations Assistance Mission Afghanistan, 2018, 1). The ACAP II program arose out of a desire to alleviate, if only partially, the suffering caused by the accidental harm of civilians and their property during combat between ISAF and Taliban forces. It was funded by USAID and implemented by International Relief and Development (IRD)

[5]Masterson and Lehmann (2018) finds little evidence that cash transfers increased mobilization for armed rebellion among Syrian refugees in Lebanon, for example.

[6]Information-centric ("hearts and minds") approaches are silent on whether civilians provide a single tip or a continual flow after experiencing harm, making it difficult to render predictions about overall patterns of violence.

in partnership with the Afghan Ministry of Labor, Social Affairs, Martyrs, and Disabled (MoLSAMD) and local Afghan NGOs. Over the course of 2011-15, ACAP II administered immediate assistance to 41,141 individuals in 7,444 families across 29 of Afghanistan's 34 provinces. Total aid disbursed reached $52.4 million (Special Inspector General for Afghanistan Reconstruction, 2018, 218). Civilian casualty incidents were identified and verified by ACAP II's own extensive monitoring system, which included local police, non-governmental organizations, journalists, district and provincial authorities, and USAID's own On-Site Monitors (OSMs) posted to major ISAF bases. For the period under consideration here, ACAP II investigated 1,061 incidents from 7 October 2011 to 14 September 2013 for possible humanitarian assistance.

ACAP II implemented a strict protocol for determining eligibility for assistance. An incident was deemed eligible for possible assistance if it met two of three criteria: (1) the harmed parties were civilians; (2) these individuals were harmed as a direct result of ISAF's actions; or (3) these individuals were harmed by the Taliban as a result of ISAF's presence in a given area. Reproduced in Section S1, ACAP II's mandate was designed to be "blame blind" in nature: "ACAP II provides assistance regardless of who is at fault, if the loss was incurred due to U.S. and Coalition Forces targeting the Taliban and other insurgent groups involved in the armed conflict or due to the Taliban and other insurgent groups targeting U.S. and Coalition Forces; however, civilians harmed by Afghan National Security Forces or solely by the Taliban and insurgents without the presence of U.S. and Coalition Forces will not be Approved for ACAP II assistance."

Examples of eligible ISAF-initiated events include: airstrikes, including accidental weapons releases; military operations and night raids that accidentally killed civilians; road accidents; and escalation of force (EOF) incidents where civilians failed to heed ISAF soldiers' traffic instructions. Taliban-initiated events include: accidental deaths arising from suicide bombings that targeted ISAF convoys and bases but missed (or failed to breach the walls); improvised explosive devices (IEDs) that failed to detonate against their intended target and instead harmed civilian passersby; Taliban offensives within villages that accidentally hurt civilians; and errant rocket attacks against ISAF bases and patrols. Table S1 summarizes these incidents by frequency and responsible party.

When authorized to provide aid (see below), ACAP II distributed immediate assistance in the form of food and household supplies, including building materials to repair damaged compounds. Cash transfers were deliberately prohibited. A total of $11,147,910 in assistance was delivered during the 2011-13 study period. An estimated 30,304 individuals from 5,488 families received assistance during 2011-13. Mean assistance was $195 per beneficiary, with beneficiaries rarely receiving more than $400. Immediate assistance was standardized in kind and value across all recipients in a given village.[7]

Aid distributions ranged from a single beneficiary who received $85 after a traffic collision to a massive $1.79 million aid package for 4,472 beneficiaries after a 23 November 2012 truck bomb that killed three, injured 120 and damaged an estimated 30 vehicles, 800 shops, 200 houses and 15 public properties. On average, a village received $10,507 worth of in-kind assistance. Aid distribution was a one-time affair, with all harmed individuals (or their representatives) gathering in a central location to collect their assistance once notified of its delivery, typically two weeks to two months after an incident. Biometric data was collected at these sites to confirm beneficiary identities, helping prevent aid diversion as well as ensuring the strict definition of a "civilian" was upheld.

3 Empirical Strategy

Humanitarian aid programs are especially difficult to evaluate in conflict settings. Potential selection effects abound: aid organizations may be barred from the most dangerous areas or, conversely, only choose to operate in them. The sudden and unpredictable nature of civilian casualty incidents can eliminate the possibility of collecting baseline data, while counterfactual observations may be difficult to identify. Randomized control trials, the gold standard of impact evaluations, are typically ruled out given ethical concerns arising from the need to withhold aid (even temporarily) from harmed populations to create control observations (Stoddard et al., 2017; Puri et al., 2014, iv-v). As one possible solution, I exploit as-if random variation surrounding the authorization of ACAP II aid delivery. Owing to bureaucratic obstacles inherent to the authorization process, nearly half of all civilian casualty events investigated by ACAP II were unfortunately abandoned by IRD, receiving no assistance despite verified civilian casualties and property damage.

Indeed, while ACAP II was responsible for identifying and investigating civilian casualty incidents, USAID mandated that ISAF's own Civilian Casualty Mitigation Team (CCMT) also confirm that ISAF units were involved in the incident before aid could be released. The

[7]Some individuals were also provided an supplemental form of tailored assistance — references to psychiatric counseling, medical assistance, and vocational training — in cases of extreme need.

CCMT lacked the capacity to conduct its own investigations, however, and a bottleneck in the approval process quickly developed. Unable to keep pace, CCMT began haphazardly issuing confirmations, resulting in the arbitrary neglect of many incidents. As a result, only 592 of the 1,061 incidents ACAP II verified (55.8%) were actually approved for assistance. The remaining cases never received a reply from ISAF, forcing IRD to abandon them. It bears emphasizing that these incidents met all the requirements for initiating assistance — all had substantial documentation from multiple eyewitnesses and agencies — and lacked only final confirmation from the CCMT to initiate programming.[8]

Claims of "as-if" randomization of assignment to treatment demand a high standard of evidence. This is especially true when a sharp discontinuity is not produced by an *ex ante* official policy threshold. Fortunately, we can confirm the plausibility of as-if random assignment in several ways.

First, interviews with stakeholders in each of these organizations, as well as direct participant observation, reveal the broken nature of the authorization process. A near consensus among IRD, USAID, CCMC, and ACAP II personnel existed around the belief that the process was "haphazard" and "chaotic." Officials cited various reasons for this state of affairs, including low bureaucratic capacity, frequent CCMT personnel turnover, and changing personal dynamics that conspired to delay authorizations. The pace of ACAP II's caseload also contributed: it submitted 1.36 requests *daily* to the CCMT for 26 months. USAID's own review of ACAP II noted that "the slow USAID incident verification process and a lack of coordination with ISAF challenged ACAP II through its first two years of programming" (Management Systems International, 2015, 21). It concluded that "ISAF was not a reliable partner for USAID from the start of the ACAP II program (p.23)." CIVIC, a non-governmental organization tasked with an external audit of the CCMT, similarly concluded that its "severely limited" capabilities could not keep pace with incident approval demands (Keene, 2014). Tellingly, USAID ultimately junked its partnership with ISAF in late-2013, asking the United Nations Assistance Mission to Afghanistan (UNAMA) to verify incidents instead to prevent so much need from going unmet.

The immediate concern, of course, is that the CCMT, or perhaps ISAF more generally, selectively authorized approval of certain incidents according to an unknown selection criteria. Perhaps the most lethal ISAF-initiated incidents were buried to avoid calling

[8]This discussion is based on interviews with USAID, IRD, and CCMT officials in Kabul as well as subsequent correspondence.

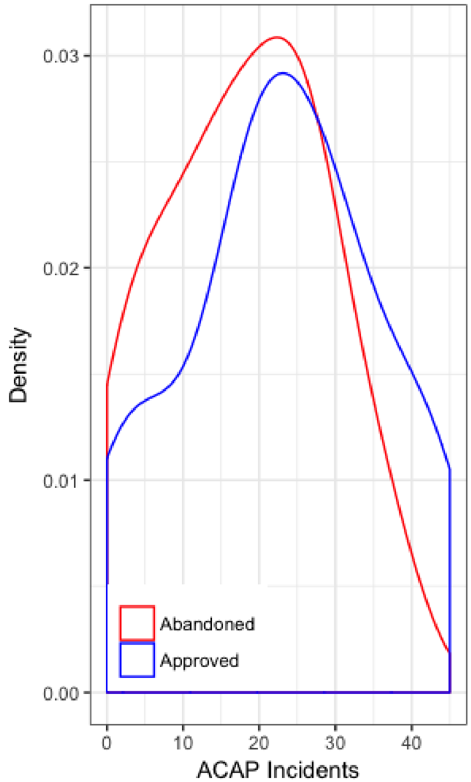
attention to ISAF's culpability, especially in light of the politically sensitive nature of civilian casualties. Alternatively, perhaps ISAF was prodded to grant aid only after public outcry, and so approval should be tied to the most destructive incidents. ISAF may also have intervened in the authorization process to ensure that all Taliban-initiated events were granted aid as a means of spotlighting Taliban cruelty for political gain.

None of these fears about selection into authorization appear warranted, however. I test balance across both incidents and villages by regressing a host of incident-level, spatial, and village-level covariates on approval status. In total, I draw on 70 covariates; only two have a difference at $p<0.05$. This compares favorably with the balance obtained in some randomized control trials (Blattman, Jamison and Sheridan, 2017, 1174), though the design cannot, by definition, ensure balance across unobserved covariates.

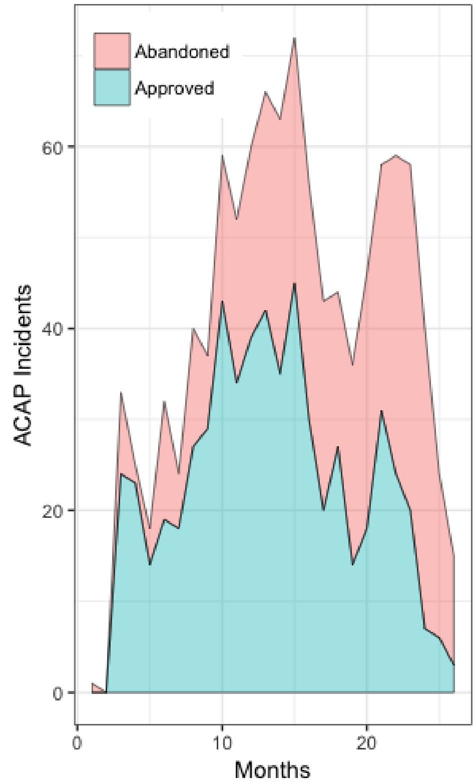
For example, the decision to approve certain incidents does not hinge on the most important (and obvious) properties of the incident itself (see Table S2). The belligerent responsible, the number of civilians killed and wounded, property damage, and the cumulative damage (casualties and property) suffered by a village all fail to predict aid assignment. Abandoned incidents had an average of 1.72 individuals killed and 2.65 wounded; approved ones had 1.5 and 3.38, respectively. Property damage occurred in 41% of abandoned incidents and 45% of approved ones. Nor was assignment sensitive to the type of incident (e.g. airstrikes, suicide bombings). Of the ten different categories of civilian casualty events, only one, "crossfires," was different at $p<0.05$.

Designed as a national program, approved and abandoned incidents were distributed similarly across Afghanistan. Programming was especially dense in eastern Afghanistan, the site of heavy fighting in 2011-13. I plot the location of all incidents in Figure S6. In many cases, approved and abandoned villages were neighbors. Moreover, 104 of the 607 villages in the sample (17%) experienced both approved and abandoned incidents, an odd pattern if ISAF was restricting aid to certain villages. I also test balance across the five Regional Commands (RCs), the top five provinces for ACAP II incidents, and a dummy variable for sharing a border with Pakistan to examine whether ISAF was cherrypicking certain regions based on strategic concerns (see Table S3). Once again, only one of these 11 covariates, the eastern province of Khost, is different at $p<0.05$.

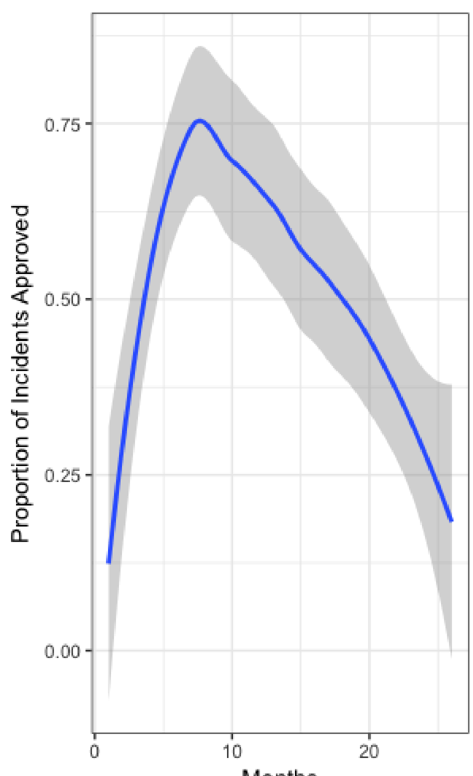
Finally, there is excellent balance across 46 village-level covariates (Table S4). Key demographic traits, including population size, dominant language spoken (a proxy for ethnicity), and a dummy Pashto indicator, are well balanced. So, too, are important spatial characteristics of these villages, including their elevation, distance from the district center, and number of neighboring villages within a 5km^2 radius. Prior aid from the National Solidarity Program (NSP), as measured by spending per capita, the number of NSP projects, and the number of beneficiary families, is also balanced. The number of ISAF and ANDSF military installations within 3km^2, 5km^2, and 10km^2 radii, as well as distance to the nearest base (in kilometers), is also similar across approved and abandoned villages. Perhaps most importantly, these villages share similar pre-incident levels of Taliban attacks against ISAF, ANDSF, and civilian targets across four different time periods: 7-day, 90-day, 180-day, and 365-day before the civilian casualty event. These similarities hold whether we use all Taliban attacks against these targets or use a subset that include only improvised explosive devices (IEDs). I also include a measure from CIDNE that tracks ISAF's own military activities in and around these villages for the same time periods. None of these 46 covariates is different at $p<0.05$; one is significant at $p<0.10$.



(a) Density Plot by Incident Status



(b) Stacked Area Plot of Approved, Abandoned, and Total Incidents



(c) Proportion of Incidents Approved

Figure 1: Distribution of ACAP II Incidents Over Time, 2011-13.

No design is without limitations, of course. One threat to inference here lies in the possibility that ISAF's authorization process created an temporal imbalance between approved and abandoned incidents. If all early incidents were approved, and all later ones abandoned, then the absence of overlap between the two types of incidents weakens the case for using abandoned incidents as counterfactuals. How concerned should we be?

Panel A in Figure 1 plots the monthly density of approved and abandoned incidents. While the overlap is not perfect, a two-sample Kolmogorov-Smirnov test for equality of distribution functions confirms that these population densities are not unequal.[9] Panel B in Figure 1 examines the monthly frequency of incidents using a stacked area plot. These trends do closely approximate one another. Crucially, both types of incidents are represented across each month for the entire program; it is not the case that each certain incidents are confined to specific months. Finally, I plot the proportion of events approved monthly in Panel C in Figure 1. This smoothed plot confirms the general pattern revealed in the interviews; the CCMT was quickly swamped by ACAP II authorization requests, falling behind as early as eight months into programming. Two points bear emphasizing. First, while the proportion of incidents approved falls over time, it never reaches zero in any month, indicating that events were still approved even in ACAP II's final stages. Second, the mean approval rate across these 26 months was 0.51. While monthly variance does occur, the overall approval rate is remarkably close to even odds over ACAP II's programming cycle. While a strong case can be made for the plausibility of abandoned incidents as counterfactuals, I nonetheless estimate all regressions using quarterly fixed effects to adjust for temporal lumpiness induced by the approval process.

4 Estimation Strategy

I adopt a Neyman-Rubin counterfactual framework that compares approved ("treated") villages with abandoned ("control") villages to generate difference-in-difference estimates of changes in Taliban violence (Rubin, 2006). Formally, for each village and for each time period I estimate:

$$\text{DD} = (Y_1^t - Y_0^t) — (Y_1^c - Y_0^c)$$

where $Y_x \in (0, 1)$ are pre- and post-treatment periods,

[9] D=0.2308, p=0.493.

T denotes the treatment group (approved aid), and
C denotes the control group (abandoned aid)

I use a purpose-built SQL program to calculate the number of Taliban attacks before and after a series of preset temporal windows ranging from seven days to two years before and after the date of the civilian casualty incident (for controls) or aid distribution (for treated villages). Spatially, all attacks within a 2km^2 for each treated/control village are counted.[10] All regressions include relevant covariates and control for time trends using quarterly fixed effects. This disaggregated empirical strategy maximizes the advantages of microlevel data in two ways. Instead of anchoring its analysis in subnational units like provinces or districts, it focuses on the appropriate unit of analysis, namely, the village that was exposed to indiscriminate violence and that (possibly) received assistance. It also introduces flexibility into its treatment of time, moving away from fixed and coarse annual trends and instead exploring variation over meaningful periods whose start dates are dictated by the incident itself.

5 Data

I draw on two event datasets to track changes in Taliban violence over time. First, I use declassified data from ISAF's own Combined Information Data Network Exchange (CIDNE), which records the date, location, and type of insurgent attack against ISAF forces and installations. These data permit highly disaggregated study of Taliban violence; most incidents are assigned geographic coordinates down to one meter resolution. There are 431,774 recorded incidents against ISAF forces (including 36,891 improvised explosive detonations) from 1 January 2008 to 1 January 2015.[11] Attacks against Afghan forces and civilians are excluded from CIDNE, however. As a result, I draw on a second dataset, iMMAP, that was compiled by international and local NGOs, media, and foreign embassies to record attacks against Afghan forces and civilians. iMMAP recorded 31,600 Taliban attacks against ANDSF targets from 1 January 2008 to 30 April 2014 and a further 14,117 attacks against civilians.[12] These datasets are not exhaustive of every Taliban attack, a

[10] For cities with $\geq$500,000 inhabitants, a 5km^2 radius is used to reflect their larger urban sprawl.
[11] For additional details on CIDNE, see Lyall, Blair and Imai 2013.
[12] To minimize treatment bias and right-censoring due to CIDNE and iMMAP time limitations, I calculate only two year and one year post-incident (aid delivery) windows for CIDNE and iMMAP, respectively. All right-censored observations are dropped from the analyses reported below.

clear impossibility given wartime conditions. They do, however, represent the best collection efforts to date by separate monitors to verify and catalogue Taliban violence across different but complementary targets and victims.

Village-level data are pooled from a variety of sources, including USAID, ISAF, and various Afghan government ministries. Population size, the village's elevation (in meters), and the dominant language spoken are included, as well as a binary indicator for Pashto-speaking villages. Spatial factors are also addressed, including the village's distance to its district capital (a measure of ruralness and difficulty of access), the length of paved roads in village's district, and the number of villages within a 5km^2 radius. I also include distance (in kilometers) to the nearest ISAF or ANDSF military installation, including forward operating bases and combat outposts, to take into account dynamics of contestation as well as the availability of military targets for the Taliban to strike. To capture the density of ISAF's presence, I generated counts for the number of military installations within various bands around each village (3km^2, 5km^2 and 10km^2). Since ACAP II was not operating in isolation, I collected data on the prior distribution of National Solidarity Program (NSP) grants to these villages, including the number of projects, spending per capita, and total number of beneficiary families. The NSP is Afghanistan's largest aid program, typically distributing block grants of around $60,000 to selected villages for investment in infrastructure and other labor-intensive activities that might boost social cohesion and resilience (Beath, Christia and Enikolopov, 2011). Finally, all models include a binary indicator for Afghanistan's so-called "fighting season" (April to September) as well as quarterly fixed effects to adjust for temporal trends in wartime violence.

6 Findings

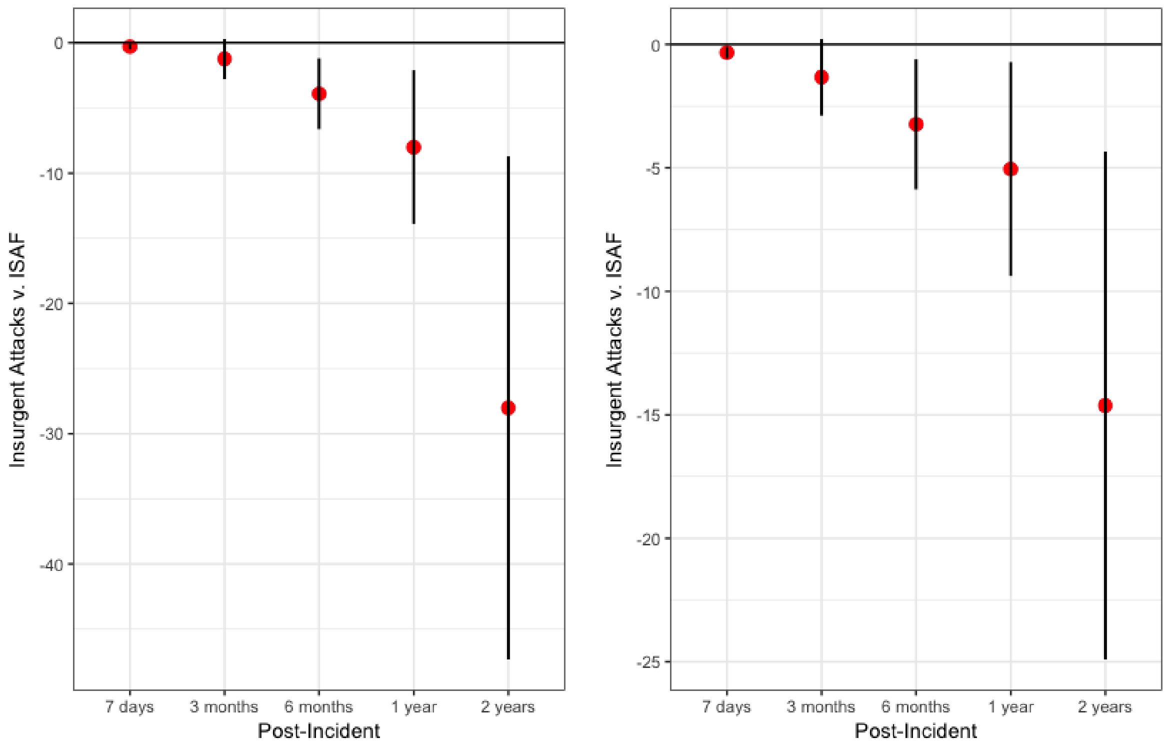
I begin by examining the association between ACAP II and Taliban attacks against ISAF in the days, months, and years following aid delivery. I then repeat the analysis for ANDSF personnel and civilian victims. In the following section, I extend these initial analyses to consider the relationship between the amount of ACAP II aid and subsequent Taliban violence, how distance to military bases conditions these effects, and whether aid remains effective when responding to incidents that involve civilian fatalities.

6.1 Insurgent Attacks Against ISAF

Is ACAP II assistance associated with changes in Taliban attacks? As illustrated in Panel A of Figure 2, the frequency of Taliban attacks drops markedly for up to two years after a village receives humanitarian assistance (see also Table S5). Just seven days after aid delivery, Taliban violence has been reduced by 0.289 attacks (with a 95% confidence interval at -0.495, -0.083). In substantive terms, this represents a 35% reduction in the mean number of attacks relative to the baseline seven days before the incident (with 95% CI at -60%,-10%). At the 90 day mark, Taliban attacks have decreased an average of 1.238 attacks (95% CI at -2.772, 0.297), representing an 11% decrease (95% CI at -25.6%, 2.6%) in the mean number of attacks over the identical pre-incident baseline.

Over time, we observe even sharper reductions in Taliban attacks. At the 180 day post-incident mark, Taliban violence has fallen by a mean 3.910 attacks (95% CI at -6.64, -1.18), representing an 18% reduction in attacks relative to the same pre-incident period (95% CI at -30.7%, -6.1%). We observe a further 8.014 reduction in mean Taliban attacks at the one year post-aid delivery benchmark (with 95% CI at -13.91, -2.12), about 19% of pre-incident Taliban violence (or -33.3%, -5.1%). These findings can be stretched to the two year post-aid delivery mark, though with some hesitation since 380 observations are right-censored due to the absence of sufficient CIDNE data to complete the full two-year time frame. Villages that received ACAP II aid are still associated with a mean reduction of 28.01 attacks (-47.33, -8.70). This represents a sizable 30.7% reduction in the mean of pre-aid delivery means (-52%, -9.5%). Two year estimates probably represent the outer edge of feasible estimation since the parallel trends assumption underpinning DD estimation likely becomes increasingly untenable as time-from-aid lengthens.

Taken together, we observe an average 23% reduction in the mean number of Taliban attacks against ISAF in approved villages across the first two years post-aid delivery (or -40.2%,-8.76%). This decrease is substantively meaningful even in the early days after ACAP II aid disbursement. For example, approved villages were collectively the site of 465 Taliban attacks in the week preceding aid delivery. The treatment effect of ACAP II aid is thus equivalent to 107 "missing" attacks in the week following its delivery (or 187, 41). The number of missing attacks scales up quickly as time elapses. Approved villages cumulatively totaled 52,099 Taliban attacks in the two years preceding aid delivery. Our estimate of ACAP II's effect would result in 11,983 "missing" attacks in the two years following aid disbursement (or 20,944 to 4,564). Even allowing for possible double-counting of attacks given repeated civilian casualty events in the same location within this two year window, ACAP II's effect is substantively large.



(a) Full Sample (N=1,061) (b) Rural Sample (N=1,004)

Figure 2: DD estimates of ACAP II effects on Taliban attacks v. ISAF over time

We might worry that these findings are driven by large cities (especially Kabul) that have been focal points for armed conflict. I therefore reestimated these models while excluding all locations with ≥50,000 inhabitants. Yet, as illustrated in Panel B of Figure 2, the same pattern holds. Taliban violence has already dropped by a mean 0.327 attacks only seven days after aid disbursement (95% CI at -0.558, -0.097). This represents a large 44% decrease from the pre-aid mean number of attacks (95% CI at -75.5%, -13.1%). By the 90 day post-aid mark, Taliban attacks have fallen by 1.328 attacks (95% CI at -2.878, 0.221), a 13% decrease from the preceding 90 days (95% CI at -28.5%, 2.2%). Six months after aid disbursement, Taliban violence continues to decrease, with a mean 3.234 reduction in attacks recorded (95% CI at -5.871, -0.597). This amounts to a 16% decrease from the pre-aid baseline (95% CI at -29.9%, -3%). A further 5.049 drop in mean Taliban attacks

(95% CI at -9.377, -0.721) is noted at the one year mark, a 13% decrease (95% CI at -25%, -1%) compared to baseline. Finally, at the extreme edge of the sample, we observe a 14.626 reduction in mean Taliban attacks two years after aid disbursement (95% CI at -24.912, -4.340), a 16% decrease (95% CI at -29.9%, -3%). Overall, ACAP II aid is associated with an average 20.8% decrease in Taliban violence across these five measurement periods (95% CI at -38%, -3.6%), a decrease comparable to that obtained in the full sample. Substantively, 371 attacks were recorded in the cumulative seven day window prior to aid disbursement. Applying the average reduction of attacks yields an estimate of about 77 "missing" attacks in the seven days following disbursement (95% CI at 141, 13). Similarly, there were 42,533 attacks in the cumulative two year pre-aid disbursement window for Approved villages, suggesting 8,847 "missing" attacks due to ACAP II aid (95% CI at 16,162 to 1,531). These rural-only findings underscore the violence of the operating environment; cities are not solely driving the observed reductions in attacks.

These findings survive multiple robustness checks. *Approved* remains statistically significant and associated with reduced post-aid Taliban attacks when all other covariates are dropped from the models, for example (Table S6). Subsetting Taliban violence to include only successful IED detonations does weaken the relationship somewhat but the general pattern remains. Post-aid IEDs are reduced by 0.045 in the first seven days after aid disbursement (95% CI at -0.087, -0.002), halving the number of observed IED detonations for this time period (95% CI at -96%, -2%). This reduction can still be seen at the two year mark, with mean IED attacks reduced by 1.454 detonations (95% CI at -2.695, -0.212), or about 17% of overall IED detonations (95% CI at -31%, -2%). Though still negative, the relationship fades in the intervening time periods, failing to reach conventional levels of statistical significance. These temporal inconsistencies, along with the modest reduction in the mean number of IED detonations, suggests that ACAP II may be insufficient to influence two aspects of IED attacks: the need for popular support to facilitate IED emplacement and the willingness of locals to withhold timely information from the counterinsurgent about Taliban intentions and activities, a point I return to below (Table S9). Finally, I conducted a placebo test by randomly reassigning new aid disbursement dates (see Table S10).. As expected, *Approved* is no longer associated with a statistically significant difference in Taliban violence after (pseudo-)aid delivery in any time period.

Additional Data: Attacks Against ANDSF and Civilians

To extend these findings, I draw on iMMAP data recording Taliban attacks against ANDSF and civilians for up to year after aid disbursement. iMMAP data has three desirable properties. First, CIDNE data simply omits these attacks, leaving us blind to possible changes in Taliban violence against these targets. Second, since ACAP II specifically excluded events that involved ANDSF units, we can use iMMAP data as a falsification check. That is, we can use a non-equivalent dependent variable that should not respond to the ACAP II intervention to test claims that the ACAP II intervention is shifting popular attitudes or material circumstances among harmed individuals. Empirically, we should observe no difference between approved and abandoned villages in terms of anti-ANDSF violence since these units were excluded from ACAP II. Third, civilians (and villages) that received ACAP II assistance may be singled out for Taliban punishment. Taliban commanders may use violence to forestall their loss of control over these villages, using ACAP II aid as a signal that a village is shifting into the pro-government camp (Hirose, Imai and Lyall, 2017; Crost, Felter and Johnston, 2014). I therefore need to test for the possibility that ACAP II assistance merely shifted the burden of Taliban violence from ISAF onto the shoulders of civilians.

As Panels A and B in Figure 3 demonstrate, violence against ANDSF is unaffected by ACAP II aid. Estimates are inconsistent across each of the temporal windows for up to a year after aid disbursement (see Table S7). Aid also appears unconnected with patterns of violence against civilians. As Panels C and D illustrate, the relationship between aid and subsequent Taliban violence against civilians is negative for each time period, though it never reaches statistical significance in any time period (see Table S8). In each case, the results are robust to the exclusion of cities with $\geq$50,000 citizens.

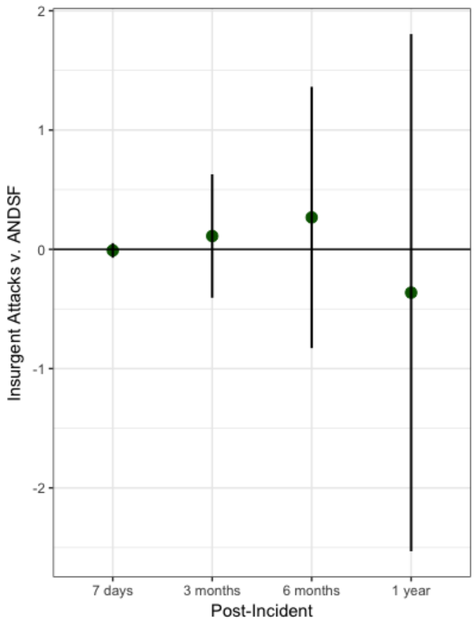
These non-findings are informative for several reasons. They suggest that the effects of aid may hinge partly in the symbolic value of the perpetrator's public acknowledgement of civilian harm; uninvolved parties may not benefit from humanitarian assistance without this visible linking of blame and restitution. It also appears possible to program in violent environments without necessarily increasing the risk to civilian beneficiaries. Aid, particularly in small quantities, non-monetary in nature, and highly tailored to specific needs, may not generate sufficient incentives for insurgents to expend efforts to disrupt or capture these resources. In fact, IRD never recorded a single attempt by Taliban or other actors to derail aid distribution, either through threats or actual violence, despite programming deep in Taliban-controlled and -contested areas and despite Taliban proclivity for targeting aid workers (Narang and Stanton, 2017).

7 Discussion

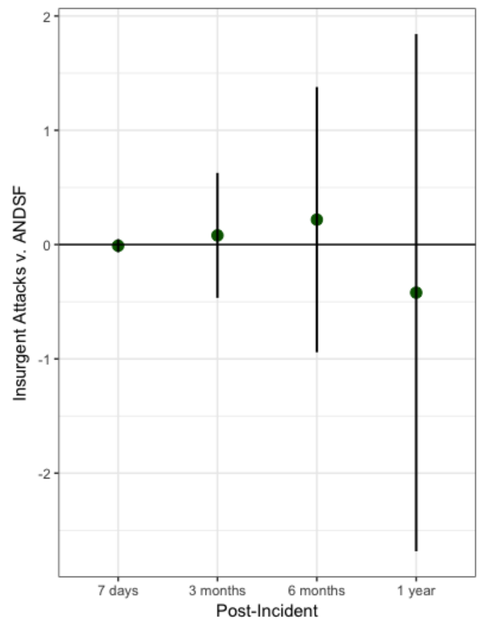
These initial findings suggest several natural extensions. I address four here.

First, the empirical strategy used here is pitched at the village, rather than individual, level. It is possible that this approach does not capture all of ACAP II's effects on Taliban violence if significant spillover between villages is present. Perhaps non-residents are harmed in an incident and then return to their home villages newly-aggrieved, ineligible for aid, and invisible to our data collection strategy since their subsequent attacks would occur outside the radius of the original location. Such concerns are misplaced, however. The small-scale and tailored nature of ACAP II assistance, along the absence of cash transfers, meant that very little aid could be physically redistributed outside the Approved village. In addition, a random sample of 2,038 aid beneficiaries from 268 of 592 approved incidents found that only 95 individuals were non-residents of the harmed village (Lyall, 2018). Low inter-village mobility, compounded by wartime travel disruptions, constrained ACAP II's reach to the immediate vicinity of the incident itself.

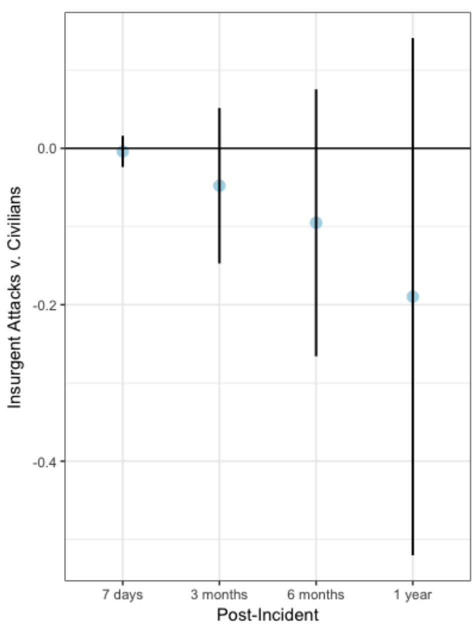
Second, it remains an open question whether these reductions in Taliban violence are sensitive to the size of the ACAP II disbursement. I therefore reestimate *Approved* as a continuous variable using the value of the aid distributed in a specific village (in $, logged), with abandoned villages assigned $0. As Table S11 reports, the main relationship observed above continues to hold, an important robustness check. Yet nearly all of ACAP II's leverage on Taliban violence is obtained when moving from no aid to some aid. Indeed, Taliban violence appears insensitive to the actual amount of spending allocated, whether measured as a function of the overall aid expenditure in each village (Table S12) or as aid per capita of a given incident location (Table S13) when we restrict our focus to Approved villages only. In fact, in nearly every model, more aid is correlated with increased insurgent attacks against ISAF, albeit rarely at statistically significant levels. This somewhat counterintuitive result stems from the fact aid disbursements track with an incident's destructiveness; the greater the harm a village suffers, the larger the disbursement, but with ACAP II



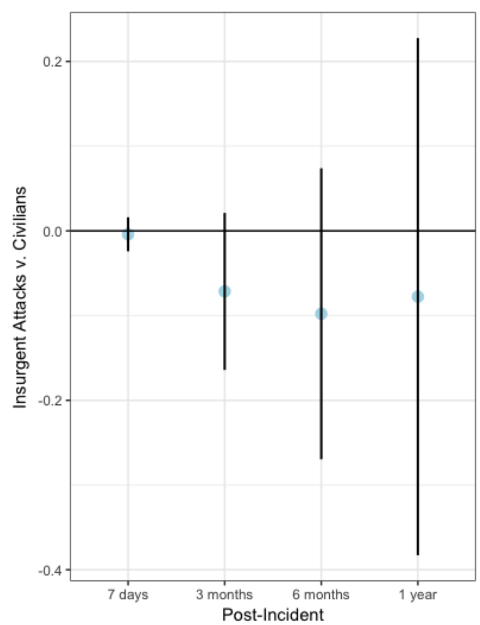
(a) Full Sample (N=1,061)



(b) Rural Sample (N=1,004)



(c) Full Sample (N=1,061)



(d) Rural Sample (N=1,004)

Figure 3: DD estimates of ACAP II effects on Taliban attacks v. ANDSF and Civilians

immediate assistance capped at about $400 per beneficiary, it is likely that such limited aid becomes less effective at higher levels of destructiveness.[13]

We can observe how ACAP II strained to respond to mass casualty events in Panel A of Figure 4. Here I plot the first difference of aid's effects on Taliban attacks against ISAF over time when we shift from an incident with five beneficiaries (the 10th percentile) to 64 beneficiaries (the 90th percentile). In each case, this shift is associated with an increased mean number of post-aid Taliban attacks, a result that approaches conventional levels of statistical significance.[14] It simply may be a bridge too far to expect small amounts of aid to stitch back together shattered lives in incidents that affect entire villages. Similarly, urban settings represent extremely difficult environments where significant numbers of civilian casualties, along with attendant property damage, are likely to swamp the modest efforts like ACAP II. Caution is warranted in extending an unmodified ACAP-style program to war-ravaged cities like Raqqa or Mosul.[15]

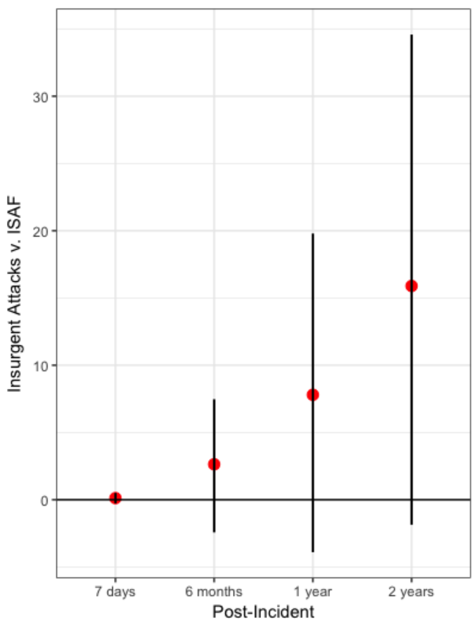
Third, these fine-grained data help add nuance to ongoing discussions about the relationship between security and aid effectiveness. Prior studies of development assistance in Iraq and Afghanistan have suggested that such programs are most likely to be violence-reducing when projects are small and counterinsurgent troop strength is high (Sexton, 2016; Berman et al., 2013). Though development and humanitarian assistance have different aims, we find a similar result with ACAP II aid. As Panel B in Figure 4 illustrates, a sharp spatial discontinuity exists in ACAP II's effectiveness: its violence-reducing properties are mostly tied to approved villages located less than four kilometers from an ISAF military installation, including small Forward Operating Bases and even smaller Combat Outposts.[16] Once this distance is exceeded, *Approved* typically turns insignificant, indicating no difference in post-aid Taliban violence when compared with abandoned villages. We should not conclude, however, that these areas are safe or secure, or that ISAF exercised uncontested control. In reality, many of these areas were dominated by the Taliban

[13]The average disbursement per incident was $11,386 once three outliers, each with over $1 million distributed, are removed.

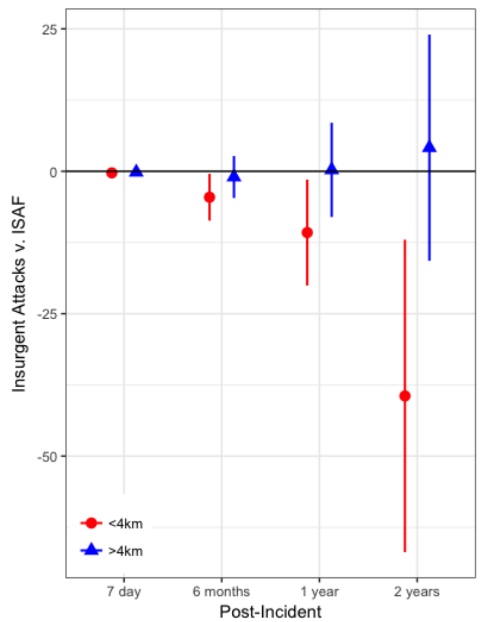
[14]Substantive interpretations were generated using first differences in *Clarify* using full models specified in Table S5. All continuous variables were set at their mean; dichotomous variables at median values. K=1000 simulations were estimated (Tomz, Wittenberg and King, 2003).

[15]This discussion also illustrates the need to consider civilian harm more holistically than simply the number of beneficiaries. Mass casualty events have emergent properties (e.g. infrastructure damage) that destroy communal resilience in ways not seen with smaller scale incidents.

[16]Mean distance in the entire sample is 4.08 kilometers; nearly 70% of all villages are $\leq$4km of one military base.



(a) First Differences: Shifts from 5 to 64 Beneficiaries (10th → 90th percentile)



(b) Distance from Nearest Base

Figure 4: ACAP II Effects on Taliban Attacks Against ISAF by Number of Beneficiaries and Distance from Military Bases

despite (and possibly because of) the presence of multiple bases. Moreover, it is perhaps unsurprising that violence reduction is steepest near ISAF bases; these are, after all, magnets for Taliban violence. Rather than treating security as a precondition for success, the ACAP II program reveals that small, tailored, programs can operate successfully in violent, insurgent-controlled, settings without large numbers of counterinsurgent forces. But there are limits; drift too far from these locations, and aid's violence-reducing effects fade.

Finally, existing studies of civilian casualties and violence typically privilege the role of fatalities as the principal measure of harm inflicted. How does ACAP II perform after fatal incidents compared to non-lethal ones? In total, 548 incidents (nearly 52% of the entire sample) of approved and abandoned incidents involved at least one civilian fatality. ISAF was responsible for 227 of these incidents; the Taliban, 277. Initial tests suggest that ACAP II remained effective in reducing insurgent attacks even after incidents that killed at least one civilian (see Table S14). These violence-reducing effects persist even when larger urban centers are excluded.

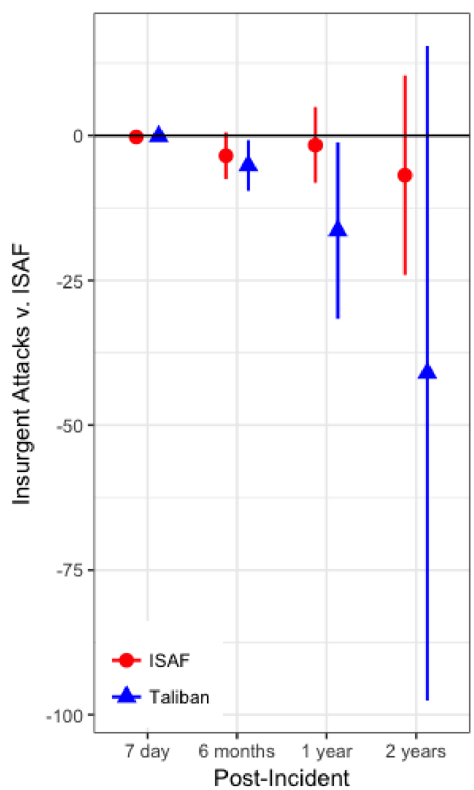


Figure 5: ACAP II Effects if Fatalities, by Perpetrators

Yet caution is again warranted. There is significant heterogeneity in the magnitude and statistical significance of this reduction when comparing ISAF- and Taliban-initiated incidents. As Figure 5 illustrates, ACAP II's effects on Taliban violence shrink considerably when considering ISAF-initiated events, with a statistically significant relationship observed in only one time period. This result is consistent with prior claims that violence by an external out-group can be especially difficult to overcome (Lyall, Blair and Imai, 2013). More optimistically, these results suggest that even in the most likely situation for witnessing grievance formation — namely, after a family member is killed — we do not observe increased Taliban attacks. Thus, while ACAP II was not sufficient to ensure reduced Taliban violence after ISAF-inflicted civilian casualties, it appears sufficient to assuage these grievances somewhat, possibly preventing the transfer of tips to the Taliban that would fuel stepped up reprisals against ISAF. This is a modest claim, but an important one, for it suggests that oft-hypothesized direct link between a counterinsurgent killing civilians and a subsequent round of increased insurgent violence can be weakened, if not severed entirely, by humanitarian aid.

8 Conclusion

Contrary to expectations drawn from the crossnational literature, the ACAP II program was associated with a marked decrease Taliban attacks against ISAF. Perhaps most remarkable is that ACAP II's conflict-reducing properties were still discernible two years after aid disbursement despite the modest amount of assistance provided. These effects do not extend to ANDSF units, however, suggesting that the "halo effect" of aid might extend only to those belligerents in a position to claim credit for its delivery. Encouragingly, ACAP II was able to deliver assistance without increasing Taliban reprisals against civilians, suggesting that some types of aid programs may not generate incentives for insurgents to disrupt them violently.

These findings suggest several theoretical and empirical avenues for further investigation. Grievance-based explanations of insurgent violence may overstate the automaticity of vengeance-seeking, for example. Even small aid packages appear able to dampen the flames of revenge for a considerable period of time. Similarly, "hearts and minds" accounts that privilege information-sharing may need to be updated to take into account the conditioning role of humanitarian aid. Indeed, in the absence of aid, Taliban attacks against ISAF were *higher* after Taliban-inflicted casualties, not lower, indicating that tips may not be flowing to the counterinsurgent as expected. Above all, there is need to deepen our theorizing to include questions of how different forms of rebel governance (Arjona, 2017) and insurgent organization (Staniland, 2014) condition the direction, magnitude, and longevity of aid's effects in wartime contexts. This will necessarily require different types of microlevel data. Attitudinal data, whether obtained via surveys or interviews, is especially important for testing the mechanisms underpinning these relationships. And while this study has privileged insurgent attacks, other behavioral measures — resilience and social cohesion, trust, collective goods provision — could be adopted to test aid's effects.

Policy recommendations also flow from these findings. Policymakers should know that even modest programs can have outsized effects on insurgent violence even in (especially in) difficult settings. ACAP II had its largest effects among rural populations near ISAF military bases after events with fairly low thresholds for casualties and property damage, providing clues about best sites for programming. Expectations should be kept reasonable: there is no guarantee that increasing the amount of assistance will reap greater dividends. The opposite may in fact be true. Practitioners should also experiment with other forms of assistance, including cash, medical assistance, and cognitive therapy, with an eye toward reducing post-incident violence even further. In an ideal world, ethical considerations would preclude any form of civilian harm in wartime. In reality, civilians suffer enormously in civil wars. These findings underscore the need to buttress moralistic appeals designed to reduce civilian casualties with a pragmatic focus on post-harm mitigation once civilians inevitably find themselves trapped between warring parties.

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Supplemental Online Appendix for "Civilian Casualties, Humanitarian Aid, and Insurgent Violence"

S1 Event Eligibility Criteria

ACAP II's eligibility criteria are reproduced below (International Relief and Development, 2012, 13). Note too that all beneficiaries are screened via using EPLS and UN Lists for identifying black listed or excluded parties. In cases where any beneficiary does not pass these checks, the database team has to notify the respected RD/DRD and exclude that nominated beneficiary.

1. Direct result of the presence of U.S. and Coalition Forces actions against Taliban or other Insurgent groups.
   (a) Aerial Incident (bombardment, accidental weapons release, property damage caused by US and Coalition Forces aircraft.
   (b) Direct US and Coalition Forces combat operations against Taliban or other Insurgent groups (day/night).
2. Direct result of the presence of U.S. and Coalition Forces responding to a potential or assumed threat. (Self-defense).
   (a) Firing on a civilian/vehicle perceived as a threat by US and Coalition Forces (vehicle approaching or overtaking military convoys or fail to follow instructions at a check point/ civilians entering or in the vicinity of a US and Coalition Forces guarded area).
   (b) Searching a suspected insurgent residence or property and accidentally harming an innocent civilian.
3. Direct result of the presence of U.S. and Coalition Forces in a given area. Civilians affected by Improvised Explosive Devices (IEDs) targeting military convoys; attacks against US and Coalition Forces bases or forces.
   (a) IED/ VBIED/ suicide /firing event against US and Coalition Forces convoys/patrols. For IED detonations, the convoy/patrol must be present within 1 km or 10 minutes of the detonation site.
   (b) IED/ suicide/ firing event against US and Coalition Forces bases/outpost. Civilian casualties/property damage must incur within a 1 km radius of the base/ outpost.

S1.1 Individual Eligibility Criteria

1. *Civilian/Non-Combatant:* Any person who is not taking a direct part in hostilities. This includes all civilians not used for a military purpose in terms of fighting the conflict. Women and children will also be considered as non-combatants and may be Approved if harmed by US and Coalition Forces.

2. Afghan civilians who are not Approved for ACAP II assistance are:

   (a) Afghan National Security forces (ANA, ANP, ALP, NDS, ABP)

   (b) Afghan Government Officials (political and office holders)

   (c) Afghans directly employed/contracted by US and Coalition Forces (translators, vendors, supply contractors, drivers)

Note: The types of ACAP II assistance given will be dependent on investigations by ACAP II staff, and the provision of one phase of ACAP II humanitarian assistance will not guarantee provision of further assistance. Thorough investigations will be made and will be case specific. Additionally, in instances in which circumstances are unclear, humanitarian assistance will be dependent on the results of a thorough ACAP II investigation.

Table S1: Approved and Abandoned Incidents, By Event Type

Event Type	*Approved*	*Abandoned*
ISAF-initiated		
Traffic Accident	57	14
Airstrike	63	67
ISAF Indirect Fire	10	6
ISAF Military Operation	140	102
Escalation of Force (EOF)	12	4
Sub-Total	282	193
Taliban-initiated		
Improvised Explosive Device (IED)	110	129
Taliban Indirect Fire	57	29
Taliban Military Operation	60	20
Suicide Bombing	43	17
Sub-Total	270	195
Unclear Responsibility		
Crossfire	40	81
TOTAL	592	469

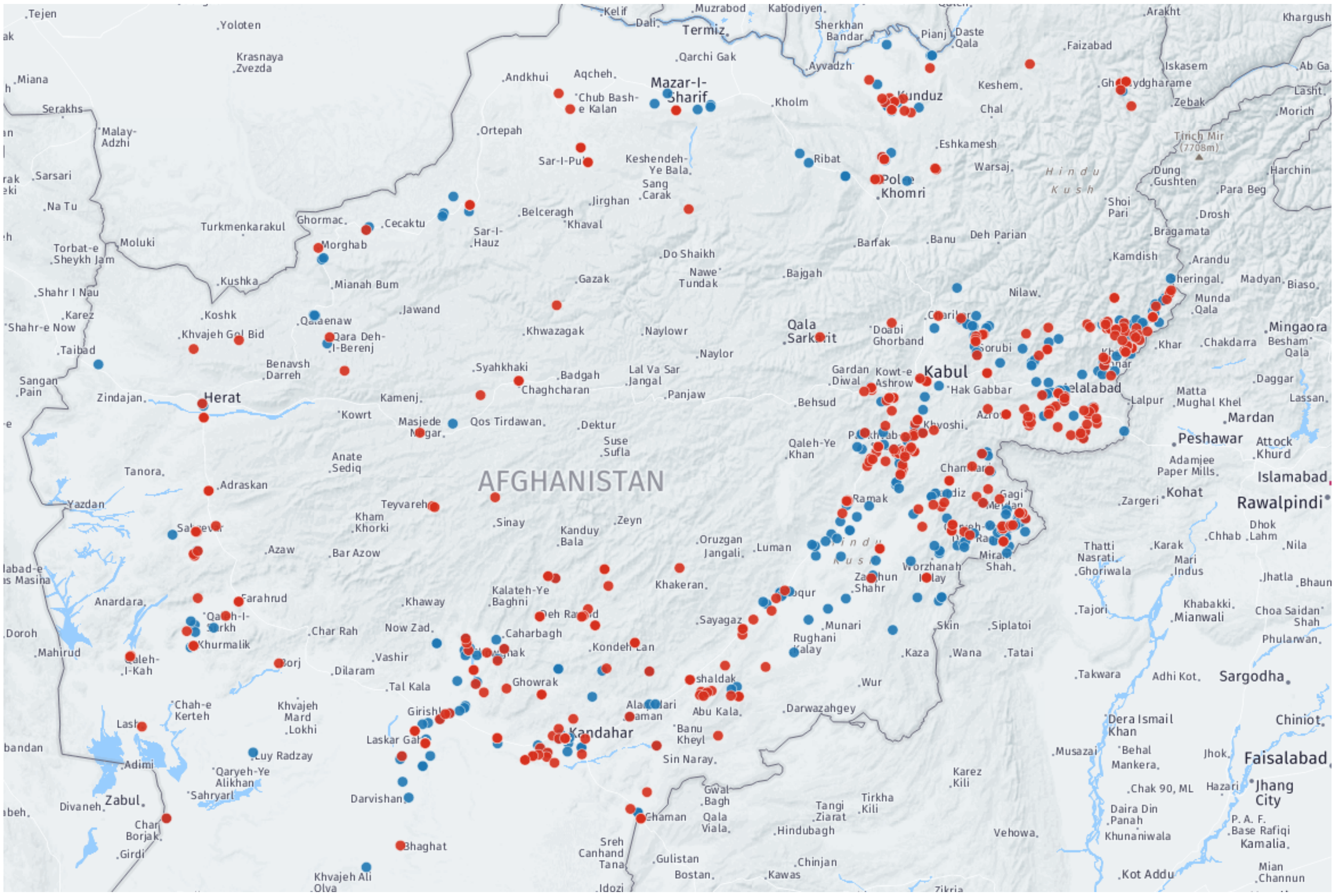

Figure S6: ACAP II Approved (Blue) and Abandoned (Red) Incidents

Table S2: As-if randomization balance test: Incident-level determinants of aid approval

		Reduced Form		By Incident Type	
	Sample Mean	Coefficient	p-value	Coefficient	p-value
	(1)	(2)	(3)	(4)	(5)
Traits					
ISAF Responsible (binary)	0.505	−0.004	0.918		
Casualties (logged)	0.095	−0.009	0.539	−0.002	0.874
Total Harm (logged)	0.920	−0.002	0.942	0.003	0.920
Property Damage (binary)	0.431	0.019	0.649	0.022	0.583
ISAF-initiated					
Military Operation	0.228			−0.043	0.724
Airstrike	0.123			−0.137	0.284
Escalation of force	0.015			0.128	0.453
Traffic Accident	0.067			0.175	0.188
Taliban-initiated					
Military Operation	0.075			0.136	0.304
Indirect Fire	0.081			0.040	0.759
Suicide Bombing	0.057			0.097	0.484
Improvised Explosive Device	0.226			−0.160	0.200
Unclear Responsibility					
Crossfire	0.114			−0.285	0.026
Adjusted r^2			0.003		0.076
p-value on F-statistic			0.621		0.000
N		940		1,004	

Note: Columns (2) and (3) report the coefficient and p-value on assignment to eligibility from a logistic regression of all variables on the treatment indicator (approved/not approved). Robust standard errors clustered by village. ISAF indirect fire is the referent category. Mean casualties per incident was 4.7 individuals killed and wounded.

Table S3: As-if randomization balance test: Spatial determinants of aid approval

	Sample Mean (1)	Coefficient (2)	p-value (3)
RC North	0.086	−0.183	0.525
RC East	0.622	−0.095	0.740
RC South	0.133	−0.317	0.274
RC West	0.074	−0.211	0.465
RC Kabul	0.013	−0.024	0.939
Pakistan Border	0.177	−0.037	0.467
Helmand	0.068	−0.055	0.849
Kandahar	0.074	0.123	0.140
Khost	0.076	0.214	0.003
Kunar	0.179	0.041	0.383
Logar	0.086	0.021	0.718
Adjusted r^2			0.023
p-value on F-statistic			0.000
N			1,061

Note: Columns (2) and (3) report the coefficient and p-value on assignment to eligibility from a logistic regression of all variables on the treatment indicator (approved/not approved). Robust standard errors clustered by village. Regional Command (RC) South West is the referent category (mean: 0.087).

Table S4: As-if randomization balance test: Village-level determinants of aid approval

	Sample Mean (1)	Coefficient (2)	p-value (3)
Population (log)	7.559	−0.017	0.149
Elevation (meters, log)	7.127	−0.038	0.332
Village Language	2.008	−0.026	0.132
Pashto (binary)	0.811	0.038	0.429
Number of Neighbors Within $5km^2$ (log)	2.291	0.001	0.883
Distance to District Center (km, log)	1.062	−0.002	0.872
Paved Roads in District (km, log)	−0.048	0.006	0.207
Latitude	33.903	−0.022	0.217
Longitude	68.290	0.017	0.052
Kabul (binary)	0.013	−0.393	0.167
NSP Spending Per Capita ($, log)	−1.109	−0.001	0.831
Number of NSP Projects	0.990	−0.004	0.351
Number of NSP Beneficiaries (families)	82.627	−0.000	0.757
Distance to Nearest Base (km, log)	7.596	−0.012	0.504
Number of Bases Within $3km^2$	1.953	0.003	0.836
Number of Bases Within $5km^2$	3.237	0.019	0.102
Number of Bases Within $10km^2$	6.781	−0.001	0.718
Prior Taliban Attacks v. ISAF (7 days ↓)	0.953	0.009	0.505
Prior Taliban IEDs v. ISAF	0.101	−0.050	0.346
Prior ISAF Attacks v. Taliban	0.037	0.008	0.919
Prior Taliban Attacks v. ANDSF	0.184	−0.011	0.795
Prior Taliban IEDs v. ANDSF	0.048	0.106	0.199
Prior Taliban Attacks v. Civilians	0.025	0.097	0.537
Prior Taliban IEDs v. Civilians	0.013	0.023	0.907

Note: Columns (2) and (3) report the coefficient and p-value on assignment to eligibility from a logistic regression of all variables on the treatment indicator (approved/not approved). Robust standard errors clustered by village.

Table S4: As-if randomization balance test: Village-level, continued

	Sample Mean (1)	Coefficient (2)	p-value (3)
Prior Taliban Attacks v. ISAF (90 days ↓)	11.575	−0.001	0.802
Prior Taliban IEDs v. ISAF	1.246	−0.018	0.356
Prior ISAF Attacks v. Taliban	0.388	0.012	0.663
Prior Taliban Attacks v. ANDSF	2.220	0.001	0.901
Prior Taliban IEDs v. ANDSF	0.581	−0.004	0.891
Prior Taliban Attacks v. Civilians	0.316	−0.006	0.922
Prior Taliban IEDs v. Civilians	0.170	−0.015	0.848
Prior Taliban Attacks v. ISAF (180 days ↓)	21.270	−0.001	0.839
Prior Taliban IEDs v. ISAF	2.321	−0.002	0.917
Prior ISAF Attacks v. Taliban	0.749	0.013	0.566
Prior Taliban Attacks v. ANDSF	4.403	−0.003	0.688
Prior Taliban IEDs v. ANDSF	1.154	0.008	0.727
Prior Taliban Attacks v. Civilians	0.623	0.068	0.211
Prior Taliban IEDs v. Civilians	0.322	−0.016	0.833
Prior Taliban Attacks v. ISAF (365 days ↓)	40.869	0.001	0.416
Prior Taliban IEDs v. ISAF	4.546	0.003	0.703
Prior ISAF Attacks v. Taliban	1.679	−0.010	0.302
Prior Taliban Attacks v. ANDSF	8.526	−0.001	0.738
Prior Taliban IEDs v. ANDSF	2.177	−0.002	0.836
Prior Taliban Attacks v. Civilians	1.106	−0.025	0.371
Prior Taliban IEDs v. Civilians	0.656	−0.019	0.653
Fighting Season (April-September)	0.579	−0.032	0.340
Adjusted r^2			0.005
p-value on F-statistic			0.266
N		1,061	

Table S5: ACAP II and Insurgent Violence Against ISAF

	7 days	90 days	180 days	1 year	2 years
	(1)	(2)	(3)	(4)	(5)
Approved	−0.289**	−1.238	−3.910**	−8.014**	−28.013**
	(0.105)	(0.781)	(1.388)	(3.003)	(9.826)
Population	0.031	0.448*	1.572**	3.535*	8.702†
	(0.033)	(0.221)	(0.602)	(1.548)	(4.745)
Elevation	0.125†	0.203	2.074†	3.958	10.218
	(0.075)	(0.562)	(1.139)	(2.802)	(9.093)
Pashto	−0.252*	−0.893	−2.167	−2.890	−17.796
	(0.107)	(0.888)	(2.084)	(4.243)	(15.364)
Neighbors	−0.012	−0.230	−0.612†	−1.159	−4.265
	(0.018)	(0.155)	(0.369)	(0.960)	(3.341)
NSP Spending Per Capita	−0.012	−0.169*	−0.517***	−1.547***	−3.582**
	(0.010)	(0.077)	(0.157)	(0.421)	(1.322)
Distance to District Center	0.019	−0.025	0.084	2.505	6.914
	(0.041)	(0.342)	(0.736)	(1.582)	(5.857)
Distance to Nearest Base	0.018	0.371	0.899	3.313	8.346
	(0.045)	(0.411)	(0.803)	(2.176)	(6.431)
Bases Within 10km	0.069***	0.513***	1.481***	4.080***	13.438***
	(0.010)	(0.092)	(0.261)	(0.887)	(2.464)
Total Harm	0.046	0.507	1.579*	5.850**	18.299***
	(0.068)	(0.374)	(0.699)	(2.082)	(3.935)
ISAF-Initiated Incident	−0.069	0.046	0.712	1.603	14.155†
	(0.108)	(0.564)	(1.120)	(2.689)	(8.420)
Prior Taliban Attacks v. ISAF	−0.369**	−0.191	−0.306**	−0.264†	−0.198
	(0.117)	(0.137)	(0.110)	(0.139)	(0.156)
Prior Taliban IEDs v. ISAF	0.074	−0.397	−0.480	0.188	−0.445
	(0.257)	(0.507)	(0.547)	(0.648)	(1.125)
Prior ISAF Attacks v. Taliban	0.335	0.859*	1.148*	−0.557	−3.442†
	(0.283)	(0.349)	(0.464)	(0.782)	(1.921)
Prior Taliban Attacks v. ANDSF	0.011	0.015	0.166	0.130	−0.633
	(0.202)	(0.219)	(0.174)	(0.255)	(0.505)
Prior Taliban IEDs v. ANDSF	−0.429	−0.213	−0.471	−2.404*	−3.706†
	(0.341)	(0.621)	(0.688)	(1.197)	(2.064)
Prior Taliban Attacks v. Civilians	−0.672	−1.989	−1.027	3.151	22.575*
	(0.697)	(1.225)	(1.122)	(2.205)	(10.584)
Prior Taliban IEDs v. Civilians	0.136	1.857	0.510	0.731	−9.247
	(0.893)	(1.165)	(1.156)	(2.342)	(9.923)
Time FE	✓	✓	✓	✓	✓
F-test	6.20***	7.37***	8.84***	5.34***	56.21***
Root MSE	1.648	11.123	20.156	43.08	116.22
r^2	0.185	0.159	0.311	0.421	0.562
N	1,061	1,061	1,061	1,061	681
Clusters	607	607	607	607	415

Note: The number of prior Taliban and ISAF attacks is tied to each temporal window (e.g., Model 1 uses 7 day pre/post temporal windows). Quarterly fixed effects are used in all models (third quarter of 2013 is the referent category). Two year estimates for some observations are right censored due to CIDNE data availability. Robust standard errors clustered on village. $^{\star\star\star}p<.001$; $^{\star\star}p<.01$; $^{\star}p<.05$; † $p<.1$

Table S6: ACAP II and Insurgent Attacks v. ISAF, No Covariates or FE

	7 days	90 days	180 days	1 year	2 years
	(1)	(2)	(3)	(4)	(5)
Approved	$-0.225^{\star}$	-0.982	$-3.232^{\star}$	$-5.025^{\star}$	$-12.929^{\dagger}$
	(0.104)	(0.780)	(1.482)	(2.418)	(7.129)
F-test	$4.65^{\star}$	1.58	$4.76^{\star}$	$4.32^{\star}$	$3.26^{\dagger}$
Root MSE	1.801	11.981	23.958	55.927	172.78
r^2	0.004	0.002	0.005	0.002	0.001
N	1,061	1,061	1,061	1,061	681
Clusters	607	607	607	607	415

Note: The number of prior Taliban and ISAF attacks is tied to each temporal window (e.g., Model 1 uses 7 day pre/post temporal windows). Quarterly fixed effects are used in all models (third quarter of 2013 is the referent category). Two year estimates for some observations are right censored due to CIDNE data availability. Robust standard errors clustered on village. $^{\star\star\star}p<.001$; $^{\star\star}p<.01$; $^{\star}p<.05$; † $p<.1$

Table S7: ACAP II and Insurgent Violence Against ANDSF

	7 days	90 days	180 days	1 year
	(1)	(2)	(3)	(4)
Approved	-0.009	0.111	0.267	0.290
	(0.031)	(0.263)	(0.557)	(1.187)
Population	0.011	0.098	0.163	0.107
	(0.014)	(0.113)	(0.189)	(0.536)
Elevation	0.003	-0.211	-0.438	-2.045
	(0.023)	(0.179)	(0.388)	(1.356)
Pashto	0.043	$0.696^{\star\star}$	$0.961^{\dagger}$	-0.388
	(0.040)	(0.258)	(0.513)	(1.085)
Neighbors	-0.002	0.034	0.082	0.073
	(0.007)	(0.050)	(0.101)	(0.197)
NSP Spending Per Capita	0.004	-0.018	-0.024	-0.177
	(0.003)	(0.033)	(0.068)	(0.131)
Distance to District Center	$-0.049^{\star\star\star}$	-0.084	-0.255	0.196
	(0.017)	(0.085)	(0.212)	(0.948)
Distance to Nearest Base	$-0.041^{\star}$	-0.077	-0.177	-0.147
	(0.017)	(0.157)	(0.323)	(0.948)
Bases Within 10km	-0.002	-0.016	-0.056	-0.094
	(0.002)	(0.019)	(0.044)	(0.088)
Total Harm	-0.007	0.072	-0.046	-0.437
	(0.014)	(0.132)	(0.253)	(0.517)
ISAF-Initiated Incident	-0.000	-0.093	0.005	0.127
	(0.029)	(0.210)	(0.362)	(0.687)
Prior Taliban Attacks v. ISAF	$0.022^{\dagger}$	$0.021^{\dagger}$	$0.027^{\dagger}$	0.009
	(0.012)	(0.013)	(0.016)	(0.017)
Prior Taliban IEDs v. ISAF	-0.082	$-0.151^{\dagger}$	-0.099	0.243
	(0.060)	(0.091)	(0.111)	(0.162)
Prior ISAF Attacks v. Taliban	$0.233^{\dagger}$	0.018	-0.061	-0.042
	(0.129)	(0.123)	(0.200)	(0.379)
Prior Taliban Attacks v. ANDSF	$-0.860^{\star\star\star}$	$-0.254^{\star}$	$-0.306^{\dagger}$	-0.116
	(0.056)	(0.130)	(0.159)	(0.100)
Prior Taliban IEDs v. ANDSF	-0.080	-0.374	-0.436	$-0.823^{\star}$
	(0.131)	(0.256)	(0.362)	(0.412)
Prior Taliban Attacks v. Civilians	0.112	0.003	0.474	-0.008
	(0.162)	(0.380)	(0.399)	(0.733)
Prior Taliban IEDs v. Civilians	0.031	$0.932^{\star}$	0.314	1.906
	(0.201)	(0.471)	(0.563)	(1.122)
Time FE	✓	✓	✓	✓
F-test	$23.78^{\star\star\star}$	$2.62^{\star\star\star}$	$2.35^{\star\star\star}$	$2.25^{\star\star\star}$
Root MSE	0.478	3.600	7.265	13.487
r^2	0.489	0.113	0.122	0.067
N	1,061	1,061	1,061	865
Clusters	607	607	607	510

Note: The number of prior Taliban and ISAF attacks is tied to each temporal window (e.g., Model 1 uses 7 day pre/post temporal windows). Quarterly fixed effects are used in all models (third quarter of 2013 is the referent category). One year estimates for some observations are right censored due to iMMAP data availability. Robust standard errors clustered on village. $^{\star\star\star}p<.001$; $^{\star\star}p<.01$; $^{\star}p<.05$; † $p<.1$

Table S8: ACAP II and Insurgent Violence Against Civilians

	7 days	90 days	180 days	1 year
	(1)	(2)	(3)	(4)
Approved	−0.004	−0.048	−0.095	−0.163
	(0.010)	(0.501)	(0.087)	(0.188)
Population	0.007**	0.086**	0.150**	0.107†
	(0.003)	(0.032)	(0.045)	(0.062)
Elevation	−0.007	−0.080	−0.126	−0.389*
	(0.010)	(0.052)	(0.091)	(0.164)
Pashto	0.009	0.056	0.114	−0.067
	(0.012)	(0.069)	(0.116)	(0.240)
Neighbors	0.002	−0.005	−0.015	−0.060
	(0.001)	(0.009)	(0.019)	(0.039)
NSP Spending Per Capita	0.002*	0.012*	0.017†	0.004
	(0.001)	(0.006)	(0.010)	(0.018)
Distance to District Center	−0.010	−0.033†	−0.029	0.008
	(0.007)	(0.018)	(0.035)	(0.086)
Distance to Nearest Base	−0.005	−0.006	−0.023	−0.037
	(0.006)	(0.021)	(0.037)	(0.093)
Bases Within 10km	−0.001	0.000	0.004	0.014
	(0.001)	(0.002)	(0.005)	(0.013)
Total Harm	0.009	0.023	0.020	−0.007
	(0.006)	(0.024)	(0.037)	(0.059)
ISAF-Initiated Incident	−0.005	0.023	0.057	−0.068
	(0.011)	(0.048)	(0.071)	(0.125)
Prior Taliban Attacks v. ISAF	−0.000	−0.004	−0.002	−0.007**
	(0.002)	(0.001)	(0.002)	(0.003)
Prior Taliban IEDs v. ISAF	0.004	0.004	0.020	0.073*
	(0.024)	(0.024)	(0.024)	(0.070)
Prior ISAF Attacks v. Taliban	−0.019**	−0.008	−0.025	0.004
	(0.007)	(0.018)	(0.025)	(0.065)
Prior Taliban Attacks v. ANDSF	0.010	0.031	0.033***	0.053**
	(0.013)	(0.012)	(0.011)	(0.017)
Prior Taliban IEDs v. ANDSF	−0.027	0.094	0.055	0.060
	(0.022)	(0.059)	(0.074)	(0.070)
Prior Taliban Attacks v. Civilians	−1.045***	−0.759***	−0.659***	−0.386
	(0.013)	(0.112)	(0.089)	(0.244)
Prior Taliban IEDs v. Civilians	0.064	−0.037	−0.067	−0.396
	(0.048)	(0.142)	(0.126)	(0.273)
Time FE	✓	✓	✓	✓
F-test	1187.47***	17.62***	10.92***	8.18***
Root MSE	0.159	0.654	1.031	1.804
r^2	0.576	0.414	0.374	0.323
N	1,061	1,061	1,061	865
Clusters	607	607	607	510

Note: The number of prior Taliban and ISAF attacks is tied to each temporal window (e.g., Model 1 uses 7 day pre/post temporal windows). Quarterly fixed effects are used in all models (third quarter of 2013 is the referent category). One year estimates for some observations are right censored due to iMMAP data availability. Robust standard errors clustered on village.*** $p<.001$; ** $p<.01$; * $p<.05$; † $p<.1$

Table S9: ACAP II and Insurgent Improvised Explosive Device (IED) Attacks v. ISAF

	7 days	90 days	180 days	1 year	2 years
	(1)	(2)	(3)	(4)	(5)
Approved	$-0.045^{\star}$	-0.039	-0.078	-0.322	$-1.454^{\star}$
	(0.021)	(0.088)	(0.158)	(0.271)	(0.631)
F-test	$16.33^{\star\star\star}$	$20.32^{\star\star\star}$	$9.73^{\star\star\star}$	$6.45^{\star\star\star}$	$6.60^{\star\star\star}$
Root MSE	0.338	1.502	2.489	3.809	7.719
r^2	0.428	0.414	0.342	0.347	0.482
N	1,061	1,061	1,061	1,061	681
Clusters	607	607	607	607	415

Note: Models include all covariates used in Table S5. The number of prior Taliban and ISAF attacks is tied to each temporal window (e.g., Model 1 uses 7 day pre/post temporal windows). Quarterly fixed effects are used in all models (third quarter of 2013 is the referent category). Two year estimates for some observations are right censored due to CIDNE data availability. Robust standard errors clustered on village. $^{\star\star\star}p<.001$; $^{\star\star}p<.01$; $^{\star}p<.05$; $^{\dagger}p<.1$

Table S10: Placebo Test: ACAP II and Insurgent Violence v. ISAF

	7 days	90 days	180 days	1 year
	(1)	(2)	(3)	(4)
Approved	0.029	0.232	0.082	−1.591
	(0.042)	(0.437)	(0.699)	(1.607)
F-test	20.58⋆⋆⋆	9.07⋆⋆⋆	12.45⋆⋆⋆	32.68⋆⋆⋆
Root MSE	0.755	6.680	14.38	28.85
r^2	0.326	0.121	0.282	0.469
N	1,061	1,061	1,061	1,061
Clusters	607	607	607	607

Note: Models include all covariates used in Table S5. The number of prior Taliban and ISAF attacks is tied to each temporal window (e.g., Model 1 uses 7 day pre/post temporal windows). The time period is set to August 2009–August 2011. Quarterly fixed effects are used in all models (third quarter of 2010 is the referent category). Two year estimates for some observations are right censored due to CIDNE data availability. Robust standard errors clustered on village. ⋆⋆⋆$p<.001$; ⋆⋆$p<.01$; ⋆$p<.05$; † $p<.1$

Table S11: Continuous Treatment: Aid Dispersed ($) and Insurgent Attacks v. ISAF (Full Sample)

	7 days	90 days	180 days	1 year	2 years
	(1)	(2)	(3)	(4)	(5)
Aid Amount (Logged)	$-0.021^{\star\star}$	-0.083	$-0.291^{\star\star}$	$-0.592^{\star\star}$	$-2.184^{\star\star}$
	(0.008)	(0.059)	(0.099)	(0.216)	(0.723)
F-test	$6.23^{\star\star\star}$	$7.36^{\star\star\star}$	$8.88^{\star\star\star}$	$5.36^{\star\star\star}$	$7.85^{\star\star\star}$
Root MSE	1.649	11.127	20.158	43.085	116.18
r^2	0.184	0.158	0.311	0.421	0.562
N	1,061	1,061	1,061	1,061	681
Clusters	607	607	607	607	415

Note: Models include all covariates used in Table S5. The number of prior Taliban and ISAF attacks is tied to each temporal window (e.g., Model 1 uses 7 day pre/post temporal windows). Quarterly fixed effects are used in all models (third quarter of 2013 is the referent category). Two year estimates for some observations are right censored due to CIDNE data availability. Robust standard errors clustered on village.$^{\star\star\star}p<.001$; $^{\star\star}p<.01$; $^{\star}p<.05$; † $p<.1$

Table S12: Amount of Aid Dispersed ($) and Insurgent Attacks v. ISAF (ACAP II Approved Villages Only)

	7 days	90 days	180 days	1 year	2 years
	(1)	(2)	(3)	(4)	(5)
Aid Amount (Logged)	0.064	0.763	1.163	3.875	7.970*
	(0.077)	(0.647)	(0.983)	(2.473)	(3.965)
F-test	6.39***	6.92***	10.33***	10.03***	7.12***
Root MSE	1.342	11.36	18.90	44.68	119.17
r^2	0.260	0.158	0.371	0.400	0.520
N	592	592	592	592	442
Clusters	407	407	407	407	322

Note: Models include all covariates used in Table S5. The number of prior Taliban and ISAF attacks is tied to each temporal window (e.g., Model 1 uses 7 day pre/post temporal windows). Quarterly fixed effects are used in all models (third quarter of 2013 is the referent category). Two year estimates for some observations are right censored due to CIDNE data availability. Robust standard errors clustered on village.$^{\star\star\star}p<.001$; $^{\star\star}p<.01$; $^{\star}p<.05$; † $p<.1$

Table S13: Aid Per Beneficiary ($) and Insurgent Attacks v. ISAF
(ACAP II Approved Villages Only)

	7 days	90 days	180 days	1 year	2 years
	(1)	(2)	(3)	(4)	(5)
Aid Amount (Logged)	0.323	−0.912	2.778	14.138	17.551
	(0.203)	(1.394)	(2.560)	(9.017)	(20.097)
F-test	6.62***	6.79***	10.39***	10.90***	7.84***
Root MSE	1.341	11.388	18.93	44.699	119.34
r^2	0.261	0.154	0.369	0.400	0.518
N	592	592	592	592	442
Clusters	407	407	407	407	322

Note: Models include all covariates used in Table S5. The number of prior Taliban and ISAF attacks is tied to each temporal window (e.g., Model 1 uses 7 day pre/post temporal windows). Quarterly fixed effects are used in all models (third quarter of 2013 is the referent category). Two year estimates for some observations are right censored due to CIDNE data availability. Robust standard errors clustered on village.*** p<.001; ** p<.01; * p<.05; † p<.1

Table S14: ACAP II Effects in Incidents With Civilian Fatalities
(Full Sample)

	7 days	90 days	180 days	1 year	2 years
	(1)	(2)	(3)	(4)	(5)
Approved	$-0.215^{\star}$	$-1.813^{\star}$	$-5.593^{\star\star\star}$	$-11.851^{\star\star}$	-33.863
	(0.103)	(0.898)	(1.444)	(4.285)	(16.071)
F-test	$22.92^{\star\star\star}$	$20.34^{\star\star\star}$	$36.51^{\star\star\star}$	$21.39^{\star\star\star}$	$48.45^{\star\star\star}$
Root MSE	1.708	10.32	19.01	40.13	127.6
r^2	0.288	0.223	0.392	0.574	0.665
N	548	548	548	548	339
Clusters	376	376	376	376	250

Note: Models include all covariates used in Table S5. The number of prior Taliban and ISAF attacks is tied to each temporal window (e.g., Model 1 uses 7 day pre/post temporal windows). Quarterly fixed effects are used in all models (third quarter of 2013 is the referent category). Two year estimates for some observations are right censored due to CIDNE data availability. Robust standard errors clustered on village. $^{\star\star\star}p<.001$; $^{\star\star}p<.01$; $^{\star}p<.05$; $^{\dagger}p<.1$

Table S15: ACAP II Effects in Incidents With Civilian Fatalities
(Rural Only)

	7 days	90 days	180 days	1 year	2 years
	(1)	(2)	(3)	(4)	(5)
Approved	-0.123	$-1.688^{\star}$	$-3.500^{\star\star}$	-2.589	$-9.688^{\dagger}$
	(0.105)	(0.841)	(1.364)	(2.092)	(5.262)
F-test	$4.24^{\star\star\star}$	$5.31^{\star\star\star}$	$7.10^{\star\star\star}$	$7.89^{\star\star\star}$	6.83
Root MSE	1.105	7.499	13.125	23.234	49.474
r^2	0.497	0.306	0.326	0.342	0.471
N	513	513	513	513	314
Clusters	368	368	368	368	244

Note: Models include all covariates used in Table S5. The number of prior Taliban and ISAF attacks is tied to each temporal window (e.g., Model 1 uses 7 day pre/post temporal windows). Quarterly fixed effects are used in all models (third quarter of 2013 is the referent category). Two year estimates for some observations are right censored due to CIDNE data availability. Robust standard errors clustered on village. $^{\star\star\star}p<.001$; $^{\star\star}p<.01$; $^{\star}p<.05$; $^{\dagger}p<.1$