Civilian Casualties and the Conditional Effects of Humanitarian Aid in Wartime*

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Can humanitarian assistance reduce—or eliminate entirely—the likelihood that civilians support or join insurgent movements after suffering indiscriminate violence? Can aid “short-circuit” the radicalization process? I adopt a nested design-based approach that examines the effects of the Afghan Civilian Assistance Program II (ACAP II), a USAID-funded program designed to render assistance to civilians harmed (in)directly from International Security Assistance Force (ISAF) actions in 2011-13. I first examine village-level program effects on insurgent violence by exploiting plausible as-if randomization in whether an incident was deemed eligible (N=592) or ineligible (N=469) for an ACAP II response. Taking advantage of the plausibly exogenous nature of victimization at the individual level, I then employ a 3,045-respondent survey experiment among both aid beneficiaries and non-recipients that explores multiple mechanisms for how aid might affect an individual’s support for, and participation in, the Taliban. ACAP II assistance is also associated with a nearly 20% reduction in insurgent attacks for approximately six months after aid distribution. I find evidence for cross-cutting individual level effects: the program increased opportunity costs for participation in the insurgency but also increased support for the Taliban.

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Can humanitarian assistance reduce—or perhaps eliminate entirely—the likelihood that civilians support or join insurgent movements after suffering indiscriminate violence? Can aid actually “short-circuit” the radicalization process? To date, our theories of civil war dynamics and violence have emphasized how civilian victimization, especially by the counterinsurgent, can have counterproductive effects. Indeed, blowback from civilian victimization has been invoked as a mechanism to explain at least seven different dynamics of civil war violence. These include (1) increased insurgent violence, typically measured in number of insurgent attacks against counterinsurgent forces; (2) the spatial diffusion of insurgent violence and state responses to new areas as anger spreads; (3) the erosion of the central government’s legitimacy, raising the “bribe” price that counterinsurgents must pay for winning hearts and minds (Mason 1996); (4) increasing the lethality and sophistication of insurgent attacks as insurgents invest in weapons (e.g. improvised explosive devices) that require some measure of local support to wield effectively; (5) increases in the size and resources of the insurgency as civilian casualties facilitate recruitment efforts; (6) rendering counterinsurgent efforts more costly and less selective as information flows dry up from aggrieved populations; and (7) creating forced population displacement and damaged civilian economies that lower the opportunity costs of supporting and joining an insurgency.

In nearly all of these accounts, civilian victimization is largely viewed as an “on-off” switch; once it occurs, these negative processes are set in motion, with little apparent regard for the possibility of conditional effects. Beyond the assumption of a mechanical relationship between the number of fatalities and the magnitude of the negative consequences, there has been little treatment of how the magnitude of these effects might vary; how much exposure to violence is necessary to generate these effects; whether there is substantial heterogeneity in these effects, particularly when we compare state-inflicted casualties with insurgent-inflicted casualties; and almost no discussion of whether these processes can be interrupted or at least mitigated through the provision of timely post-incident humanitarian assistance. Nor are these accounts especially clear on which mechanisms underpin the assumed relationship between civilian casualties and increased insurgent violence or how these mechanisms interact.

Even the role that humanitarian assistance might play in defusing these escalatory dynamics remains unclear. In fact, recent crossnational evidence suggests that humanitarian assistance can actually *increase* the incidence and duration of civil wars (Narang 2015).
Moreover, evaluating humanitarian efforts is made difficult by the nature of their (violent) settings. There are almost no studies of immediate humanitarian assistance—one meta-analysis of 39 studies finds only six of immediate relief efforts, and only three in (post-)conflict settings. More broadly, to date studies of civilian victimization and its effects are typically pitched at national or subnational levels and lack any measure of an individual’s exposure to harm or receipt of assistance. Without individual level data, scholars have been forced to input behavior from presumed victimization, and have relied on a multitude of often behaviorally equivalent mechanisms to link violence to observed outcomes. Key issues, including how to measure aid’s effects on sensitive topics such as support for the insurgency or counterinsurgent, are difficult to tackle in these environments, especially with direct survey questions. And the potential politicization of humanitarian aid, including its use as a face-saving mechanism for “reputation protection,” likely only intensifies already severe selection biases, confounding our estimates of aid’s causal effects.

The Afghan Civilian Assistance Program II (ACAP II) provides a rare window into the effects of exposure to violence in the immediate aftermath of 1,061 civilian casualty events in Afghanistan (2011-13). This $64 million US program was designed by the United States Agency for International Development (USAID) to render timely humanitarian assistance in the immediate aftermath of civilian casualty events caused by either the International Security Assistance Force (ISAF) or the Taliban.

I draw on a design-based approach that exploits plausibly exogenous variation in individual-level exposure to violence and village-level eligibility for ACAP II assistance to assess whether humanitarian assistance can “short-circuit” post-incident radicalization. I begin at the village level to explore whether receipt of ACAP II altered patterns of insurgent violence compared to villages where incidents occurred but, due to bureaucratic inertia, ACAP II was unable to deliver assistance. I then use a 3,045 respondent survey experiment to compare individuals who received ACAP II assistance to individuals who were present during the incident but who were not harmed (and thus received no assistance) to test the microfoundations linking aid to attitudes and behavior. The combination of plausibly exogenous variation at the individual and village level creates a nested design that

1These arguments assume that aid fuels conflict because it is captured by rebels. Here, the aid distributed is non-monetary and too small/difficult to be subject to rebel capture.
integrates individual-level mechanisms with more macro-level village outcomes in wartime settings where randomized control trials are infeasible or unethical.

Several findings emerge from this study. First, receiving ACAP II assistance is associated with a 14-17% reduction in insurgent attacks after ISAF-inflicted incidents (but not Taliban ones) for up to six months after aid distribution. This reduction in insurgent attacks against counterinsurgent forces is likely due to changes in Taliban tactics; increased support for the Taliban after ISAF incidents allows the Taliban to shift their violence to other, pro-counterinsurgent, villages. Second, ACAP II’s effects at the individual level are highly conditional on which combatant (ISAF or the Taliban) inflicted harm on civilians. Evidence suggests that ACAP II worked to restore beneficiaries’ pre-incident income levels, perhaps increasing the opportunity costs for participation in the insurgency. Receipt of ACAP II assistance is also associated with increased support for the Taliban after certain events; humanitarian assistance is clearly not sufficient to overcome deeply-entrenched in-group biases among Pashtun aid recipients. Nor does ACAP II assistance improve support for the US government. At best, this aid program appears to have dampened grievances and to have returned harmed individuals to levels of support for the US that are consistent with individuals who were not harmed by these events. Proponents of humanitarian assistance as a tool for winning over “hearts and minds” are thus likely to be disappointed by these much more modest findings.

1 A Retrospective Theory of Combatant Support: Why the Effects of Aid are Conditional

Civilian casualty events create both motive and opportunity for individuals to (re)assess their relative support for combatants in wartime. These incidents represent sharp, clear, and immediate opportunities for retrospective judgements by harmed individuals of each combatant’s accountability and relative competency in mitigating the cognitive and economic effects of these events. In particular, civilian casualty events force individuals to engage in retrospective assessments of whether to update their relative support for each combatant and whether they are willing to engage in sanctioning behavior. At bottom, harmed individuals use these incidents to (re)assess the relative legitimacy of warring com-

\[ \text{Achen and Bartels (2016, 90-115).} \]
batants as well as combatants’ relative competency in responding to these events through the provision of post-incident humanitarian assistance.

In violent settings, the assignment of responsibility and blame for the incident, along with assessments of the effectiveness of the policy response, is a contested, political, process. Combatants seek to shape and enforce dueling narratives in order to deflect blame and reap credit for policy responses that (ideally) underscore their competency to harmed individuals and communities. In this instance, humanitarian assistance acts as an index of incumbent performance in which both combatants are jockeying to seize the tactical and strategic advantages that accrue for post-incident programming. These efforts are made difficult by the nature of post-incident contexts marked by high degrees of emotion, uncertainty, false information, and noise. Individuals are thus likely to adopt relatively simple heuristic devices to facilitate decision-making in these settings, including the use of in-group biases.

Harmed individuals use these retrospective judgments to inform their decisions about (1) current levels of support for each combatant and (2) the degree to which they will sanction the responsible party. In this view, “support” is viewed as an attitudinal measure that tracks an individual’s perception of the legitimacy of each combatant’s claims and his or her preference for being governed by each combatant. “Sanction” refers to behavioral measures that flow from attitudinal support, encompassing a range of possible outcomes from sharing (or withholding) “tips” about the other combatant’s activities to engaging in violence (e.g., insurgent attacks against government or occupying forces).

A naive (“blind”) retrospection model might anticipate what Achen and Bartels (2016) term a “kicking the dog” reaction — individuals simply punish the side responsible for the event and cast their support to the other side in symmetrical fashion. Humanitarian assistance in this view might have little or no affect after government-initiated violence against civilians since these amends for the harm being insufficient to remedy the responsibility for causing the event in the first place.³

I argue instead for a more nuanced account in which retrospective assessments — and thus the effects of humanitarian assistance itself — are conditional on two contextual variables: the severity of the in-group bias toward (or against) the responsible party; and

³We might imagine an even more “naive” account where harmed individuals sanction the government regardless of who was responsible since it has a mandate to protect the population (unlike insurgents). Paradoxically, if this account is correct, then efforts to impose accountability on the government may falter since it will be blamed under all conditions, undercutting its incentives to invest in tactics that limit civilian casualties.
spatial distribution of control exercised by the combatants over the individual’s surrounding area (typically, a village). The interaction of these two factors creates a set of scenarios where we might anticipate humanitarian assistance to have positive, neutral, and negative effects on relative support for insurgents.

First, I argue that group identification conditions how violence is experienced—and, as a result, how aid is received among victimized civilians. In particular, the composition of the perpetrator-victim dyad can powerfully shape not only political attitudes but subsequent decisions about whether to support (or join) an insurgent organization. Following Lyall, Blair and Imai (2013), I argue that harm by members of an out-group (here, the counterinsurgent) will have very different effects than similar levels of victimization by one’s own group (typically, though not always, the insurgent organization). More specifically, individuals will shift their support toward the insurgency after experiencing out-group victimization; humanitarian aid is unlikely to win back hearts and minds in these settings. By contrast, individuals harmed by their own group may (weakly) punish insurgents by a (modest) loss of support but will not transfer their support to the counterinsurgent.\(^4\) As a result, humanitarian aid after civilian casualty events is unlikely to overcome deep-seated bias that favor in-groups over out-groups. Put differently, there may be no hearts and minds to win, even after insurgent-initiated incidents that kill or wound large numbers of civilians.

Wartime renders in-group biases, whether ideological or ethnic in nature, even more salient.\(^5\) The fluid nature of conflict settings increases the importance of accurate, if probabilistic, assessment of others’ intentions and behavior. In our view, these biases will persist under wartime conditions rather than fade away, as expected by existing theories. The “sticky” nature of coethnic bias also suggests that counterinsurgents, especially external interveners, will struggle to win civilian hearts and minds. These biases will affect how humanitarian assistance is viewed. Pro-insurgent biases will thus dilute the effects of humanitarian assistance; civilians are likely to severely discount the motives and sincerity behind such efforts. To be sure, civilians may accept such aid, but support levels for the insurgency will (at best) remain unchanged and (at worst) will increase substantially (Lyall, Shiraito and Imai, 2015). Aid provided by an external counterinsurgent may be

\(^4\)In this view, harm by the counterinsurgent is viewed as dispositional evidence. Harm by the insurgent, however, is discounted as situational in nature.

\(^5\)On the need for our models of civil war to take ideology seriously, see Sanin and Wood (2014).
especially problematic since it underscores the inability of the central government to care for its own citizens at a moment of great need. Such public failures are likely corrosive to central government legitimacy. Simply accepting aid is therefore not a sign of attitudinal change nor a useful predictor of future behavior.⁶

Second, the distribution of territorial control (Kalyvas 2006; Staniland 2012) across combatants conditions the effects of humanitarian assistance in two ways. On the one hand, territorial control establishes the range of opportunities that aggrieved civilians have for taking up arms in vengeance. It is considerably more difficult to take revenge on counterinsurgent forces when no rebel organizations are present and when the government’s own ability to monitor the local population is high than to join an existing organization in a rebel-dominated space, for example. On the other hand, territorial control also shapes the ability of combatants (typically the counterinsurgent) to control delivery of the humanitarian assistance and to control the narrative about the violent incident. Programming in rebel-dominated areas raises the possibility that insurgents can capture the resources — or, equally as important, the credit for their delivery — that were intended to provide post-incident relief.⁷

The combination of group identification (as measured by the perpetrator’s identity relative to the harmed civilian) and the distribution of territorial control creates four categories of possible aid effects (see Table 1).

**Stranded vengeance:** In settings marked by the counterinsurgent’s indiscriminate use of violence within areas it already dominates, we are likely to observe increased support for insurgent organization. This mirrors the classic account of indiscriminate violence generating grievances among the local population. Humanitarian assistance in these settings may have marginal effects in suppressing the shift of support to the insurgents but is unlikely to satisfy public expectations. Given counterinsurgent control, however, we would not necessarily expect an increase in subsequent insurgent attacks. Aggrieved civilians lack outlets to armed groups and may find collective action much harder to organize in light of the counterinsurgent’s monitoring capabilities.

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⁶Indeed, accounts of insurgencies are rife with anecdotes of “two-faced” behavior whereby civilians accept aid but then continue their violent anti-government activities at night.

⁷Note that this argument implicitly assumes that harmed individuals are recruited into insurgent organizations and carry out their attacks in the same locality.
Table 1: The Conditional Effects of Aid on Insurgent Support: Four Scenarios

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<th>Control</th>
<th>Perpetrator of Violence</th>
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<td>Counterinsurgent</td>
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<td></td>
<td>Increased Support</td>
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<td>(&quot;Stranded Vengeance&quot;)</td>
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<td>Rebel</td>
<td>Strongly Increased Support</td>
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<td>(&quot;Credit Capture&quot;)</td>
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**Credit capture:** Settings characterized by the counterinsurgent’s use of indiscriminate violence in a rebel-dominated space are likely to yield the worst possible outcomes (from the counterinsurgent’s perspective). We are likely to witness a substantial increase in support for the insurgents in the aftermath of these events. In these situations, the effect on support for insurgents is magnified by the fact that insurgents can engage in “credit capture” by claiming that the delivery of humanitarian assistance is due to their forbearance. Counterinsurgent violence here not only reinforces preexisting biases against the out-group but also creates opportunities for insurgents to subvert counterinsurgent programming for their own purposes. Counterintuitively, however, this increase in support for the insurgents likely coincides with decreased insurgent violence. From the insurgent’s standpoint, the counterinsurgent’s violence has solidified support in the given area, reducing the need to conduct attacks against these forces to demonstrate their resolve and strength. Instead, insurgents are more likely to redirect their violence against relatively more pro-government areas (Hirose, Imai and Lyall [2014]). Humanitarian assistance may therefore displace insurgent violence rather than ameliorate it.

**Wedge strategy:** A combination of insurgent indiscriminate violence and counterinsurgent dominance represents the “best case” scenario for humanitarian assistance to boost
government support and reduce insurgent attacks. In these settings, aid can be used as a “wedge strategy” to separate civilians from the insurgents by blaming rebels for harming civilians. Counterinsurgents also retain much higher control over the delivery of assistance as well as the narrative surrounding its delivery. Preexisting bases of identification with the insurgents may limit the magnitude of attitudinal shifts; counterinsurgents may be setting themselves up for disappointment if they expect sharp swings in public sentiment. Insurgents enjoy a cushion of support among their fellow in-group members that dampens their loss of support. Nonetheless, these small shifts toward supporting the counterinsurgent may shift insurgent targeting, resulting in increased attacks as insurgents seek to forestall the further erosion of their support in these areas.

**Home team discount:** A mixture of insurgent indiscriminate violence and territorial control represent something of a “null” category. If civilians and insurgents share social ties and common identification, as is typically the case, then civilians here are likely to punish rebels weakly, if at all, for their indiscriminate violence. This “home team discount” is the product of civilian beliefs about the situational nature of insurgent violence — namely, that insurgents are forced to carry out these types of attacks because of the counterinsurgent — that creates a floor of support that is difficult to breach. Moreover, insurgent control creates additional opportunities for credit capture, dampening any possible pro-counterinsurgent shifts that might spring from the provision of aid. We can expect that both civilian relative support for the insurgents and the overall pattern of violence to remain relatively unchanged in these areas after the provision of humanitarian assistance.

In short, this categorization scheme of the conditional effects of humanitarian assistance suggest that the range of settings where aid has positive effects (from the counterinsurgent’s perspective) is fairly narrow. Humanitarian assistance may have other effects on at-risk civilian attitudes and behavior (see next section). But aid efforts are likely to fall short of desired outcomes, and may even prolong the conflict, if they are implemented in the wrong areas. In particular, these aid programs are unlikely to have the intended political effects due to the combination of (1) prior ideological or ethnic bonds that create in-group bonds with insurgents; (2) exposure to victimization by the out-group, which reinforces these biases; and (3) the possibility that aid will be captured or subverted by insurgent organizations to bolster their own standing among local populations.\(^8\)

\(^8\)This conditional view of aid’s effects also injects a significant measure of politics back into our anal-
2 Alternative Explanations

Three broad theoretical camps connect civilian victimization to increased support for, and participation within, an insurgency. These theories rest upon multiple individual-level mechanisms (sometimes shared across camps) that, for reasons of data availability, have rarely been tested directly. These theories implicitly assume that these increased support and participation are conditional on the presence of armed groups in neighboring areas where victimized individuals could act upon their (new) preferences. In addition, these camps tacitly imply that individual-level attitudes and behaviors “scale up” to produce observable patterns in insurgent violence.

Grievances. Perhaps the most common explanation for the link between civilian victimization and subsequent insurgency is also the simplest: revenge. Inflicting harm among civilians indiscriminately has frequently been identified as pushing fence-sitting individuals into the insurgency as a vehicle for satisfying revenge motives (Petersen, 2002; Longo, Canetti and Hite-Rubin, 2014; Kalyvas, 2006; Kocher, Pepinsky and Kalyvas, 2011; Condra and Shapiro, 2012; Bennett, 2008; Baicells, 2011; Nagl, 2005). More specifically, indiscriminate violence by the counterinsurgent is thought to increase support for the insurgency, dry up the flow of tips to the counterinsurgent, and boost the frequency and lethality of insurgent attacks as aggrieved individuals cast their weight behind the insurgency. So-called “hearts and minds” approaches also rest on this logic of aggrieved civilians taking up arms to exact vengeance on their victimizers. Counterinsurgents seek to counterbalance the negative effects of their violence while taking advantage of insurgent indiscriminate actions by providing assistance in the aftermath of civilian casualty events (Department of the Army, 2014; Beath, Christia and Enikolopov, 2011; Berman, Shapiro and Felter, 2011; Akerlof and Yellen, 1994; Vanden Eynde, 2013; Findley and Young, 2007). As a result, we should expect a symmetrical response from aggrieved individuals in the form of (1) decreased support for the responsible party; (2) increased support for the non-responsible party; and (3) the provision of assistance to reduce the loss of support (if harmed by the counterinsurgent) or increase its magnitude (if harmed by the insurgents). All else equal, violence should be reduced in locations that receive assistance relative to similar locations.
that did not.

**Opportunity Cost.** Civilian victimization also causes economic hardship, leaving individuals more prone to support or join insurgent organization since the opportunity costs of doing so have been lowered (Becker 1968; Gurr 1970; Blattman and Ansan 2014). Sudden unemployment and the destruction of property — particularly farms in agrarian societies — or remaining capital may conspire to raise the attraction of joining insurgencies to receive a regular income (Lyall 2013; Banarjee and Duflo 2011; Walter 2004). Exposure to indiscriminate violence may also shape an individual’s sense of future economic prospects, breeding a sense of fatalism that may render individuals more vulnerable to insurgent recruitment pitches. In this view, receipt of post-event humanitarian assistance increases the opportunity costs of participating an insurgency, leading to reduced support for insurgent groups and decreased violence in recipient communities.

**Trauma.** An emerging literature now seeks to explore how trauma restructures an individual’s cognitive processes in the aftermath of exposure to violence (Canetti and Lindner 2014). An individual’s willingness to accept risk, for example, may be affected by violence; the lower the risk acceptance, the less likely an individual is to join an insurgency in most circumstances. Exposure to violence may also result in the hardening of attitudes toward the out-group (e.g., an external occupier), greater acceptance of radical solutions to political conflict, and a greater desire for stability that may translate into greater support or participation within insurgency to escape the state’s use of indiscriminate violence (Kalyvas and Kocher 2007). In this view, exposure to violence traumatizes individuals, lowering their support for the responsible party. This trauma should manifest itself as a heightened sense of powerlessness and a willingness to consider risky actions, including aiding (or even joining) the non-responsible party. Timely humanitarian assistance should “short-circuit” this radicalization-through-trauma mechanism, restoring optimism and a sense of agency and thus reducing post-event attacks against the counterinsurgent relative to other similar but non-recipient locations.

9An emerging literature has argued, however, that exposure to violence can increase individual altruism and prosocial behavior (Blattman 2009; Gilligan, Pasquale and Samii 2014). If correct, then trauma may lead individual to seek peaceful alternatives to violence, reversing the supposed link between state-inflicted trauma and subsequent support for and participation in an insurgency.
3 Intervention: The ACAP II Program

There was little warning that the rapid toppling of Afghanistan’s Taliban regime in the immediate aftermath of the 11 September 2011 would embroil the United States (along with its NATO allies) in the longest war of its history. Yet the fall of Kabul created a political vacuum that gave breathing room for a weakened Taliban to reconstitute its forces. By 2006, the ISAF was confronted by a widening, and grinding, insurgency that forced a “surge” of combat forces in 2009 to bolster Kabul’s flagging fortunes. Tied to a mercurial President Karzai, ISAF and various international agencies and institutions sought to marry the selective use of violence with large sums of development funds (now exceeding the Marshall Plan in cost) to convince Afghans of the central government’s legitimacy. By early 2014, however, progress at extending Kabul’s writ had proven illusive at best as emboldened Taliban forces consolidated their gains throughout eastern and southern Afghanistan. While ISAF’s mission officially ended in December 2014, the US has retained at least 5,000 soldiers in Afghanistan until end of 2016, if not later.

ACAP II was designed to alleviate suffering from indiscriminate violence that met two of three criteria: (1) the injured parties were civilians; (2) these individuals were harmed via direct action by ISAF; or (3) these individuals were harmed by the Taliban due to the indirect presence of ISAF in a given area. Examples of direct ISAF actions include errant airstrikes, ISAF-initiated military operations in villages, and traffic accidents. Events in which the Taliban attack ISAF convoys or military facilities but miss — say, a roadside improvised explosive device fails to detonate in a timely fashion, missing a passing ISAF patrol and instead striking a trailing civilian bus — are also deemed eligible due to the “indirect” presence of ISAF forces. Though intended as a humanitarian program, its model of aid closely resembles counter-violent extremism (CVE) programming that aims to reduce the “push” and “pull” factors into an insurgency by replacing material losses, “hardening” individuals against insurgent recruitment appeals.

More specifically, ACAP II distributed two aid packages: (1) Immediate Assistance, which consisted of household supplies, building materials (e.g. shelters), and foodstuffs; and (2) Tailored Assistance, which involved more specialized additional services, including references to psychiatric counseling, medical assistance, vocational training, small business grants, and livestock. All Tailored Assistance beneficiaries also received Immediate Assistance. All assistance was in-kind; no cash was provided. The approximate value
of immediate assistance was $300 US per beneficiary (typically the head of household or family-nominated representative), while tailored assistance averaged approximately $4,000 US per beneficiary.

The program was funded by the United States Agency for International Development (USAID) and implemented by International Relief and Development (IRD) in partnership with the Afghan Ministry of Labor, Social Affairs, Martyrs, and Disabled (MoLSAMD). ACAP II retained its own extensive monitoring and incident verification system comprised of local police, non-governmental organizations and police, along with district and provincial authorities and USAID On-Site Monitors (OSM). While ACAP II was solely responsible for identifying and investigating civilian casualty events, USAID required ISAF’s Civilian Casualty Tracking Cell (CCTC, later the Civilian Casualty Mitigation Team, CCMT) to confirm the presence of ISAF forces before an event was deemed eligible for assistance, a point I return to below.

In total, ACAP II investigated 1,061 incidents involving civilian victimization from 7 October 2011 to 14 September 2013. A total of $9.3 million ($5.4 million was Tailored Assistance) in assistance was delivered over this time period. An estimated 5,488 families received assistance over this time period, totaling 29,467 individuals, including 8,314 men, 5,910 women, and 15,423 children.

4 Empirical Strategy

There’s clearly no shortage of methodological and logistical hurdles to overcome when estimating the causal effects of humanitarian programs in wartime. A short list of obstacles would include: the inability to collect individual baseline data given the unexpected nature of civilian casualties; selection bias arising from the non-random targeting of locations or civilians; the potential absence of relevant counterfactuals; attrition bias arising from difficulties locating beneficiaries after aid has been distributed; recall bias that might be serially correlated with the severity of the incident; and the need to elicit truthful answers to sensitive questions when incentives encourage preference falsification and misrepresentation. Moreover, our usual response — use a randomized control trial to estimate aid’s effects — is infeasible given ethical and logistical issues (Puri et al. 2014, iv-v).

10 Total program costs were estimated to be nearly $29 million, 2011-13. Office of Inspector General 2014.
Prior studies have sought plausibly exogenous mechanisms by which individuals or locations are exposed “as-if” randomly to indiscriminate violence. These mechanisms include (1) inaccurate Hamas’ rocket fire into Israel (Getmansky and Zeitzoff 2014), random Russian artillery shelling in Chechnya (Lyall 2009), German aerial bombardment during the London Blitz (Clarke 1946), block-by-block street crime in New York City (Sharkey et al. 2014), random variation in successful assassination attempts (Jones and Olken 2009), the haphazard abduction of recruits into rebel organizations in Uganda (Blattman 2009), and rough terrain’s constraining effects on combatant violence in Nepal (Gilligan, Pasquale and Samii 2014).

I build upon these tradition using a nested design that integrates as-if random assignment of individuals and villages to ACAP II humanitarian aid eligibility. The treatment assignment here has two levels, one nested inside the other (summarized in Figure 1). First, incident eligibility at the village level was subject to ISAF verification, a process that was highly haphazard during the 2011-12. As detailed below, the result is that only about half of the eligible incidents actually received ACAP II assistance as ISAF’s cumbersome and understaffed verification process could not conform incidents in time to meet ACAP II’s deadlines for rendering “timely” assistance. Hundreds of incidents were therefore summarily abandoned even though they met ACAP II’s own criteria for programming, creating “control” observations that experienced civilian casualty events but were denied aid. Next, I exploit the inherent randomness of indiscriminate violence in causing civilian casualties and property damage. If truly as-if random, then exposure to indiscriminate violence should be orthogonal to an individual’s traits, creating a two-control group comparison between those who receive ACAP II assistance and those that do not, as well as those that receive immediate assistance and those that receive more extensive tailored assistance.\footnote{“True” controls — individuals harmed but denied ACAP II assistance — are excluded on ethical grounds.}

This nested approach combines survey data among 3,045 recipients and non-recipients with declassified village-level event data on insurgent attacks against ISAF forces to estimate the effects of ACAP II assistance. Qualitative interviews with ACAP II, ISAF, and USAID personnel were also conducted to validate these as-if assumptions and to shed additional light on allocation decisions to beneficiaries (Dunning 2012, 212-14; Lyall 2015, 199-203.)
Civilian Casualty Incidents (N=1,061)
- Not eligible (N=469)
- Eligible (N=592)
  - Not Harmed (No Assistance) (N=1,007)
  - Harmed (Received IA) (N=1,314)
    - (Received IA + TA) (N=724)

Village Level (Event Data) N=607 Villages

Individual Level (Survey Data) N=3,045 Respondents (268 Incidents)

Figure 1: Nested Design. “A” is randomness of civilian victimization (yes/no) and its properties. “B” involves ACAP II’s abandoning of incidents due to ISAF’s delay in confirming incident eligibility. “C” is plausibly exogenous exposure to indiscriminate violence at the individual level. “D” is program-level “as-if” randomness into selection of immediate or tailored assistance.

4.1 Treatment Assignment

Several aspects of ACAP II programming facilitate both the identification of plausibly exogenous exposure to violence and an appropriate sample for testing its effects.

Village

ACAP II investigated a broad class of incident types where civilians were hurt and property damaged accidentally as a result of ongoing hostilities. Eligible ISAF-initiated events include: airstrikes (including accidental weapons releases); military operations and night raids that accidentally killed civilians, including during night raids; road accidents; and es-
calation 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 killed or wounded passersby on highways; military operations that accidentally hurt civilians; and rocket attacks against ISAF bases and patrols where the (crude) rockets missed their targets. Actions where the Taliban deliberately targeted civilians (i.e. reprisals) but where ISAF was not present were excluded.

Second, ACAP II provided assistance after both ISAF- and Taliban-initiated events; there is no selection effect arising from focusing on one combatant or class of incident. As ACAP II’s own eligibility criteria state: “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 eligible for ACAP II assistance.” Nor was ACAP II’s mandate geographically restricted. Instead, ACAP II responded to both ISAF- and Taliban-initiated events in Taliban, contested, and ISAF-dominated areas. For example, a simple comparison of mean insurgent attacks reveals almost identical levels of pre-incident Taliban violence — 3.58 attacks in Taliban-initiated events versus 3.28 attacks in ISAF-initiated events — in the days preceding the civilian casualty event.

Unusually, however, authority for determining incident eligibility was not held by ACAP II but rather by ISAF itself. Once notified of an incident—either by local actors/media or their own sources—on-site managers (OSMs) stationed on military installations would first investigate the incident. They then would request confirmation of civilian and ISAF involvement from ISAF’s own Civilian Casualty Tracking Cell (CCTC—later, CCMC). Because the CCTC’s own investigative capacity was limited, however, a severe bottleneck in determining eligible status quickly emerged as ACAP II programming expanded. In fact, the CCTC was chronically short-staffed, with an initial staff of only three individuals. Though that total would increase to approximately eight individuals over time, the CCTC did not possess the ability to initiate its own investigations and relied on ISAF for incident notification and verification.

USAID’s own performance review of the ACAP II program 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](https://www.management-systems.org), 2015, 21), concluding 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 a review of the CCTC, similarly concluded that ISAF had “severely limited” ability to do on-site investigations due to the drawdown of soldiers and that it simply could not keep pace with the demands for incident verification ([Keene](https://www.civic.org), 2014).

As a result, only 592 of the 1,061 incidents ACAP II investigated would actually be deemed eligible. In nearly all cases of non-eligibility, ISAF never issued a verdict. Instead, ACAP II was forced to abandon these cases because it could no longer mount a timely response; too much time had elapsed from the incident date. In fact, by 2013 so many pending cases had grown “stale” that ACAP II was essentially forced to wipe its books clean and start over with a new arrangement for confirming casualties, this time in partnership with the United Nations Assistance Mission to Afghanistan (UNAMA). These “ineligible” cases met all the requirements for initiating assistance — indeed, nearly all had substantial paper trails from OSMs, local police and media, and UNAMA — but simply did not receive ISAF verification within the allotted time frame for rendering immediate assistance. In this framework, only ISAF could determine eligibility; these additional third-party sources could be used only for generating, not confirming, incident reports. [12]

Table 2 provides an overview of the (in)eligible incidents broken down by responsible party. It is worth emphasizing that the eligible events are divided almost evenly between ISAF- and Taliban-initiated events. The program was not, for example, selectively responding to only one combatant’s actions, nor was it privileging responses to particular types of events such as high-profile mass-casualty events such as suicide bombings. And as Figures 2 and 3 illustrate, ACAP II investigated and programmed throughout Afghanistan, though southern and (especially) eastern Afghanistan received the majority of programming given the extremely violent nature of these areas.

[12] This discussion is based on interviews with ACAP II, CCTC, USAID, and MISTI officials. All sides expressed frustration with ISAF rulings, citing its “haphazard” and “chaotic” nature and noting how low bureaucratic capacity, frequent personnel turnover, and changing personal dynamics all contributed to long delays in determining eligibility.
Table 2: Eligible and Ineligible Incidents, By Event Type

<table>
<thead>
<tr>
<th>Event Type</th>
<th>Eligible</th>
<th>Ineligible</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>ISAF-initiated</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Traffic Accident</td>
<td>57</td>
<td>14</td>
</tr>
<tr>
<td>Airstrike</td>
<td>63</td>
<td>67</td>
</tr>
<tr>
<td>ISAF Indirect Fire</td>
<td>10</td>
<td>6</td>
</tr>
<tr>
<td>ISAF Military Operation</td>
<td>140</td>
<td>102</td>
</tr>
<tr>
<td>Escalation of Force (EOF)</td>
<td>12</td>
<td>4</td>
</tr>
<tr>
<td><strong>Sub-Total</strong></td>
<td>282</td>
<td>193</td>
</tr>
<tr>
<td><strong>Taliban-initiated</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Improvised Explosive Device (IED)</td>
<td>110</td>
<td>129</td>
</tr>
<tr>
<td>Taliban Indirect Fire</td>
<td>57</td>
<td>29</td>
</tr>
<tr>
<td>Taliban Military Operation</td>
<td>60</td>
<td>20</td>
</tr>
<tr>
<td>Suicide Bombing</td>
<td>43</td>
<td>17</td>
</tr>
<tr>
<td><strong>Sub-Total</strong></td>
<td>270</td>
<td>195</td>
</tr>
<tr>
<td><strong>Unclear Responsibility</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Crossfire</td>
<td>40</td>
<td>81</td>
</tr>
<tr>
<td><strong>Total</strong></td>
<td>592</td>
<td>469</td>
</tr>
</tbody>
</table>
Figure 2: Distribution of ACAP II eligible (blue) and ineligible (red) civilian casualty events, 2011-13.
Figure 3: Density plot of ACAP II eligible programming.
Individual

ACAP II’s eligibility criteria sought to create a uniform population of civilians who were targeted accidentally as a by-product of ISAF-Taliban violence; victims were not directly targeted by either warring party. “A civilian,” as defined by ACAP II, was defined as “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 eligible if harmed by US and Coalition Forces.” As a result, individuals in any Afghan security force, government officials, or directly employed by US/ISAF forces were not eligible to receive assistance. ACAP II also worked to vet potential beneficiaries to ensure that they did not have a prior history of involvement in the insurgency, though these efforts by definition cannot be considered complete. As a result, the sample population is composed exclusively of civilians rather than soldiers or insurgents where assumptions of as-if random targeting are less credible.\[^{13}\]

In short, these civilian casualty events had shared properties that make assumptions of “as-if” randomness credible: they possessed a high degree of contingency in terms of who was hurt, how badly, where, and by which combatant, and the sample is composed exclusively (to the best of our knowledge) of civilians, individuals who are at-risk of supporting or becoming insurgents due to their exposure to violence.

4.2 Data

Village Level

I measure insurgent violence using declassified data from ISAF’s Combined Information Data Network Exchange (CIDNE), which records the precise location, date, and type of insurgent attack against ISAF forces and installations across 17 specific categories. I use counts of these attacks before and after specific time windows (up to 180 days before/after the delivery of assistance) and spatial buffers (1-km, 5-km, and 10-km radii around the village) to generate difference-in-difference estimates of changes in insurgent attacks across recipient and non-recipient villages. I also disaggregate insurgent violence into attacks using improvised explosive devices (IEDs) and those drawing on other attack types, including

\[^{13}\text{If an individual is killed during one of these incidents, the family appoints a representative (typically another family member) to act as the beneficiary.}\]
small arms fire and indirect fire\textsuperscript{14}

I also draw on multiple village-level covariates to adjust for possible imbalances between eligible and ineligible incidents. I record the number of civilians killed and wounded in a given incident (logged) and use a binary indicator to reflect whether property had been damaged during the incident. I also include measures for the estimated village population (logged), its elevation (logged), and its distance in logged kilometers from the capital city, Kabul, as a measure of remoteness. To control for conflict-related factors, I include measures for the distance in logged kilometers between the village and the nearest ISAF or Afghan National Security Force (ANSF) military installation; the logged number of bases within a 3 km\textsuperscript{2} radius\textsuperscript{15} and include a binary indicator denoting the Afghan “fighting season” (April to September). I also control for prior ISAF military operations using a count variable of actions over specified temporal and spatial windows around the village. I also record each village’s district and province to control for processes at more aggregate levels of political organization.

\textbf{Individual Level}

A 3,045 respondent survey was conducted in four cycles (March, June, September, November/December) during 2013 to measure ACAP II’s individual level effects. Respondents were chosen randomly from ACAP II’s list of beneficiaries. Our sample includes 1,314 immediate assistance and 724 tailored assistance recipients. To create a baseline, 1,007 interviews were also conducted from randomly sampled individuals who lived in the same village as the beneficiaries and who were present during the civilian casualty incident but who were not harmed and thus received no assistance\textsuperscript{16} In total, respondents were drawn from a randomly-selected 268 of ACAP II’s 592 eligible incidents (45%). Surveys were conducted in 16 provinces, ensuring excellent geographic coverage. The three most heavily sampled provinces are Wardak (23%), Khost (16%), and Farah (7%). Further sample breakdowns by cycle, province, and aid type are provided in the Appendix in Tables \textsuperscript{8}

\textsuperscript{14}For additional details on CIDNE, see \cite{Lyall, Blair and Imai 2013}.

\textsuperscript{15}As a robustness check, I reestimate the models with the number of bases (logged) within 1km\textsuperscript{2} and 10km\textsuperscript{2}.

\textsuperscript{16}These individuals were identified using Kish grid random sampling. Two screening questions were employed: (1) were you present during incident X? and (2) Have you or your family members received assistance from ACAP II, a program that is run by IRD that is designed to help families recover from violent incidents?
Beneficiaries became eligible for these surveys once their case had been “closed,” meaning that assistance had been delivered.

While tracking individuals in war zones can be a difficult proposition, ACAP II’s field teams managed to contact and interview nearly all of the selected beneficiaries; only 4.8% could not be found. The response rate on the non-beneficiaries was similarly high; 81% of those initially contacted participated, while 16% were deemed ineligible because they were not present during the incident (only 3% refused to participate). We continued to select non-beneficiaries until we reached 1,007 respondents. Nearly 21% of all surveys were subjected to quality control call-backs. There were four versions of the survey instrument; these were balanced across the entire sample. The vast majority of our respondents were Pashtun (82%); the next largest ethnicity was Tajik (11%). All but 28 respondents were male. The average respondent had six years of formal schooling and access to slightly more than four hours of electricity per day.

We might worry about selection effects creeping into our sampling design via attrition: the most violent areas might be off-limits for programming, for example. In fact, this was rarely the case; we experienced very little difficulty in actually entering villages and identifying respondents. Moreover, beginning in our second wave, we allowed for telephone interviews to be conducted if beneficiaries resided in completely inaccessible areas. Only 142 of our respondents (4.6%) were surveyed in this fashion. A full 84% (N=2,550) were conducted face-to-face in their homes. The remaining interviews were conducted in the district (N=326, 11%) or provincial (N=27, 1%) centers.

In addition to standard demographic questions, purpose-built modules were constructed to measure recipient grievances against the local and central government; mental health and resiliency; post-event economic recovery and outlook; and relative support for combatants using an indirect questioning techniques known as an endorsement experiment. A module was also designed to measure the recipient’s satisfaction with the ACAP II program (i.e., the nature and delivery of economic assistance) and his understanding of its aims and procedures. Taken together, the survey had 25 management questions, 3 screener questions, 53 substantive questions, and 21 demographic questions. The survey was designed to be answered quickly to avoid re-traumatizing beneficiaries or creating undue security risks for

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17 Note, however, that non-beneficiary interviews could not be conducted in inaccessible areas, creating some differences in the control/aid distribution. That said, the control and aid receipt interviews balance nearly identically on prior levels of violence, suggesting that ACSOR was using criteria other than violence to determine accessibility.
enumerators and recipients alike. Average completion time was 32 minutes with a range of 20-76 minutes.

4.3 Checking “As-if” Randomization

While qualitative evidence is important for assessing the validity of as-if random assumptions, these survey and event data can be used to interrogate more formally these exogeneity claims at the individual and village level.

Village

We also treat the as-if random assignment of village-level eligibility with caution. Perhaps ISAF merely acknowledged the least violent incidents to maintain plausible deniability, if only with Western audiences. Or, conversely, perhaps ISAF was prodded to action only by the most egregious incidents that could not be ignored for political reasons\textsuperscript{18} Or perhaps ISAF was predisposed to only acknowledge Taliban-initiated incidents, burying its own mistakes in bureaucratic delay.

None of these concerns hold, however. In fact, as Table \ref{table:balance} demonstrates, coefficients from regressing eligible status on nearly 40 different covariates suggest remarkably good balance across eligible and non-eligible incidents, as expected if bureaucratic delays were orthogonal to the nature of these incidents. These covariates include casualty counts (logged), property damage, village-level characteristics, a measure of distance from Kabul to indicate likelihood of an incident being picked up in the (inter)national media (Weidmann, 2015), and fixed effects for year and for each province (excluding those with only one observation).

The design is not perfect, of course. Six covariates (out of 37) exhibit statistically significant differences at the \textit{p}=.05 level, higher than what we might expect from a randomized experiment. There’s some regional heterogeneity in eligibility, for example, with certain provinces (Kapisa, Ghazni, Khost) exhibiting a greater probability of their incidents being included. Indicator variables for 2011 and 2012 both confirm the problems with eligibility; it became less likely that incidents would be deemed eligible in 2013 compared to past years as ACAP II struck a large number of incidents from its roster (Management Systems International, 2015, 22).

\textsuperscript{18}This practice is known as “reputation protection” in ISAF’s parlance.
Substantively, however, eligible and non-eligible incidents are very similar. Villages where incidents were deemed non-eligible were only 14 meters higher than villages with eligible incidents, for example, and were only an average of 26 kilometers farther from Kabul. An average of 1.72 individuals were killed and 2.65 wounded in non-eligible incidents compared with 1.5 and 3.38 in eligible incidents, respectively. Property damage is also similar: 41% of incidents had property damage in non-eligible incidents, compared with 45% in eligible incidents. Eligible and non-eligible incidents had nearly identical levels of prior ISAF and insurgent violence, including the use of improvised explosive devices. And as Table 12 reveals, the eligible and non-eligible samples are balanced both in terms of combatant and incident type (with the exception of crossfire incidents). These similarities notwithstanding, I use regression analysis to control for remaining imbalances across the sets of villages.

Individual

If an individual’s exposure to violence is plausibly exogenous to their traits and location, we should expect to observe balance on a rich set of covariates between those who received and did not receive ACAP II assistance. We should also observe balance between individuals who received IA and TA since their assignment to treatment type was orthogonal to their traits.  

Are these groups in fact balanced? Table 10 reports coefficients and p-values on assignment to aid eligibility from a logistic regression with 25 different covariates on treatment indicators for receiving aid (versus non) and immediate assistance (versus tailored assistance). These covariates include individual level traits such as age, ethnicity, employment status, income levels, hours of electricity daily, education (state and religious), and size of household. I also include incident level data, including whether individuals were killed, wounded, or property damaged.

Village level data, including population, elevation, pre-incident levels of ISAF and insurgent violence, and the degree of control exercised by the Taliban over the given location, is also included. Indicator variables are also included for whether ISAF-initiated the event,

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19ACAP II had its own criteria for assigning IA or TA to beneficiaries: they investigated whether “the losses are affecting the family livelihood (yes/no)” and explored the affect of the incident on a family’s monthly income. In practice, however, these criteria were difficult to observe uniformly, and IA and TA recipients are extremely similar to one another, including on the dimension of family income.
whether the event occurred during the fighting season, the village’s proximity to ISAF military bases, and the distance between the village and its district center (a measure of remoteness). Finally, survey level information is also included, including the time lag between the incident and the survey, whether the individuals lived outside the district where the incident occurred, and the month and length of the survey itself.

As expected, there are very few differences (measured as statistically significant differences at the $p=0.05$ level) between individuals who did and did not receive assistance. Similarly, the IA and TA recipients are also remarkably similar (columns 7 and 8), though there is some imbalance in terms of the incident level measures of harm. A closer look at individual level measures of harm—ACAP II recorded whether the beneficiaries experienced a death in the family; physical injuries; and property damage — suggest that TA recipients are more likely to have experienced a fatality, that they are nearly identical in terms of injuries, and that IA recipients were more likely to have property damage. While these differences likely reflect programming decisions, it is noteworthy that these differences are not reflective of ACAP II’s own criteria for selecting aid types. Nonetheless, I use regression analysis below to control for any potential confounding differences arising from possible selection into different aid types.

5 Results

5.1 Village-Level Effects

What effects, if any, did ACAP II programming have on subsequent insurgent violence? And is there a link between the individual level mechanisms and more aggregate patterns of violence? The plausibly as-if random nature of assignment to eligibility at the village level creates the possibility of identifying causal effects via difference-in-difference estimation. In the analysis below, I first estimate the effects of ACAP II programming on insurgent attacks against ISAF in the immediate aftermath of a civilian casualty event, when ACAP II has announced that aid will be forthcoming but none has ben received yet (Crost, Felter and Johnston, 2014). I then estimate ACAP II effects once aid has been disbursed.

20 Despite the moniker “immediate” assistance, it does take a while to get the aid to the civilian casualties. In 2011: 134 days to get IA to the village (n=61), in 2012, 112 days (n=381), and only 58 days in 2013 (n=150).
use SQL to generate different temporal windows dynamically to assess changes in attacks, including up to six months after aid has been distributed. All counts of insurgent violence are taken within a 2km radius for settlements under 50,000 inhabitants; a wider 5km radius is employed for locations with $\geq 50,000$ inhabitants. I first report the pooled effect across all types of incidents, then separate by ISAF and Taliban-responsible.

**Post-Announcement**

Beginning with the announcement period (Table 3), we observe a negative relationship between ACAP II assistance and insurgent violence at both the 7- and 30-day post-announcement marks. Substantively, a -0.220 reduction in mean insurgent attacks at the 7-day post-announcement mark (Model 2) represents approximately a 23% reduction (with 95% confidence interval at [52%, 1%]) in insurgent attacks. At the 30-day mark, the introduction of ACAP II assistance is associated with a -0.678 reduction (Model 2) in mean insurgent attacks, representing about a 17% decrease (with 95% CI at [36%, 1%]) in subsequent insurgent attacks.

Our proposed conditional effects theory of humanitarian aid would anticipate, however, that ACAP II effects hinge on the perpetrator’s identity. I therefore split the samples into ISAF- and Taliban-initiated events and then reestimate the same models. While both ISAF- and Taliban-initiated events are associated with a decrease in mean insurgent attacks, these results are only statistically significant for ISAF-initiated events. At the 30-day mark, the announcement of ACAP II assistance is associated with a 30% reduction in mean insurgent attacks after ISAF events (95% CI at [60%, 6%]). By contrast, the magnitude of the decrease in mean attacks is far smaller for Taliban-initiated events and does not reach conventional levels of significance in any model.\(^{21}\)

\(^{21}\)In keeping with the closeness of fit between treated and control villages, few village-level covariates are consistently statistically significant. I therefore report only the treatment estimate.
Table 3: ACAP II Effects on Insurgent Violence After Aid Announcement

<table>
<thead>
<tr>
<th>Variables</th>
<th>All Events</th>
<th>ISAF</th>
<th>Taliban</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Model 1 (ALONE)</td>
<td>Model 2 (ALL)</td>
<td>Model 3 (ALONE)</td>
</tr>
<tr>
<td>Eligible: 7 days</td>
<td>-0.206* (0.103)</td>
<td>-0.220* (0.110)</td>
<td>-0.224 (0.166)</td>
</tr>
<tr>
<td>Constant</td>
<td>0.140 (0.100)</td>
<td>4.721*** (1.025)</td>
<td>0.104 (0.127)</td>
</tr>
<tr>
<td>F-test</td>
<td>4.00* (0.368)</td>
<td>6.50*** (0.313)</td>
<td>1.81 (0.586)</td>
</tr>
<tr>
<td>Root MSE</td>
<td>1.963 (0.317)</td>
<td>1.828 (3.452)</td>
<td>1.684 (0.530)</td>
</tr>
<tr>
<td>r²</td>
<td>0.003 (0.684*)</td>
<td>0.145 (–1.593)</td>
<td>0.004 (0.979†)</td>
</tr>
</tbody>
</table>

| Eligible: 30 days | -0.775* (0.368) | -0.678* (0.313) | -1.1458* (0.586) | -1.186* (0.479) | -0.175 (0.463) | -0.624 (0.448) |
| Constant  | 0.684* (0.317) | -1.593 (3.452) | 0.979† (0.530) | 2.615 (4.833) | 0.138 (0.382) | 0.015 (5.474) |
| F-test    | 4.42* (0.684*) | 1.00 (–1.593) | 3.82 (0.979†) | 0.99 (2.615) | 0.14 (0.138) | 2.71*** (0.015) |
| Root MSE  | 5.570 (0.317) | 5.555 (3.452) | 5.497 (0.530) | 5.474 (4.833) | 4.666 (0.382) | 4.572 (5.474) |
| r²        | 0.005 (0.684*) | 0.023 (–1.593) | 0.010 (0.979†) | 0.047 (2.615) | 0.000 (0.138) | 0.069 (0.015) |
| N         | 1061 (1061) | 1061 (1061) | 474 (474) | 474 (474) | 465 (465) | 465 (465) |
| Clusters  | 607 (607) | 607 (607) | 334 (334) | 334 (334) | 288 (288) | 288 (288) |

Note: Robust standard errors clustered on unique village id. † Significant at 10% * Significant at 5% ** Significant at 1% *** Significant at .01%.
Post-Implementation

We observe a similar pattern once aid has actually been distributed (Table 4). In the pooled estimate (Models 1-2), we witness a marked reduction in mean insurgent attacks across 7, 90, and 180 days after aid disbursement. At the 180 post-disbursement mark, the 2.99 decrease represents about a 14% reduction in pre-aid mean insurgent attacks (95% CI at [32%, 1%]).

Table 4: ACAP II Effects on Insurgent Violence After Aid Disbursement

<table>
<thead>
<tr>
<th>Variables</th>
<th>All Events</th>
<th>ISAF</th>
<th>Taliban</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Model 1 (ALONE)</td>
<td>Model 2 (ALL)</td>
<td>Model 3 (ALONE)</td>
</tr>
<tr>
<td>ELIGIBLE: +7 DAYS</td>
<td>−0.231* (0.104)</td>
<td>−0.337*** (0.098)</td>
<td>−0.236 (0.157)</td>
</tr>
<tr>
<td>Constant</td>
<td>0.147 (0.100)</td>
<td>4.284*** (0.974)</td>
<td>0.119 (0.127)</td>
</tr>
<tr>
<td>F-test</td>
<td>4.89* (0.004)</td>
<td>13.71*** (0.167)</td>
<td>2.31 (0.005)</td>
</tr>
<tr>
<td>Root MSE</td>
<td>1.802 (0.004)</td>
<td>1.658 (0.167)</td>
<td>1.594 (0.005)</td>
</tr>
<tr>
<td>( r^2 )</td>
<td>0.004 (0.004)</td>
<td>0.167 (0.167)</td>
<td>0.005 (0.005)</td>
</tr>
</tbody>
</table>

ELIGIBLE: +90 DAYS

| Constant | −1.814* (0.757) | −1.923** (0.674) | −2.722*** (1.055) | −3.036*** (1.099) | −1.317 (1.332) | −0.750 (1.091) |
| F-test | 5.74* (0.006) | 3.35*** (0.033) | 6.65** (0.014) | 2.74*** (0.109) | 0.98 (0.002) | 1.71* (0.048) |
| Root MSE | 11.95 (0.006) | 11.867 (0.033) | 11.369 (0.014) | 10.969 (0.109) | 13.178 (0.002) | 13.139 (0.048) |
| \( r^2 \) | 0.006 (0.006) | 0.167 (0.033) | 0.005 (0.014) | 0.399 (0.109) | 0.002 (0.002) | 0.185 (0.048) |

ELIGIBLE: +180 DAYS

| Constant | −3.232* (1.482) | −2.993† (1.669) | −3.478† (2.024) | −2.507 (1.642) | −4.976† (2.123) | −5.757* (2.808) |
| F-test | 4.76* (0.004) | 12.68*** (0.295) | 2.95 (0.099) | 7.08 (0.274) | 3.23† (0.005) | 24.79*** (0.410) |
| Root MSE | 23.958 (0.004) | 20.316 (0.295) | 17.652 (0.099) | 15.385 (0.274) | 27.722 (0.005) | 21.747 (0.410) |
| \( r^2 \) | 0.004 (0.004) | 0.295 (0.295) | 0.009 (0.099) | 0.274 (0.274) | 0.005 (0.005) | 0.410 (0.410) |

N | 1061 | 1061 | 474 | 474 | 465 | 465 |
Clusters | 607 | 607 | 334 | 334 | 288 | 288 |

Note: Robust standard errors clustered on unique village id. † Significant at 10% *Significant at 5% **Significant at 1% ***Significant at .01%.

Subsetting the data by the perpetrator’s identity uncovers a now-familiar pattern:
ACAP II is associated with a statistically significant decrease in mean insurgent attacks after ISAF-initiated events — four of six regressions return results significant at conventional levels, while the remaining two just miss — while Taliban-initiated events less frequently reach statistical significance. Moreover, all of the statistically significant findings in the Taliban-only regressions are due to the inclusion of Kabul in the sample. The Taliban were assigned responsibility for 12 events in the capital, compared to only two by ISAF. Once these events are dropped from the sample, there are no models in which ACAP II is associated with a statistically significant reduction in insurgent attacks after Taliban-initiated events, while every model in the ISAF-initiated subset returns a statistically significant result. Moreover, the magnitude of the decrease associated with Taliban-initiated events is far smaller than ISAF-only events.

These village-level findings are difficult to reconcile with oft-cited claims that civilian casualties are associated with increased insurgent attacks. They also challenge the implicit assumption that these grievances, once formed, are immutable. On the surface, it does appear that the counterinsurgent is able to reduce the blowback from civilian casualty events, at least after its own indiscriminate violence.

Yet these findings also appear to run counter to the opportunity cost evidence suggested by the survey data, in two ways. First, it is unclear why we would observe asymmetrical responses in magnitude, if not direction, according to perpetrator identity if ACAP I had successfully increased the opportunity costs for supporting or joining the Taliban. Why do we only observe a statistically significant decrease after ISAF-initiated events? Opportunity cost logic would dictate that individuals would be less likely to support the insurgency, perhaps increasing the flow of tips to the counterinsurgent that in turn would permit more selective targeting, reducing insurgent attacks. And increased opportunity costs should also reduce the recruit pool for the Taliban, making it more difficult to replenish their losses and leading to a reduction in overall capacity for generating violence. Second, the magnitude of the decrease in insurgent attacks is not tied to the severity of the events (as measured by casualties or property damage) nor the number of beneficiaries in the village (or the total cost of the ACAP II outlay). If the opportunity cost logic is at work, we should observe more sizable reductions in insurgent attacks as the number of beneficiaries or aid disbursement increases. Here, too, however, we fail to link the individual level survey data with more aggregate patterns of insurgent violence.

These findings are, however, consistent with a “credit capture” interpretation of the
conditional effects of humanitarian assistance. The upswing in Taliban support identified by the endorsement experiments after ISAF-initiated events suggests that ACAP II may be displacing insurgent attacks. The Taliban, now possessing a relatively higher degree of support (and perhaps control) in ISAF-victimized villages, are free to turn their attention to attacking villages that exhibit more pro-government sentiment, a pattern observed in other studies (Lyall, Shiraito and Imai, 2015). As a result, insurgent violence is being redistributed away from ACAP II aid locations and toward locations where the Taliban possess relatively lower levels of support. The modest decline in Taliban attacks after Taliban-initiated incidents may reflect its acknowledgement that civilian casualties can erode its standing (recall the modest downturn in support for Taliban after these events). But as expected we do not witness a sharp downturn in violence (or support), suggesting that the Taliban feel far less pressure from society to curb its violence than ISAF. As a result, ACAP II assistance appears to have only marginal effects on violence after Taliban-initiated events while having counterproductive effects after ISAF-events by consolidating Taliban support and freeing its capacity to strike other, more pro-government, locations.

5.2 Individual-Level

I begin the investigation of ACAP II’s effects at the individual level, comparing ACAP II beneficiaries (both IA and TA) with non-beneficiaries who were present during the violent incident but unharmed. I first estimate the average effects for ACAP II when moving from non-receipt to receipt of assistance. I then reestimate each model with subsets for ISAF- and Taliban-initiated events. Results are presented in Table 5.

All models include standard individual level covariates: age (years, logged); a binary variable for ethnicity (Pashtun or not); binary for fully employed; family income, on a 12-point scale (logged); hours of electricity per day; years of state education and year of madrassa education (logged, both); and number of individuals within the household (logged). Incident level covariates include dummies for killed, wounded and property damage and a “fighting season” (April to September) indicator. Village level factors include logged population, elevation, the number of bases within 3 km of the village (logged) and the distance in kilometers to the district center (logged). Survey-specific measures include time (in days) from the incident to the survey, the actual survey month (to control for time effects across the survey’s four waves), and the length of the interview (minutes, logged).
Standard errors are clustered on village location.

**Grievances**

I measure grievances using a four-fold index of an individual’s level of satisfaction with the provision of goods and services by the district government and Kabul. These four services are: clean drinking water; the level of agricultural assistance; the maintenance of roads; and the provision of electricity. Values range from “service not provided at all” (coded as a “0”) to very satisfied (a “4”). These services were chosen because they are frequently cited as key sources of dissatisfaction that can delegitimize a local or central government. They are also unconnected to ACAP II assistance and so should views should not be affected by prior receipt of this assistance.

Notably, receipt of ACAP II assistance is not associated with increased satisfaction in either the district or central government. In fact, the opposite appears true: nearly all of the regressions are signed in the wrong direction, indicating a decrease in satisfaction. Nor are there any differences across ISAF- and Taliban-initiated events. We cannot conclude, then, that ACAP II aid is “short-circuiting” the creation of grievances toward the Afghan government.\(^{22}\)

**Opportunity Costs**

I assess opportunity cost arguments by investigating an individual’s beliefs about his/her current household income and future economic prospects. Specifically, respondents were asked: “Compared to the time before the incident, do you believe that your household income has increased a lot (2), increased a little (1), remained unchanged (0), decreased a little (-1), or decreased a lot (-2)?” And, second, respondents were also asked: “In the next six months, do you believe your household income will increase a lot (2), increase a little (1), remain unchanged (0), decrease a little (-1), or decrease a lot (-2)?”

Evidence emerges that ACAP II assistance is associated with beliefs that household income has increased since the violent incident, though beliefs about future prospects remain unchanged between aid and non-aid respondents. Intriguingly, the results are strongest af-

\(^{22}\)In a subsequent analysis, I subset these results by territorial control. Evidence suggests that receipt of ACAP II assistance is associated with a sharp decrease in satisfaction in both district and central government service provision after Taliban-initiated attacks in Taliban-dominated villages, indicating that aid as a “wedge strategy” is not working as intended.
ter ISAF-initiated events; these positive benefits of receiving ACAP II assistance do not transfer to Taliban-initiated events. The effect is a sizable one as well: a 0.938 shift in beliefs about current income represents almost a full notch on the 5-point scale of beliefs about improvements to current income.

**Trauma and Socio-Psychological Mechanisms**

I test the effects of trauma and socio-psychological mechanisms using a battery of four questions. These include: (1) an individual’s beliefs about whether they are powerless in their daily lives (*Powerlessness*); their self-assessment of their tolerance for taking risks (*Risk*); their optimism about the future (*Optimism*); and their current interest in activities they previously enjoyed, a common measure for clinical depression (*No Interest*). Once again, the baseline comparison is to individuals who were present in the village during the violent incident but who were not harmed at that time.

I find little evidence that ACAP II assistance has affected either risk preferences or optimism about the future. By contrast, I find a negative effect for beliefs about powerlessness and perceived interest in past activities: ACAP II recipients are more likely to agree that they are powerless in their daily lives, and more likely to report that they have lost interest in past activities, when compared with non-aid beneficiaries. While we cannot conclude that ACAP II assistance is necessarily responsible for these negative effects, we can conclude that receiving ACAP II is insufficient to overcome the negative cognitive effects of exposure to violence. Taken together, these findings suggest that exposure to violence, far from empowering individuals as recent research has argued, is instead generating serious cognitive issues that would appear to point away from prosocial activities. It is likely that an increased sense of powerlessness and apathy would lead individuals away from participation in an insurgency, though we cannot rule out the possibility that these same traits make individuals attractive recruits for certain types of insurgent organizations (principally ideologically driven ones that provide meaning and purpose for an alienated individual).
Table 5: Mechanisms: No Aid to Aid

<table>
<thead>
<tr>
<th></th>
<th>Grievances</th>
<th>Opportunity Cost</th>
<th>Socio-Psychological Mechanisms</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>District</td>
<td>Kabul Current Income</td>
<td>Future Income</td>
</tr>
<tr>
<td><strong>TREATMENT</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Model 1</td>
<td>−0.043</td>
<td>0.325**</td>
<td>0.024</td>
</tr>
<tr>
<td>Model 2</td>
<td>−0.042</td>
<td>(0.037)</td>
<td>(0.118)</td>
</tr>
<tr>
<td>F score</td>
<td>18.73***</td>
<td>11.82***</td>
<td></td>
</tr>
<tr>
<td>Wald χ²</td>
<td></td>
<td>98.71***</td>
<td>54.09***</td>
</tr>
<tr>
<td>Log pseudolikelihood</td>
<td></td>
<td>−4147.07</td>
<td>−4114.46</td>
</tr>
<tr>
<td>r²</td>
<td>0.20</td>
<td>0.16</td>
<td>0.02</td>
</tr>
<tr>
<td>Respondents</td>
<td>3034</td>
<td>3034</td>
<td>2946</td>
</tr>
<tr>
<td>Villages</td>
<td>200</td>
<td>200</td>
<td>200</td>
</tr>
<tr>
<td><strong>TREATMENT (ISAF only)</strong></td>
<td>0.050</td>
<td>0.938***</td>
<td>0.292</td>
</tr>
<tr>
<td>Model 1</td>
<td>(0.083)</td>
<td>(0.065)</td>
<td>(0.173)</td>
</tr>
<tr>
<td>Model 2</td>
<td>(0.045)</td>
<td>(0.133)</td>
<td>(0.124)</td>
</tr>
<tr>
<td>F score</td>
<td>6.45***</td>
<td>4.93***</td>
<td></td>
</tr>
<tr>
<td>Wald χ²</td>
<td></td>
<td>62.41***</td>
<td>90.57***</td>
</tr>
<tr>
<td>Log pseudolikelihood</td>
<td></td>
<td>−907.56</td>
<td>−897.26</td>
</tr>
<tr>
<td>r²</td>
<td>0.20</td>
<td>0.16</td>
<td>0.03</td>
</tr>
<tr>
<td>Respondents</td>
<td>677</td>
<td>677</td>
<td>677</td>
</tr>
<tr>
<td><strong>TREATMENT (Taliban only)</strong></td>
<td>−0.052</td>
<td>0.169</td>
<td>−0.032</td>
</tr>
<tr>
<td>Model 1</td>
<td>(0.048)</td>
<td>(0.133)</td>
<td>(0.124)</td>
</tr>
<tr>
<td>Model 2</td>
<td>(0.043)</td>
<td>(0.133)</td>
<td>(0.124)</td>
</tr>
<tr>
<td>F score</td>
<td>25.66***</td>
<td>20.71***</td>
<td></td>
</tr>
<tr>
<td>Wald χ²</td>
<td></td>
<td>175.37***</td>
<td>130.48***</td>
</tr>
<tr>
<td>Log pseudolikelihood</td>
<td></td>
<td>−3113.76</td>
<td>−3105.64</td>
</tr>
<tr>
<td>r²</td>
<td>0.23</td>
<td>0.20</td>
<td>0.03</td>
</tr>
<tr>
<td>Respondents</td>
<td>2308</td>
<td>2308</td>
<td>2308</td>
</tr>
<tr>
<td>Villages</td>
<td>99</td>
<td>99</td>
<td>99</td>
</tr>
</tbody>
</table>

Note: All covariates in the model with robust standard errors clustered on village. 3,045 total respondents. Missing and refused answers dropped in this analysis. † Significant at 10% * Significant at 5% ** Significant at 1% *** Significant at .01%.
Support for the Combatants

I draw on an indirect survey technique to elicit (more) truthful answers to questions about an individual’s support for the government or the Taliban. As a first cut, I pool together three endorsement experiments into an index of support for these actors. Individuals are read questions about three policy issues: electoral reform; an anti-corruption initiative; and prison reform. Individuals are randomly assigned into one of two groups: policies that are endorsed by the Afghan government or by the Taliban.

I subset the results of these endorsement experiments by the identity of the perpetrator and the distribution of territorial control to illustrate the conditional effects of ACAP II assistance. Table 6 captures support for the Taliban; Table 7 records support for ISAF.

Preliminary analysis suggests that receipt of ACAP II assistance is associated with a sharp increase in support for the Taliban after ISAF-initiated events. By contrast, Taliban support dips slightly after Taliban-initiated events, though this difference is not statistically significant. These findings are consistent with the conditional argument proposed here; namely, the meaning of violent events, and thus ACAP II’s ability to mediate these effects, hinge on preexisting group allegiances. Harmed Pashtuns are unlikely to shift their support behind the counterinsurgent even if repressed by the Taliban. By contrast, the Taliban reap a groundswell of support after an ISAF-initiated civilian casualty event. ACAP II assistance appears unable to overcome these preexisting in-group biases. Moreover, there is evidence that villagers in Taliban-controlled areas credit the Taliban for allowing ACAP II to access these villages. The Taliban are therefore able to “credit capture” ACAP II assistance while ISAF retains the blame for initiating the incident.

Nor does ACAP II assistance translate into improved views of the United States government. Using an additional three endorsement questions (with the United States and the Afghan government as endorsers), I created a second index for support for these combatants. There is no increase in support for the US government relative to the Afghan one after either ISAF- or Taliban-initiated violence. Indeed, these non-findings persist even among individuals who (randomly) received a prompt reminding them that their assistance had been “brought to them by the American people.”

The next version will include estimates using ideal point estimation for these endorsement experiments, following (Lyll, Blair and Imai 2013; Shiraito and Imai 2012).

This encouragement design was meant to mimic USAID branding, which is often thought to diminish legitimacy for the local government. For this debate, see Dietrich and Winters (2015).
reduce violence at the village level, it is unlikely to be working through the mechanisms commonly suggested by hearts and minds approaches.
Table 6: The Effect of ACAP II on Taliban Support, By Perpetrator and Control

<table>
<thead>
<tr>
<th>Control</th>
<th>Perpetrator of Violence</th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>ISAF</td>
<td>Taliban</td>
<td></td>
</tr>
<tr>
<td>ISAF-Dominated</td>
<td>0.202$^+$</td>
<td>0.004</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.078)</td>
<td>(0.089)</td>
<td></td>
</tr>
<tr>
<td>Taliban-Dominated</td>
<td>−0.064</td>
<td>0.271$$^{**}$</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.149)</td>
<td>(0.088)</td>
<td></td>
</tr>
</tbody>
</table>
Table 7: The Effect of ACAP II on US Support, By Perpetrator and Control

<table>
<thead>
<tr>
<th>Control</th>
<th>Perpetrator of Violence</th>
<th>ISAF</th>
<th>Taliban</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td><em>ISAF-Dominated</em></td>
<td></td>
<td>−0.202</td>
<td>0.060</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.146)</td>
<td>(0.062)</td>
</tr>
<tr>
<td><em>Taliban-Dominated</em></td>
<td></td>
<td>0.089</td>
<td>0.048</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.179)</td>
<td>(0.081)</td>
</tr>
</tbody>
</table>
6 Discussion

6.1 Spillover

We might worry that the assumption of relatively immobile individuals is too strict, however. If individuals are highly mobile, then the presumed link between individual-level mechanisms and more aggregate village-level patterns of insurgent attacks will become tenuous. We may also underestimate the true extent of ACAP II’s effects if radicalized individuals leave their original homes, acting as potential conveyers of anti-ISAF sentiment that are invisible to our data collection efforts.

Yet this concern is largely muted by the fact that only 95 individuals (3%) in the entire sample are from a different district than where the violent incident occurred. Moreover, only 16 individuals hail from a different province than where the incident occurred. In short, the assumption of low mobility and of localized effects appears justified in our case.

6.2 Treatment Heterogeneity

What about differences within ACAP II beneficiaries? Do recipients of immediate assistance react differently than individuals who received the more comprehensive tailored assistance? I repeat the same statistical models from above to facilitate within-group comparison between IA and TA recipients (in Appendix Table 13).

In total, there are few differences between these recipients, suggesting that additional assistance is not translating into improved (from ACAP II’s viewpoint) effects. There is some evidence to suggest that receipt of TA assistance does improve an individual’s perception of future income, though only after Taliban-initiated events. Similarly, TA recipients do score better on questions about their current interest in past activities (Model 6). And there is suggestive, if modest, evidence that TA assistance is associated with improved perceptions of the United States relative to the Afghan government when compared to IA recipients. Overall, however, the comparison between no aid and aid recipients is more meaningful than differentiating between IA and TA recipients. These mostly null findings underscore the weakness of the opportunity cost argument: harmed individuals appear largely insensitive to the size and

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For the IA/TA comparison, I added a 4-point question about whether the family received all of the assistance that it was promised and whether they understood the type of assistance they would receive to separate out beliefs about the quality of ACAP II implementation from its effects.
type of ACAP II assistance they received.

6.3 Event Heterogeneity

There is also an important amount of heterogeneity within ISAF- and Taliban-initiated events that bears emphasizing. For example, most of the reduction in insurgent attacks after ISAF-initiated events is associated with ground operations (such as clearing operations) and indirect fire. The magnitude of the decrease in insurgent attacks is far less after ISAF airstrikes, however; indeed, in certain model specification, airstrikes are associated with increased insurgent attacks, the only ISAF category of event to do so. We cannot assume, then, that humanitarian assistance has uniform effects across classes of events, suggesting that programmers need to take into account not only the initiator of the incident but also the type of indiscriminate violence when choosing to respond to civilian casualty events.

6.4 Regional Effects

To be done.

6.5 Generalizability

There are, of course, limits to the generalizability of single country studies. Do these claims about humanitarian assistance travels to other contexts? There are at least three ways to assess generalizability here: as a function of Afghanistan’s specific properties; the war’s characteristics; and the nature of the humanitarian assistance itself.

First, as demonstrated by Hirose, Imai and Lyall (2014), Afghanistan is not an outlier on any of the six dimensions that Fearon and Laitin (2003) use to catalogue civil wars. These include: per capita income, population, mountainous terrain, the regime’s polity score, and ethnic and religious fractionalization. There is little reason, then, to assume that these results are driven by Afghanistan-specific properties.

Nor is the war itself an outlier; it shares many properties of long-running insurgencies since 1945. For example, at least one-quarter of all insurgencies since 1945 have witnessed armed intervention by a third-party counterinsurgent like ISAF (Lyall and Wilson, 2009). Most of these counterinsurgency efforts—including prominent examples in Pakistan, Iraq, Colombia, Mexico, Yemen, and the Philippines—have included extensive “hearts and
minds” campaigns to win over public support. And while crossnational data on civilian victimization is poor, the current war in Afghanistan is not an outlier in terms of the magnitude of civilian deaths. The best public estimates suggest that 2,000—3,500 civilians have been killed each year, most by the Taliban (United Nations Assistance Mission in Afghanistan 2015), placing the war squarely in the middle of the distribution of civil war casualties.

Finally, there is little unique about the nature of ACAP II assistance. Indeed, its model of in-kind, non-cash based, assistance in the aftermath of civilian casualties has been adopted in a wide range of conflicts, including Syria, Yemen, Pakistan (Shah and Shahbaz 2015), and Iraq26. In fact, the ACAP II program was judged so successful that a three-year $30.2 million US successor program (ACAP III) was launched in Afghanistan in September 2015.

7 Conclusion

This paper reaches several conclusions. Above all, it is clear that humanitarian assistance in wartime does not have uniform effects. In fact, in several instances, notably under conditions of Taliban control after ISAF indiscriminate violence, efforts to provide humanitarian assistance to injured parties can be counterproductive, at least when viewed from a war-fighting rather than moral perspective. Moreover, while receipt of ACAP II assistance was associated with a reduction in insurgent attacks relative to similar villages without aid, we must be cautious in concluding that the aid itself was responsible for this decline. Instead, the marked increase in support for the Taliban often coincided with decreased violence, suggesting an alternative mechanism is at work: namely, insurgents are shifting their targets away from villages that, thanks to civilian victimization, are now more firmly in the insurgent camp and toward villages that are still relatively more pro-government in their ideological orientation. In this interpretation, these aid efforts have displaced rather than “solved” insurgent violence, freeing insurgents from the need to ensure popular support through the threat (or actual use) of violence.

These findings suggest several research avenues. The dynamics of blame attribution and, in particular, how meaning is attached to civilian casualty events are still poorly

26Section 8127 of the Consolidated Appropriations Act of 2014 allocated funding to the Pentagon to create a program to help civilians harmed by US actions in wartime settings.
understood and nearly totally absent from existing theories of civil war violence. Simple distinctions between in- and out-group perpetrators of violence can profoundly shape how an event is understood by victims, its effects on support for in- and out-group members, and how these events may (and may not) scale up to produce violence itself. Yet our current theories make little of this distinction, while empirical tests routinely pool together violent incidents regardless of the identities of perpetrators and victims, as if our default assumption should be that violence has homogenous effects. These data also suggest that our “microlevel” theories are typically not microlevel enough. Without individual level data, preferably as panel data, we are often blind to the actual mechanisms and mediating effects that produce and complicate the relationship between violence, aid, combatant support and eventual violence. Theorizing the links between individual and more aggregate data (i.e. at the village level) will enable scholars to scale up these individual level findings into spatially bounded patterns of combatant support and violence. Indeed, a clear picture of the effects of ACAP II humanitarian assistance only emerged when individual data was paired with village level data; without evidence for the individual level mechanisms, for example, we might have erroneously concluded that violence decreased in these recipient villages because “hearts and minds” had been won.

Several policy recommendations also flow from these findings. One lesson is the importance of humility: if the argument proposed here is correct, then there are few situations in which humanitarian assistance will result in a significant shift in popular attitudes toward the counterinsurgent. Reasonable expectations about what this aid can achieve are key. Counterinsurgents adopting such programs are likely to have the greatest chance of success if they program after their own civilian casualty events in locations that they predominantly control. Avoiding credit capture will remain an important operating constraint for many, perhaps most, civilian casualty events among at-risk populations in areas with at least nominal insurgent presence. Similarly, there is evidence of considerable heterogeneity across event types within each combatant’s own civilian casualty events. For counterinsurgents, airstrikes remain an especially difficult counter-programming situation; events marked by direct fire between counterinsurgents and rebels appear to have more favorable post-aid outcomes. Perhaps most importantly, the possibility of counterproductive and cross-cutting effects raises an important prior question for counterinsurgents: are

A shift to providing assistance via cash transfers offers one possible technology that bypasses credit capture opportunities for insurgents and local politicians alike. 

\[27\]
these programs intended to have humanitarian-only effects, in which effects on relative combatant support are ignored, or are these programs designed to win over hearts and minds? If the latter is the case, then the scenarios where aid can be effectively wielded by the counterinsurgent represent only a small subset of the overall range of situations that will confront policymakers in these wartime settings.
References


Appendix

A Event Eligibility Criteria

ACAP II’s eligibility criteria are reproduced below (International Relief and Development, 2012). 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 (International Relief and Development, 2012, 13).

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.
A.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 eligible if harmed by US and Coalition Forces.

2. Afghan civilians who are not eligible 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.

---

28 Those civilians who “Assume Risk” by working with/or for government or security entities.
B Sample

B.1 Sample, by Cycle and Type

Table 8: Sample, by Cycle and Type

<table>
<thead>
<tr>
<th>Cycle</th>
<th>IA</th>
<th>TA</th>
<th>No Aid</th>
<th>N</th>
</tr>
</thead>
<tbody>
<tr>
<td>Cycle 1 (March)</td>
<td>186</td>
<td>142</td>
<td>139</td>
<td>467</td>
</tr>
<tr>
<td>Cycle 2 (June)</td>
<td>397</td>
<td>191</td>
<td>368</td>
<td>956</td>
</tr>
<tr>
<td>Cycle 3 (Sept)</td>
<td>407</td>
<td>189</td>
<td>289</td>
<td>885</td>
</tr>
<tr>
<td>Cycle 4 (Nov-Dec)</td>
<td>324</td>
<td>202</td>
<td>211</td>
<td>737</td>
</tr>
<tr>
<td><strong>Total</strong></td>
<td>1,314</td>
<td>724</td>
<td>1,007</td>
<td>3,045</td>
</tr>
</tbody>
</table>

50
### B.2 Sample, by Province and Type

Table 9: Sample, by Province and Type

<table>
<thead>
<tr>
<th>Province</th>
<th>Cycle IA</th>
<th>Cycle TA</th>
<th>No Aid</th>
<th>N</th>
<th>% of Respondents</th>
</tr>
</thead>
<tbody>
<tr>
<td>Badghis</td>
<td>0</td>
<td>16</td>
<td>8</td>
<td>24</td>
<td>1%</td>
</tr>
<tr>
<td>Farah</td>
<td>96</td>
<td>53</td>
<td>76</td>
<td>225</td>
<td>7%</td>
</tr>
<tr>
<td>Faryab</td>
<td>8</td>
<td>21</td>
<td>32</td>
<td>61</td>
<td>2%</td>
</tr>
<tr>
<td>Ghazni</td>
<td>61</td>
<td>40</td>
<td>62</td>
<td>163</td>
<td>5%</td>
</tr>
<tr>
<td>Helmand</td>
<td>32</td>
<td>68</td>
<td>31</td>
<td>131</td>
<td>4%</td>
</tr>
<tr>
<td>Herat</td>
<td>33</td>
<td>42</td>
<td>33</td>
<td>108</td>
<td>4%</td>
</tr>
<tr>
<td>Kabul</td>
<td>99</td>
<td>30</td>
<td>83</td>
<td>212</td>
<td>7%</td>
</tr>
<tr>
<td>Kandahar</td>
<td>0</td>
<td>24</td>
<td>24</td>
<td>48</td>
<td>2%</td>
</tr>
<tr>
<td>Kapisa</td>
<td>40</td>
<td>48</td>
<td>44</td>
<td>132</td>
<td>4%</td>
</tr>
<tr>
<td>Khost</td>
<td>237</td>
<td>97</td>
<td>149</td>
<td>483</td>
<td>16%</td>
</tr>
<tr>
<td>Kunar</td>
<td>44</td>
<td>39</td>
<td>44</td>
<td>127</td>
<td>4%</td>
</tr>
<tr>
<td>Laghman</td>
<td>18</td>
<td>20</td>
<td>36</td>
<td>74</td>
<td>2%</td>
</tr>
<tr>
<td>Logar</td>
<td>114</td>
<td>40</td>
<td>63</td>
<td>217</td>
<td>7%</td>
</tr>
<tr>
<td>Nangarhar</td>
<td>86</td>
<td>58</td>
<td>56</td>
<td>200</td>
<td>7%</td>
</tr>
<tr>
<td>Paktia</td>
<td>73</td>
<td>16</td>
<td>44</td>
<td>133</td>
<td>4%</td>
</tr>
<tr>
<td>Wardak</td>
<td>373</td>
<td>112</td>
<td>222</td>
<td>707</td>
<td>23%</td>
</tr>
<tr>
<td><strong>Total</strong></td>
<td>1,314</td>
<td>724</td>
<td>1,007</td>
<td>3,045</td>
<td>100%</td>
</tr>
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</table>
C Testing “As-if” Randomization: Individual Level
<table>
<thead>
<tr>
<th>Variables</th>
<th>Control Mean</th>
<th>Control</th>
<th>Aid</th>
<th>Control</th>
<th>IA</th>
<th>Control</th>
<th>TA</th>
<th>IA</th>
<th>TA</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>Coefficient</td>
<td>p value</td>
<td>Coefficient</td>
<td>p value</td>
<td>Coefficient</td>
<td>p value</td>
<td>Coefficient</td>
<td>p value</td>
</tr>
<tr>
<td>Age (log)</td>
<td>3.526</td>
<td>0.289</td>
<td>0.152</td>
<td>0.295</td>
<td>0.204</td>
<td>0.188</td>
<td>0.361</td>
<td>-0.108</td>
<td>0.553</td>
</tr>
<tr>
<td>Pashtun</td>
<td>0.811</td>
<td>-0.099</td>
<td>0.640</td>
<td>-0.151</td>
<td>0.542</td>
<td>-0.138</td>
<td>0.591</td>
<td>-0.021</td>
<td>0.940</td>
</tr>
<tr>
<td>EmployedFull</td>
<td>0.814</td>
<td>-0.122</td>
<td>0.416</td>
<td>-0.231</td>
<td>0.184</td>
<td>-0.020</td>
<td>0.287</td>
<td>0.233</td>
<td>0.222</td>
</tr>
<tr>
<td>Income Level (log)</td>
<td>1.637</td>
<td>-0.150</td>
<td>0.078</td>
<td>-0.155</td>
<td>0.133</td>
<td>-0.068</td>
<td>0.538</td>
<td>-0.063</td>
<td>0.668</td>
</tr>
<tr>
<td>Hours of electricity</td>
<td>4.885</td>
<td>-0.030</td>
<td>0.303</td>
<td>-0.470</td>
<td>0.213</td>
<td>-0.033</td>
<td>0.318</td>
<td>0.019</td>
<td>0.640</td>
</tr>
<tr>
<td>State education (years)</td>
<td>6.319</td>
<td>-0.015</td>
<td>0.313</td>
<td>0.000</td>
<td>0.969</td>
<td>-0.033</td>
<td>0.047</td>
<td>-0.035</td>
<td>0.017</td>
</tr>
<tr>
<td>Madrassa education (years)</td>
<td>0.970</td>
<td>0.037</td>
<td>0.067</td>
<td>0.037</td>
<td>0.090</td>
<td>0.030</td>
<td>0.215</td>
<td>0.010</td>
<td>0.664</td>
</tr>
<tr>
<td>Size of household (log)</td>
<td>2.350</td>
<td>0.301</td>
<td>0.092</td>
<td>0.288</td>
<td>0.191</td>
<td>0.439</td>
<td>0.019</td>
<td>-0.058</td>
<td>0.723</td>
</tr>
<tr>
<td>Killed (binary)</td>
<td>0.806</td>
<td>0.076</td>
<td>0.665</td>
<td>-0.355</td>
<td>0.201</td>
<td>0.286</td>
<td>0.203</td>
<td>0.847</td>
<td>0.010</td>
</tr>
<tr>
<td>Wounded (binary)</td>
<td>0.907</td>
<td>0.167</td>
<td>0.438</td>
<td>0.922</td>
<td>0.005</td>
<td>-0.401</td>
<td>0.142</td>
<td>-1.343</td>
<td>0.000</td>
</tr>
<tr>
<td>Property (binary)</td>
<td>0.732</td>
<td>0.407</td>
<td>0.027</td>
<td>0.860</td>
<td>0.003</td>
<td>-0.107</td>
<td>0.637</td>
<td>-0.982</td>
<td>0.001</td>
</tr>
<tr>
<td>Population (log)</td>
<td>8.913</td>
<td>-0.054</td>
<td>0.113</td>
<td>-0.034</td>
<td>0.488</td>
<td>-0.104</td>
<td>0.086</td>
<td>0.053</td>
<td>0.509</td>
</tr>
<tr>
<td>Elevation (log)</td>
<td>7.285</td>
<td>0.026</td>
<td>0.898</td>
<td>0.252</td>
<td>0.284</td>
<td>-0.456</td>
<td>0.113</td>
<td>-0.606</td>
<td>0.103</td>
</tr>
<tr>
<td>Prior insurgent violence (7 days)</td>
<td>1.156</td>
<td>0.051</td>
<td>0.252</td>
<td>0.014</td>
<td>0.777</td>
<td>0.115</td>
<td>0.074</td>
<td>0.007</td>
<td>0.901</td>
</tr>
<tr>
<td>Prior ISAF violence (7 days)</td>
<td>0.008</td>
<td>-0.106</td>
<td>0.582</td>
<td>0.065</td>
<td>0.768</td>
<td>-0.241</td>
<td>0.441</td>
<td>-0.428</td>
<td>0.214</td>
</tr>
<tr>
<td>Prior IED detonations (7 days)</td>
<td>0.069</td>
<td>0.275</td>
<td>0.237</td>
<td>0.214</td>
<td>0.599</td>
<td>0.379</td>
<td>0.031</td>
<td>0.607</td>
<td>0.024</td>
</tr>
<tr>
<td>ISAF-initiated event</td>
<td>0.208</td>
<td>0.296</td>
<td>0.206</td>
<td>0.234</td>
<td>0.369</td>
<td>0.337</td>
<td>0.267</td>
<td>-0.089</td>
<td>0.803</td>
</tr>
<tr>
<td>Village control</td>
<td>2.518</td>
<td>-0.068</td>
<td>0.201</td>
<td>-0.457</td>
<td>0.464</td>
<td>-0.070</td>
<td>0.309</td>
<td>-0.103</td>
<td>0.099</td>
</tr>
<tr>
<td>Fighting season</td>
<td>0.543</td>
<td>0.230</td>
<td>0.167</td>
<td>0.329</td>
<td>0.097</td>
<td>0.236</td>
<td>0.318</td>
<td>-0.182</td>
<td>0.575</td>
</tr>
<tr>
<td>Bases within 3km² (log)</td>
<td>0.104</td>
<td>-0.006</td>
<td>0.871</td>
<td>0.009</td>
<td>0.866</td>
<td>-0.178</td>
<td>0.773</td>
<td>-0.068</td>
<td>0.338</td>
</tr>
<tr>
<td>Distance to district center (km, log)</td>
<td>0.726</td>
<td>-0.073</td>
<td>0.234</td>
<td>-0.102</td>
<td>0.134</td>
<td>-0.091</td>
<td>0.329</td>
<td>0.056</td>
<td>0.574</td>
</tr>
<tr>
<td>Time lag (incident → survey, log)</td>
<td>5.645</td>
<td>-0.096</td>
<td>0.338</td>
<td>-0.497</td>
<td>0.000</td>
<td>0.936</td>
<td>0.009</td>
<td>1.347</td>
<td>0.001</td>
</tr>
<tr>
<td>Outside district?</td>
<td>0.028</td>
<td>0.135</td>
<td>0.522</td>
<td>-0.230</td>
<td>0.491</td>
<td>0.627</td>
<td>0.056</td>
<td>1.019</td>
<td>0.088</td>
</tr>
<tr>
<td>Survey month</td>
<td>7.589</td>
<td>0.048</td>
<td>0.018</td>
<td>0.050</td>
<td>0.031</td>
<td>0.043</td>
<td>0.405</td>
<td>-0.047</td>
<td>0.403</td>
</tr>
<tr>
<td>Survey length (log)</td>
<td>3.442</td>
<td>0.317</td>
<td>0.315</td>
<td>-0.263</td>
<td>0.339</td>
<td>0.670</td>
<td>0.160</td>
<td>0.496</td>
<td>0.180</td>
</tr>
</tbody>
</table>

Note: Columns (2) and (3) report the coefficient and p-value on assignment to eligibility from a logistic regression of all covariates on the treatment indicator (eligible/not eligible). Standard errors clustered by unique village id. 3,045 respondents over October 2011-October 2013. Survey measurement occurred in four waves in 2013.
## D Checking “As-if” Random Assignment of Eligibility at the Village Level

Table 11: Baseline Summary Statistics and “As-if” Randomization Balance Test: Village-Level

<table>
<thead>
<tr>
<th>Baseline Covariate</th>
<th>Control Mean (1)</th>
<th>Treatment Difference (2)</th>
<th>p-value (3)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Civilian casualties (log)</td>
<td>−0.165</td>
<td>−0.023</td>
<td>0.346</td>
</tr>
<tr>
<td>Property (binary)</td>
<td>0.405</td>
<td>0.159</td>
<td>0.270</td>
</tr>
<tr>
<td>Population (log)</td>
<td>7.567</td>
<td>−0.077</td>
<td>0.092</td>
</tr>
<tr>
<td>Elevation (log)</td>
<td>7.127</td>
<td>−0.149</td>
<td>0.360</td>
</tr>
<tr>
<td>Distance to Kabul (km, log)</td>
<td>5.174</td>
<td>−0.083</td>
<td>0.144</td>
</tr>
<tr>
<td>Distance to nearest base (km, log)</td>
<td>7.704</td>
<td>0.012</td>
<td>0.897</td>
</tr>
<tr>
<td>Number of bases within 3km(^2) (log)</td>
<td>−1.527</td>
<td>0.098</td>
<td>0.022</td>
</tr>
<tr>
<td>Fighting season</td>
<td>0.597</td>
<td>−0.252</td>
<td>0.130</td>
</tr>
<tr>
<td>Prior insurgent violence (7 days)</td>
<td>0.874</td>
<td>0.039</td>
<td>0.359</td>
</tr>
<tr>
<td>Prior ISAF violence (7 days)</td>
<td>0.029</td>
<td>0.145</td>
<td>0.590</td>
</tr>
<tr>
<td>Prior IED detonations (7 days)</td>
<td>0.108</td>
<td>−0.159</td>
<td>0.364</td>
</tr>
<tr>
<td>Prior insurgent violence (30 days)</td>
<td>4.586</td>
<td>−0.025</td>
<td>0.038</td>
</tr>
<tr>
<td>Prior ISAF violence (30 days)</td>
<td>0.130</td>
<td>0.150</td>
<td>0.255</td>
</tr>
<tr>
<td>Prior IED detonations (30 days)</td>
<td>0.498</td>
<td>−0.096</td>
<td>0.256</td>
</tr>
</tbody>
</table>

*Note:* Columns (2) and (3) report the coefficient and p-value on assignment to eligibility from a logistic regression of all covariates on the treatment indicator (eligible/not eligible). Standard errors clustered by village. Missing population data are imputed at the median. N=1,061 incidents (592 eligible; 469 ineligible) over October 2011-October 2013.
Table 12: Baseline Summary Statistics and “As-if” Randomization Balance Test: Village-Level by Incident Type

<table>
<thead>
<tr>
<th>Baseline Covariate</th>
<th>Control Mean (1)</th>
<th>Treatment Difference (2)</th>
<th>p-value (3)</th>
</tr>
</thead>
<tbody>
<tr>
<td>ISAF-initiated</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>ISAF military operation</td>
<td>0.215</td>
<td>-0.030</td>
<td>0.947</td>
</tr>
<tr>
<td>Airstrike</td>
<td>0.142</td>
<td>-0.418</td>
<td>0.367</td>
</tr>
<tr>
<td>Accident</td>
<td>0.030</td>
<td>1.047</td>
<td>0.052</td>
</tr>
<tr>
<td>Escalation of force</td>
<td>0.008</td>
<td>0.742</td>
<td>0.258</td>
</tr>
<tr>
<td>Taliban-initiated</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Insurgent military operation</td>
<td>0.043</td>
<td>0.741</td>
<td>0.133</td>
</tr>
<tr>
<td>Insurgent indirect fire</td>
<td>0.062</td>
<td>0.319</td>
<td>0.516</td>
</tr>
<tr>
<td>Suicide bombing</td>
<td>0.036</td>
<td>0.571</td>
<td>0.261</td>
</tr>
<tr>
<td>Improvised explosive device</td>
<td>0.275</td>
<td>-0.516</td>
<td>0.253</td>
</tr>
<tr>
<td>Unclear Responsibility</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Crossfire</td>
<td>0.173</td>
<td>-1.062</td>
<td>0.023</td>
</tr>
</tbody>
</table>

*Note:* Columns (2) and (3) report the coefficient and p-value on assignment to eligibility from a logistic regression of all covariates on the treatment indicator (eligible/not eligible). Standard errors clustered by village. N=1,061 incidents (592 eligible; 469 ineligible) over October 2011-October 2013. ISAF indirect fire is the referent category.
E Within-Group Comparison
Table 13: Mechanisms: Within-Group Comparison

<table>
<thead>
<tr>
<th>Opportunity Cost</th>
<th>Socio-Psychological Mechanisms</th>
<th>Hearts and Minds</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Current Income</td>
<td>Future Income</td>
</tr>
<tr>
<td>Treatment</td>
<td>Model 1</td>
<td>Model 2</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>0.149</td>
<td>0.193</td>
</tr>
<tr>
<td></td>
<td>(0.141)</td>
<td>(0.155)</td>
</tr>
<tr>
<td>Wald $\chi^2$</td>
<td>99.06***</td>
<td>131.95***</td>
</tr>
<tr>
<td>Log pseudolikelihood</td>
<td>-2736.61</td>
<td>-2735.34</td>
</tr>
<tr>
<td></td>
<td>0.02</td>
<td>0.02</td>
</tr>
<tr>
<td>Villages</td>
<td>200</td>
<td>193</td>
</tr>
<tr>
<td>Wald $\chi^2$</td>
<td>43.16**</td>
<td>76.85***</td>
</tr>
<tr>
<td>Log pseudolikelihood</td>
<td>-616.53</td>
<td>-618.12</td>
</tr>
<tr>
<td></td>
<td>0.03</td>
<td>0.04</td>
</tr>
<tr>
<td>Respondents</td>
<td>470</td>
<td>458</td>
</tr>
<tr>
<td>Wald $\chi^2$</td>
<td>199.25***</td>
<td>647.11***</td>
</tr>
<tr>
<td>Log pseudolikelihood</td>
<td>-2032.63</td>
<td>-2017.98</td>
</tr>
<tr>
<td></td>
<td>0.03</td>
<td>0.03</td>
</tr>
<tr>
<td>Respondents</td>
<td>1523</td>
<td>1472</td>
</tr>
<tr>
<td>Villages</td>
<td>98</td>
<td>92</td>
</tr>
</tbody>
</table>

Note: All covariates in the model with robust standard errors clustered on unique village id. † Significant at 10% *Significant at 5% **Significant at 1% ***Significant at .01%.